nature portfolio

Peer Review File

Anthropogenic impact on terrestrial and marine environments linked to societal change in Ancient Greece

Corresponding Author: Dr Andreas Koutsodendris

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr Koutsodendris,

Your manuscript titled "Anthropogenic impact on terrestrial and marine environments linked to societal change in Ancient Greece" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, and meet our editorial thresholds as outlined below, we would be happy to look at a substantially revised manuscript.

In the following, we list our main editorial thresholds.

****Editorial threshold 1: Provide firm and sufficient evidence to demonstrate that the observed peaks in Pb concentrations reflect pollution

****Editorial threshold 2: Provide firm and sufficient evidence to demonstrate that the presence of Olea is due to cultivation, which indicates human-induced landscape changes.

Additionally, we encourage you to incorporate archaeological and archaeobotanical evidence of plant resources exploited by past human communities in the region, as we believe this would strengthen the paper.

If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred.

Important: The response to reviewers must not include any figures, tables or graphs. If you wish to respond to the reviewer reports with additional data in one of these formats, please add them to the main article or Supplementary Information, and refer to them in the rebuttal. Due to current technical limitations, any figures, tables, or graphs embedded in your rebuttal will not be included in the peer review file, if published.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

Link Redacted

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Yiming Wang, PhD
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Carolina Ortiz Guerrero, PhD
Associate Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

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• Life sciences
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For your information, you can find some guidance regarding format requirements summarized on the following checklist: (https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

In this manuscript, Koutsodendris et al., present a series of interesting Pb and pollen records from terrestrial and marine Greece. Their data suggest early pollution (prior to much of the rest of Europe) in the Bronze age, and region-wide uptick at 2100 yr BP. From what I can tell, the data are reliable, the findings are certainly of interest, and the manuscript is well written and clear. I am delighted to see another group working on this region's palaeopollution history, and the work will be of great value. I am certain the work should be published, but in its current form it is not quite there. My comments are below, with a number of issues I think must be addressed prior to publication (major comments) followed by some smaller concerns.
Major comments
• The primary finding of an uptick in Pb content at c. 2100 yr BP is interesting and very worthy of focussing on. However, I find the lack of statistical analysis means it is a slight challenge to be sure the uptick truly is significant. I would suggest adding some changepoint analysis to the work, which would help narrow down exactly when statistically significant shifts appear in the Pb datasets, and confirm the existence of this change at 2100 yr BP.
• This leads on to another major issue – the presentation of un-normalised Pb data as a proxy for metallurgy/pollution. It is standard in this field to normalise to a lithogenic element (Sc, Zr etc. – see Shotyk et al. 2000). I think this must be done

before analysis of the trends is completed – it may be the early Pb peaks are just lithogenic influx rather than pollution.
• The early peaks are especially interesting in this regard – as the authors point out, this sort of Pb enrichment has not been observed anywhere else, even in the earliest enrichment in the Serbian record presented in comparison. It would be fascinating to prove the fidelity of these peaks, but without calculation of enrichment factors and PbAnthro levels, it's hard to see what is just natural. Are these peaks they truly reflective of very early metallurgy/pollution? Or do they indicate the bog does not have "extraordinary fidelity in recording atmospheric signals (L78)"? Ombrotrophy in a bog can be proven via various methods – I'd just like to see some more evidence.
• I would also like a section discussing why there is no evidence of pre-Roman Pb. Surely the Classical & Hellenistic Greek periods, known to have been extremely advanced, would have left a signal?
• Comparison with the data from Crveni Potok is suitable, but direct comparison of absolute Pb levels is not (L174-177). Absolute Pb can be impacted by a range of things, including the accumulation rate and the location. In addition to calculating PbAnthro and EFs, I think Pb accumulation rates would help when directly comparing (see methods in Longman et al., 2018).
• Figure 3 is nice but I think a summary/synthesis of the dates would really help the authors make their conclusions clear. For example, taking the 'dark blue' maxima, and plotting a cumulative probability diagram would really help to identify when each of these peaks occur. This could then help the interpretation of the data.
• In a similar vein, some moving averages would help the reader to understand the primary variability in the Pb datasets – all the marine cores have high variability so it's quite hard to see what is just noise and what is truly Pb enrichment/depletion.
• I'd like to see the ICP-MS data presented in the main manuscript, rather than relegated to the SI. The use of core scanning XRF for Pb-based palaeopollution reconstructions is not yet a standard method, and so the ICP data are appreciated to ground-truth the assertions. Maybe these data could be added to Fig. 2?
• I think the introduction needs a bit of streamlining. I include specific comments below, but there are bits which I think are unnecessary and/or should go into other sections (e.g. the Br data should not be in the intro).

Minor Comments
L28: What do you mean by 'previously thought'? Maybe say 'earlier than previous archaeological evidence'
L42: The timing of the Bronze Age varies across Europe – clarify here you mean the Greek Bronze Age.
L48-49: I think this is largely as marine cores have issues distinguishing natural vs anthropogenic signals. Compared to an ombrotrophic peat core which has mainly atmospheric input marine cores are a much more mixed provenance. I'd add a line here to maybe explain this.
L52: I don't think you can say you reconstruct impact, rather the signal of the activity. You don't look at how the marine environment responds to humans.
L60-63: This can be removed – it reads like a wrap-up sentence which should come at the end of the intro, not halfway through.
L67: I think citing our synthesis here may make this point clearer (Longman et al., 2020, Sci Reps).
L64-99: This is a very long paragraph and ends up quite meandering. I would suggest restructuring in to a couple of shorter paragraphs which cover 1) the work one Pb you discuss in lines 64-70. I would then have 2) a paragraph on the archaeology discussed at the end and then 3) the study sites and methodologies.
L100-104: This is results – the Br data and chronology should not be discussed until then.
L104: Here there should be a 'Here we present' style sentence leading into the remainder of the introduction.
L122-123; Can you do some statistical comparison, i.e. binning the XRF data and constructing regressions to compare to the ICP? I agree that the data look similar, but eyeballing isn't hugely statistically rigorous.
L128: I find this title a bit odd – this section focusses on the uptick around 2100 yr BP, but the following paragraphs discuss the earlier peaks (e.g. 5200 yr BP – L170). Also, as commented on above, working out enrichment factors and PbAnthro values can help to identify what is anthropogenic and what is natural Pb.
L132: And also to different locations, and sources of pollution? As your sites are spread around a bit it's to be expected they will have variability controlled by forces beyond radiocarbon uncertainty.
L147: Make it clear why monetized society could control this shift.
L162: An interesting contrast – suggests different centres of metallurgy at this time.
L172-174: As mentioned above, this claim needs better evidence. Is it statistically significant? Is it not just natural Pb input.
L176: Direct comparison of Pb content makes no sense – need to consider accumulation rates.
L188: Be specific – which mining centres are you talking about? I also think this section could be expanded – it's a fascinating finding but is sort of glossed over.
L206: Can you add in a little more about post-Roman pollution in the cores?
L214: I find it a bit odd that no charcoal data are presented in this section. If available, I think they would help support the claims.
L215: Can you also comment on the early Pb pollution peaks, and whether the palynology shows any changes related to them?
Fig. 2 needs some units on the left side of the graph

