

Please find our response to the comments of the Reviewer in blue italic font below.

Please find the implemented changes in red italic font below. Line numbers refer to the track changes version of the manuscript.

Editor: Alberto Reyes

Public justification (visible to the public if the article is accepted and published):

Dear Dr. Meijers,

Thank you for addressing the reviewer comments/suggestions in your detailed responses. My sense is that the major thematic points of reviewer concern (clarifying spatial scales; discussions/evaluation of the robustness of the geochronology framework; details for allowing readers to evaluate data quality) can be addressed with relatively minor revision.

I think Reviewer 2 makes an important comment about providing as much information as possible to allow readers to evaluate the samples re: diagenesis and sampling, so I'm glad some of the details/context provided in the response will be included in the revised manuscript (along with citation to the newly accepted EPSL paper).

There's also a good point made about including more context on the geochronology that underpins the manuscript. This is important enough that I recommend including this in main text (the supplementary table of geochron data is fine where it is). If you prefer for a detailed discussion to be in a supplementary text file instead, please do consider at least including a high-level summary of this contextual discussion in main text.

Please prepare and submit a revised manuscript along the lines of your responses, and also taking into account my recommendations above. I anticipate making a decision on the revised manuscript without another round of review.

Sincerely,
Alberto Reyes

Dear Prof. Dr. Alberto Reyes,

Hereby we send you our revised manuscript 'Climate change drove Late Miocene to Pliocene rise and fall of C₄ vegetation at the crossroads of Africa and Eurasia (Anatolia, Türkiye)'. We very much appreciate the extra time we received to finalize the manuscript.

We have revised our manuscript according to the insightful and constructive reviews by the two Reviewers and we believe that we have properly addressed the points raised.

We have clarified the spatial scales and added a supplementary text file that details the geochronology. Additionally, we added a composite stratigraphy of the southern Central Anatolian Volcanic Province which also includes pictures of representative outcrops. We also added a table to the main text to detail the subdivision of the datasets, as well as to give a brief overview of the chronologies and $\delta^{13}\text{C}$ values per site (as well as averaged per time interval). We hope this supports and clarifies the text. We have also included an extensive discussion about diagenetic alteration and now refer to the $\delta^{18}\text{O}$ values in the published EPSL paper. We detailed the implemented changes below the Reviewers' comments.

We apologize that the track-changes version of the manuscript includes duplicate citations throughout. We removed the duplicates from the final version.

We hope that you will find this revised manuscript acceptable for publication in *Climate of the Past*.

Sincerely,
Maud Meijers

On behalf of all authors

Reviewer: Funda Akgün

Dear Editor,

I have read entitled “Climate change drove Late Miocene to Pliocene rise and fall of C4 vegetation at the crossroads of Africa and Eurasia (Anatolia, Türkiye)” detailed in all my good intentions for the study. This study was focused on using *duraylı* isotope data, mammal and palaeobotany/paleoclimate based on previous studies. It includes major corrections noted on the manuscript (given below). The main problem is that approaches have been made on the Anatolian and Aegean scale based on data from narrow areas with sedimentological characteristics, which are not specified in the results and abstract. The need for narrowing and simplification of these approaches has been tried to be pointed out in the manuscript. In conclusion, this manuscript is suitable for publication in your journal after the major corrections and edits indicated.

We thank the Reviewer for their comments and suggestions. Our response can be found below.

Major Corrections:

- Table S2 should be updated as Karacaören- early Pliocene, Bingol-Halifan-late Pliocene, Erzurum-Horosan-late Pliocene (Original Time Slice) as stated in the related manuscript. In the related references, the ages of the samples from these locations are given as listed. Comments and figures related to this correction should be revised.

In our manuscript, we refer to the ages that were provided for sites in the most recent publications while also citing the original publications. Therefore, the age ranges of the three sites above are larger (and as such more conservative) than in the original publications. We will add this information to the column ‘Age Remarks’ in Table S2.

Information was added to Table S2.

- It is not clear how the concepts of Plateau and Coastal given in Table S2 are evaluated within the interpretations of the manuscript.

