

The timing of the ca-660 BCE Miyake solar-proton event constrained to between 664 and 663 BCE

Corresponding Author: Dr Irina Panyushkina

Version 0:

Decision Letter:

**** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors ****

Dear Dr Panyushkina,

Your manuscript titled "663 BCE is the start date of the Miyake solar-proton event at ca. 660 BCE" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't 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 referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the 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 ****

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still 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.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Alienor Lavergne, PhD
Associate Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: [Policy requirements](https://www.nature.com/documents/nr-editorial-policy-checklist.pdf)
(Download the link to your computer as a PDF.)

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist: <https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf> Communications Earth & Environment formatting checklist

and also in our style and formatting guide <https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf> Communications Earth & Environment formatting guide.

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (<http://www.copdess.org/enabling-fair-data-project/>). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at <http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf>.

In particular, the Data availability statement should include:

- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at <http://www.nature.com/sdata/policies/repositories>.

If a community resource is unavailable, data can be submitted to generalist repositories such as <https://figshare.com/> or <http://datadryad.org/> Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at <http://www.nature.com/authors/policies/availability.html>.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of Panyushkina et al., '663 BCE is the start date of the Miyake solar-proton event at ca. 660 BCE'

14C in tree rings can identify strong solar proton events (SPE) on annual/sub annual scale, and several SPEs have been found in the past 12500 years. Among them, the SPE at ca. 660 BC appears unusually prolonged over several years. Several studies already discussed this observation, suggesting several SPEs over three years.

This study uses trees of two specially selected sites, one of high North latitude and one of high altitude, to consider 14C dispersion in the atmosphere and tree species aspects of carbon uptake at the 660 BCE interval. In addition, four published studies of the 660 BC SPE are discussed in detail in the work.

The authors assign the extension of the 14C signal in the 663 BC event to the dispersion of 14CO₂ between stratosphere and troposphere, latitudinal mixing of 14CO₂ in the troposphere, and tree-species specific uptake of CO₂ during the growing interval. Hence diverse characters of solar events would not be required from the 14C sequence of the 660 BC SPE. The study documents well planned sample choice, and presents detailed discussion of 14CO₂ propagation in the atmosphere and uptake by photosynthesis, which is not present in previous studies. These aspects definitively will guide future research using 14C to identify SPEs.

I fully recommend publication of the paper as it is. It is an important contribution to the study of SPEs, using cosmogenic isotopes.

Reviewer #2 (Remarks to the Author):

Dear Editor,

Thank you for sending me this very interesting paper by Panyushkina et al on the 660 BCE Miyake Event. It is timely and important and executed well. It certainly merits publication with minor revisions detailed below.

I do note that it might require some light language editing for grammar.

Kind regards,

The Reviewer

Major Comments:

- This is a good paper and I have mainly methodological comments and comments on presentation.
- The general thrust of the paper is that the new high latitude tree samples show short events in 660 BCE, rather than the prolonged events claimed in previous literature. This is pretty exciting! In terms of presentation, the figures could get this point across *much* more clearly with a few minor changes. It is hard to read captions or small legends to distinguish which curves/panels correspond to the new Altai and Yamal datasets and which are from the literature - I suggest you display two warm shades (red, orange?) for these, and cool shades (blue, purple, green...) for the literature events, or vice versa, keeping colour consistent throughout the paper to make it clear which dataset is which (eg all the way through to Fig 4).
- Likewise Figures 2, 3, it is unclear which panel is which dataset - titles on each panel, grouping data by old and new rather than alphabetic, and consistent use of colour would be helpful.
- It is an important result that some but not all of the 660 BCE trees show extended events. It is worth noting that Zhang et al 2022 found this to be the case for the 774 and 993 CE events as well - and without a clear connection to species or latitude, including Siberian larch with both extended and short events in both. I would encourage the authors to address this question, though I do not necessarily expect them to solve it!
- In Figure 3, I have a suspicion there is a numerical problem, hard to tell how serious. Notice that all of the posterior predictive samples for $\Delta^{14}\text{C}$ have hard, flat bottoms - this is because of a spread in phase at a fixed amplitude and baseline for the curve. Allowing the baseline $\Delta^{14}\text{C}$ and the amplitude of the solar cycle to vary and inferring this by MCMC, or by pinning these to more carefully determined values, I expect you can probably improve the fit in the region immediately before the Miyake Event starts to rise (notice how it seems to systematically overestimate the ^{14}C in the 1-2 years before the spike)
- this might be important for the duration estimate. One other thing that probably does deserve mention is the extremely high outlier right at the top in panel a for the Altai larch. What is happening there? If you exclude that point does it improve the fit?
- Figure 4 is very interesting and I have a number of suggestions, beyond colour and formatting noted above, about how to improve it. First, there are no units on the top two panels (presumably astronomical year and years) and 'times' for the production PDF should really be something explicit like q_0 yr as used in Zhang et al 2022 if you are using similar outputs from ticktack, or else in appropriate units like atoms/cm² or kg production if you prefer. If this is in the same q_0 yr units as in Zhang et al, the explanation in the body text is unclear about being x "times higher than the 11 year average production" - it should properly say x times the "average annual production".
- Looking at the phase PDF, I think I can see a problem: the Altai larch is cut off strictly at zero years and the Choukai early wood strictly at 11 years. This implies hard edges to the prior on phase, where you really want it to be cyclic so that 0 and 11 years wrap around. Fixing this may lead to improvements in the fits.
- Overall, in light of the comments above, I think it would be beset to try a joint fit to all 5 datasets to see if the joint fit has improved posteriors; and to display the multidimensional, covariant posteriors from the individual and joint fits as a corner plot (grid of 2d histograms) using for example chainconsumer or corner.py. This set of visualizations is likely to be very informative.
- Straying a little from my expertise, I cannot comment on the correctness of the botany in the later sections but it is very interesting. I think it is easy to see how different plants in different regions would show a different amplitude event, as explained very well by the text. I struggle to see why this would change the apparent *duration* of the event though - perhaps the text could explain this more clearly.
- A major implication of this work is that archaeological dating of events using Miyake events as tie-points (eg Dee & Pope 2016, Kuitens et al 2021) is actually not as precise as archaeologists might have hoped. If the apparent duration and start date of these events varies from tree to tree by ~ a year or two, this isn't disastrous for archaeology but is likely to be a significant impact on their methods and the confidence of their interpretations. The impact of this paper will be higher if the authors point out this problem in the text.

Minor Comments:

- Might be a bit controversial to say six Miyake events when the Bard et al 2023 event of 14.3 cal year BP is published now.
- It is probably also a little too strong to say that Miyake et al citations 4 and 5 conclusively demonstrated that 774 and 993 were SPE; this wasn't really a consensus until Usoskin 2013 and then later the beryllium data, at least from memory.
- I'm not sure why the astronomical year with a zero year is used rather than BCE in line with other publications on these data.
- One possibility not addressed by the text is that a poor fit to the data could be model error - an incorrect carbon box model, in that the preindustrial carbon cycle may have different reservoirs to the Brehm 22 box model employed. There isn't much we can do about it here, but it should be noted - future work might try to directly infer preindustrial carbon box model parameters from tree-ring or other data, and this is likely to be very valuable. It is also worth noting that general circulation models to predict the detailed kinematics may explain why some regions experience different sub-annual and few-year kinematics.
- The 1-month timesteps noted in the appendix on use of ticktack seem a little short when we are dealing with very detailed questions of time resolution - perhaps going a factor of a few finer in time resolution might be good for numerical robustness

(though I don't think this is likely to be a major issue). It is probably also worth citing emcee (Foreman-Mackey et al) for the MCMC sampler.

** Visit Nature Research's author and referees' website at www.nature.com/authors for information about policies, services and author benefits**

Communications Earth & Environment is committed to improving transparency in authorship. As part of our efforts in this direction, we are now requesting that all authors identified as 'corresponding author' create and link their Open Researcher and Contributor Identifier (ORCID) with their account on the Manuscript Tracking System prior to acceptance. ORCID helps the scientific community achieve unambiguous attribution of all scholarly contributions. You can create and link your ORCID from the home page of the Manuscript Tracking System by clicking on 'Modify my Springer Nature account' and following the instructions in the link below. Please also inform all co-authors that they can add their ORCIDs to their accounts and that they must do so prior to acceptance.