References
Longman, J., Veres, D., Finsinger, W. and Ersek, V., 2018. Exceptionally high levels of lead pollution in the Balkans from the Early Bronze Age to the Industrial Revolution. Proceedings of the National Academy of Sciences, 115(25), pp.E5661-E5668.
Longman, J., Ersek, V. and Veres, D., 2020. High variability between regional histories of long-term atmospheric Pb pollution. Scientific Reports, 10(1), p.20890.
Shotyk, W., Blaser, P., Grünig, A. and Cheburkin, A.K., 2000. A new approach for quantifying cumulative, anthropogenic, atmospheric lead deposition using peat cores from bogs: Pb in eight Swiss peat bog profiles. Science of the Total Environment, 249(1-3), pp.281-295.

Jack Longman

Reviewer #2 (Remarks to the Author):

This paper highlight the early and significant human-induced environmental changes in the Aegean region, demonstrating the extensive impact of ancient societies on their surroundings. The novelty of this work is the application of Pb pollution analysis. This study shows a significant increase in Pb pollution around 2,100 cal. years BP, with up to a threefold rise. This pollution left a clear imprint across both terrestrial and marine environments in the Aegean region.

What I find should be improved is the Correlation with Human Activities. The authors state that the first signs of marine pollution coincide with maximum deforestation and agricultural expansion. But agricultural expansion is based in some cases only in the expansion of Olea. It is important to note that the pollen of Olea could belong to cultivated Olea europaea but also to a wild variety. The authors should justify why they think Olea is cultivated and is not expanding due to climate conditions. The decline in deciduous Quercus and subsequent expansion of Olea could be motivated by a drier climate, a more prolonged summer drought, etc. Is this cultivation of Olea supported by archaeological evidence? The use of wood of Quercus deciduous for metallurdy is supported by archaeological evidence? If yes, please mention it explicitly. If this vegetation change is documented in a vast region (also in coastal Asia minor), perhaps is related to significant natural changes in regional vegetation? I need to see more strenght in the arguments related to human-induced landscape changes...

In lines 84-85, the authors say "the vast majority of pollen and spores are deposited within a few tens of km from their parent plants". This is not true at all, there are a lot of plants whose pollen is dispersed by animals/insects, and even autogamous plants (i.e. cereals), and the dispersion range of these plants is restricted to less than 1km, even a few tens of metres. Please rephrase it considering diverse and complex mechanisms of pollination.

In conclusion, the authors should make an effort on the justification of human-induced landscape changes. I suggest using archaeological information to compare changes in vegetation dynamics with archaeobotanical evidence of plant resources which were exploited by past human communities.

Reviewer #3 (Remarks to the Author):

In the article, “Anthropogenic impact on terrestrial and marine environments linked to societal change in Ancient Greece”, Koutsodendris et al., have conducted an interdisciplinary effort to establish the nature and timing of anthropogenic Pb pollution and environmental impacts around the Aegean Sea, including a variety of methods comprising radiocarbon calibration and age-depth modelling, ICP-MS, XRF, and palynological assessments. This pushes back the suggested earliest onset of Pb pollution in the region to 5,200 BP, but demonstrates that for several thousand years the environmental impacts of humans were limited to relatively small areas surrounding settlements. A “jump” in Pb concentrations is evidenced in many of the records around 2,100 BP, which the authors suggest reflects an intensification of metallurgy at the transition from the Hellenistic to the Roman Period. Pollen records from the surrounding region are also presented, and their age-models reconstructed, to suggest coincident human-induced vegetation change. This is stated as a “turning point” in the environmental history of the region: when human impact became more significant than natural drivers. Overall, the paper is excellent, written well, and confidently uses diverse datasets to reach a reasonable conclusion. Further, I think it is well within the scope of the journal and suitable for publication there. There are only two changes which I deem essential: (1) being more transparent about the chronologies of the records and (2) being more conservative about the presentation of 2,100 BP as an isochron marker, which I explain fully in the attached document. I also provide a few suggestions for changes that I believe could strengthen the manuscript, but these are not essential. Therefore, I recommend the paper to be accepted to Communications Earth and Environment following minor revisions.

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Version 1:

Decision Letter:

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Dear Dr Koutsodendris,

Your revised manuscript titled "Anthropogenic impact on terrestrial and marine environments linked to societal change in Ancient Greece" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewer #3. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Yiming Wang, PhD
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Carolina Ortiz Guerrero, Ph.D.
Associate Editor
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thanks to the authors for making all suggested changes. I am happy to recommend publication.

