Climate of the Past Discussions

Clim. Past Discuss., community comment CC1
https://doi.org/10.5194/cp-2021-83-CC1, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

# Comment on cp-2021-83

Hemmo Abels

Community comment on "Terrestrial carbon isotope stratigraphy and mammal turnover during post-PETM hyperthermals in the Bighorn Basin, Wyoming, USA" by Sarah J. Widlansky et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-83-CC1, 2021

Herewith my review of Widlanski et al. submitted to Climate of the Past.

The paper brings an important subject interesting for a relatively wide audience of paleontologist and paleoclimate workers, both terrestrial and marine. The early Eocene hyperthermals are well studied while little data are present concerning their impact on the continents both geochemically, as environmentally and faunally. The paper is a continuation of earlier work published in Climate of the Past (Chew 2015) where an attempt was made to correlate faunal records of the central Bighorn basin, Wyoming, to a carbon isotope record from more northern parts of the same basin. The faunal analysis of that previous paper is now supplemented by a series of carbon isotope sections much closer or directly at the mammal sites in the central parts of the basin and with that potentially making the correlations between fauna and isotope changes more straightforward. The authors face the problem that, in the central parts of the basin, outcrops and so carbon isotope samples and mammal sites are scattered over large areas due to low topography. A nearly 30-year old composite meter level system is used since then to anchor different data sets to the meter-level system over this entire area, while structural dips are reported to occassionally change within the area without much notice. The composite level stratigraphy (BCM named here) has thus a relative large uncertainty and mammal sites and carbon isotope records may erroneously overlap or be separated in stratigraphy while in reality that is not the case. Therefore, the current authors additionally use absolute dGPS levels within their sections to also tie sections together. Subsequently, a composite isotope stratigraphy is constructed where the mammal analysis of Chew 2015 is placed along and mammal and isotope changes are discussed and placed in a more global perspective.

Correct stratigraphy is thus the key to produce solid results in this paper as in many other papers. The difficult outcrops make that the stratigraphic approach and data should be produced and communicated with even more care. How do isotope and mammal finds stack together into stratigraphy? The authors must have spend considerable time to produce the current work, however, the written work under review here did not take away my concerns about the mammal and isotope composite stratigraphy produced. I think this is partly because the workflow and data are not sufficiently backed-up with maps and data in the paper or supplement. There are large stratigraphic thickness changes between steps within the workflow of up to 30% for individual sections. How is that possible? One would expect that dGPS data or Jacob-Staff data should be able to work at lower uncertainty? Were structural dips of the layering not determined sufficiently? The isotope

records are at places of relatively high resolution, although there is no explanation of sampling strategy. Where all pedogenic nodules sampled? The carbon isotope records show some very good CIEs, but also some interval difficult to interpret. The resulting composite isotope record however is far from easy to interpret. Due to the stacking of series that do not have very similar carbon isotope results, the composite stratigraphy is blurry. Relating CIE1 to ETM2 and CIE2 to H2 seems logic with the additional stratigraphic constraints, but I wonder whether correlation to I1-I2 would be possible? CIE sizes have so-far been showing to be very stable, such as even similar PETM CIE body d13Cped values in the northern basin as in the southern basin. The paper has long discussion on what could have caused different CIE sizes within the basin, but while I am still doubting the stratigraphy, interpretations and composite stratigraphy, it is difficult to believe the remainder of the paper.

The seeming lack of documentation of stratigraphic data within the paper and appendix can be solved. Mammal sites, carbon isotope sections, and magnetostratigraphic sections should be plotted on a map(s) that is(are) detailed enough to see how different site (may) relate. Figure 1 is small and the background of it vague. In the supplement or paper, more detailed maps could help to show such relations. In these map figures, it is necessary to show topographic lines, the key mammal sites, the isotope sections and the structural dip of layering. This allows to understand how these result in stratigraphy. Besides, photographs of the sections sampled should be added such to see the outcrops and sampling trajectories. Are these far away even within single sections? It seems so, if I see the North Fork section on Figure 1 and try to find it using Google Earth, I only see very low hills with 5-10 meters of stratigraphy. What is the uncertainty in thickness of single sections when measured in the field by Jacob Staff? There misses a table with the dGPS data that are used and including GPS locations of the tops and bases of the (sub-)sections.