We subdivided the sites in Coastal and Plateau sites because a decrease in e.g., calculated cold month temperatures (CMT) and mean annual temperatures (MAT; see Fig. 2B,C) at sites located on the modern plateau may result from surface uplift of the CAP (Fig. 2D) rather than from regional climatic changes.

Unfortunately, the low number of sites in the coastal regions (i.e. the regions that should not be affected by surface uplift-induced cooling) and the large uncertainties in CMT and MAT estimates do not allow us to draw firm conclusions (see lines 253-255). We are happy to clarify this in the manuscript.

*Information was added to **line 320** ('in the CAP interior') and to the caption of Fig. 2*

- The locations marked in the Figure 1 are all in Central Anatolia, Rhodes, Ermenek, Adana are in the Mediterranean and Pikermi, and Samos is in the Aegean and Mediterranean belt. Aegean is used for Western and GB Anatolia. For this reason, the term Anatolian-Aegean region should be replaced by another term to include all locations. Otherwise, it is understood that the data used in the manuscript belong to the Aegean region of Anatolia. Also, it is useful to evaluate it later in the manuscript.

Yes, indeed, we should have been more precise and explicitly labeled the sites on the Mediterranean coast. We will change this throughout the manuscript.

Changed in captions of Fig. 1 and Fig. 2. The term does not occur in the remainder of the manuscript.

- In the Miocene and Pliocene, as in the present day, different microclimatic conditions (such as topography, elevation, distance to the sea, etc.) were effective from region to region within Anatolia. For this reason, the interpretations made in the Aegean region throughout the manuscript are confusing due to the inadequacy in the definition of Anatolia-Aegean region.

Please see our replies to the previous two comments. Resolving these two comments should make it clear to the reader which local variations may have affected microclimate.

No changes required.

- The multilayered presence of Central Anatolian volcanic activity must have had a more localized impact on the vegetation in CAP region than the general climatic conditions. The notion that this influence was such that C4 vegetation dominated the whole of Anatolia should be used with caution.

First, as detailed in section 4.2 (lines 214-233), we conclude that intramontane floodplains in Anatolia were dominated by C₄ vegetation between 7.1 and 4.9 Ma, while the mountain ranges were dominantly covered with C₃ vegetation (as reconstructed from paleobotanical datasets). We also address the preservation bias in floodplain sediments, which leads to an overrepresentation of C₄ vegetation in our soil carbonate $\delta^{13}\text{C}$ -based vegetation reconstructions. At no point do we claim that the whole of Anatolia was dominated by C₄ vegetation, we consistently refer to the floodplains (see, e.g., Abstract).

Second, there is independent evidence for the (increased) presence of C₄ vegetation during the Late Miocene in areas surrounding Anatolia, which were not affected by extensive volcanism

(including the Aegean/Crete, Iran, Cyprus; lines 201-206). As such, this refutes the hypothesis that volcanism played a decisive role in shaping the dominant vegetation type (with respect to photosynthetic pathway) on geological time scales.

Third, we are not aware of studies that suggest a direct link between volcanism and the rise of C₄ vegetation (in contrary to studies that show that fire activity led to the expansion of (C₄) grasslands in many parts of the world, see e.g., Guarino et al. (2024, Biogeosciences) and Loughney et al. (2023, Geology) for contemporary and Middle Miocene case studies). As such, including volcanism as a potential driver would be speculative.

No changes required.

Best regards

Funda AKGÜN

Reviewer: Anonymous Referee #2

In this manuscript, the authors present a new record of pedogenic carbonate $\delta^{13}\text{C}$ data from the Anatolian floodplains. This paper fills in a critical regional gap in the geologic record and provides contrast with African and Asian records of C₃/C₄ turnover. My largest concern with this manuscript is that the way the information on samples and data are currently published make it challenging to evaluate the quality of the $\delta^{13}\text{C}$ data. There is useful data in the supplemental tables that should be moved to the main text to make the manuscript easier to evaluate.

We thank the Reviewer for their constructive comments and suggestions. Please find our response below.