Jack Longman

Reviewer #2 (Remarks to the Author):

The authors have addressed perfectly the feedback from reviewers and have improved substantially the manuscript. I recommend its publication in its current form

Reviewer #3 (Remarks to the Author):

I am satisfied with the authors' response. Adjusting to my comments, and the feedback of the other reviewers, has vastly improved the quality of the manuscript and I believe it is ready for publication.

The only point of mine still to be corrected (which is not essential but I think important) is the fact that 1σ uncertainties are utilized should be mentioned within the methodology or results of the main paper, rather than just in the supplementary. Chronologies are a vital part of the paper's argument and thus total transparency is required.

Response to reviewers' comments (in blue)

Reviewer #1 (R1; Jack Longman)

In this manuscript, Koutsodendris et al., present a series of interesting Pb and pollen records from terrestrial and marine Greece. Their data suggest early pollution (prior to much of the rest of Europe) in the Bronze age, and region-wide uptick at 2100 yr BP. From what I can tell, the data are reliable, the findings are certainly of interest, and the manuscript is well written and clear. I am delighted to see another group working on this region's palaeopollution history, and the work will be of great value. I am certain the work should be published, but in its current form it is not quite there. My comments are below, with a number of issues I think must be addressed prior to publication (major comments) followed by some smaller concerns.

We thank R1 for his review of our manuscript. His constructive criticism and suggestions have helped us to further improve its quality, notably with regard to the statistical significance of the results supporting our interpretations and conclusions. We have considered all points raised by R1 in order to maximize the quality of the manuscript, and we provide specific, point-to-point responses to all his comments and suggestions below.

Major comments

The primary finding of an uptick in Pb content at c. 2100 yr BP is interesting and very worthy of focussing on. However, I find the lack of statistical analysis means it is a slight challenge to be sure the uptick truly is significant. I would suggest adding some changepoint analysis to the work, which would help narrow down exactly when statistically significant shifts appear in the Pb datasets, and confirm the existence of this change at 2100 yr BP.

We appreciate the suggestion off carrying out change-point analysis in order to underscore the robustness of our interpretations, and we have now performed change-point analyses for all dated records. The results suggest that the increase in Pb occurs at 2150 cal BP. We note that the c. 50 years difference from the results as presented in our original submission (i.e., the Pb increase occurring at c. 2100 cal BP) is well within the uncertainties of radiocarbon dating. Importantly, the now slightly older age does not affect the conclusions about, and implications of, the Pb pollution in the terrestrial and marine realms of the Aegean region. We have revised our manuscript accordingly, providing information on the methodology of the change-point analysis (lines) as well as presenting the results and implications (lines 135–138, 142–146, 177–178). The change-point analysis results are also provided in the supplementary information (i.e., Dataset S6).

This leads on to another major issue – the presentation of un-normalised Pb data as a proxy for metallurgy/pollution. It is standard in this field to normalise to a lithogenic element (Sc, Zr etc. – see Shotyk et al. 2000). I think this must be done before analysis of the trends is completed – it may be the early Pb peaks are just lithogenic influx rather than pollution.

Following this suggestion of R1, we have normalized the Pb data to Zr. The results confirm that even the earliest Pb peaks at the Tenaghi Philippon peat record (at c. 5200 BP) represent pollution. We have revised our manuscript accordingly in the Methods (lines 446–456) and Results/Discussion sections (lines 116–121, 128–132, 137, and 144), as well as Figures 2, 3, and 4.

The early peaks are especially interesting in this regard – as the authors point out, this sort of Pb enrichment has not been observed anywhere else, even in the earliest enrichment in the Serbian record presented in comparison. It would be fascinating to prove the fidelity of these peaks, but without calculation of enrichment factors and PbAnthro levels, it's hard to see what is just natural. Are these peaks they truly reflective of very early metallurgy/pollution? Or do they indicate the bog does not have "extraordinary fidelity in recording atmospheric signals (L78)"? Ombrotrophy in a bog can be proven via various methods – I'd just like to see some more evidence.

We thank R1 for this comment. To comply with his suggestion, we have calculated the enrichment factors and $Pb_{anthropogenic}$ levels following the methods described by Longman et al. (2018). The results support that the early Pb increase at Tenaghi Philippon indeed reflects early Pb pollution in the Aegean region. Considering the outcome of the change-point analysis (please see above), the onset of this pollution is at c. 5200 cal BP. We have revised our manuscript accordingly in the Methods (lines 446–456) and Results/Discussion sections (lines 178–179), as well as Supplementary Figure 2.

I would also like a section discussing why there is no evidence of pre-Roman Pb. Surely the Classical & Hellenistic Greek periods, known to have been extremely advanced, would have left a signal?

We agree with R1 that the Classical and Hellenistic periods should have left a Pb pollution signal. This is clearly documented in the Tenaghi Philippon record, where anthropogenic Pb increases above the natural, early Holocene baseline for the first time during the Bronze Age (at c. 5200 cal BP) and strongly increases during the Classical period (at c. 2350 cal BP). We note, however, that the Pb concentrations remain lower during the Iron Age as well as Classical and Hellenistic periods prior to 2350 cal. BP in comparison to the Bronze Age levels. Our hypothesis is that the sources of Pb pollution during that time may have been located further away from Tenaghi Philippon, e.g., in the southern Aegean region such as in Lavrion and on Sifnos island, therefore leaving a weaker pollution signal in this archive of the North Aegean borderlands.

Comparison with the data from Crveni Potok is suitable, but direct comparison of absolute Pb levels is not (L174-177). Absolute Pb can be impacted by a range of things, including the accumulation rate and the location. In addition to calculating PbAnthro and EFs, I think Pb accumulation rates would help when directly comparing (see methods in Longman et al., 2018).