The Results section misses a detailed description of stratigraphy and stratigraphic results. The dGPS stratigraphies differ quite a bit from the BCM levels supplemented by Jacob Staff measurements. In Figure 4, both stratigraphies are provided, there is a description of the isotope stratigraphies of both methods to come to stratigraphy, but there lacks a description of the stratigraphic impact itself between the two methods. Basin Draw is shortened by 33% it seems in the dGPS method, Kraus Flats is shortened by 20%, while North Fork is expaned by 33%. One would expect that Jacob-Staff data are not that inaccurate? Why would this happen? Another argument could be that the approximate levels of the dGPS are indeed reliable, so still lining up the supposed ETM2 level, but for thicknesses the Jacob Staff data are quite good? What about structural dips of the bedding? In relation, there misses an impact on mammal site stratigraphy of the two stratigraphic compilations made. Part of these results are presented in the discussion, but these should really be in the Results section. Missing is a table with mammal site content in the new stratigraphic order listing the most important mammal species through stratigraphy, it must be different with these new approaches from Chew, 2015? Grey bars indicate BioB, B1 and B2, but the mammal sites where this is based upon should provided.

In addition, the authors do not include the notion of precession forcing of stratigraphy on the floodplain stratigraphy of the Bighorn Basin. Abdul Aziz et al. 2008 suggest precession forcing in what seems one of the currently studied sections, Red Butte. 20 kyr would correspond roughly to 8 meters of section according to the work of Abdul Aziz. For more northern sites, such numbers have been documented since. Also, Abels et al. 2016 discuss a ca 35m cycle in the carbon isotope record studied that would be in line with the precession forcing of the smaller scale cycles. Both these numbers give a fairly good control on sedimentation rates and from that point of view the isotope records could be analysed and interpreted. This is not used nor discussed. Line 353-355 are a little too easily stating that there are "overall differences in sediment thickness across the basin".

Yes, there are clearly differences, and also hiatuses particularly at the basin margins, but sedimentation rates in the basin centres have been shown to be relatively constant. In the centres, sed rates depend on subsidence rates that controlled net accommodation space generation. One would not expect rapid changes in sedimentation rates at above 10^4 yr and below 10^6 yr time scales as are suggested by the correlation of CIE3 to I1. There is discussion on this in 5.2 and 5.3, but it is quite unstructured now also discussing the same things in different sections (line 360-366 in 5.3 and similar discussion with different arguments in 5.2). As a minimum, the authors should refer to the work on astronomical forcing of these series and why they think not to use these arguments in building stratigraphic framework for their series and for the interpretation of their carbon isotope records. The authors claim lower sedimentation rates by observing lower spacing between CIE1 and CIE2 in their series. If these are correlated to CIEs in marine records, it also imports 100-kyr eccentricity age control on the Fifteenmile Creek series that could be used to interpret the remainder of the series.

The second half of section 5.3 does not include the possibility that there may be remaining uncertainties in the study. There are at least some serious stratigraphic doubts to be placed along the composite stratigraphy with the dGPS stratigraphy so largely different from the Jacob's Staffed thicknesses. If the thicknesses of individual section can be different up to 30% between methods because of uncertainties in structural dip changing through the area and because of very low topography of outcrops, correlations between sections must contain serious uncertainties. The composite isotope record is far from clean likely because of these uncertainties. Thicknesses of intervals and sections are different also in the composite. How can the authors be certain about the composite? Should the composite actually be made or is it better to discuss the results from the individual sections as those are uncertain enough in themselves?

What could improve the carbon isotope data analysis is nailing down better the baseline carbon isotope values. This is around -10 per mille it seems. If a vertical line is placed at the -10 permille d13C value in all plots, it much better allows to identify excursions, also when records are relatively short.

