Climate of the Past Discussions

Clim. Past Discuss., author comment AC2
https://doi.org/10.5194/cp-2021-45-AC2, 2021

Reply on RC2

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-AC2, 2021

Overview:

This study reports on an ensemble of paleosol-based paleoclimate proxies and new geochronologic dates of Eocene strata of Wyoming. The use of multiple proxies and critical assessment of the performance of these proxies through comparison to leaf physiognomy and isotope proxies for paleoclimate is a robust contribution towards methodology and an understanding of the Early Eocene Climatic Optimum. The authors advance a proposal that volcanic degassing of CO_2 likely contributed to the sustained warming during EECO based on carbon isotopes of foliar material collected in these strata.

We appreciate Dr. Gulbranson for taking the time to review our manuscript, and for the positive and constructive feedback on our work and its importance in the paleoclimate context of the area.

The stable carbon isotope analysis is likely the weakest part of this study, however, I do not think this is a fatal flaw. Tempering of the significance of the implications of the stable carbon isotope results will help this manuscript achieve its greatest impact without sacrificing credibility. I recommend enhancing the focus of the stable carbon isotopes on species specific trends (if they exist) among the studied taxa.

We thank the reviewer for this positive feedback. We have tempered the significance of the implications to appropriately reflect our confidence, and emphasize that this is useful only in the context of marine proxy reconstructions. We unfortunately did not have a high enough sampling density of specific species to make reconstructions based on these, although we do agree that would be ideal.

The writing can be improved substantially in several sections of this manuscript, and in some particular cases avoid unnecessary confusion or distraction from key aspects of this study.

We appreciate the reviewer's comments and have made modifications throughout to streamline the message.

Major-level comments:

None.

Moderate-level comments:

Redrafting of several sections in this manuscript is advised to increase the clarity of the writing and strength of the arguments presented.

Thank you for this suggestion. We have clarified the unclear sections.

Paleosol descriptions are lacking.

We agree that more details are needed, and because most of the readers will not be paleosol specialists we have added a Supplemental Table S3 including our detailed paleosol observations.

Reliance on Arens et al., 2002 may be a critical weakness in the security of the conclusions on volcanic degassing.

Yes, we agree that the transfer function is not particularly precise. However, this is why we ran 34 analyses. The 34 analyses mean that the uncertainty on the reconstructed atmospheric value is much smaller than any single value based on the proxy by itself.

Minor comments:

See below.

Line-by-line comments:

Line 22: Given how this reads it is inaccurate to say that provenance and parent material was "reconstructed". Neither of these variables has been reconstructed (i.e., if I reconstructed provenance of a sediment, then I would attempt to create or synthesize an erosion, transport, and deposition scenario that mimics what is seen in the rock record), but they have been studied to identify the source of sediment and composition of the parent material in these Eocene strata.

We have replaced this word with studied.

Line 24: There are two possible isotopic systems in CO_2. Be specific about which system, carbon in this case, is being calculated via proxy.

Clarified.

Line 25: This sentence should be broken up into two sentences with the second sentence discussing the comparison.

Done.

Line 28: Comparing paleosol to foliar-based paleoclimate proxies makes me think of time-averaging (irrespective of the uncertainty in each proxy). How comparable are foliar-based paleoclimate proxies to paleosol paleoclimate proxies if the paleosol represents 100 years, 1000 years, 10000 years, etc.? Moreover, at this early stage of the paper I'm also wondering if these paleosols may be polygenetic and thus integrate geochemical archives of different climate states, or are these solitary profiles where we can be certain that the paleosol developed in equilibrium with the state factors at that time? I'm interested to learn more about this in this paper, but this is an opportunity to clarify these issues for the reader in the Abstract.

Clarified in text. There are no thousand year old leaves on trees, so time-averaging on trees is irrelevant for that type of data, but is relevant for the paleosols.

Line 30: It is apparent now that I'm not clear on what the purpose is of this study, the problem or hypothesis that was to be tested or evaluated. I re-read the earlier parts of the abstract to see if I missed something, but the purpose of this study I think is more implied than a direct statement. Please consider revising the first 1/3 of the Abstract to better elucidate this.