We agree with R1. Following his suggestion, we have calculated the enrichment factor (EF) and $Pb_{anthropogenic}$ levels following the methods of Longman et al. (2018) (please see also comment above) in order to facilitate adequate comparison with the Crveni Potok data. Unfortunately, however, we cannot precisely calculate the density of the Tenaghi Philippon sediments because of the highly heterogenous nature of the unconsolidated fen peat that consists of leaves, roots and other plant remains (see Christanis, 1983; Pross et al., 2015). As such, we refrain from calculating changes in the accumulation rates using the formula from Longman et al. (2018). We note, however, that the radiocarbon-based age model for Tenaghi Philippon does not indicate significant changes in sedimentation rates between 1700 and 7000 years BP. We are therefore confident that this factor plays only a subordinate role in the Pb accumulation rates at the site. We elaborate on the application of the EF and $Pb_{anthropogenic}$ in our revised manuscript in lines (128–132, 446–456) and Supplementary Figure 2.

Figure 3 is nice but I think a summary/synthesis of the dates would really help the authors make their conclusions clear. For example, taking the 'dark blue' maxima, and plotting a cumulative probability diagram would really help to identify when each of these peaks occur. This could then help the interpretation of the data.

Following this suggestion of R1 we have now created a cumulative probability plot using the cumulative probability function (CDF) on the mean ages of the Pb increase as derived from the change-point analysis. This new plot is presented in the supplementary information (Figure S4) of our revised manuscript.

In a similar vein, some moving averages would help the reader to understand the primary variability in the Pb datasets – all the marine cores have high variability so it's quite hard to see what is just noise and what is truly Pb enrichment/depletion.

To comply with R1's suggestion we have added a 10-times moving average in the Pb datasets and revised Figures 2 and 4 accordingly.

I'd like to see the ICP-MS data presented in the main manuscript, rather than relegated to the SI. The use of core scanning XRF for Pb-based palaeopollution reconstructions is not yet a standard method, and so the ICP data are appreciated to ground-truth the assertions. Maybe these data could be added to Fig. 2?

We appreciate this suggestion of R1. However, Figure 2 already contains a substantial amount of information, and we feel that the addition of further complexities (such as adding the comparison of the XRF- and ICP-MS-based Pb contents) will make it very difficult to read. Moreover, the ICP-MS data are only used to confirm the sensitivity of the XRF core scanning in detecting changes in Pb contents. For these reasons we prefer to keep the comparison in the supplementary information (Figure S2). Please also note that we have added the anthropogenic and lithogenic Pb as well as the EF to this figure to comply with R1's suggestions (see comment above).

I think the introduction needs a bit of streamlining. I include specific comments below, but there are bits which I think are unnecessary and/or should go into other sections (e.g. the Br data should not be in the intro).

We have considered R1's suggestions on how to better streamline the Introduction section. In the following, we provide specific information on the changes made in the revised manuscript after each specific point that R1 has raised.

Minor Comments

L28: What do you mean by 'previously thought'? Maybe say 'earlier than previous archaeological evidence'

Following R1's comment we have rephrased this sentence as suggested (line 29).

L42: The timing of the Bronze Age varies across Europe – clarify here you mean the Greek Bronze Age.

We agree with R1 that the timing of the Bronze Age varies across Europe, and we have slightly rephrased the respective sentence to provide greater clarity (lines 43–44).

L48-49: I think this is largely as marine cores have issues distinguishing natural vs anthropogenic signals. Compared to an ombrotrophic peat core which has mainly atmospheric input marine cores are a much more mixed provenance. I'd add a line here to maybe explain this.

We agree with R1 that marine sediments derive from mixed provenances and hence are less sensitive in detecting the earliest pollution in comparison to peat sediments. We elaborate on this aspect later in the Introduction (lines 78–79) as well as in the Results/Discussion section (lines 200–205). Hence, we have not made changes in the respective sentence.

L52: I don't think you can say you reconstruct impact, rather the signal of the activity. You don't look at how the marine environment responds to humans.

To correct for this grammatical mistake, we have slightly rephrased the sentence (line 53).

L60-63: This can be removed – it reads like a wrap-up sentence which should come at the end of the intro, not halfway through.

We agree with R1 and have removed this sentence.

L67: I think citing our synthesis here may make this point clearer (Longman et al., 2020, Sci Reps).

We have added the suggested paper as recommended.

L64-99: This is a very long paragraph and ends up quite meandering. I would suggest restructuring in to a couple of shorter paragraphs which cover 1) the work one Pb you discuss in lines 64-70. I would then have 2) a paragraph on the archaeology discussed at the end and then 3) the study sites and methodologies.

We agree with R1 that this was a long paragraph. To comply with his suggestion and achieve maximum clarity for the readers, we have split it into three shortened paragraphs. We have not, however, changed the internal structure as we feel that the geochemical methods should be discussed separately from the palynological methods.

L100-104: This is results – the Br data and chronology should not be discussed until then.

We respectfully disagree with R1 on this point: This half sentence does not present results – it only mentions methods that will be used in order to pave the way for what the paper is aiming for, i.e., resolving human-induced environmental change at timescales relevant for sociocultural transformations. We have therefore refrained from removing it from our revised manuscript (lines 100–102).

L104: Here there should be a 'Here we present' style sentence leading into the remainder of the introduction.

We thank R1 for the suggested change in the style of writing. However, we have refrained from changing the specific sentence – please note that such a 'Here we present' sentence already appears in line 53.

L122-123; Can you do some statistical comparison, i.e. binning the XRF data and constructing regressions to compare to the ICP? I agree that the data look similar, but eyeballing isn't hugely statistically rigorous.

We appreciate this comment. However, we believe our argument that the Pb increase in the XRF records as defined by change-point analysis is synchronous to a more than 10-fold increase in Pb concentrations as measured by ICP-MS to be clear, reproducible and indisputable. As such, we do not think that further statistical analysis for the comparison of the XRF and ICP-MS data would improve clarity.

L128: I find this title a bit odd – this section focusses on the uptick around 2100 yr BP, but the following paragraphs discuss the earlier peaks (e.g. 5200 yr BP – L170). Also, as commented on above, working out enrichment factors and PbAnthro values can help to identify what is anthropogenic and what is natural Pb.