Largest concerns:

1. The amount of data published in the supplement does not make it possible to evaluate the quality of the $\delta^{13}\text{C}$ data published.
 1. In the methods section the authors should describe what kinds of corrections were done on the data (i.e. linearity, drift, scale corrections). I suggest creating a new supplementary table where the authors publish *at least* the raw measured values of **both** samples and standards, as well as the corrected values. Best practice would be to keep a record in that spreadsheet of each correction step. That way, if standard values are updated in the future, it is possible to bring this data into a new reference frame. If there is an associated R or Python script for these corrections, this should be published as well.
 2. Were the data all collected in one session? Multiple sessions? Please be clear about how that influenced data correction.

The samples, and as such the data, were acquired over multiple years and multiple sessions that included unrelated carbonate samples from other projects. As mentioned in the manuscript, we follow the procedures in Spötl & Vennemann (2003), which includes scale, linearity, and drift corrections. We included international reference material NBS 18 (IAEA) in all our measurements. We also use a synthetic Merck standard ($\delta^{13}\text{C} = -35.69\text{‰}$ (V-PDB), $\delta^{18}\text{O} = 12.38\text{‰}$ (V-SMOW)) and an in-house Carrara marble standard ($\delta^{13}\text{C} = 2.01\text{‰}$, $\delta^{18}\text{O} = -1.74\text{‰}$), which was

weighed in for four different sample sizes for the linearity correction. We will communicate this information more clearly in the manuscript.

The IAEA has established high-precision values for their NBS18 standards over the past decades. As such, the standards are not expected to further improve such that the data need to be reprocessed in the future. Therefore, we do not deem it necessary to upload all measurement files as supplementary materials.

We implemented the changes above in the Methods section (2.1 and 2.2; lines 96-121). The $\delta^{18}\text{O}$ values were added to Supplementary Table S1.

More detail is needed about the nodules themselves is needed. What drove sampling choices? Why did the authors run more nodules from some samples and not others? More info on diagenetic evaluation is needed – are there any microscopy or CL images? Please include any images of nodules in the supplement. What diagenetic signs were you looking for? (line 168). How did that influence drilling choices? Given burial depth (line 170), I think the authors are probably right on diagenesis, but just a little more detail would put readers at ease.

Sampling choices were entirely based on the availability of pedogenic carbonates in outcrops, which were taken over several months of fieldwork. When drilling the samples, only micritic parts devoid of clasts were drilled (i.e. sparry calcite in veins -if present- was not sampled).

We do not have microscopic or CL images of the samples. However, we will provide pictures of representative pedogenic samples for the supplementary materials. The $\delta^{18}\text{O}$ values associated with the $\delta^{13}\text{C}$ values presented in this manuscript do not show any signs of diagenetic alteration. The manuscript with these $\delta^{18}\text{O}$ values was accepted for publication in Earth and Planetary Science Letters on May 01 2025 (see also reply to next comment). Additionally, the accepted publication that includes the $\delta^{18}\text{O}$ data includes two dual clumped isotope (Δ_{47} , Δ_{48}) temperatures for ca. 5.4 Ma pedogenic carbonate (10CKK64) and for ca. 5.4 Ma lake carbonate (sampled in the Adana basin in southern Turkey). Both analyzed samples plot within error of the dual clumped isotope equilibrium line, which implies that their isotopic compositions are devoid of significant kinetic biases. Second, their Δ_{47} temperatures are $24.1 \pm 3.5^\circ\text{C}$ and $15.6 \pm 3.3^\circ\text{C}$ for the lacustrine (AVA08) and the pedogenic (10CKK64) carbonate, respectively. The obtained temperatures show that the carbonates precipitated under conditions conformable with soil formation rather than with elevated diagenetic temperatures. More evidence for the primary origin of the sampled carbonates comes from the $\delta^{18}\text{O}$ values and thin sections of lake carbonate that was sampled in the region (in some cases in the same sections, see Meijers et al., 2018, Earth and Planetary Science Letters), as well as the associated $\delta^{13}\text{C}$ values (Meijers et al., 2020, Geosphere), which show no evidence of diagenesis.