<https://www.springernature.com/gp/researchers/orcid/orcid-for-nature-research>

For more information please visit <http://www.springernature.com/orcid>

If you experience problems in linking your ORCID, please contact the Platform Support Helpdesk.

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Panyushkina,

Your manuscript titled "663-662 BCE is the exact timing of the Miyake ca. 660 BCE solar-proton event" has now been seen by 1 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. In particular, for publication in Communications Earth & Environment to be appropriate, we ask that you fully resolve reviewer 2's concerns regarding the boundary conditions used in the prior over the phase of the solar cycle, to compellingly date the Miyake events in question to 663-662 BCE.

Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't 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 referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the 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 **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still 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.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: [Policy requirements](https://www.nature.com/documents/nr-editorial-policy-checklist.pdf) (Download the link to your computer as a PDF.)

For Manuscripts that fall into the following fields:

- Behavioural and social science
- Ecological, evolutionary & environmental sciences
- Life sciences

An updated and completed version of our Reporting Summary must be uploaded with the revised manuscript

You can download the form here:

<https://www.nature.com/documents/nr-reporting-summary.zip>

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist: [Communications Earth & Environment formatting checklist](https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf)

and also in our style and formatting guide [Communications Earth & Environment formatting guide](https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf) .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (<http://www.copdess.org/enabling-fair-data-project/>). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at <http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf>.

In particular, the Data availability statement should include:

- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at <http://www.nature.com/sdata/policies/repositories>.

If a community resource is unavailable, data can be submitted to generalist repositories such as [figshare](https://figshare.com/) or [Dryad Digital Repository](http://datadryad.org/). Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at <http://www.nature.com/authors/policies/availability.html>.

REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

Dear Editor,

Thank you for sending me this revised manuscript. It is much improved and nearly ready for publication - there are a couple of minor points raised in the resubmission that might require additional comment.

Kind regards,

The reviewer

Main Comments:

- In the authors' response, it seems like they may have misunderstood my comment about the phase PDF. I am not concerned about the length of the time-series - but about the boundary conditions used in the prior over the phase of the solar cycle. It appears that this is a uniform prior in (0, 11) years, but it needs to be cyclic appropriately - the PDF for the Altai larch seems to be running up against 0 in a way that would indicate it is being cut off. This is nothing to do with the length of the time series but only to do with the choice of prior - and this is my most important comment - if this is not fixed, the results of that fit are definitely going to be wrong.
- If the authors have a joint fit to all datasets, which has been excluded for space reasons, I think this is important to include as an appendix/supplementary online material/in whatever form is seen appropriate by the authors and the editor - but it is certainly important that this is available somewhere. The phase relative to solar maximum may be better constrained there.
- Likewise, the marginal posteriors in Fig 4 are interesting, but it is important (even if only in an appendix) that the authors display the full covariant posteriors with corner plots, such as from chainconsumer or pairplots.jl.
- I would still do finer timesteps than 1 month in the use of ticktack, for numerical stability of the solver - it's not about whether finer time resolutions are irrelevant because of the trees, but whether the numerics of the solver are correct.
- Figure 2 I maybe don't quite understand - this is a step plot, which is the format produced by the inverse-solver in ticktack, but the caption says this is with MCMC and control points - which interpolates a smooth function with a Gaussian Process. There is a mismatch between the caption and the figure that needs to be clarified.