We are admittedly not entirely sure what R1 would like to suggest here. The title in line 128 'Onset of anthropogenic Pb pollution in the Aegean region' (line 134 in the revised manuscript) refers to a section focussing exclusively on the Pb increase at c. 2150 cal BP. Instead, the discussion on the earlier time intervals comprises a different section with a different title, i.e., 'Pb pollution in the Aegean region within a historical context' (line 150). As such, we have not changed the structure of this part of the discussion. However, we agree with R1 on the importance of elaborating on the enrichment factors and $Pb_{anthropogenic}$ (please see also comment above), and we have revised the text accordingly (lines 128–132, 446–456; Figure S2).

L132: And also to different locations, and sources of pollution? As your sites are spread around a bit it's to be expected they will have variability controlled by forces beyond radiocarbon uncertainty.

We agree with R1 and have slightly rephrased the respective sentence (line 141).

L147: Make it clear why monetized society could control this shift.

We agree with R1 that it is important to achieve more clarity for the readers on why monetized societies would be responsible for the onset of Pb pollution. We have therefore slightly rephrased this sentence (line 152) in order to pave the way for a detailed explanation that we provide in the section 'Pb pollution in the Aegean region within a historical context' (lines 150–211).

L162: An interesting contrast – suggests different centres of metallurgy at this time.

We are pleased that R1 agrees with us on this very interesting result.

L172-174: As mentioned above, this claim needs better evidence. Is it statistically significant? Is it not just natural Pb input.

As discussed above, this finding is statistically significant based on Pb normalisation, enrichment factors, and $Pb_{anthropogenic}$. Moreover, the timing of this increase is confirmed by the results of the change-point analysis (see Supplementary Dataset 6). With the incorporation of this information in various parts of the revised manuscript (e.g., lines 128–132, 135–138, and 178–179) the aspect commented on by R1 has been strengthened substantially.

L176: Direct comparison of Pb content makes no sense – need to consider accumulation rates.

We thank R1 for this comment. Unfortunately, it is not possible to correct the Pb contents considering accumulation rates using the formula of Longman et al. (2018) because the density of the samples cannot be precisely calculated (see comment above). However, we note that our radiocarbon-based model as well as previous radiocarbon- and cryptotephra-based age models (Müller at al., 2011; Wulf et al., 2018) for the Tenaghi Philippon record allow to exclude significant changes in accumulation rates across the interval corresponding to the Bronze Age (i.e., c. 5,200–3,200 BP). Both aspects argue against a strong effect of changing sedimentation rates on the Pb content. Nevertheless, and in an effort to avoid any misinterpretation of the Pb signals from Tenaghi Philippon and Crveni Potok we have decided to delete the respective sentence from the revised version of the manuscript.

L188: Be specific – which mining centres are you talking about? I also think this section could be expanded – it's a fascinating finding but is sort of glossed over.

We appreciate this suggestion of R1, and we have now specified several mining sites that are known to have been active before the Late Bronze and Iron Ages (lines 193–194). These include (i) Limenaria on Thasos Island, which is located c. 40 km south of Tenaghi Philippon and is one of the earliest mining centers in the Aegean region since (at least the Chalcolithic; Maran, 2022); (ii) Pangaion mountain, which delimits the Tenaghi Philippon peatland to the west and was mined already during the Bronze Age (Unger, 1987); and (iii) SE Bulgaria, which is widely considered a mining center during the Bronze Age and even earlier, but is yet lacking robust archaeological evidence (e.g., Stos-Gale and Gale, 2009; Maran, 2022). We also provide references for each of these mining centers in our revised manuscript such that interested readers will have straightforward access to in-depth information on the history of these centers.

L206: Can you add in a little more about post-Roman pollution in the cores?

A discussion on the post-Roman Pb pollution is provided in the beginning of the respective section of both our original and revised manuscript (lines 161–175).

L214: I find it a bit odd that no charcoal data are presented in this section. If available, I think they would help support the claims.

We appreciate this comment of R1. Charcoal data that could be added to the discussion are not available for the records presented here. We would like to note, however, that charcoal can be transported to the study sites from a very wide catchment area, and hence it can originate from fires occurring in very distal settings (e.g., Clark, 1988). Moreover, charcoal concentrations depend on the type and availability of burning biomass (e.g., Marlon et al., 2006; Vanniere et al., 2011). Additionally, the size of charcoal may be significantly altered during transport (e.g., Clark, 1998; Finsinger et al., 2014). Together, a direct comparison of charcoal records from the study sites may not be a straightforward method for unraveling the timing and extent of deforestation in the Aegean Sea sub-regions.

L215: Can you also comment on the early Pb pollution peaks, and whether the palynology shows any changes related to them?

The early Pb pollution peaks at Tenaghi Philippon during the Bronze Age fall within a period of deforestation and expansion of agricultural as documented by the pollen data and further supported by archaeobotanical evidence (please see also responses to the comments of R2 below). We elaborate on these vegetation changes in various parts of our revised manuscript (e.g., lines 286–290). However, considering the merely local nature of this vegetation change at that time (please see in-depth discussion in lines 275–313) and the lack of Pb isotope data that could confirm the origin of the Pb pollution in the northern Aegean borderlands, we have refrained from expanding the discussion on this issue in our manuscript.

Fig. 2 needs some units on the left side of the graph

We understand the notion of R2 regarding this point. However, normalised XRF elemental data do not have units (see also Weltje & Tjallingii, 2008; Bertrand et al., 2024); hence, we have kept Figure 2 of the manuscript unchanged.

Reviewer #2 (R2)

This paper highlight the early and significant human-induced environmental changes in the Aegean region, demonstrating the extensive impact of ancient societies on their surroundings. The novelty of this work is the application of Pb pollution analysis. This study shows a significant increase in Pb pollution around 2,100 cal. years BP, with up to a threefold rise. This pollution left a clear imprint across both terrestrial and marine environments in the Aegean region. What I find should be improved is the Correlation with Human Activities. The authors state that the first signs of marine pollution coincide with maximum deforestation and agricultural expansion. But agricultural expansion is based in some cases only in the expansion of Olea.

