Climate of the Past Discussions

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Comment on cp-2021-45

Erik Gulbranson (Referee)

Referee comment on "Climate & 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-RC2, 2021

Referee Report CP-2021-45

Manuscript: Climate & Ecology in the Rocky Mountain Interior After the Early Eocene Climatic Optimum

Authors: Stein et al.

Journal: Climate of the Past

Reviewer: Erik L Gulbranson

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.

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.

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.

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.

Paleosol descriptions are lacking.

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

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.

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.

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

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.

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.

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.

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?

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

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.

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…"

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

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.

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

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

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.

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

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.

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".

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?

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

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

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.

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

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

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

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?

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

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

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.

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.

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.

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).

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.

Line 367: What were these oxygen isotopes measured on?

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.

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).

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

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.

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

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).