nature portfolio

Peer Review File

Multidisciplinary analyses and ancient DNA reveal social inequality and mobility in the Central Plains during the Eastern Zhou period in China

Corresponding Author: Professor Chuan-Chao Wang

Version 0:

Decision Letter:

4th March 2025

Dear Professor Wang,

Thank you once again for your manuscript, entitled "Multidisciplinary analysis reveals the social inequality patterns in the Central Plains of the Eastern Zhou Dynasty in China", and for your patience during the peer review process.

Your Article has now been evaluated by 3 referees. You will see from their comments copied below that, although they find your work of potential interest, they have raised quite substantial concerns. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version if you are willing and able to fully address reviewer and editorial concerns.

We hope you will find the referees' comments useful as you decide how to proceed. If you wish to submit a substantially revised manuscript, please bear in mind that we will be reluctant to approach the referees again in the absence of major revisions. We are committed to providing a fair and constructive peer-review process. Do not hesitate to contact us if there are specific requests from the reviewers that you believe are technically impossible or unlikely to yield a meaningful outcome.

To guide the scope of the revisions, the editors discuss the referee reports in detail within the team, including with the chief editor, with a view to (1) identifying key priorities that should be addressed in revision and (2) overruling referee requests that are deemed beyond the scope of the current study. We hope that you will find the prioritised set of referee points to be useful when revising your study. Please do not hesitate to get in touch if you would like to discuss these issues further.

Specifically, please prioritize the following:
- Reviewer #1 suggested potential extension in how genetic analysis can also contribute to illuminate social inequality besides past migration. Please improve your methods description and framing in response to the Reviewer #1’s comments.
- Both Reviewer #2 and Reviewer #3 commented on the small sample size and non-representativeness of your sample. Please respond to their comments, discuss this limitation and tune down your claims to be suggestive in your revision.
- Reviewer #3 raised several inaccuracies in your historical contextualization, please revise the terms accordingly.
- Both R1 and R2 are concerned about omitted literature, please include relevant references as suggested.

Finally, your revised manuscript must comply fully with our editorial policies and formatting requirements. Failure to do so will result in your manuscript being returned to you, which will delay its consideration. To assist you in this process, I have attached a checklist that lists all of our requirements. If you have any questions about any of our policies or formatting, please don't hesitate to contact me.

If you wish to submit a suitably revised manuscript, we would hope to receive it within 4 months. I would be grateful if you could contact us as soon as possible if you foresee difficulties with meeting this target resubmission date.

With your revision, please:

• Include a “Response to the editors and reviewers” document detailing, point-by-point, how you addressed each editor and referee comment. If no action was taken to address a point, you must provide a compelling argument. When formatting this document, please respond to each reviewer comment individually, including the full text of the reviewer comment verbatim followed by your response to the individual point. This response will be used by the editors to evaluate your revision and sent back to the reviewers along with the revised manuscript.

• Highlight all changes made to your manuscript or provide us with a version that tracks changes.

Please use the link below to submit your revised manuscript and related files:

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Note: This URL links to your confidential home page and associated information about manuscripts you may have submitted, or that you are reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage.

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

Sincerely,

[redacted]
Nature Human Behaviour

Reviewer expertise:

Reviewer #1: ancient genetics/aDNA

Reviewer #2: ancient genetics/aDNA, archaeology-social inequality in ancient China

Reviewer #3: archaeology-social inequality in ancient China

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Zhang et al. Explore patterns of social inequality from the Zhou Dynasty in China by putting together a set of different experimental approaches on 32 skeletons from a cemetery from that period that include six nobles and 26 sacrificial victims. Carbon and nitrogen isotopic analyses provide information on human diet and mobility, while genomic analysis provide information of ancestry composition and kinship. In addition, paleoproteomic analysis on some individuals provide information about genetic sex that can be correlated with anthropological inference. Some of the results point to long-distance migration patterns in the elite class as well as conspicuous differences on diet -including meat intake and millet consumption- between nobles and sacrificial victims. Kinships analysis uncovers links between nobles but also, intriguingly, with one of the victims.

Overall, I think this is an interesting study on past human society that offers a solid depiction on how different techniques can offer now a nuanced view of past social inequality. The introduction is nicely written and brings together all these potential sources of information. If anything, I feel the authors could do a better job explaining how genetic analysis can also contribute to illuminate social inequality, besides the usual application to past migrations. Ancient DNA can illuminate not only kinship relationships in any archaeological site that can be correlated with grave goods, like have been explored in Mittnik et al. (2019) (Science) in Bronze Age Germany, but also if particular population movements were sex biased, like was explored by Olalde et al. (2019) (Science) in the Bronze Age Iberian Peninsula. In particular, it is possible to explore autosomal ancestry turnover and compare it to uniparental markers such as Y chromosome and mtDNA that behave differently in sex-biased migrations. Also, it is possible to estimate a specific ancestry in the X chromosome (which is in two thirds in women at any given population with equal sex ratios) and compare this ancestry in the autosomes of the same individuals. Some of these ideas have been explored in book such as: Lalueza-Fox (2022) "Inequality: A Genetic History" or in Reich (2018) "Who We Are and How We Got Here". Needless to say, these approaches offer also the possibility of building an interdisciplinary approach to the study of the human past, in collaboration with archaeologists.

