

We are glad to read the reviewer supports publication of our work. Below we reply on the comments made.

1) The authors twice cite Schubert and Jahren (2012) (p1863, line 14; and p 1872, line 5), but this publication is not included in their reference list.

The citation will be added to the reference list in the revised version of the manuscript.

2) The context suggests that the authors may have meant to have cited Schubert and Jahren (2013, Nature Communications, "Reconciliation of marine and terrestrial carbon isotope excursions based on changing atmospheric CO<sub>2</sub> levels," doi: 10.1038/ncomms2659) with the citation of Schubert and Jahren (2012) on page 1863: that paper first showed how the pCO<sub>2</sub> effect can reconcile differences in the magnitudes between marine and terrestrial CIEs, and the analysis was applied to the PETM specifically.

The correct citations will be used in the revised version of the manuscript.

3)  $\Delta p$  increases with increasing pCO<sub>2</sub> (Schubert and Jahren 2012), but the text repeatedly presents this backwards (as a decline in  $\Delta p$  with increasing pCO<sub>2</sub>) (see: p1872, lines 5-7, 11-13; p1873, lines 13-15; p1875 lines 1-3).

The text will be corrected.

A major claim by the authors is that, "In conclusion, our model results indicate that the reduction [should be increase] of carbon isotope fractionation in plants cannot explain the observed difference in terrestrial-marine CIE scaling for the PETM relative to the subsequent hyperthermals." However, this conclusion is based on an incorrect assumption of static background pCO<sub>2</sub>. Here I show that the pCO<sub>2</sub> effect can reconcile the offsets between the paleosol carbonate and marine records for all five CIEs, including the PETM, and that the pCO<sub>2</sub> levels required to do so match, in both trend and absolute value, the other pCO<sub>2</sub> proxy estimates available for this time period.

We appreciate the reviewer's perspective and acknowledge that there are scenarios involving changing background pCO<sub>2</sub> that can reconcile the CIE magnitudes. As stated in the original manuscript, our analysis was predicated on the simplifying assumption that background pCO<sub>2</sub> remained constant across the study interval. Clearly this may not be the case (in fact it seems unlikely) but it was our assertion that the available proxy data did not permit any robust, quantitative specification of changing background CO<sub>2</sub> levels (e.g., Fig. 2 plotted in the review). Without this, the model for photosynthetic fractionation is flexible enough that one could come up with combinations of background values and hyperthermal pCO<sub>2</sub> change that would reproduce the observed patterns, but we felt this was somewhat *ad hoc*. We acknowledge that it is a possibility, though, and as the reviewer suggests it's perhaps a reasonable interpretation that deserves further consideration. Based on the encouragement of both reviewers we will present a further analysis that considers changing background pCO<sub>2</sub> as one potential explanation, in addition to contrasting regional climatic changes, for the difference in CIE scaling between the PETM and the other hyperthermals.

The authors recognize that background pCO<sub>2</sub> levels were likely not static throughout the study interval (p1863, 19-22), and changing pCO<sub>2</sub> across this interval is supported by recent proxy complications (e.g., Beerling and Royer 2011, Nature Geoscience, vol 4, their figure copied below). The δ18O record (e.g., Zachos et al. 2008, Nature, doi:10.1038/nature06588) also supports a general warming trend across the first ~5 million years of the Eocene (which spans the interval with the CIEs studied here), and is also at odds with an assumption of static pCO<sub>2</sub>. Yet, the authors assume that background pCO<sub>2</sub> (pCO<sub>2,i</sub>) was in fact static (p1863, 22). Static background pCO<sub>2</sub> is a fundamental assumption within Eq. (2) of their manuscript and without static pCO<sub>2,i</sub>, the calculated values for ΔpCO<sub>2</sub> are difficult to reconcile. I illustrate how changing pCO<sub>2,i</sub> yields conflicting results on the calculated values for ΔpCO<sub>2,I2</sub> and ΔpCO<sub>2,H2</sub> using pCO<sub>2,i</sub> = 700 and 500 ppm for I2 and H2, respectively, and maintaining ΔpCO<sub>2,PETM</sub> constant at 1000 ppm (all values within the ranges explored in this manuscript).