Minor Comments:

- Re having six events - I see that where I had misunderstood that the authors excluded Bard, I realize it was the 5410 BCE event. It merits comment as to why this is excluded.
- The authors argue that the statement about Miyake "conclusively" demonstrating "The statement is not linked to citations [4] and [5] and the demonstration that 774 and 993 were SPE. This statement are related to the successful use of 14C measurements in tree rings and placed next to the quote from P. Damon paper. No action is taken." The plain language reading of the text in the paper is that it links to citations 4 and 5, and there is ongoing discussion about whether these events were SPE. I strongly recommend removing the word "conclusively", if only that word.
- Why does the domain of the duration PDF in Fig 4 extend to negative values?
- Agree with the author comments about CBMs vs GCMs - all I mean is that it would strengthen the future work section of the paper to say something about it there, even something more or less verbatim like the rebuttal letter.

** Visit Nature Research's author and referees' website at www.nature.com/authors for information about policies, services and author benefits**

Communications Earth & Environment is committed to improving transparency in authorship. As part of our efforts in this direction, we are now requesting that all authors identified as 'corresponding author' create and link their Open Researcher and Contributor Identifier (ORCID) with their account on the Manuscript Tracking System prior to acceptance. ORCID helps the scientific community achieve unambiguous attribution of all scholarly contributions. You can create and link your ORCID from the home page of the Manuscript Tracking System by clicking on 'Modify my Springer Nature account' and following the instructions in the link below. Please also inform all co-authors that they can add their ORCIDs to their accounts and that they must do so prior to acceptance.

<https://www.springernature.com/gp/researchers/orcid/orcid-for-nature-research>

For more information please visit <http://www.springernature.com/orcid>

If you experience problems in linking your ORCID, please contact the Platform Support Helpdesk.

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Version 2:

Decision Letter:

Dear Dr Panyushkina,

Your manuscript titled "664-663 BCE is the exact timing of the Miyake ca. 660 BCE solar-proton event" has now been seen by the original reviewer 2, whose comments are appended below. Unfortunately, they continue to raise substantial concerns about your methodology and we therefore regret to inform you that we cannot publish your manuscript in Communications Earth & Environment.

In light of these ongoing concerns, we are unable to conclude that your study is sufficiently compelling to justify publication in Communications Earth & Environment.

We are sorry that we cannot be more positive on this occasion and thank you for the opportunity to consider your work.

Best regards,

Dr Alireza Bahadori
Associate Editor
Communications Earth & Environment

Reviewers' comments:

Reviewer #2 (Remarks to the Author):

Thanks for sending me this revised manuscript. The authors have addressed most of the concerns.

There remain a few statistical problems, which I think arise in misunderstandings of my previous referee report.

- The authors have amended the caption of Figure 4 to read "Gaussian fitting of nonparametric controlpoints", and refer to Gaussian fits in the text. There are two issues with this:

- a Gaussian Process is not the same thing as a Gaussian, and the ticktack code uses a Gaussian Process. If the authors did use a Gaussian Process, they need to refer to it as such. I direct the authors to Aigrain & Foreman-Mackey 2023 if they want an overview of this.

- Nevertheless, Figure 4 does not resemble the Gaussian Process outputs from ticktack, but does resemble the Inverse Solver, which is not based on a GP but actually a direct inverse of the forward model. It is essential that the authors are clear and correct about what they did here, as these are all quite different things. It could be helpful to link to a GitHub (or other provider) repository with a Jupyter Notebook that they used for the analysis.

- Re the issue with the solar cycle phase: in the recent response the authors note "We used a fixed duration of 12-year solar cycle, and the bounding values of solar cycle phase are 0 and 11." This is not the issue (though I note that if you are using a 12 year solar cycle period, the phase should extend from 0 to 12 not 0 to 11). The issue is that this is a hard edged prior, but if the peak is close to 0 it should appropriately know that phase = -1 is actually the same as phase = (max phase - 1). This is important and the paper cannot be accepted while there is clearly still a problem here.

If the authors do not wish to implement a cyclic prior themselves, they could raise an issue at <https://github.com/SharmaLlama/ticktack/issues>; or there is an even easier way. Where the posterior peak is close to the bound, just move the bounds and recalculate; eg if it's close to 0 or 12 and getting cut off, you could set the bounds to be (-6, 6) so that they are now centred on 0.