We have added a sentence before launching into the details of the study "Using this well-preserved basin deposited during a period of tectonic and paleoclimatic interest, we employ multiple proxies to study trends in provenance, parent material, weathering and climate throughout one million years." on line 18. This is after the background about why we care about the Eocene, but before the logistics of the study.

Line 47: This is overly generalized and inaccurate. The PETM included pronounced regions of aridification and associated landscape, floristic, and vertebrate changes. This also establishes a contradiction with the next sentence, which also lacks crucial clarity as to the mechanism(s) for why an already dry climate may become drier under increasing atmospheric temperatures.

Addressed by alluding to the complexities of this time period. This makes the transition to the concern about hydroclimate smoother.

Line 50: There is more than one desert in this broad region. Is it true that all of these deserts are equally affected in terms of response, timing, and magnitude to a given climate forcing?

Done.

Line 59: What specific mechanism(s) led to the formation of a series of large lakes?

Clarified in text, increased and changed fluvial flow due to uplift of mountains..

Line 65: A point of clarity, the Laramide structures probably did not contribute water to anything at the Earth surface, rather (and I'm assuming the original meaning), as uplifted blocks they may have influenced the transport of atmospheric moisture and groundwater flow paths in the region.

Clarified in text to state that they influenced atmospheric transport.

Line 69: This is a very precise paleolatitude, 41.82ºN, what is the uncertainty on this estimate? However, with more definite knowledge of the modern latitude of the region, the comparison should be more definite than "is thought…"

The uncertainty exceeds the utility of having that precision, so we have corrected it to say ~42N.

Line 70: This sentence can end after the word "latitude".

Done, thank you.

Lines 73–75: I understand the purpose of this introduction, but it requires some revision: 1) consistent format for references; 2) breaking the reference to specific proxies out of the parentheses and into a sentence or two; 3) describing more of the connection of an observation (e.g., isotope value) to an interpretation.

Done.

Line 75: How are the quality of organic specimens determined?

Clarified.

Lines 76–78: This is overly vague and lacks key references.

Added references and clarified the importance of this statement.

Lines 78–79: Again, overly vague. Could this section instead be rolled into the Methods section? In this version of the manuscript this section doesn't really add any information.

This section could be placed in the introduction or methods, depending on the desired message. In this case, we are emphasizing the importance of using multiple proxies to understand an environment. As such, we are leaving it where it is but changing the tone and focus.

Line 96: What are the uncertainties on these ages, and have these ages been corrected so that they are comparable to U-Pb ages?

Yes, all the ages we report or discuss have all been calculated using the 28.201 Ma age for Fish Canyon tuff sanidine standard (Kuiper et al., 2008). We actually did this in a 2010 paper, which we cite in the text: *Radioisotopic ages reported or discussed in this contribution have all been calculated using the 28.201 Ma age for the Fish Canyon tuff sanidine standard, and are thus comparable with modern U-Pb geochronology (Kuiper et al., 2008; Smith et al., 2010).*

Line 98: Siliciclastic is a general term that implies a quartz-rich clastic sediment. When I read about potential sediment source areas I am generally surprised to see siliciclastic as the first potential source listed without specific mention of sediment recycling. Regarding provenance I think it makes sense to start with the most fundamental data available, sediment composition, and then work backwards to identify probable sources of that sediment.

These potential sources exist and have been characterized previously. There isn't an explicit sandstone petrography dataset being interpreted here. Some more detail on the siliciclastic sediments include: a) the most common detrital feldspar ages in the sand are similar to depositional age (i.e., recently erupted volcanic grains) and b) there are lots of euhedral volcanic biotite and felsic volcanic lithic grains (small pumice clasts) in Bridger Fm sandstones (Chetel et al., 2011). Additional characterizations are included in Smith et al., 2008; Smith et al. 2015:

Smith, M. E., Carroll, A. R., and Mueller, E. R., 2008, Elevated weathering rates in the Rocky Mountains during the Early Eocene Climatic Optimum: Nature-Geoscience, v. 1, p. 370-374.

