Reply on RC1

Rebekah A. Stein et al.

Author comment on "Climate and ecology in the Rocky Mountain interior after the early Eocene Climatic Optimum" by Rebekah A. Stein et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-45-AC1, 2021

General Comments:

The study is interesting and addresses important scientific questions surrounding global versus regional environmental responses to past warm intervals and intervals of abrupt climate change. It is certainly relevant to and deserving of publication in Climate of the Past. This work contributes to the greater understanding of the North American terrestrial environmental response to carbon emissions, a timely topic when observations of modern shifts in the hydrologic cycle are considered. Further, it provides (1) new early Eocene proxy-based quantitative environmental constraints, and (2) new age constraints in a geologically significant area. The authors do a good job introducing the geologic setting and explaining their approach for environmental reconstructions and geochronology. The paper is also fairly well-structured and laid out in general. The explanation of various weathering indices was particularly well-written and concise. However, generally, the manuscript is only moderately well written, and would improve greatly with grammatical and sentence structure revision. Some of the arguments leading to main conclusions about atmospheric carbon sources in the early Eocene and Paleogene are weak or non-existent.

We thank the anonymous reviewer for taking the time to review this manuscript and for the positive and constructive feedback.

Generally, the majority of my critique involves the following:

- The study motivation and significance could be more clearly and effectively communicated. I give specific details below.

We have added sentences to emphasize the motivation and significance throughout, including the suggested locations.

- Information on the approach to analyzing for bulk geochemistry is opaque and needs to be expanded.

We appreciate this suggestion. We have provided the information that we were able to garner regarding error and analysis from ALS laboratories. We do know that error was calculated based on the maximum error of duplicate and standard tolerance, but that is the extent of publicly-available information.

- Sentence structure and grammar needs to be improved. I aimed to give thorough and specific recommendations.

We appreciate these suggestions and have made modifications according to specific feedback below.

- Propagation of uncertainty and specifics on reported precision needs to be addressed, or at least better defined throughout the manuscript, with respect to paleo reconstructions.
- Discussion of environmental results and structure of discussion could improve.
- Some arguments leading to major conclusions are incomplete.

As stated above, this article is suitable for Climate of the Past and will be of interest to readers as it provides new paleoenvironmental constraints on an important interval. Based on the above critique, and the lack of thorough revision prior to submission, I recommend this article is reconsidered following major revisions.

Specific Comments:

-The study motivation could be improved or expanded upon. For example, the authors state how this warm interval may prove useful as an analogue for modern climate change for several reasons, but give the reader a weak connection between modern and past warming at that location using inconsistent plant fossils and hydrologic cycle comparisons. The reader is left wondering: "Why was it wetter then even though it was warmer and it's drying out now?", but the study doesn't specifically address this question.

We have added text to clarify the connection, rather than asking audience to read between the lines, e.g., lines 61-82 "From the Paleocene to early Eocene, it has been inferred that there were extensive temperate forests dispersed throughout North America (Smith et al., 2012; Breedlovestrout et al., 2013; Greenwood et al., 2016; West et al., 2020) up to high latitudes 65 °N (Dillhoff et al., 2013). However, the nearby Bighorn Basin is inferred to have undergone aridification based on magnetic properties in paleosols (Maxbauer et al. 2016; Carmichael et al. 2017), and global climate models predict low and lower-middle latitude sites, including areas like central Utah to experience aridification due to changes in meridional vapor transport distribution (Pagani et al., 2006). As the planet warms, there is increasing concern about water availability and dry climates getting drier. For example, the North American Southwest, composed of a series of deserts and dry ecosystems, is at risk for having its already severe droughts increased in frequency and severity (Poore et al., 2005; Coats et al., 2015; Cheeseman 2016). Therefore, study of ancient climate and ecosystems in these hydrologically vulnerable areas can provide examples for what may happen to these ecosystems in the context of emerging climate and societal challenges."