One thing that strikes me as strange is the fact that the authors determine sex by oligopeptide analysis and later on retrieve genome-wide data from the skeletons, thus obtaining, of course, the genetic sex. As it stands now, it looks more than a laundry list, where different things done are listed one after the other. I think the authors could improve the narrative of the study, like in the previous section, if they explain first the genomic data and only if some individuals failed to have genetic sex, explain them the paleoproteomic approach. Otherwise, I would move this section to the supplementary and use it as a confirmation for the genetic sex attribution. And again, some review reference on the possibilities of paleoproteomics should be nice (I suggest Cappellini's review on Annual Review of Biochemistry 2018, or Frido Welker's review on Quaternary Science Reviews 2018).

It is also surprising the authors do not mention the Y chromosome or mitochondrial DNA attribution of any of the individuals. It seems they only use these markers for contamination detection. Is it possible these markers are totally un-informative in this period/geographical region? Specially while discussing possible patrilocality/matrilocality they should be mentioned in the main text.

Minor points

The authors mention that millet consumption is associated to high status, but the reason is unclear. Is it more nutritious? Why? A potential reference supporting this could be nice.

It is unclear why the authors include the Chinese characters after some concepts. I think it is nice if, for instance, an English translation is complex or diffuse, but maybe they could explain the reason somewhere (in the Acknowledgments?).

-Line 213. “was highly erratic” maybe is better “was variable” or similar.

The paper ends with a section on the results of tomb18 that shows a shifting on diet during her infancy. I suggest to add a Conclusions section to finish the paper, maybe with general overview on the potential of these new techniques, including ancient DNA, on the understanding of past social inequalities in a wide range of situations and periods, as well as potential work ahead (enlarging samples, studying other dynasties, etc).

Reviewer #2 (Remarks to the Author):

The paper has an extensive amount of data from a very interesting archaeological site. The way the paper is presently structured it is mainly descriptive. The question or hypothesis of the paper is weak. The Zhou dynasty was a state level archaeological culture with a known level of social stratification. The authors propose so show a detailed level of inequality through multiple types of isotopes. The paper needs to have more information about the populations of the Zhou dynasty to provide a baseline for comparison. What proportion of the population was nobility, commoner, slaves? Another issue is the extremely small sample size of 32 individuals. The Zhou dynasty had a population of about 30 million people. This sample is predominantly nobility and sacrificial victims. How is this a representative sample of a living Zhou village/city? The authors imply the sacrificial victims were slaves, but their expensive grave goods and evidence of long-distance travel imply otherwise. The difference in diet composition (C3/C4 plants and meat) is not convincing as a simple difference in social status. Meat consumption in East and South Asia is complicated and not simply associated with high status. High status individuals in later periods often refrained from meat consumption. Millet and wheat/barley are not grown in the same regions and so a difference in diet is more complicated than a simple status difference. The dietary difference may reflect environmental change over time and crop adjustments. So simply stating the results are proof of inequality is not enough. There is a gap between the results, discussion, and conclusion. There are too many holes. The paper also did not include English language papers on Chinese bioarchaeology or paleoethnobotany. The word waist is a part of the body, it should be switched to waste which means trash.

Reviewer #3 (Remarks to the Author):

The results of the authors’ scientific analyses are of great interest. However, the historical contextualization still leaves to be desired. In the first place, the criteria according to which certain individuals are identified as “nobles” and “victims” should be clearly spelled out. Otherwise, the reader cannot fend off the impression that there is circular reasoning going on. Moreover, the authors should face the fact that the samples with which they are working are not statistically significant; what the study presents is interesting evidence of an impressionistic sort, which may be suggestive of social trends during the period under analysis, but is insufficient to prove them. The reader cannot help having the impression that the authors consider the fact that their interpretations fit, more or less, with what is known from traditional texts amounts to be a proof of their validity. Instead, of course, this could be just as likely a case of interpretations that are molded by received ideas—an echo chamber. The study would profit from a more rigorous conceptual separation of the scientific part from the historical part (the latter being in need of strengthening, see below); a synthetic interpretation could be offered in the conclusion.
It might be worth offering more explicit comparisons of the new data here reported with other studies on contemporaneous materials that have already been undertaken. This could be done in the text as well as by adding additional tables that position the Songzhuang results as part of a wider universe of data.
The historical context as given in ll. 97-118 is overly schematic and seems naïve. “Eastern Zhou Dynasty” should read “Eastern Zhou period”; note that there are very large differences between the early (Springs and Autumns) and later (Warring States) parts of that period. In any case, absolute dates should be provided for the tombs analyzed. The results are certainly not going to be applicable for “Eastern Zhou” as a whole, but only for a specific chronological segment thereof, which should be clearly defined. At the very least, the discussion should be confined specifically to one or the other of the two subperiods of “Eastern Zhou.” “Zhou emperor” is certainly wrong; it should read “king.” Instead of “noble class”, “ranked part (or segment) of the population” would probably be safer, given that at least the Springs and Autumns-period probably still was a lineage society rather than a class society. Whether there were “various types of slaves” is quite unclear; we know neither what the economic rôle of slavery was, nor whether “slaves” constituted a separate stratum in the society.
As a point of formatting: current Sinological standards require the use of fanti (complex) character forms, both in the text and in the bibliography.
In the bibliography, (1) the names of Chinese authors should be given in full; giving only their initials makes it often impossible to identify them, due to the large number of homonyms. (2) Titles of works that were published in Chinese should be cited in the original language (transcription followed by characters). Optionally, a translation may be provided; if it is, this should be done consistently.
All in all, this is a study of considerable potential. The analytic methods employed are certainly of great usefulness, and they seem to have been used competently (this is not something the present reviewer can judge, however). The results, even if only suggestive rather than truly representative for the social situation of the period under analysis. The article merits publication once the conceptual issues raised above are properly addressed.