- The authors note about the bug in allowing negative durations in Figure 4 that "We think the negative value is a statistical artefact generated by the Gaussian function. The duration parameter derived from this time series is near 0 meaning that the Gaussian bell curve is centered near "0", so the early part of the bell curve went to minus values." This is not true. There should be no Gaussian function or bell curve here. This is an MCMC output and no Gaussian function is used. It should be impossible to get a duration less than 0 and this indicates that there is a problem, whether in the analysis code or the plotting. As the corner plots in supplementary material don't show this, I suggest it is the plotting code; perhaps the authors have missed that duration is done in logarithmic units? If the authors wish to get support for their use of their code, the development team are available at <https://github.com/SharmaLlama/ticktack/issues> and responsive to issues.

- The manuscript uses "transgressive" in a sense that does not seem natural.

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Version 3:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Panyushkina,

Your manuscript titled "664-663 BCE is the exact timing of the Miyake ca. 660 BCE solar-proton event" has now been seen by our adjudicator, reviewer 3, 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 under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to fully address all of the remaining concerns of 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.

EDITORIAL REQUESTS:

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

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

SUBMISSION INFORMATION:

In order to accept your paper, we require the files listed at the end of the Editorial Requests Table; the list of required files is also available at <https://www.nature.com/documents/commsj-file-checklist.pdf>.

OPEN ACCESS:

Communications Earth & Environment is a fully open access journal. Articles are made freely accessible on publication under a [CC BY license](http://creativecommons.org/licenses/by/4.0) (Creative Commons Attribution 4.0 International License). This license allows maximum dissemination and re-use of open access materials and is preferred by many research funding bodies.

For further information about article processing charges, open access funding, and advice and support from Nature Research, please visit <https://www.nature.com/commsenv/article-processing-charges>

At acceptance, you will be provided with instructions for completing this CC BY license on behalf of all authors. This grants us the necessary permissions to publish your paper. Additionally, you will be asked to declare that all required third party permissions have been obtained, and to provide billing information in order to pay the article-processing charge (APC).

Please use the following link to submit the above items:

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 **

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

Best regards,

Dr Alireza Bahadori
Associate Editor
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

Review of:
664-663 BCE is the exact timing of the Miyake ca. 660 BCE solar-proton event

T J Heaton

Summary:

This paper introduces two new time series of ^{14}C measurements in annual tree-rings over the ca. 660BCE Miyake event. They investigate the timing and size of the event in different locations. This is done visually and also using the ticktack software.

It seems there is some disagreement between reviewer 2 and the authors – specifically on ticktack. My reading is that some of this disagreement is due to a misunderstanding by the authors of what the reviewer is actually suggesting/requesting.

It seems like the reviewer is not particularly asking for more work to be done, just clarity and further explanation on what is actually being presented. I tend to agree with the reviewer that the way the ticktack various models were fitted to these trees is hard to follow for a reader and needs more explanation (but critically, not more actual work).

On the other hand, I think reviewer 2 might be getting a little too focussed on the details of ticktack itself regarding the queries about the phase parameter for one tree (I am guessing that they might be involved in that software in some way). I highly doubt anything requested by reviewer 2 on this point will change any of the conclusion – most of the conclusions are directly evident simply from the $\Delta^{14}\text{C}$ plots of the trees (Fig 1 and 3) and I would be very dubious about the reliability of ticktack if the application of that software changed them substantially)

View on TickTack

I should add that I have not used the ticktack software. My assumption is that perhaps reviewer 2 is an author of that software so would somewhat defer to their expertise with the specifics of using that package.

My general view of ticktack is that it has some really nice ideas, but I do not believe that all the complexity it introduces is critical to inference for many aspects of these events, and some of the complexity (incorporating strong other modelling assumptions such as the spin-up and uniformity of 11yr solar cycles) probably introduces considerable biases in the estimates in other unforeseen ways.