The BioB, B1 and B2 'events' are used without much discussion on their reliability. There should be minimally some discussion on these data and interpretations, such as why sample size and different types of diversity go hand-in-hand in the records of Chew 2015. Plotting the average sample size plot on top of the diversity plots in the Figure 4 of Chew, 2015, lets them merge, which despite the statistics places some serious doubts along the B1 and B2 events. And, does the new stratigraphy not impact those previous results warranting a new analysis?

- the title is too general now and should be refined to cover the content of the paper. The paper is not improving carbon isotope stratigraphy of post-PETM time intervals. A reference to 'terrestrial' or 'continental' and the geographic location is needed.
- in Figure 1. Would there be the possibility to both show topographic lines and structural dip of bedding? It would give some more notion of the stratigraphic transects / sections measured
- in Figure 1. It would be good to add some trails / tracks / roads to provide the reader the opportunity to better orient
- in Figure 1. Legend is missing the light brown shading, from the caption that seems to be 'unshaded' Quaternary?
- section 3.1 is unnecessary, items can be discussed when applicable in the results interpretation or discussion sections
- Line 196. It is not clear what was the strategy to come to 'sampling sites'. Where all nodules encountered sampled, or was a certain stratigraphic resolution chosen?
- Line 212 refers to pedogenic carbonate sampling again, right? Should it be in 3.2 then?
- Line 276. I would not regard the whole range between -8 and -12 per mille d13C as

background. There is clearly noise on these records, but not 4 per mille.
- Line 275-280. Plotting against Trimble positions suggests there is no structural dip in the area?
- Figure 3. 'colors represent outcrop color' is not a very clear statement, I suppose you mean after removing weathered surface material? Or you mean color of the weathered surface?
- Figure 1 and 3. How does the Red Butte section correlate to the Red butte section of Abdul Aziz et al. 2008 in Geology? As the same name is used here, and it seems to be in the same area, it should be indicated how these relate, or another name should be used for the section. There are workers around who can help identify the location of that previous Red Butte section.
- Figure 3: why is the level at 45m in North Fork not identified as a CIE? And 35m and above in Red Butte? It is clearly not baseline of -10 per mille
- Figure 3: in Basin Draw D1459 is given as 29 meters from D1460, but in the panel c of Basin Draw it is no more than 15m without visible uncertainty, how is that possible? The same occurs with D1822 and D1204top, where a separation is given of 18m while in the panel c it is no more than 4 meters
- Figure 3: it would be good to label the levels in panels c with the 1,2,3,4 numbers
- Figure 3: Basal/D1350 has only few dGPS points while it is 50 m thick, why are there so few points and is the whole section not covered by calibration points?
- Figure 4: for simplicity, delete -10 and -14 from all x-axis labels, it makes it easier to read
- Figure 4: the panel b misses biostratigraphic information, while these would just relate to the isotope data as they do in panel a, right?
- Line 330 is the PETM really an option? it seems a bit unnecessary to exclude the PETM as an option in the discussion I suppose? The PETM should be so much lower in stratigraphy? The early Eocene has a good bunch of CIEs to correlate to other than the PETM. Why are these Fifteenmile Creek CIEs not I1 and I2, their size would make that plausible at least.
- Figure 5: magnetostratigraphy should be plotted along Fifteenmile Creek Composition Section
- the distance between the centre of CIE2 and CIE3 in FCCSection, has decreased to just over 20m while nearly 40 m in North Fork section, the only section where the interval from CIE2 to CIE3 is measured in a continuous manner, how is the possible? It has a very big impact on the interpretation of the CIEs
- Figure 5: it would be good to place the marine isotope stratigraphy to the far left as a baseline, including labels for hyperthermals, the MCP record next it, and the FCCS to the right.
- Lines 360 to 366 is doubling of discussion with the 5.2 section.
- Line 429-430: this sentence is written as if it is needed to have every CIE requires faunal change. I would be happy to believe that though I doubt whether we have much at hand at this stage to confirm anything close to that.