Version 1:

Decision Letter:

Our ref: NATHUMBEHAV-25010354A

11th July 2025

Dear Dr. Wang,

Thank you for submitting your revised manuscript "Multidisciplinary analysis reveals the social inequality patterns in the Central Plains during the Eastern Zhou in China" (NATHUMBEHAV-25010354A). It has now been seen by the original referees and their comments are below. As you can see, the reviewers find that the paper has improved in revision. We will therefore be happy in principle to publish it in Nature Human Behaviour, pending minor revisions to satisfy the referees' final requests and to comply with our editorial and formatting guidelines.

We are now performing detailed checks on your paper and will send you a checklist detailing our editorial and formatting requirements within two weeks. Please do not upload the final materials and make any revisions until you receive this additional information from us.

Please do not hesitate to contact me if you have any questions.

Sincerely,

[redacted]
Nature Human Behaviour

Reviewer #1 (Remarks to the Author):

I think the authors have made a successful revision not only answering my comments but also from those from the other two reviewers, greatly improving the narrative flow of the paper. I found also interesting the addition of a Conclusions section with a paragraph on future research directions.

Reviewer #3 (Remarks to the Author):

l. 89: " Consequently, being "superior in life" is as crucial as being "superior in death."--In the present context, this is misleading because it suggests that the bioarchaeological phenomena here reported are the result of funerary customs, when in fact they are entirely a reflection of how the humans in question lived before they died.
l. 370: " patriarchal clan system" should read " patriarchal lineage system"
l. 416-417: "The Zhonghang and Fan clans, neglecting common people's hardships,
sought to monopolize the state of Jin"--"clans" should read "lineages"; "monopolize the state of Jin" sounds funny, you may want to check a previous translation for a better formulation. The classical loci referred to throughout the article should all be properly referenced to standard editions.

Version 2:

Decision Letter:

Dear Professor Wang,

We are pleased to inform you that your Article "Multidisciplinary analyses and ancient DNA reveal social inequality and mobility in the Central Plains during the Eastern Zhou period in China", has now been accepted for publication in Nature Human Behaviour.

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With best regards,

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Dear Editors and Reviewers:

We sincerely appreciate your letter and the reviewers' constructive feedback regarding our manuscript entitled "Multidisciplinary analysis reveals the social inequality patterns in the Central Plains of the Eastern Zhou Dynasty in China". The insightful comments have been invaluable in helping us improve the quality of this work and will continue to guide our future research endeavours.

We have carefully considered each suggestion and have implemented comprehensive revisions to address the reviewers' concerns. We believe these substantial improvements have significantly strengthened the manuscript's scientific rigor and clarity. The detailed modifications and our point-by-point responses to all comments are outlined below.

Reviewer #1

Comment 1: The introduction is nicely written and brings together all these potential sources of information. If anything, I feel the authors could do a better job explaining how genetic analysis can also contribute to illuminate social inequality, besides the usual application to past migrations. Ancient DNA can illuminate not only kinship relationships in any archaeological site that can be correlated with grave goods, like have been explored in Mittnik et al. (2019) (Science) in Bronze Age Germany, but also if particular population movements were sex biased, like was explored by Olalde et al. (2019) (Science) in the Bronze Age Iberian Peninsula. In particular, it is possible to explore autosomal ancestry turnover and compare it to uniparental markers such as Y chromosome and mtDNA that behave differently in sex-biased migrations. Also, it is possible to estimate a specific ancestry in the X chromosome (which is in two thirds in women at any given population with equal sex ratios) and compare this ancestry in the autosomes of the same individuals. Some of these ideas have been explored in book such as: Lalueza-Fox (2022) “Inequality: A Genetic History” or in Reich (2018) “Who We Are and How We Got Here”. Needless to say, these approaches offer also the possibility of building an interdisciplinary approach to the study of the human past, in collaboration with archaeologists.