The main use of ticktack in the original ticktack paper, is to assume that the atmospheric signal is part of a large carbon cycle. They then assume that the production of ^{14}C is a combination of:
a mean trend
an 11yr periodic cycle (of entirely fixed period and amplitude)
a sharp ESPE-type spike (which they model as some kind of super-Gaussian)

They then try and separate out these three solar production components from v. short tree-ring signals. This includes some nice physical components that may offer inference improvements but one could equally argue that the solar cycles aren't uniform in reality, the spin-up of their carbon cycle could easily be wrong, the carbon cycle isn't linear,

Ultimately, I am doubtful that one has to use ticktack in order to understand in which year the spikes in ^{14}C occurred (this is simply evident from the plots); or generally how big the production spikes were (again I think this is likely just a function of the size of the jump from one year to the next). Indeed, I would be quite dubious if the inferences were not fairly robust to many of the elements of ticktack.

Ticktack also has a non-parametric approach which I found to not be described quite as clearly in the original ticktack paper, but it appears they might fit a Gaussian Process (GP) to the raw data, and then try and differentiate that GP to estimate production.

Specifics of Paper – Figure 2 (What Does it Show?)

The description in this 600BCE paper of how ticktack was fitted (pg 5-7) to their tree rings is very unclear. I think that much of the disagreement between reviewer 2 and the authors would disappear if the authors simply explained more clearly the specific models that they fitted (and perhaps just gave the equations).

The description contains a lot of jargon which doesn't tell the reader much. There are lots of statistical words but they don't actually provide sufficient detail to specify the exact model - tell the reader exactly model you actually fitted with an equation. Parametric/non-parametric tells me nothing of real note – what is the parametric model?.

I agree with reviewer 2 that the plot in Figure 2 is not what the non-parametric version of ticktack normally seems to produce.

My guess (as reviewer 2 also suggests) is that this is NOT the non-parametric fitting procedure as described in the ticktack paper (that using a Gaussian process). Instead, I am assuming (like reviewer 2) that it is the production rate that would be required in any single year to interpolate the raw data (obtained by inverting the carbon cycle model).

I (and I am guessing reviewer 2) think this inverted annual production estimate is absolutely fine/valid to show (actually my view is that it's probably the most useful thing to show as an initial exploratory plot) – it is simply the accompanying explanation which needs to be corrected.

I do not understand the response from the authors here – they say it isn't the above, that it is a Gaussian process, but I am still not convinced as I don't know how they would have got from a Gaussian process (smooth and continuous) to a set of annual boxes.

Suggestion to Authors: Provide a much clearer explanation of the two models that was fitted, perhaps split into two sections. These would be much better presented using equations (which for the parametric model are explicitly given in the ticktack paper) rather than statistical jargon. Provide real clarity on what the output of Figure 2 is

Phase of Solar Cycle in Figure 4b

Here, I have more sympathy for the authors if they have simply applied the default ticktack. I don't think it is reasonable to expect them to be able to edit and improve the software (although they do need to fully understand what it does and describe it). Also, my feeling is that, ultimately the main results are really not reliant upon this (it seems minutiae of the method).

Firstly, I am assuming reviewer 1 actually means the Yamal larch (in yellow) as it is this which very sharply drops to zero as though it has been cut-off. The Altai larch (red) has no such cut-off.

I entirely agree that the underlying method should recognise the cyclicity of the phase parameter. However, this doesn't seem to be an issue with the prior – it's surely to do with the proposal distribution and the mixing within the MCMC? In this specific case:

1) It isn't actually clear whether there is a problem with the mixing and there should be a wrap-around mass to 11 in this specific case. It's not a totally cut-off drop at 0 but rather a very sharp drop and there does appear to be some wrap-around/multimodality from the purple (Choukai) tree. Perhaps the plotted yellow Yamal posterior is correct (and it definitely is 0 – 1 yrs after the maximum), and the MCMC mixing is working fine and it really is that concentrated in this case

2) Alternatively this is an issue with the proposal distribution in ticktack (or the mixing) and, in my view, it's not reasonable for the authors to be expected to fix and rewrite the software.

I don't know how the original ticktack authors implemented their method, but I am assuming the method includes a recognition that a proposal to move the phase parameter can wrap around (rather than considering it on the whole line and then taking a modulus afterwards). If so, then certainly a prior $U[0,11]$ is the right prior and should allow wrap-around if ticktack mixes correctly.