We thank R2 for her/his constructive comments that have helped to further improve our manuscript. We agree that we have mostly used the *Olea* pollen abundances as an indicator of agricultural practices. This is based on the fact the olive trees produce a high amount of pollen grains that spread on regional scales and hence can be used to compare sites across the Aegean region. Following R2's suggestions below, we also elaborate on signals from other agricultural indicators such as cereals and wine, which comprise lower pollen producers than *Olea*, in our revised manuscript (lines 224–235).

It is important to note that the pollen of Olea could belong to cultivated Olea europaea but also to a wild variety.

We thank R2 for this comment. Although it is not possible to differentiate between the wild and cultivated *Olea* based on palynological analysis, such a differentiation can be achieved through the analysis of the morphological structure of olive stones (e.g., Terral et al., 2004, 2021; Margaritis and Jones, 2008; Newton et al., 2014). The comparison of olive stones found at archaeological sites from Neolithic to Hellenistic times provides evidence for the expansion of *Olea* cultivation during the Bronze Age, first in the southern Aegean region, and later in northern Greece during the Iron Age and historical times (please see in-depth discussion in Valamoti et al., 2018); these findings are in agreement with the timing of the increase in *Olea* pollen percentages across the Aegean region. We elaborate on this and have also added relevant references in our revised manuscript in lines 225–230 and 264–266.

The authors should justify why they think Olea is cultivated and is not expanding due to climate conditions. The decline in deciduous Quercus and subsequent expansion of Olea could be motivated by a drier climate, a more prolonged summer drought, etc. Is this cultivation of Olea supported by archaeological evidence?

We agree with R2 that the expansion of *Olea* and the decrease in deciduous *Quercus* could principally be related to a shift to drier climate conditions. However, an enhancement of dry conditions would also cause the expansion of other Mediterranean evergreen taxa that have similar climatic tolerances and occur in similar habitats as *Olea*, such as evergreen *Quercus*, *Pistacia*, and *Phillyrea* (e.g., San-Miguel-Ayanz et al., 2016). Because the pollen data do not document an increase of these taxa, a natural, climatic trigger for the expansion of *Olea* at the expense of deciduous *Quercus* can be firmly ruled out.
To provide maximum clarity on this issue we have further expanded the discussion and provided relevant references on (i) the paleoclimatic results that allow to exclude a climatic trigger for the expansion of *Olea* and (ii) on the time-transgressive deforestation and arboriculture in the Eastern Mediterranean region (Beyşehir Occupation Phase; see response to comments of R3 below) in our revised manuscript (lines 299–306). We now also further elaborate on the fact that there is archaeobotanical and anthracological evidence for the cultivation of *Olea* during the Bronze Age in the Aegean region (e.g., Renfrew et al., 2013; Valamoti et al., 2018; Mavromati, 2022) (lines 228–230, 264–266).

The use of wood of Quercus deciduous for metallurdy is supported by archaeological evidence? If yes, please mention it explicitly.

Indeed, there is archaeological evidence for the use of *Quercus* wood in metallurgy already for the Neolithic period. Horne (1982) reports the existence of written texts dating back to the Sumerian and Akkadian periods that *Quercus* wood made the best charcoal for smelting. Healy (1978) provides support for the use of *Quercus* for charcoal and smelting in Ancient Greece. Even more specifically for the Bronze Age of the Aegean region, the integration of archaeobotanical, phytolith and anthracological studies of ovens in pyrotechnological workshops at Tiryns (NE Peloponnese) has provided evidence for the use of *Quercus* wood alongside *Pinus* and *Abies* wood for fire making (Buckley et al., 2021). We now elaborate on these findings, which further strengthen our interpretation, in lines 248–250 and 266–268 of our revised manuscript also adding relevant references.

If this vegetation change is documented in a vast region (also in coastal Asia minor), perhaps is related to significant natural changes in regional vegetation? I need to see more strenght in the arguments related to human-induced landscape changes...

As mentioned above, we discuss the absence of regional climate change as the trigger for the vegetation change in the Eastern Mediterranean region based on both paleoclimatic and archaeological information. We now elaborate on this issue more extensively in lines 299–306 of our revised manuscript (please see also response to R3 comments below) also adding relevant references.

In lines 84-85, the authors say "the vast majority of pollen and spores are deposited within a few tens of km from their parent plants". This is not true at all, there are a lot of plants whose pollen is dispersed by animals/insects, and even autogamous plants (i.e. cereals), and the dispersion range of these plants is restricted to less than 1km, even a few tens of metres. Please rephrase it considering diverse and complex mechanisms of pollination.

We agree with R2 that most pollen grains (and even more so spores) are deposited very close to their parent plants. However, it is important to note that a small fraction of pollen grains (and also spores) does become dispersed much further – up to hundreds or even thousands of kilometers (see, e.g., Sofiev et al., 2013, for an overview on the different scales of pollen dispersal). The long-distance transport does not only affect wind-pollinated taxa, but also insect-pollinated taxa (albeit to a lesser extent given the fundamentally different dispersal strategies). An example specifically for the Aegean region is the work of Kotthoff et al. (2008), which documented pollen from entomophilic taxa in a marine core from the northern Aegean Sea. While these aspects are clearly beyond the scope of the present paper, they had prompted us to use the phrasing “the vast majority of pollen and spores are deposited within a few tens of kilometres from their parent plants”. Importantly, this phrasing is fully compliant with what we feel R2 wishes to emphasize, i.e., the limited dispersal distance of many pollen grains. The dispersal distances mentioned by R2 all fall within the meaning of our phrasing – hence, there is no contradiction between the phrasing and what R2 states. In an effort to achieve maximum clarity for the readers, we have slightly rephrased this sentence to “the vast majority of pollen and spores are deposited within a few tens of kilometres from their parent plants or even less for some cultivars (e.g., Cerealia and *Vitis*)” (lines 85–86), and we have provided supporting references in the revised version of our manuscript.