Response: We sincerely appreciate the reviewer's insightful suggestions regarding the expanded applications of genetic analysis in the study of social inequality. In response, we have incorporated relevant discussions into the manuscript, particularly in lines 64-70, where we elaborate on how genetic data can illuminate kinship patterns and sex-biased migrations. Additionally, we have explored the analysis of sex-biased admixture by comparing ancestry proportions on the X chromosome and autosomes. Currently, qpAdm-based modeling of X chromosomes is more frequently applied in studies involving East-West Eurasian interactions (Jianxue Xiong, 2024, "Inferring the Demographic History of Hexi Corridor Over the Past Two Millennia from Ancient Genomes") or among genetically more differentiated European populations (Joscha Gretzinger, 2023, "Evidence for Dynastic Succession Among Early Celtic Elites in Central Europe"). In East Asia, sex-biased analyses pose more challenges due to the relatively limited genetic differentiation among regional populations and the limited number of SNPs on the X chromosome in previous publications, which may affect the resolution and robustness of the modeling results. Unfortunately, we were unable to obtain stable or interpretable results in our study, and thus did not include this

part in the main text. We performed qpAdm modeling on the X chromosome for both the Songzhuang and Songzhuang2 groups, and the results are summarized in the table.

target	source1	source2	ratio1	ratio2	std.error1	std.error2	tail	Jackknife1	Jackknife2
Songzhuang2	Taiwan_Hanben		0.000		0.000		0.2511	1.0000	
Songzhuang2	ANA	Taiwan_Hanben	0.320	0.680	0.198	0.198	0.3974	0.3654	0.6345
Songzhuang	ANA	Taiwan_Hanben	0.288	0.712	0.056	0.056	0.0856	0.2928	0.7071

We applied *qpAdm* models for single sample.

target	Rank	source1	source2	ratio1	ratio2	std.error1	std.error2	tail	Jackknife1	Jackknife2
Songzhuang_H110808	human sacrifice	YR_MN		0.000		0.000		0.5390	1.0000	
Songzhuang_H110808	human sacrifice	Taiwan_Hanben		0.000		0.000		0.4142	1.0000	
Songzhuang_H110808	human sacrifice	YR_MN	Taiwan_Hanben	0.531	0.469	0.238	0.238	0.8578	0.5478	0.4521
Songzhuang_HB2331	human sacrifice	YR_MN		0.000		0.000		0.4465	1.0000	
Songzhuang_HB2331	human sacrifice	Taiwan_Hanben		0.000		0.000		0.4902	1.0000	
Songzhuang_I52904	human sacrifice	YR_MN		0.000		0.000		0.2626	1.0000	
Songzhuang_I52904	human sacrifice	YR_MN	ANA	0.684	0.316	0.220	0.220	0.3261	0.6493	0.3506
Songzhuang_I60814	human sacrifice	YR_MN		0.000		0.000		0.0845	1.0000	
Songzhuang_I60814	human sacrifice	Taiwan_Hanben		0.000		0.000		0.0681	1.0000	
Songzhuang_I82701	Noble	YR_MN		0.000		0.000		0.1393	1.0000	
Songzhuang_I90306	human sacrifice	Taiwan_Hanben		0.000		0.000		0.0924	1.0000	
Songzhuang_I92001	human sacrifice	YR_MN		0.000		0.000		0.0547	1.0000	
Songzhuang_I92001	human sacrifice	Taiwan_Hanben		0.000		0.000		0.1326	1.0000	
Songzhuang_I92001	human sacrifice	ANA	Taiwan_Hanben	0.368	0.632	0.153	0.153	0.4993	0.3625	0.6374
Songzhuang_I92002	human sacrifice	YR_MN		0.000		0.000		0.1071	1.0000	
Songzhuang_I92002	human sacrifice	Taiwan_Hanben		0.000		0.000		0.7667	1.0000	
Songzhuang_I92004	human sacrifice	Taiwan_Hanben		0.000		0.000		0.4585	1.0000	
Songzhuang_I92004	human sacrifice	YR_MN	Taiwan_Hanben	0.304	0.696	0.205	0.205	0.6444	0.3191	0.6808
Songzhuang_I92004	human sacrifice	ANA	Taiwan_Hanben	0.250	0.750	0.110	0.110	0.8178	0.2522	0.7477
Songzhuang_IA0603	human sacrifice	YR_MN		0.000		0.000		0.9458	1.0000	
Songzhuang_IA0604	Commoner	YR_MN		0.000		0.000		0.0518	1.0000	
Songzhuang_IA0605	human sacrifice	ANA		0.000		0.000		0.0655	1.0000	
Songzhuang_IA0605	human sacrifice	Taiwan_Hanben		0.000		0.000		0.1950	1.0000	
Songzhuang_IA0605	human sacrifice	ANA	Taiwan_Hanben	0.391	0.609	0.241	0.241	0.3391	0.3973	0.6026
Songzhuang_IA1726	human sacrifice	YR_MN		0.000		0.000		0.7105	1.0000	
Songzhuang_IA1726	human sacrifice	Taiwan_Hanben		0.000		0.000		0.3295	1.0000	
Songzhuang_IA1726	human sacrifice	YR_MN	Taiwan_Hanben	0.637	0.363	0.254	0.254	0.8106	0.6276	0.3723
Songzhuang2_I52903	human sacrifice	Taiwan_Hanben		0.000		0.000		0.0726	1.0000	
Songzhuang2_I52903	human sacrifice	ANA	Taiwan_Hanben	0.513	0.487	0.249	0.249	0.2183	0.5999	0.4000

