Climate of the Past Discussions

Clim. Past Discuss., author comment AC1
https://doi.org/10.5194/cp-2021-83-AC1, 2021

Reply on CC1

Sarah J. Widlansky et al.

Author 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-AC1, 2021

Thank you for your comment and for taking the time to review this work. We will incorporate your feedback in the final version. The next version will include an additional supplemental table of GPS coordinates as well as more detailed field maps showing the sampling locations and places where we traced beds between subsections. We agree that adding these maps will help clarify some of the uncertainty in the stratigraphy and strengthen our argument for choosing to correlate the sections as we did. When measuring sections with a Jacob Staff we usually assumed a dip of zero degrees. In local areas where strike and dip could be measured, we found that dip was very close to zero. When dip is close to zero, it is extremely difficult to accurately calculate the direction of strike. Thus, we do not have measurements of structural dip that can be placed on the maps. Most variation in dip in the Fifteenmile area is minor and localized and does not reflect broader structural patterns. If local differences in dip along the section transect were detected (by shooting a horizontal line between exposures of a marker bed along the transect), adjustments to dip were made on the Jacob Staff's clinometer. It should be noted that in most cases, section thickness was measured on slopes of 30 degrees or more, where beds were well exposed, and small differences in dip would have had only a small effect on overall section thickness. These small, local subsections were tied together with bed traces, sometimes over long distances.

We appreciate that you pointed out the need to identify other potential correlations for the CIEs. While the magnitudes are more consistent with the lower CIEs being I1 and I2, this isn't supported by the biostratigraphy – this interval occurs immediately above Biohorizon B and there is no evidence for a significant unconformity in the region that would prevent the preservation of ETM2 and H2.

There are differences between the thicknesses measured with a Jacob staff and using the differential GPS, but a better way to compare them would be to compare the "pure" Jacob staff method, where all samples are shown according to their measured local meter level (Figure 3), and the differential GPS method (lower panel in Figure 4) where the relative spacing of sample sites is maintained while adjusting for dGPS tie points. The BCM levels (upper panel in Figure 4) offer an alternative way to transform the original measurements where the relative spacing is again maintained, but the levels are instead adjusted using the measured stratigraphic levels for fossil localities from Bown et al. (1994). Both using

dGPS data and using the Bown et al. (1994) measurements provide a framework for correlating between the five local sections we measured, but they each have their own uncertainty, as described in the text. When you compare the thicknesses measured in our local sections separately to each method, the differences are reduced significantly. We will incorporate more of this discussion into the final text.

We also appreciate your suggestion of incorporating precession forcing in our discussion of stratigraphic constraints and sedimentation rates as well as the nearby Red Butte section from Aziz et al. (2008). We agree that including these constraints will help us strengthen our discussion of basin deposition and comparisons to McCullough Peaks. We will also be sure to more clearly highlight places where significant uncertainty still remains.

We feel that re-doing the faunal analyses of Chew (2015), given this new chemostratigraphy, is outside the scope of the current study, however we do identify it as a promising area for future work. However, we will expand the discussion on the limits of interpreting the previous faunal work in the context of the new chemostratigraphy. A significant amount of additional field work is necessary to incorporate all of the > 400 fossil localities used by Chew (2015) into a detailed chemostratigraphy like what we provide for the 18 fossil localities in our study. An important note though – the basic premise of the Chew (2015) paper holds true: she proposed that two intervals of observed faunal turnover corresponded to the ETM2 and H2 CIEs, and our results generally support this (with some uncertainties that are discussed in the text). A more precise assessment of the faunal changes that took place during these hyperthermals can be revisited in a later study when the results presented here can be expanded to include the many additional fossil localities in the area that have yet to be directly correlated to a nearby chemostratigraphy.

Lastly, we appreciate the line-specific recommendations and suggestions for making the figures easier to interpret. We will address these specifically as we make the changes in the final version.