In conclusion, the authors should make an effort on the justification of human-induced landscape changes. I suggest using archaeological information to compare changes in vegetation dynamics with archaeobotanical evidence of plant resources which were exploited by past human communities.

We agree with R2 that our arguments about human-induced environmental change can benefit from additional archaeological evidence. We have therefore revised our manuscript to include more archaeobotanical and anthracological evidence from archaeological sites across the Aegean region (e.g., lines 228–230, 232–235, and 264–268) that unequivocally support agricultural practices and human-induced deforestation.

Reviewer #3 (R3)

In the article, "Anthropogenic impact on terrestrial and marine environments linked to societal change in Ancient Greece", Koutsodendris et al., have conducted an interdisciplinary effort to establish the nature and timing of anthropogenic Pb pollution and environmental impacts around the Aegean Sea, including a variety of methods comprising radiocarbon calibration and age-depth modelling, ICP-MS, XRF, and palynological assessments. This pushes back the suggested earliest onset of Pb pollution in the region to 5,200 BP, but demonstrates that for several thousand years the environmental impacts of humans were limited to relatively small areas surrounding settlements. A "jump" in Pb concentrations is evidenced in many of the records around 2,100 BP, which the authors suggest reflects an intensification of metallurgy at the transition from the Hellenistic to the Roman Period. Pollen records from the surrounding region are also presented, and their age-models reconstructed, to suggest coincident human-induced vegetation change. This is stated as a "turning point" in the environmental history of the region: when human impact became more significant than natural drivers. Overall, the paper is excellent, written well, and confidently uses diverse datasets to reach a reasonable conclusion.

We appreciate R3's thorough, very positive assessment of our paper, and we would like to thank her/him for the constructive comments and suggestions.

Further, I think it is well within the scope of the journal and suitable for publication there. There are only two changes which I deem essential: (1) being more transparent about the chronologies of the records and (2) being more conservative about the presentation of 2,100 BP as an isochron marker, which I explain fully in the attached document.

We thank R3 for highlighting these two essential aspects for our manuscript. We have carefully elaborated on the chronological uncertainties of our records and have toned down the Pb increase as an isochron marker in our revised manuscript. Below, we provide specific information on the changes made in the manuscript to accommodate R3's suggestions.

I also provide a few suggestions for changes that I believe could strengthen the manuscript, but these are not essential. Therefore, I recommend the paper to be accepted to Communications Earth and Environment following minor revisions.

We thank R3 for her/his very positive assessment of our manuscript. Below we provide specific, point-to-point responses to all her/his comments and suggestions on the attached file.

(1) Chronology:

The cores discussed are reasonably well-dated. However, some important pieces of information are missed in the discussion and supplementary that are required for transparency.

- It needs to be made clear whether the radiocarbon age uncertainties are given as 1σ or 2σ. I am assuming 1σ based on the relatively low uncertainties given, this is important for other researchers utilizing your data in new studies.

To comply with this suggestion of R3 we have provided the radiocarbon age uncertainties (i.e., 1σ) in Supplementary Table S2 and Dataset S1.

- The chronologies for all sites were (re)calculated according to dates recalibrated according to IntCal20. These recalibrated dates should be included in Supplementary Table 2 and the other Supplementary file.

To comply with R3's suggestion we have provided the recalibrated radiocarbon ages of the previously published pollen data in Supplementary Dataset S2.

- It might also be easier for the reader if you include an additional column with the references for different radiocarbon dates used, rather than including them in the Table S2 caption.

To comply with this suggestion of R3 we have added the references for different radiocarbon ages in Supplementary Table S2.

(2) Isochron Marker:

- I appreciate the idea of Pb as a "new" marker for comparison between many archives: sediment cores, tree rings, speleothems and ice-cores (lines 289-292). This is a great point and a proxy currently under-utilised (alongside many others!).

We are pleased that R3 agrees with our suggestion to use Pb as a marker for the comparison of different types of depositional archives.

- However, I don't think 2,100 BP can be used as chronological marker (isochron) for new records or to improve chronologies because even within the records you discuss, there is not agreement on the exact timing – they are all around 2,100 BP. The timing of the start of Pb pollution would not be the same across all sites, nor would the start of exploitation of the various Pb ore deposits you indicate in Figure 1.

We agree with R3 that the available records cannot provide an unequivocal onset for the Pb pollution in the Aegean region. However, considering the uncertainties in radiocarbon dating of several decades (e.g., Hajdas et al., 2021) and the lack of datable material in some deposits, we do feel that the Pb increase at c. 2150 BP can serve as a direct tie point across a wide variety of environmental settings. A similar concept has been extensively used in the Eastern Mediterranean region with regard to Sapropel S1 (e.g., Marino et al., 2009) despite the fact that the timing of deposition (i.e., onset and end) varies across different basins (e.g., Rohling et al., 2015; Filippidi et al., 2016). To provide more clarity we have elaborated on the above in the revised version of our manuscript (lines 319–321).

- Using the Pb isochron as a marker for the start of extensive anthropogenic impact across the whole Eastern Mediterranean (Lines 308-311) is even more of a stretch given the focus of this study (Aegean). You should merely suggest this could be the case and argue for additional research in other regions such as Anatolia and the Levant, or keep your conclusions to the Aegean region.

We agree with R3 and have toned down our conclusions accordingly; we call for additional research in neighbouring regions such as Anatolia, the Levant and the Italian Peninsula (lines 342–344).

Beyşehir Occupation Phase:

- An important concept worth discussing in the sections on vegetation is the Beyşehir Occupation Phase (BOP). This period of decline in deciduous tree-pollen and increased grass pollen (incl. cereals) and cultivated trees (OJCV) is primarily discussed for other regions of the Eastern Mediterranean (Anatolia, the Levant etc.), but matches nicely with your pollen data (you discuss this, but do not name it, for Belevi, Elaia, Bafa). Varied onset dates are given between ~3,800 and ~2,150 BP, with the onset of many of the studied sites centered around 2,300 BP (see Roberts, 2018).