Our findings suggest that Songzhuang2 can be modeled as a descendant of a southern lineage (Taiwan_Hanben), supporting the significant southern ancestry component in this group. At the individual level, both the noble (I82701) and the commoner (IA0604) are best explained by a single-source model from the Yellow River Middle Neolithic farmers (YR_MN), which may indicate the prominence of northern women in this context. Due to the differences in source populations between

our group-level and individual-level qpAdm simulations, interpretations of sex-specific migration and admixture dynamics should be approached with caution. Therefore, we have chosen not to include these findings in the main text. We appreciate your understanding. As the quality and quantity of reference data improve or as representative groups become available (for example, there is currently a lack of sex chromosome data from the East Asia southeast coastal regions during the Neolithic period), we will continue to monitor this matter in our future work.

Comment 2: One thing that strikes me as strange is the fact that the authors determine sex by oligopeptide analysis and later on retrieve genome-wide data from the skeletons, thus obtaining, of course, the genetic sex. As it stands now, it looks more than a laundry list, where different things done are listed one after the other. I think the authors could improve the narrative of the study, like in the previous section, if they explain first the genomic data and only if some individuals failed to have genetic sex, explain them the paleoproteomic approach. Otherwise, I would move this section to the supplementary and use it as a confirmation for the genetic sex attribution. And again, some review reference on the possibilities of paleoproteomics should be nice (I suggest Cappellini's review on Annual Review of Biochemistry 2018, or Frido Welker's review on Quaternary Science Reviews 2018).

Response: Thank you for your insightful feedback. We have revised the manuscript to improve the narrative flow. DNA data is now primarily used for sex determination, with paleoproteomic analysis results integrated as a supplementary method for cases where DNA data is insufficient (see lines 134-141). Furthermore, we have relocated the detailed paleoproteomics methodology and related discussions to the supplementary information section (see lines 450-455).

Comment 3: It is also surprising the authors do not mention the Y chromosome or mitochondrial DNA attribution of any of the individuals. It seems they only use these markers for contamination detection. Is it possible these markers are totally un-informative in this period/geographical region? Specially while discussing possible patrilocality/matrilocality they should be mentioned in the main text.

Response: Thank you for your thoughtful reminder and suggestion. We have incorporated the relevant results into the main text (see lines 238-246).

Comment 4: The authors mention that millet consumption is associated to high status, but the reason is unclear. Is it more nutritious? Why? A potential reference supporting this could be nice.

Response: We sincerely apologize for not thoroughly explaining the connection between millet consumption and elevated social status. One plausible explanation is that the rudimentary processing techniques for the newly introduced wheat resulted in a coarse texture and unpleasant taste. As a result, wheat-based foods were primarily consumed by lower-status individuals who faced dietary constraints. For further details, please refer to Lines 299-319.

Comment 5: It is unclear why the authors include the Chinese characters after some concepts. I think it is nice if, for instance, an English translation is complex or diffuse, but maybe they could explain the reason somewhere (in the Acknowledgments?).

Response: The Chinese characters have been removed from the manuscript, and brief English explanations have been added where necessary to clarify the concepts.

Comment 6: I suggest to add a Conclusions section to finish the paper, maybe with general overview on the potential of these new techniques, including ancient DNA, on the understanding of past social inequalities in a wide range of situations and periods, as well as potential work ahead (enlarging samples, studying other dynasties, etc).

Response: Thank you for suggesting the addition of a Conclusions section. We have now included this in the manuscript. In the updated Conclusions section (see lines 423-441), we emphasize the valuable insights gained from applying these advanced techniques. Additionally, we underscore the need for future research to enhance sample diversity by improving temporal and spatial density and broadening the range of social strata studied.

Comment 7: Line 213. "was highly erratic" maybe is better "was variable" or similar.

Response: Thank you for your suggestions. We have revised the manuscript accordingly.

Reviewer #2

Comment 1:

The paper needs to have more information about the populations of the Zhou dynasty to provide a baseline for comparison. What proportion of the population was nobility, commoner, slaves? Another issue is the extremely small sample size of 32 individuals. The Zhou dynasty had a population of about 30 million people. This sample is predominantly nobility and sacrificial victims. How is this a representative sample of a living Zhou village/city?

Response: We sincerely appreciate the reviewer's constructive feedback regarding population baseline data and sample representativeness. However, estimating the population and structure of the Zhou Dynasty presents significant challenges due to two main factors. First, there is a notable lack of documented records. Current estimates of the Eastern Zhou population rely on scattered sources, such as troop numbers and population data for certain cities, as well as broad speculations based on territory and political and economic conditions[1-3]. These sources, however, are not entirely reliable for determining accurate population figures. Additionally, mortuary practices tend to disproportionately represent privileged groups. For instance, at the Songzhuang site, numerous noble tombs have been uncovered, while commoner burials are comparatively rare. Moreover, few archaeological sites provide statistically meaningful material for fully reconstructing the social hierarchy of the time, as seen at the Shangma Cemetery[4]. Consequently, determining the exact population demographics of the Zhou Dynasty, including the proportions of nobility, commoners, and slaves, remains challenging due to limited historical records. We have used an estimated figure of tens of millions of people to provide a general sense of the population scale, but specific proportions for each social class remain difficult to ascertain. We have briefly mentioned the potential social strata in lines 77-81.