Equation (2) is given as:

$$\Delta pCO_{2,h} = pCO_{2,i} + \Delta pCO_{2,PETM} \times CIE_{benth,h} / CIE_{benth,PETM} \quad (2)$$

Using the values above, for I2:

$$\Delta pCO_{2,I2} = 700 + 1000 \times 0.73 / 3.38$$

$$\Delta pCO_{2,I2} = 916 \text{ ppm}$$

and for H2:

$$\Delta pCO_{2,H2} = 500 + 1000 \times 0.97 / 3.38$$

$$\Delta pCO_{2,H2} = 787 \text{ ppm}$$

This shows that if pCO<sub>2,i</sub> is not held constant (in this case pCO<sub>2,i</sub> increased from 500 to 700 ppm from H2 to I2), then one can calculate a bigger ΔpCO<sub>2</sub> value for I2 (916 ppm) than H2 (787 ppm), despite a smaller CIE recorded for I2 (0.73‰) than for H2 (0.97‰). This is a problem (provided one assumes a common forcing among the events) and requires strong justification for why pCO<sub>2,i</sub> should be kept constant. A reference or description of from where Eq. (2) is derived is also needed.

We thank the reviewer for carefully checking these equations and pointing this out. In fact equation 2 as written in the manuscript gives the peak pCO<sub>2</sub> value during each hyperthermal event, not the change in pCO<sub>2</sub> as suggested in the manuscript. We will correct this error in the revised version.

Constant pCO<sub>2,i</sub> also causes significant issues when using the pCO<sub>2</sub> effect to reconcile the marine and terrestrial CIEs. The authors state on page 1873 (lines 12-15), “In conclusion, our model results indicate that the reduction [increase] of carbon isotope fractionation in plants cannot explain the observed difference in terrestrial-marine CIE scaling for the PETM relative to the subsequent hyperthermals.” Yet, if pCO<sub>2,i</sub> is allowed to change, then the pCO<sub>2</sub> effect can reconcile the offsets between the paleosol carbonate and marine records for all five CIEs (including the PETM). Using the data provided in Table 1, I calculate that the pCO<sub>2</sub> effect not only reconciles the offsets reported here between CIE<sub>benth</sub> and

CIEpaleosol, but the reconstructed  $pCO_{2,i}$  values I calculate match previous proxy estimates and trends across this time period, which suggest a  $pCO_2$  minimum between the PETM and ETM2, followed by an increase in  $pCO_2$  across the 4 events following the PETM (see Figures 1-2, below).

We had conducted a similar set of calculations and as described above acknowledge that there are scenarios of background  $pCO_2$  and during-event  $pCO_2$  change that can reproduce the observed pattern of CIE application across all events. The solution space looks somewhat different than reported in the review if one uses the correct form of equation 2 (our fault, see above), and as also noted above we don't necessarily share the opinion that the proxy record (of  $pCO_2$ ) provides a clear indication of pattern against which the results can be tested. Nonetheless, we value the reviewer's perspective and agree that it would be reasonable to present this as one possible interpretation. This will lead us to temper the statement quoted in the reviewer's comment, which was predicated on the conditions of the analysis presented in the paper.

Similar to the authors' work, my above calculations assume no changes in environmental conditions (e.g., climate, plant communities, productivity) and attribute the entire offset between CIEpaleosol and CIEbenthic as being caused by the  $pCO_2$  effect. However, as the authors correctly identify, this assumption may not be valid, especially when looking at individual sites, as is the case here. For example, the authors cite evidence for drying in their study region during the PETM (and they find little precipitation change for the other CIEs). Drying could dampen the magnitude of the CIE at the PETM at this site through diminished carbon isotope fractionation and reduced productivity.

Evidence for drying and a dampening of the CIE at the PETM is also provided by the nalkane CIE = 4.2‰ for this site. This value is smaller than most of the other reported nalkane CIEs for the PETM (median = 5.0‰, McNerney and Wing, 2011), which is also consistent with drying. In fact, using a larger CIE (e.g., 5.0‰ versus 4.2‰) gives a better match across lower  $pCO_{2,i}$  between the model estimated terrestrial CIE and the measured value than is shown in Fig. 5A of the text. All else being equal, increasing the absolute magnitude of the terrestrial CIE results in a lower  $pCO_{2,i}$ .