We thank R3 for this comment and now elaborate on the Beyşehir Occupation Phase in the revised version of our manuscript (lines 299–304).

Climate:

- The main period you discuss, of surging Pb and BOP-esque pollen results, is centered on 2,100 BP. Many authors have discussed the potential impact of the Roman Climatic Optimum (starting ~2,200 BP) on these developments. For example, Ören (2023) discusses the BOP in the context of climate and suggests “favourable and stable climatic conditions from 250 BCE to 300 CE” played a role in intensification of agriculture. This paper, and many others, are rather deterministic and paint with a wide brush. Other authors (e.g., Jacobson et al., 2022; Labuhn et al., 2018) have argued against this link, suggesting that, at least in some regions, similar changes occurred despite significantly drier conditions. You already state on lines 225-228: “At the same time, the percentages of steppic taxa (i.e., Artemisia, Chenopodiaceae, Ephedra) do not increase in any of the two records, which excludes climatic forcing as a reason for the decrease of deciduous tree-pollen percentages.” This is great, but does not exclude a “climate -> human land use” mechanism for the decrease in tree pollen, as is more commonly claimed.

Palaeotemperature records in the region are generally lacking – in terms of number, resolution and dating accuracy – however, some are derived from the cores you mention. On the other hand, palaeohydrological records are abundant (see below table for some of the records) in the surrounding region (including from some of the lakes you mention).

Archive	Citation	Archive	Citation
Southern Balkans		Pheneos	(Seguin et al., 2020b)
Shkodra	(Zanchetta et al., 2012)	Stymphalia	(Heymann et al., 2013; Seguin et al., 2020b)
Dojran	(Francke et al., 2013)	Kapsia	(Finné et al., 2014)
Ohrid	(Lacey et al., 2015)	Lerna	(Katrantsiotis et al., 2019)
Prespa	(Leng et al., 2010)	Asea	(Unkel et al., 2014)
Skala Marion	(Psomiadis et al., 2018)	Agios Floros	(Norström et al., 2018)
M2	(Gogou et al., 2016)	Gialova	(Katrantsiotis et al., 2018)
Butrint	(Morellón et al., 2016)	Mavri Trypa	(Finné et al., 2017)
Ioannina	(Frogley et al., 2001)	Alepotrypa	(Boyd, 2015)
SL148	(Kuhnt et al., 2007)	**Western Anatolia**	
The Peloponnese		Sofular	(Fleitmann et al., 2009)
Trichonida	(Seguin et al., 2020a)	Çubuk	(Ocakoğlu et al., 2016)
Klisova	(Emmanouilidis et al., 2022)	Simav	(Ocakoğlu et al., 2022)
Aliki	(Emmanouilidis et al., 2020b)	Salda	(Danladi and Akçer-Ön, 2018)
Vouliagmeni	(Emmanouilidis et al., 2020a)	Kocain	(Jacobson et al., 2021)
Limnon	(Peckover et al., 2019)	Gölhisar	(Eastwood et al., 2007)
Kaisari	(Seguin et al., 2020b)	Dim	(Ünal-İmer et al., 2015)

I think the previously argued climate-environment theories deserve a little bit of further exploration in the paper, but are still relatively easy to disregard due to the heterogeneity of climatic conditions across the region and through time (i.e., there is no ubiquitous period of “favourable” climatic conditions across the entire region). There is no need to cite specific records, but mentioning this point would further invalidate climate as a factor in the vegetation changes. I think this could be resolved in a couple of sentences.

We appreciate R3’s comment on the debate on climate variability as the trigger for vegetation change in the Eastern Mediterranean region. We fully agree with R3 that the Aegean region is characterized by considerable spatiotemporally heterogenous climate variability since the Neolithic. By extension, although climate change may have affected vegetation locally, it appears very unlikely that it was the trigger for regional deforestation and arboriculture since the Bronze Age. We elaborate on this issue in our revised manuscript (lines 299–306), and have also added relevant references.

- For the Eastern Mediterranean, this heterogeneity of hydroclimatic conditions has been emphasized in many studies, both in discussions of the present day (e.g., Ulbrich et al.,2012) and the Mid-Late Holocene (e.g., Jacobson et al., 2021; Katrantsiotis et al., 2019). In general, we expect the climate to vary significantly across relatively small distances, so this is not a shock.

We fully agree with R3 about the hydroclimate heterogeneity in the Aegean region. To comply with the R3’s suggestion, we cite the most review study on the climate variability in the Aegean Sea during the Holocene (Jacobson et al., 2024), that documents the absence of prolonged dry periods across the entire Aegean region since the Neolithic (lines 302–306).

Minor Comments:

- Line 155-8 – surely this is the result of proximity to the mining activities that were disrupted? You state this on lines 177-80, but I think it should be introduced here.

We agree with this suggestion and have modified our revised manuscript providing information on proximal to Tenaghi Philippon mining centers (lines 165–166).

- I would encourage the authors to also upload their data to a database (e.g., Neotoma or Zenodo) and include a reference to this in the data availability section.

We agree with R3 that the data should be made publicly available (in fact, this is something that we do with regard to all papers published by our group). In addition to providing the data as supporting information that comes along with our paper, we have also submitted them to the SeaDataNet database (www.seanoe.org) (https://doi.org/10.17882/102344).

English: The English is great throughout, three small comments:

- Line 202 doesn't read quite right, I would use either "by the subordination of Greece to the Roman Empire after 146 BCE." Or "by the subordination of Greece to Rome after 146 BCE."

We thank R3 for the linguistic guidance and have corrected the sentence as suggested (line 208).

- Line 218 – the phrasing here is a little confusing, reads as if the Pb surge corresponds to the onset of agricultural indicators that were already present.

To achieve maximum clarity for the readers we have corrected the sentence (line 225).

- Line 330 I believe should read "foraminifera"

Corrected as suggested (line 364).

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