We fully concur with the reviewer's observation regarding the limited sample. Our study tries to address this concern through four key considerations:

1. Our study analyzed all 32 available individual remains from the archaeological site. While this sample size seems small relative to the estimated tens of millions of people in the Zhou Dynasty, these remains are highly significant. They represent two distinct social strata common in Eastern Zhou society: the ruling elite and their sacrificial victims. This contrast between privileged aristocrats and subordinate sacrificial groups provides crucial insights into the hierarchical structure and funerary practices of the period.

2. Our research, although limited to 32 individuals, employs a systematic, interdisciplinary analytical approach for the first time to extract maximum life history data from each specimen during this critical period in East Asia. This methodology significantly expands our multi-dimensional understanding across different life stages. For instance, ancient DNA analysis reconstructs genetic lineages, kinship networks, and social organization patterns; carbon/nitrogen isotopic profiling differentiates childhood dietary patterns from terminal consumption habits, revealing nutritional transitions and social stratification; strontium/oxygen isotopic mapping traces lifetime mobility trajectories and settlement patterns. This comprehensive dataset transcends the limitations of sample volume, enabling us to draw meaningful inferences about population dynamics and cultural practices from a relatively small but intensively analyzed dataset.

3. To address data limitations more effectively, we incorporated additional published datasets from the Zhou Dynasty. This includes sex ratio information on individuals involved in human funerary sacrifices, comprising a database of 207 individuals from 26 archaeological sites, and carbon-

nitrogen isotopic profiling data from 327 individuals across six sites. By integrating our original dataset with these external records, we can better illuminate two key patterns in the middle and lower Yellow River region: 1) the consistent gender composition trends among funerary sacrifice populations, and 2) the dietary differences between various social strata, which align with hierarchical status. These multi-site data aggregations provide cross-validated evidence for the social dynamics reflected in our primary sample, enhancing the robustness of interpretations about ritual practices and subsistence strategies in Eastern Zhou society.

4. Transparent Limitations: We acknowledge that our findings are restricted in terms of time and space, specifically to the middle and lower Yellow River regions during the Eastern Zhou period. Additionally, we explicitly state that our sample primarily consists of nobility and sacrificial victims, with limited representation of commoners. We emphasize the need for future research to increase sample diversity by improving temporal and spatial sampling density and expanding the range of social classes studied (see lines 423-441).

Comment 2: The authors imply the sacrificial victims were slaves, but their expensive grave goods and evidence of long-distance travel imply otherwise.

Response: We appreciate the reviewer's insightful observation regarding the interpretation of sacrificial victims.

First, to clarify any potential misunderstandings, we have revised the terminology used in the manuscript. Instead of the term "slaves," which may imply a specific social categorisation, we now use "people of the lower class, who were in a subordinate position" to describe these individuals.

Second, we apologize for not providing comprehensive details about the burial goods in the original text. We have now included this information in the attached supplementary tables, providing readers with a comprehensive overview of each individual's burial assemblage. Among the 26 sacrificial victims analyzed, 15 (57.7%) had no burial goods, while 11 (42.3%) were interred with only minimal decorative items, predominantly stone bead necklaces and bone hairpins. Such sparse and humble grave goods clearly indicate that these individuals were not wealthy, reinforcing the conclusion that they belonged to a subordinate, lower social class.

Finally, it's important to note that only a small proportion of sacrificial victims show evidence of non-local origins. As indicated by the strontium-oxygen isotope analysis, among the 26 individuals studied, only two (M1X1 & M8X2) may have experienced long-distance migration.

Comment 3: The difference in diet composition (C3/C4 plants and meat) is not convincing as a simple difference in social status. Meat consumption in East and South Asia is complicated and not simply associated with high status. High status individuals in later periods often refrained from meat consumption. Millet and wheat/barley are not grown in the same regions and so a difference in diet is more complicated than a simple status difference. The dietary difference may reflect environmental change over time and crop adjustments. So simply stating the results are proof of inequality is not enough. There is a gap between the results, discussion, and conclusion. There are too many holes. The paper also did not include English language papers on Chinese bioarchaeology or paleoethnobotany.

Response: We sincerely appreciate the reviewer's thoughtful and constructive feedback. We have carefully addressed each concern, and our responses are organized as follows:

Meat Consumption in the Eastern Zhou Period: While it is true that meat consumption patterns

varied across different historical periods and regions, isotopic evidence from various studies indicates a strong link between high-status individuals and increased animal protein consumption from the Shang to the Eastern Zhou period[5]. This aligns with historical texts, such as the "Zuo Zhuan," where the elite are described as "meat-eaters."