We agree with the reviewer that there are a number of lines of evidence for changes in climate, and hydroclimate specifically, within the Bighorn Basin records (though interpretation of the  $d_{13}C$  records in this context has proven challenging, see the conflicting interpretations of (Bowen et al., 2004) and many subsequent paleo-precipitation proxy studies (e.g. Kraus et al. 2013). Our analysis is framed around the time-series of CIEs for all events recorded in the Bighorn Basin, however, and the potential global-scale vs. local-scale controls on the pattern of the excursions, so we will maintain our reference to the data from this basin in our revised analysis.

I am somewhat confused, however, to how Figure 5 of the text is presented. Panel A shows how well the  $pCO_2$  effect reconciles a 4.2‰ magnitude terrestrial CIE and panel B shows how well it reconciles a 9.5‰ magnitude terrestrial CIE. It is noted that for panel B, that the  $pCO_2$  effect cannot reconcile this large of a CIE within the explored parameter space (bottom of p1872); however, the  $pCO_2$  effect can reconcile this 9.5‰ CIE with the 3.38‰ benthic CIE outside of this parameter space (e.g.,  $pCO_{2,i}$  = 267 to 372 ppm and  $\Delta pCO_2$  = 937 to 3145 ppm). As shown in Figure 2 (above),  $pCO_{2,i}$  values in this range

are not inconsistent with previous proxy estimates for background pCO<sub>2</sub> in the late Paleocene. I do not understand why these plots are limited to pCO<sub>2,i</sub> > 500 ppmv? The Y-axis should be extended to include lower pCO<sub>2,i</sub>.

Although there are mathematical solutions that will reconcile the sequence of paleosol carbonate CIEs within the range of values noted by the reviewer, they produce plant tissue CIE values that do not match the observed values from the BHB, see response to the next comment.

Further, it is not clear to me what the authors are trying to demonstrate with the comparison of Figure 5A and 5B. On page 1873, lines 1-5, the authors imply that pCO<sub>2,i</sub> and ΔpCO<sub>2</sub> should match across Fig. 5A and 5B. However, these cannot match nor should they match, as widely different CIE values are used in both (4.2‰ from n-alkanes versus 9.5‰ from extrapolated paleosol values). Schubert and Jahren (2013) pointed out that “The very large CIE measured in paleosol carbonate (average = -5.5‰, median = -6.3‰) may reflect a combination of the enhanced fractionation by plants under high pCO<sub>2</sub>, diffusion of increased pCO<sub>2</sub> levels into the soil and increased productivity.” Such a claim could be made here as well that the paleosol carbonate CIE reflects the pCO<sub>2</sub> effect plus additional effects from a change in productivity. Thus, the paleosol CIE = 5.9‰, while the n-alkane CIE = 4.2‰ for the same site.

Indeed the point we are trying to make with comparison of the two figures is that one cannot reconcile both the observed PETM plant (alkane) CIE and the observed PETM paleosol carbonate CIE (in the context of the scaling relationship across events) with reference only to changes in photosynthetic carbon isotope discrimination (via the Schubert relationship and in the context of the analysis presented). In other words, something else has to be changing among events, which is the primary conclusion of the analysis. We focus mainly on the potential role of hydroclimate in the original manuscript. We propose to broaden this somewhat to consider the potential contribution of changing background pCO<sub>2</sub>, as encouraged by the reviewers and discussed above, in the revised manuscript. Either way, the primary result here is to establish a framework for the various factors which ought to be considered in trying to integrate the data from both organic and inorganic systems across these events, to point out that the relevant ‘solution space’ includes changes in multiple systems and/or over multiple timescales, and to give some indication of what types of data would provide further and more powerful constraints on the problem (something which we propose to emphasize more strongly in the revision).

Finally, we will adopt the minor technical comments in our revision.

Bowen, G.J., Beerling, D.J., Koch, P.L., Zachos, J.C., Quattlebaum, T., 2004. A humid climate state during the Palaeocene-Eocene thermal maximum. *Nature*, 432: 495-499.

Kraus, M. J., McInerney, F. A., Wing, S. L., Secord, R., Baczynski, A. A., and Bloch, J. I.: Paleohydrologic response to continental warming during the Paleocene–Eocene Thermal Maximum, Bighorn Basin, Wyoming, *Palaeogeogr. Palaeoclimatol.*, 370, 196–208, 2013.