The connection between understanding this particular environment/location at this specific time and its significance to modern change is vague (especially with respect to the concluding sentence of section 1.1). The authors could build a stronger argument for study significance by stating that their study fits in a greater framework of understanding the global versus regional responses to carbon emissions and subsequent climate change, particular with respect to a shifting hydrologic cycle (i.e., observations of modern shifts in N. America hydrologic cycle can be better understood if given paleo-context). Further, this region and the Cenozoic sediments it contains are well-studied. In the introduction, the manuscript would benefit from a more thorough explanation of the significance of this study with respect to previous work and understanding of the region. The authors do a good job of contextualizing this data in the discussion (section 5.1). However, this should

also be laid out as a study motivator in the introduction, not just the paleo analogue argument, in my opinion.

Thank you for this suggestion. We have clarified our motivations in the introduction, finishing section 1.1 "Observations of modern shifts in the North American Southwest hydroclimate can be better informed with a paleo-context, and as such, we focus on paleo-hydroclimate changes in this region, contextualized with similar regional studies from this time throughout the Rocky Mountain region (e.g., Leopold & MacGinitie 1972; Wing & Greenwood 1993; Greenwood & Wing 1995; Inglis et al., 2017; Murphey et al., 2017; Allen 2017a/b). This study fits in a greater framework for understanding global and regional responses of terrestrial climate, and more particularly, terrestrial hydroclimate, to carbon emissions."

-Line 45: Cite refences here which constrain the interval of warming you state. I recommend looking into Westerhold et al., 2018 or Cramwinckel et al., 2018.

Thank you for the suggestion, we have added both reference suggestions.

-Line 170: The elements analyzed should be listed in this section.

Added, thank you for the suggestion.

-Line 173: Please explain what you are using for "internal standards." Is this an in-house multi-element solution standard at ALS? Also, how is precision defined here? How is it determined? For example, is it determined using 2SE of long-term reproducibility in solution consistency standards? Or, perhaps, 1sd of multiple measurements of an individual sample across many analytical sessions? Generally, this section needs some more details for the ICP-informed reader.

Samples were analyzed in a commercial laboratory (ALS National Laboratories in Vancouver, BC) according to their proprietary methods. We do know that error was calculated based on the maximum error of duplicate and standard tolerance, but that is the extent of publicly-available information. This detail was added to section 3.2.4 (line 251-252).

-Line 206-208: I cannot make sense of this sentence. It needs revising. Certainly, consider removing the word "so" and/or state "U/Th is redox-sensitive" parenthetically rather than in commas.

Corrected, thank you.

-Line 315: You should refer readers here to the sedimentary geochemistry data which you make available.

We have added the data repository at the start of this section, thank you for this suggestion. Mendeley Data Repository, Doi: 10.17632/z6twpstz4r.3

-Line 337: How is the precision in temperature reported? Does this standard deviation you report consider the analytical uncertainty in bulk geochemistry used in the PWI calculation (i.e., 0.2 wt %)? Does it include calibration uncertainty in the equation which translates PWI to temperature? Is it simply based on temperature reproducibility (i.e., the standard deviation of multiple sample temperature values) with no propagation of analytical or calibration error? Sorry for all of the questions, but it is important to be transparent here. If PWI-temperature calibration uncertainty was not previously constrained, it may be best to give the reader an estimate of the fit of the calibration regression by providing an

R^2 value from Gallagher and Sheldon, 2013. If uncertainty is constrained in this relationship, please utilize it by propagating into the temperature uncertainty and state that you are doing so.

Clarified in text. The standard deviation does not include the uncertainty in bulk geochemistry used in PWI. The standard deviation just looks at multiple sample temperature values and is based on reproducibility. We then compare it to the calibration uncertainty translating PWI to temperature and demonstrate that the standard deviation is within error of calibration uncertainty.

The standard deviation on replicate analyses on six profiles from the same paleosol was smaller than the error on the proxy itself. The analytical uncertainty on PWI is that of ALS laboratories (see above), so the error is dominated by error in the model calibration.

-In figures where error bars are being used, more details with respect to error propagation is needed similar to the critique above on bulk geochemistry reproducibility.

Figure captions have been updated appropriately.

-Line 384: I don't think you can say "slightly" here given your MAAT of 11ºC from a paleolat in the low 40ºs N, and the 35ºC MAAT from 36º N latitude.

Clarified in text, slightly to moderately depending on the latitude.