*-We have added the following text to materials and methods section 2.1 (lines 96-98):
'Sampling choices were based on the availability of pedogenic carbonates in outcrops, which were taken over several months of fieldwork.'*

*-We have added the following text to materials and methods section 2.2 (lines 103-104):
'Only micritic parts devoid of clasts were drilled for analysis'*

-The part in the discussion referring to diagenesis (section 4.1) now reads as follows (lines 204-221):

'We interpret obtained $\delta^{13}\text{C}$ values (and associated $\delta^{18}\text{O}$ values; Meijers et al., 2025) as primary (i.e. reflecting the isotopic ratios upon the formation of the carbonates), because the values are within a range typical of soil carbonates (e.g., Cerling and Quade, 1993). Additionally, the paleoaltimetry study by Meijers et al., (2025) that is based on the $\delta^{18}\text{O}$ values associated with the $\delta^{13}\text{C}$ values of this study includes two dual clumped isotope (Δ_{47} , Δ_{48}) temperatures. First, results for the ca. 5.4 Ma pedogenic carbonate sample (from CAVP site 10CKK, see also this study) and lake carbonate sample (from the Adana basin in southern Turkey) plot within error of the dual clumped isotope equilibrium line, which implies that their isotopic compositions are devoid of significant kinetic biases. Second, the obtained Δ_{47} temperatures of $15.6 \pm 3.3^\circ\text{C}$ (CAVP) and $24.1 \pm 3.5^\circ\text{C}$ (Adana basin) show that the carbonates precipitated under conditions conformable with soil formation rather than with elevated diagenetic temperatures. More evidence for the primary origin of the sampled soil carbonates comes from the $\delta^{18}\text{O}$ and $\delta^{13}\text{C}$ values and thin sections of lake carbonate that was sampled in the same or similar fluvio-lacustrine host lithologies, which show no evidence of diagenetic alteration (Meijers et al., 2018; Meijers et al., 2020). Moreover, all sites within the Central Anatolian Plateau interior were buried only 170 m or less (Supplementary Table S1), except for Mustafapaşa (ca. 9.9 ± 0.8 Ma; burial depth ca. 400 m). Low burial depths result from relatively low sedimentation rates (ca. 25–80 m/Myr; Supplementary Table S1) and rapid drainage integration and incision of the basins within maximal 2.5 Myrs after the latest deposition of the soil carbonate host lithologies (Brocard et al., 2021; Meijers et al., 2020).'

-We have also added pictures of representative outcrops to Supplementary Figure S1, in which we also included a composite section of the southern Central Anatolian Volcanic Province to show the relationship between all sampled sections in that region.

References:

Meijers et al., 2018, EPSL:

<https://www.sciencedirect.com/science/article/pii/S0012821X18303236>

Meijers et al., 2020, Geosphere:

<https://pubs.geoscienceworld.org/gsa/geosphere/article/16/2/490/580000/Paleoenvironmental-conditions-and-drainage>

Meijers et al., 2025, EPSL:

<https://www.sciencedirect.com/science/article/pii/S0012821X25002134>

2. The authors should also strongly consider putting the $\delta^{18}\text{O}$ data in the supplement, even if it's not discussed in the manuscript so those data are freely available to future users. Improvements in pedogenic carbonate modern calibrations (using $\Delta^{17}\text{O}$ and Δ_{47}) are showing that those data might be more useful than previously thought. Make sure that it is clear how those data were corrected as well.

The manuscript with the $\delta^{18}\text{O}$ results was recently accepted for publication in Earth and Planetary Science Letters. The data were interpreted in terms of surface uplift (stable isotope paleoaltimetry). We can therefore include the $\delta^{18}\text{O}$ values in this manuscript, such that the entire dataset is available in one location. The $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ results were, of course, measured and evaluated simultaneously.

The $\delta^{18}\text{O}$ values were added to Supplementary Table S1, as well as a reference to the recently published EPSL manuscript with the $\delta^{18}\text{O}$ values (see response above).

Line by line comments:

Line 23-24: This sentence is confusing, please clarify

Yes, indeed, we will change this to: 'The transition to C₄ in Anatolia coincides with a similar shift from C₃ to C₄ vegetation in southern Asia and is generally attributed to the Late Miocene Cooling in response to decreasing atmospheric pCO₂.'

Changed.

Figure 1: The hexagon is hard to differentiate, consider adding color (maybe something in a cool color spectrum?) or changing the shape so it stands out.

Yes, we will do this.

Changed to a triangle.

Lines 64 – 75: I recognize that this is a stylistic choice, but I find it jarring to put the conclusions into the introduction of a paper in this format. I think it makes sense for shorter format papers, but here it feels like putting the cart before the horse. Consider removing or reframing

We will reframe this.