Smith M.E., Carroll A.R., Scott J.J. (2015) Stratigraphic Expression of Climate, Tectonism, and Geomorphic Forcing in an Underfilled Lake Basin: Wilkins Peak Member of the Green River Formation. In: Smith M., Carroll A. (eds) Stratigraphy and Paleolimnology of the Green River Formation, Western USA. Syntheses in Limnogeology, vol 1. Springer, Dordrecht. https://doi.org/10.1007/978-94-017-9906-5_4

Line 109: What about the roots, and anatomical attachment? I would temper this statement to refer to excellent plant fossil preservation without the qualifier of "all plant organs".

Done.

Line 110: This is broader critique I have with paleobotanical references to biomes in general. Given the paleolatitude being in the mid-latitude region, this cannot be a subtropical biome, sensu stricto. Rather, the flora contained in this biome may contain elements consistent with biomes at lower paleolatitudes, which says something important about the Etp/MAP balance, seasonality, MAT, etc. What it doesn't say, which is what subtropical suggests, is that the incident angle of solar radiation was the same at ~41ºN as it is between 0º and ~25º, and remains with a finite difference through one full rotation around the Sun. It also doesn't say that Hadley Cell circulation was different, where the descending limb extends to ~50ºN, which is what is implied by calling this biome subtropical. Instead, we have a mid-latitude region, with mid-latitude sunlight seasonality and power, with mid-latitude atmospheric circulation (or lackthereof), with a flora that previously (and afterwards) inhabited only the equatorial latitudes. Personally, I think this showcases the significance of this latitudinal shift in flora and points to some of the key aspects to study the who/how/and why about how these ecosystems came to develop here. If the mid-latitudes truly became subtropical in every sense of the word, then that would be likewise astounding, but is that what we're saying here?

This is an interesting and helpful point. Rather than saying we found subtropical ecosystems at 41N, we have corrected the text to say ecosystems comparable to modern subtropical ecosystems, an important distinction!

Line 112: We don't know that there are quarries or what these are quarries of, this sentence needs a segue of some sort.

Clarified.

Line 121: How do the authors know that these are volcaniclastic beds?

A couple of reasons: a) the most common detrital feldspar ages in the sand are similar to depositional age (i.e., recently erupted volcanic grains) and b) there are lots of euhedral volcanic biotite and felsic volcanic lithic grains (small pumice clasts) in Bridger Fm sandstones (Chetel. Et al. 2011).

Line 122: The blue-green marker needs a more precise and archivable definition. How would a person unaffiliated with this research team find this bed? I see more specifics later on, but at this juncture there should be a reference to a figure or something to direct our attention to where this bed is.

Done.

Line 139: What is meant by "updated stratigraphic column"?

Done.

Line 140: Arbitrary sampling is fine, but what was the rationale for this choice in sampling?

This gives us reasonable statistical coverage relative to the thickness of the section. We were not looking for a discrete event, so we did not need higher resolution sampling than this.

Lines 152–153: Epipedons are preserved in all of these paleosols?

Clarified.

Line 165: Which references were used for the C isotope analysis and what was the performance of those references on the Picarro over the time range of the analysis of these samples?

Clarified.

Line 210: Please change soils to paleosols (admittedly, I refer to paleosols as soils all the time, but it is not appropriate).

Done.

Line 219–220: It would be more concise to cite this dissertation along with a description of the method used.

The geochronologists on this paper said this was the community standard.

Line 240: It's hard to quote that R^2 and be confident in the results. However, what really crushes my confidence in the approach of Arens et al. (and helps explain the low R^2) is the fact that they hold constant the variable that plants modulate to respond to climate (Ci/Ca). It's just not a sound approach. I know it is used widely, but, that's just not a sufficient reason for me to agree with it. Paleoclimate is the goal for many of us, but it's how we arrive at our conclusion that matters. As an analogy, I can measure the stable isotopes of carbon in practically any carbon-bearing substrate. Those techniques are pretty easy, but if my data is to mean something I need to carefully select my samples and process them in such a way to preserve the signal I hope to extract from these samples.