-Lines 390-394: Here, seasonality in temperature is brought up, and despite that this is the discussion, no discussion on potential cold or warm season biases in the authors' temperature reconstructions are brought up. This is necessary.

Elemental chemistry should not have a seasonal bias because of the time scale of soil formation (hundreds to thousands of years). The principle on which the paleosol-based proxies are based (see Sheldon et al., 2002) is that given enough time B-horizon chemistry reaches equilibrium with the environment. Even in the tropics (where weathering intensity is typically higher), equilibrium, once reached, is also maintained >50 ka (see work by Oliver Chadwick and Peter Vitousek) and longer in continental settings. Thus, these types of analyses represent long-term integration without short-term seasonal biases) has been shown to persist on time scales and one of the pros, and the cons, depending on the resolution that you seek, is that elemental chemistry is unable to reflect seasonality. Of note, clumped isotopes in carbonates (and carbonate nodules in general) do have a seasonal bias related to time of year they form dependent on timing of precipitation and temperature. We made this explicit in the text.

-Line 396: What paleosol-based results? All of them or just the temperatures? Confusing as written.

Clarified to include temperature and precipitation.

-Line 399: Your temperature results are similar to Wing et al. (2005)? It does not appear so to me. To which PETM data in Wing do you refer: min body CIE temps or pre-/post-event temps? Surely your data can't be similar to both considering the warming at the PETM and your reported precision… that is, if temperature is what is being discussed here.

We meant the ecosystems present were comparable, which we clarified in text.

-Line 408: Yes, because they are within error, but also because of data scarcity and

sampling frequency, no?

Based on Dzombak et al. (2021) in P3, we found that paleosol-based reconstructions based on the number of sampled paleosols (n = 6) was sufficient to minimize error (see Dzombak et al., in press, Palaeogeography, Palaeoecology, Palaeoclimatology). The threshold varies for reliability is 3-4 profiles based upon that work

-Line 412-415: Please expand with citations. How does a past warmer climate allow for inceptisols to form in warmer conditions? Without details or a mechanism, this comes across circular and non-scientific.

Taken out.

-Lines 423-424: As written this is a bold statement given the dataset. How are you sure that you simply didn't sample shorter-term climate variability? This needs a timescale associated with it, such as: "climate was likely generally steady (+/− < 5ºC) on 100kyr+ timescales."

Clarified in text.

-Line 426-427: There is not much of a debate if you only include one reference here. This statement falls within the realm of marine work. The most reliable reconstructions of the early Eocene in terms of temp and pCO_2 are from marine archives and they should be cited and discussed here (e.g., Anagnostou et al., 2017; Cramwinckel et al., 2018).

We have included these additional references.

-Line 429: The methane release hypothesis needs a citation (probably a Jerry Dickens paper), and volcanism could use a few other citations (e.g., Gutjahr et al., 2017 and a recent article constraining the magnitude of North Atlantic Igneous Province volcanism).

Added additional references (Dickens 2011; Gutjahr et al., 2017, Jones et al., 2019).

-Line 432-434: Unfortunately, as written this is incorrect and a very surficial explanation of the complexity of the scientific problem at hand. What you state about reconstructing the C source using $\delta^{13}C$ is not possible without an additional constraint on parameters such another constraint on surficial carbon cycling (e.g., CCD) or temperature + climate sensitivity.

One of the advantages of using a plant-based reconstruction technique is that it does not rely on things like the CCD. We agree that there are complexities in using fossil forams to reconstruct the atmosphere. We find it heartening that this technique, which is independent/separate from ocean chemistry, finds similar answers to reconstructions based on ocean chemistry. While we agree that it is not possible to fully reconstruct all sources to the atmosphere without more information, we disagree that we have over-interpreted. As they stand, the discussion of atmospheric CO2 sources states that this study provides additional evidence that the source had an isotopic value of the mantle.