We reframed the text and removed the conclusions. However, we did leave a teaser in the text. The Introduction now reads (lines 67-75):

'Our results of 447 pedogenic carbonate samples from sixteen sites show that C₄ plants spread to dominance in Anatolian floodplains during the Late Miocene and diminished to similar-to-modern proportions during the Pliocene. We compare the timing (Quade and Cerling, 1995) of floral overturn with the few available Anatolian (Türkiye) and Aegean (Greece) soil carbonate $\delta^{13}\text{C}$ records, as well as with records of faunal overturn. We then assess the drivers of vegetation dynamics in terms of components of C₄ and C₃ vegetation in Anatolia and the Aegean by comparing their timing with other soil carbonate $\delta^{13}\text{C}$ records from the Old World, as well as global marine climate records.

Line 88: did you measure any rhizoliths? If so, need to clearly differentiate that data.

We certainly analyzed not only carbonate nodules but also carbonate concretions that formed from rhizoliths (root casts), as they were always collected when found in the field. Whenever identifiable, we will add that information to the data table.

Information about the type of sampled soil carbonate was added to Supplementary Table S1. Additional information for Central Anatolia was added to Supplementary Figure S1 and includes a composite stratigraphy in which we show the sampled interval of all sites that were sampled in that area, as well as pictures of representative outcrops.

Line 88: It would be more appropriate to use Munsell soil colors here

Unfortunately, no Munsell soil color chart was available during field work and field pictures do not allow for proper color analysis. In general, we believe that color information would benefit our study of soil carbonate $\delta^{13}\text{C}$ values.

No changes possible.

Line 109: Because so much of the manuscript relies on the geochronology being right, it is worth it to describe in a little greater detail in the manuscript. I know its published, but make it easier for the readers to evaluate the quality of the geochron data. What kind of radiometric dating? What kind of age model?

The details (i.e. type of radiometric dating, minerals dated etc.) are all available from Supplementary Table S1. However, we will add a supplementary text file which gives a clearer overview of the detailed chronologies and the underlying ages for each of the sites.

Supplementary File S1 was created and referred to in the text.

Line 123: Stronger justification of the exclusion of this data is needed since so much of the final conclusion is based on hydrologic change in the region.

We will do this for the one Asian site that we excluded (out of eleven) based on the original multi-proxy interpretation of the authors (Zhuang et al., 2011, Earth and Planetary Science Letters) that the high soil carbonate $\delta^{13}\text{C}$ values are a direct result of low soil respiration rates.

The text now reads as follows (lines 145-149):

We also excluded elevated $\delta^{13}\text{C}$ values from one multiproxy study on the Qaidam basin, which are attributed to low soil respiration rates in response to increased aridification (Zhuang et al., 2011). As such, the $\delta^{13}\text{C}$ values may not be only interpreted in terms of variations in the dominating photosynthetic pathways (see also 4.1).

Results section (lines 131 – 159):

1. I would suggest that the authors consider significantly shortening the results, and instead translate much of the text into a table. There is a lot of useful information hidden in table S1 that should be moved to the main text (for example, moving some of the geochron details would help alleviate the line 109 comment).

We will move the information to a table. As suggested for the comment on line 109, we can move the geochronological information to a supplementary text file, as including the information in the main text would fill too much space (ca. 1.5 page).

We significantly shortened the results section (section 3) and translated the information to Table 1 in the main text, which also includes basic chronological information (we present detailed chronologies in the new file 'Supplementary Text S1'). We also included averages and means for the data from different age groups.

2. It may be more useful to describe the salient trends that we can see in the data rather than the range of isotope values.

We will put more emphasis on the trends after moving the details to a table (see previous comment).

The results section has been adjusted and shortened (see comment above).

3. The authors present sample means for each of their samples, but the underlying data structure is clearly not a normal distribution (some samples are surely biased by outliers), and so the authors should report median values as well as mean values. The authors should strongly consider calculating and reporting the 2 standard error value for their samples. This is becoming the more common-place error statistic to report in the stable isotope community.

We will certainly do this and test our data accordingly. However, this does not change any of the trends we describe in the manuscript.