C3/C4 Plant Consumption and Social Stratification: We have expanded our analysis to explain the divergence in C3/C4 plant consumption, focusing on the following aspects:

1. Climate Change and Agricultural Structural Transformation: At the end of the Holocene climatic optimum (around 3,000 BP), the climate became cooler and drier, negatively impacting rice cultivation. This, combined with population growth during the Zhou Dynasty, particularly in the Eastern Zhou period, likely contributed to the increased cultivation of wheat as a high-yield dryland crop. Multiple archaeobotanical analyses show that millet and wheat were the dominant crops in the middle and lower Yellow River regions during the Eastern Zhou period.

2. Underdeveloped Wheat Processing Techniques: The underdeveloped processing techniques of wheat may have been the primary reason why the aristocracy consumed less of it. During the initial phase of wheat introduction, it was mainly prepared as "whole grains," which failed to fully separate the husk from the wheat kernels, resulting in a coarse texture and poor digestibility.

We hope that by adding this explanation (see lines 299-319), we can strengthen the logical connections between our findings, analysis, and conclusions. Additionally, we have further emphasized the temporal and spatial limitations of this conclusion to provide a clearer context for our findings (see lines 332-337).

Comment 4: The word waist is a part of the body, it should be switched to waste which means trash.

Response: In this study, the term "waist pit" denotes a pit excavated near the waist of the deceased within a tomb. This burial custom is observed in specific ancient Chinese tombs, notably from the Shang and Zhou periods.

(1) Zhao, W. & Xie, S. *Zhongguo Renkou Shi* (People's Publishing House, 1988).

(2) Wang, Y. *Zhongguo Renkou Shi*. (Jiangsu People's Publishing House, 1995).

(3) Ge, J. *Zhongguo Renkou Shi*. (Fudan University Press, 2002).

(4) Falkenhausen, L. V. *Chinese Society in the Age of Confucius (1000-250 BC): The Archaeological Evidence*. (Chinese society in the age of Confucius (1000-250 BC) :, 2006).

(5) Li, X. *et al.* Dietary shift and social hierarchy from the Proto-Shang to Zhou Dynasty in the Central Plains of China. *Environmental Research Letters* **15** (2020). https://doi.org/10.1088/1748-9326/ab6783

Reviewer #3

Comment 1: In the first place, the criteria according to which certain individuals are identified as "nobles" and "victims" should be clearly spelled out.

Response: Thank you for your feedback. The distinction between "nobles" and "victims" in burial contexts primarily relies on spatial hierarchy and the specifications of burial structures. Nobles are typically interred within a central, independent coffin chamber, as seen with the tomb owner in M4, and are enclosed by a multi-layered structure, such as inner and outer coffins. In contrast, victims are found in peripheral areas outside the main coffin chamber, such as second-tier platforms or waist pits, and are accompanied by basic burial containers, like wooden coffins, or none at all. We have revised the relevant sections of the manuscript to clarify these distinctions (see lines 108-115).

Comment 2: Moreover, the authors should face the fact that the samples with which they are working are not statistically significant; what the study presents is interesting evidence of an impressionistic sort, which may be suggestive of social trends during the period under analysis, but is insufficient to prove them.

Response: In response to your valuable suggestions, we have implemented the following actions:

1. Statistical Significance in Core Analysis

Several key comparisons in our study yielded statistically significant results. For instance, carbon-nitrogen isotopic differences between noble individuals and funerary sacrifice groups revealed significant distinctions, indicating systematic dietary stratification. Additionally, within the funerary sacrifice group, carbon isotopic variations across different burial contexts demonstrated statistical significance, suggesting nuanced social hierarchies even among these populations.

2. Strengthening Conclusions Through Multi-Dataset Integration

To address sample limitations, we integrated published datasets, including a sex ratio database of 207 individuals from 26 sites and carbon-nitrogen isotopic data from 327 individuals across 6 sites. This cross-site analysis revealed consistent patterns in the middle-lower Yellow River region: uniform gender composition trends among funerary sacrifice populations (predominantly female) and persistent dietary disparities between nobles and sacrificial victims, correlated with social rank. These findings are not impressionistic but are supported by aggregated data across spatial and temporal scales, enhancing the credibility of our conclusions within a broader archaeological context.

3. Acknowledging Limitations

While certain results, such as dietary variations within sacrificial victim subgroups, showed statistical significance in our dataset, we recognize that small sample sizes for specific subgroups may limit generalizability. Therefore, we emphasize the spatiotemporal limits of our conclusions and stress the importance of increasing sample density in future research to enhance representativeness (see lines 332-337, 423-441).

Comment 3: It might be worth offering more explicit comparisons of the new data here reported with other studies on contemporaneous materials that have already been undertaken. This could be done in the text as well as by adding additional tables that position the Songzhuang results as part of a wider universe of data.

Response: We sincerely appreciate the reviewer's insightful comment. In response to the suggestion of providing more explicit comparisons with other studies on contemporaneous materials, we would

like to clarify that our article already includes relevant isotope and gender data from contemporaneous sites in northern China. This information is detailed in Supplementary Tables 7, 11, and 12. Additionally, we have integrated discussions related to these comparative data in the main text. If there are specific aspects of these comparisons that the reviewer would like to see further emphasized, we are more than willing to make the necessary adjustments.