We agree with the reviewer that the R^2 value in the Arens et al. (2000) model is low, though significant. Of note, we agree with the reviewer and Arens et al. that

C_i/C_a is likely not variable, despite being held constant for this model. However, the complexity of this measurement is not yet constrainable in geologic time. As such, we used a large number of measurements to assess the *most likely* $\delta^{13}C_{atm}$ value for comparison to marine $\delta^{13}C_{atm}$ reconstructions. We would not interpret any of the individual measurements as reasonable, however based on the clustering of the data, the uncertainty on the reconstructed atmospheric value is much smaller than anything based on the proxy by itself. We are heartened by the comparable marine-based reconstructions.

Section 4.2 Where are the O horizons? For paleosol 1, it is missing a B horizon, but does this mean that it has an A horizon over a C horizon, where the A and C horizons are separated by an erosional contact? If so, then there are many possibilities for what that profile may represent, but, it wouldn't represent a continuum of soil-forming processes. For paleosol 4, I highly doubt that erosion of the A horizon took place during the burial process, which as the name implies, indicates burial of the strata. It is more often than not the case that the epipedon of paleosol profiles are removed via erosion when those profiles are formed in overbank regions of fluvial environments or proximal to shorelines of lakes/shorefaces. After that erosion, and subsequent deposition of new material the profile may be buried, preserving its truncated form. This sections needs a systematic description of the paleosol profiles, followed by their diagnosis against your taxonomic scheme of choice.

We have added Supplemental Table S3 with details on the paleosols. All questions should be addressed there.

Line 295: Without a systematic presentation of the paleosol observations and their lateral variation it is not clear how these profiles represent Inceptisols rather than Entisols. Also, the Soil Survey Staff, 2014 should be cited here.

See Table S3 for details, and Soil Survey Staff citation has been added.

Line 335: What was the % difference in CIA-K in the A or B horizons relative to the parent material? I use an arbitrary cutoff of 5%, with the idea that the greater the difference of the subsoil relative to the parent material indicates a greater likelihood that the soil formed closer to equilibrium with its environment (and thus that the solid state major element concentrations reflect all of those lovely contributions of weathering energies from water and organic acids).

The two paleosols excluded from the analysis (19BRWY1 and 2) did not show CIA-K values >5% of the parent material, but the remaining ones, 19BRWY3-19BRWY6, had values >9% throughout, indicating they were in equilibrium with their environment.

Line 340: Phew, I was really hoping to read this statement (species-specific tests). What I mean is, the authors have identified these plant taxa with wide ranging ecology, and it would be expected/anticipated to see carbon isotope variation among them (maybe clueing us into ecosystem processes as a function of functional diversity). I'm excited to read more.

Thanks!

Line 367: What were these oxygen isotopes measured on?

Micritic lacustrine carbonates. We have added this, thank you!

Line 375: This makes sense as the name Blue-green marker bed suggests a sedimentary unit with either stratified or variegated color of blue-green, which is indicative of reducing conditions.

Thank you for this comment. We have added that clarification.

Line 379: R^2=0.2 suggests that this explanation does not satisfactorily explain the variance in the data. Moreover, this explanation is fairly weakly held as a taphonomic difference could also explain the high/low carbon content. If the authors wish to further test this, then a compound-specific analysis (maybe via pyr-gc or solid-state 13C NMR) could be informative on the composition of the organic carbon (granted you'd be looking for the diagenetic products of specific ensembles of organic acids).

This is a great point. We have changed the language to reflect that while this correlation is interesting (albeit weak), this could be related to taphonomic difference.

Figure 3C: It is difficult to read the text superimposed on the image.

Thank you, we have fixed this.

Figure 5B: Why is the scale set to 0.002? This is an exaggerated scale when none of the data plot even half the way to this value.

Set to a more reasonable value to show the very small amount of variability.

Figure 7: A strike and dip symbol would be instructive on this image.

We added a strike and dip symbol as well as the location of the stratigraphic column for context. Strike measurements were not consistent, and dip was close to 0.

Figure 9: Is the color spectrum just the same representation as the y-axis? If so, it is redundant, confusing, and should be discarded in a favor of a more simplistic visualization of this data (e.g., without color).

We have simplified the figure, thank you to the reviewer for this feedback.