-Line 445-446: Please explain how $\delta^{13}C_a$ of ~−5.3 to ~−5.8 provides evidence that increases in atmospheric pCO_2 over the LPEE were driven by a volcanic source. Your data do not support this conclusion without other constraints on climate or the carbon cycle, and there is no clear argument provided in the text to support this conclusion. In addition to an atmospheric $\delta^{13}C$ value, one must understand and constrain the global exogenic

carbon cycle to know the long-term driver. If you are arguing that (1) your values are similar to what Tipple et al. (2010) came up with, and (2) That study claimed to constrain the driver of long-term pCO_2 increases, thus your value supports that hypothesis, you are incorrect in your written statement and should remove this sentence. If this is not your intention, please more clearly explain why your new values help support this previous finding. Please also see Komar Zeebe and Dickens (2013) for a detailed study involving geochemical constraints on the long-term drivers of LPEE pCO_2 increase using C cycle box model.

We have added this reference and included a sentence very explicitly mentioning these limitations.

-Besides small local volcanics, if you state that your data supports a certain C source, you should point out and discuss (e.g., magnitude of C) the hypothesized source of volcanism for the Paleogene: North Atlantic Igneous Province Volcanism.

We have noted the presence of the NAIP and added a citation.

-Line 446-447: Citation for "period of elevated rate of volcanism" needed. This sentence states that global CO_2 and temperature drove a slowing of volcanism written as is. I don't think that is intended by the authors, and it should be revised. Also, the Zachos et al. (2008) citation is suboptimal and a more recent study which investigates the cause of EOT cooling should be utilized. Zachos et al. (2008) do not specifically point to a decrease in volcanism to be the driver of the EOT.

We have clarified in text the period of elevated volcanism we meant.

Technical Corrections:

-Line 11: Confusing/redundant to say that increasing temperatures "accompany" modern climate change. Consider revising.

Done.

-Line 14: Here you spell "analog" and below in section title 1.1 you spell "Analogue."

Done.

-Line 24: "at that time" is confusing as it refers to when you went about reconstructing environmental conditions written as is. Consider removing phrase.

Done.

-Line 35 and throughout: You are using hyphens (-) instead of negative signs (−).

Thank you for pointing this out. We have addressed this.

-Line 179: Equation numbers appear misaligned with those below (possible formatting issue).

Done.

-Line 192: Above there is an extra line spacing after equations; it is missing here.

Done.

-Line 201: "Was" should be "were."

Done.

-Line 203: "The molar ratio" should be "The molar ratios."

Done.

-Line 220: Citation "2017b" with no author. Double check this is the appropriate format for CotP. I am uncertain since the text mentions the coauthor by initials.

Done.

-Line 250: New paragraph needs indentation.

Done.

-Line 289: "Figs. 4-7" Use em dash instead of hyphen.

Done.

-Line 290: Remove "anywhere."

Done.

-Line 295: "Inceptisols" paleosol capitalized throughout. I do not think this is common practice, but I could be wrong.

USDA soil orders are considered proper nouns and are capitalized.

-Line 302: Change to: …typical of values… or revise sentence.

Done.

-Line 305: "Demonstrated" confusing. Consider changing to "Displayed?"

Done

-Line 318: Here you are using "percent C" and "percent N", but above they were "%C" and "%N". Reminder to keep things consistent.

-Line 322: "This specific field excursion (2019)" is a bit confusing. Consider rewording to "the 2019 field excursion."

-Line 329: Missing word. "located at" or similar instead of "located. "

Done.

-Line 344-347: Extra word: "are", and many other confusing errors with this sentence. Requires revision.

Done.

-Line 365: "which can be interpreted to mean that" can be more concise. For example, "which may suggest".

Done.

-Line 371: Remove "actually" (informal/needless).

Done.

-Line 372: Vague. How are they consistent? Consider rewording sentence to state that "Changes in X element ratios are consistent with…"

Done.

-Line 384-385: Confusing, grammatically incorrect sentence.

Done.

-Line 385-387: State that this is the range in temperatures for the early Eocene (correct?).

Done.

-Line 399: Capitalize "Thermal" and "Maximum."

Done.

-Line 410: I don't think it's common practice to capitalize these paleosol names.

It is common practice to capitalize any paleosol name that overlaps with modern USDA taxonomy.

-Line 421: As written, this reads as if the "discrepancy" "represents modest actual change…" rather than the data/reconstruction.

Done.

-Line 432: "…processes and landscapes" change to "…processes and landscapes to be mobilized into the atmosphere" or similar. As is, this sentence is unclear.

Clarified.