Comment 4: The reader cannot help having the impression that the authors consider the fact that their interpretations fit, more or less, with what is known from traditional texts amounts to be a proof of their validity. Instead, of course, this could be just as likely a case of interpretations that are molded by received ideas—an echo chamber. The study would profit from a more rigorous conceptual separation of the scientific part from the historical part (the latter being in need of strengthening, see below); a synthetic interpretation could be offered in the conclusion.

The historical context as given in ll. 97-118 is overly schematic and seems naïve. “Eastern Zhou Dynasty” should read “Eastern Zhou period”; note that there are very large differences between the early (Springs and Autumns) and later (Warring States) parts of that period. In any case, absolute dates should be provided for the tombs analyzed. The results are certainly not going to be applicable for “Eastern Zhou” as a whole, but only for a specific chronological segment thereof, which should be clearly defined. At the very least, the discussion should be confined specifically to one or the other of the two subperiods of “Eastern Zhou.” “Zhou emperor” is certainly wrong; it should read “king.” Instead of “noble class”, “ranked part (or segment) of the population” would probably be safer, given that at least the Springs and Autumns-period probably still was a lineage society rather than a class society. Whether there were “various types of slaves” is quite unclear; we know neither what the economic rôle of slavery was, nor whether “slaves” constituted a separate stratum in the society.

Response: We sincerely appreciate the reviewer's insightful observations, which underscore an important methodological consideration for ensuring the rigor of our interpretation. In light of your valuable feedback, we have implemented the following revisions to enhance the clarity and academic rigor of our manuscript:

Firstly, we have adopted the terminological changes you suggested. By replacing "Eastern Zhou Dynasty" with "Eastern Zhou period," "Zhou emperor" with "Zhou king," "noble class" with "ranked part of the population," and "various types of slaves" with "people of lower class, who were in a subordinate position," we aim to present a more accurate and nuanced view of the historical context.

Secondly, regarding the issue of the chronological range raised by the reviewer, we would like to clarify that the dating results of the tombs indicate they span both the Spring and Autumn period and the Warring States period. This aligns with the archaeological conclusion that the cemetery dates from the late Spring and Autumn period to the mid-Western Zhou period. Therefore, the samples in this study cannot be simply attributed to a specific stage of either the "Spring and Autumn" or the "Warring States" period. A more detailed study of this issue awaits more precise sampling in the future.

Finally, we have refined the presentation of some results to better delineate the scientific and historical sections. Additionally, we have supplemented the discussion, particularly in explaining the relationship between diet and social status, to improve the logical connection between data and conclusions.

Comment 4: As a point of formatting: current Sinological standards require the use of fanti (complex) character forms, both in the text and in the bibliography.

In the bibliography, (1) the names of Chinese authors should be given in full; giving only their initials makes it often impossible to identify them, due to the large number of homonyms. (2) Titles of works that were published in Chinese should be cited in the original language (transcription followed by characters). Optionally, a translation may be provided; if it is, this should be done consistently.

Response: Thank you for your feedback. We have updated the bibliography based on your suggestions.

Remaining reviewer comments

Our guidance:	Your response:
Pleases address the remaining concerns raised by Reviewer #3 and discuss reference [39] in more depth. In particular, please elaborate the difference between the current study and reference [39].	Reviewer #3: Q1: 89: “ Consequently, being "superior in life" is as crucial as being "superior in death.”--In the present context, this is misleading because it suggests that the bioarchaeological phenomena here reported are the result of funerary customs, when in fact they are entirely a reflection of how the humans in question lived before they died. R1: Thank you for your valuable comment. We agree that the original sentence was not clear enough and caused ambiguity. Therefore, we have revised it in the manuscript to explicitly emphasize our focus on the understanding of individuals' lifestyles before their death (see lines 140-142). Q2: 370: “ patriarchal clan system” should read “ patriarchal lineage system” R2: We have revised "patriarchal clan system" to "patriarchal lineage system" as requested. Q3: “The Zhonghang and Fan clans, neglecting common people's hardships, sought to monopolize the state of Jin"--"clans" should read "lineages"; "monopolize the state of Jin" sounds funny, you may want to check a previous translation for a better formulation. The classical loci referred to throughout the article should all be properly referenced to standard editions. R3: In response to your revision suggestions, we have adjusted the relevant

	expressions in the original text and supplemented citations from authoritative literary sources. Q4: Pleases discuss reference [39] in more depth. In particular, please elaborate the difference between the current study and reference [39]. We have discussed Reference [39] in more depth in the revised manuscript. R4: We have discussed Reference [39] in more depth in the revised manuscript (Lines 148-157). Specifically, Reference [39] focuses on dietary differences among various populations in the period preceding death, analyzed within a broad framework of population identities. In contrast, our research emphasizes a multi-dimensional investigation of social inequality, encompassing dietary patterns, mobility, kinship, and genetic structure. With respect to diet in particular, while Reference [39] centers on pre-death diets, our study primarily concentrates on dietary differences and long-term stability during the early life stage. This shift in temporal focus allows us to probe developmental aspects of social differentiation that are not captured by analyses limited to terminal dietary signals.