Table 1 was created and added to the main body of the manuscript. It includes the median values, the corresponding 25% and 75% quartiles, as well as the 2σ SD uncertainty corresponding to the averages. We also added a line to section 2.2 (line 121-123) and to the start of the results section (lines 163-165) that clearly state that we evaluate the data at the 1 sigma SD level.

4. Be clearer as results are presented if you continue to just report 1 SD (again, this would be easier in a table)

We are not sure we understand this comment, but we hope that adding the table will resolve the issue.

Table was created and we clearly indicated in the Results section that the data are being discussed at the 1σ SD level.

5. Reference figure 2 earlier in the results

Yes, we will do this.

Reference was added (line 163).

Line 170: please also edit this sentence to reflect the burial depth for Mustafapaşa (400 m).

Consider it done.

Sentence was adjusted to include the information (line 218).

Line 176: are there any paleosol characteristics that support or refute this? Color, texture, morphology, etc.

Paleosol classification was not systematically recorded for each sampling site. The data available do not permit to support or refute this hypothesis, which is based on paleobotanical data.

No changes possible.

Figure 2:

1. In A: please add a better minor grid and make the average values solid to make them easier to see and differentiate?
2. In B & C – please make the data points a size or two larger to make it easier to read and see.

Yes, we will take care of both suggestions above.

Figure 2 was adjusted accordingly (minor grid adjusted, symbol size and stroke as well).

Line 194: include citations here

We could add them. However, they are already listed in the Table S2 and include 20 citations, which might be a bit much for a caption.

No changes required.

Lines 214 – 232: The way this paragraph is written (juxtaposing the contradicting data) is a little confusing, and needs a little reorganization and refining.

We will do this.

Rewritten in lines 278-296.

Line 244: is this a decrease of up to 10°C?

The reviewer refers to line 254. Yes, indeed, a decrease of up to 10°C. We will point this out more clearly.

Changed.

Line 264: indent new paragraph

Consider it done.

Done.

Figure 3: please make it clearer what AMOC + and MSC in c) are in the caption. Strongly consider swapping the x axis direction, with 0 Ma on the right side. This ultimately is stylistic, but it is quite standard in paleoclimate (e.g. Westerhold et al., 2020). It would make your plot easier for other scientists to use.

Yes, we will improve the explanation of AMOC+ and MSC. We agree that swapping the horizontal axis is appropriate and will do so.

The figure and caption were edited accordingly.

Line 314: This mechanism has been shown in modern experimental grasslands. The authors should make reference to Knapp et al., 2020 (<https://doi.org/10.1073/pnas.1922030117>)

Thank you very much for pointing out this study. We will gladly add a summarizing line and the reference.

Study was added (lines 391-394):

'Such a change to more cold season precipitation has been shown to result in a strong and rapid (3–4 years) effect on the relative proportions of C₃ and C₄ vegetation during the 1930s Dust Bowl and was successfully reproduced in experimental studies in modern grasslands in the Great Plains (United States; Knapp et al., 2020).'

Table S1:

- Please make the headings for latitude and longitude clearer (rather than N / E), please also include the reference frame (e.g. WGS84)

Yes, we will do this.

We added the information to all tables.

- Please justify the merging of the Bakali sites more clearly. It is also unclear where the age for this sample came from. Please clarify.

The samples of the sites were all taken from time-equivalent deposits in a small area (± 2000 m horizontally), hence we grouped them. The Quaternary age of the samples was derived from the 1:500,000 geological map of Adana, as referred to in line 111 in section 2.2 ('Pedogenic carbonate chronology').

The information was added to Table 1 and Table S1, as well as the detailed chronologies to the new Supplementary Text S1.

- Are there any estimates of paleoelevation? If so, please include

The paleoelevation estimates are part of a manuscript that was accepted for publication in Earth and Planetary Science Letters (see our reply further above).

Pointed out in the Methods (2.2) and included in the discussion (4.1). (lines 108-109, 205, and 208-209).

- Chronologies: If there are samples where adjustments for sedimentation rate were made, please make that clear

In terms of uncertainty of the assigned ages, we regarded the entire interval between dated ignimbrites. We will point this out more clearly.

Now clearly stated in the new file Supplementary Text S1.