As a reviewer, I can't really comment on whether the authors have correctly implemented ticktack. I am willing to take the authors at their word here (if they say they have implemented the parametric ticktack properly) and the responsibility lies with them to have done so correctly.

Importantly, whether the phase for the ESPE in this tree is actually 0.5 or 10.5, I do not believe it makes any substantial different to the main results in the paper about the timing or the size of the ESPE (and if it did then I would have very considerable doubt about the reliability of ticktack as an approach/package). This perhaps seems to be a case that reviewer 2 has become a little too focussed on a technical aspect of a model which doesn't affect the bigger picture/inference (especially as in reality solar cycles aren't uniform as in ticktack).

Suggestion to Authors:

Again, I think this is resolved just by:

- A) Being clearer about the method (using equations to specify both the production model and the specific priors) when fitting the parametric model.
- B) Providing the code on, e.g., Github, so people can reproduce the results (and choose different priors if they so wish)

I don't think it is reasonable to expect the authors to go in and edit ticktack and address/investigate potential coding/mixing problems in the MCMC within it.

Overall, I think the authors really need to:

A) Improve the clarity of exposition of the methods on pg5-7 (to remove jargon, add an equation or two to formally specify their model for solar production rates and priors).

B) To upload their code to Github (or some other repository) so that the analysis can be repeated (and if reviewer 2 thinks their phase analysis is wrong and makes a big difference then they can check/redo it)

Importantly, I believe that their main conclusions (about the varying timing of the ESPE in the different trees) are highly robust to the specific of ticktick phasing as ultimately they are evident from the first plot in Fig 1a.

My other general comments:

More Major:

1) Very confusing notation for time periods when using – to denote interval. Sometimes, e.g. 993 – 4, it appears to be used to represent/imply a single year. However, in other places, e.g. 664 – 663 in the abstract it is supposed to denote two years.

2) Where is the data/analysis from the Vistula Oak?

3) I think it is worth noting how poorly the Altai larch is fitted (Fig 3a). This seems much more critical whether the phase of Yamal in comparison to a solar cycle is off by 0.5 yrs

Minor/Typos:

1) In Figure 3, if possible it would be useful to have a consistent and common $\Delta^{14}\text{C}$ scale in the y-axis (so you can compare the sizes of the events to one another more easily)

2) My understanding is that you are somewhat inconsistent in your definitions of ME and ESPEs compared with the wider literature. I was under the impression that a Miyake Event is used to refer to a sharp jump in ^{14}C (above a particular threshold). It becomes an ESPE if it is possible to identify the was caused by a solar particle event.

3) Pg 11 – you mean 993/4 CE event (not 934 CE)

Finally, I would say that it is actually not the case that the 660 BCE event is the only event which seems to have differential timing in the ^{14}C spike in different locations/trees. The 993 CE event also has similar properties as discussed in an upcoming perspective article:

Extreme Solar Storms and the Quest for Exact Dating with Radiocarbon Heaton, Bard, Bayliss, Blauuw, Bronk Ramsey, Reimer, Turney & Usoskin (Nature, to appear)

Not sure when this perspective will be out (hopefully next month or so).

Have also sent this as a Word document as the copying/poasting here has altered the typesetting/layout

** Visit Nature Research's author and referees' website at www.nature.com/authors for information about policies, services and author benefits**

Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Open Access This Peer Review File is licensed under a Creative Commons Attribution 4.0 International License, which permits use, sharing, adaptation, distribution and reproduction in any medium or format, as long as you give appropriate credit to the original author(s) and the source, provide a link to the Creative Commons license, and indicate if changes were made.

In cases where reviewers are anonymous, credit should be given to 'Anonymous Referee' and the source.

The images or other third party material in this Peer Review File are included in the article's Creative Commons license, unless indicated otherwise in a credit line to the material. If material is not included in the article's Creative Commons license and your intended use is not permitted by statutory regulation or exceeds the permitted use, you will need to obtain permission directly from the copyright holder.

To view a copy of this license, visit <https://creativecommons.org/licenses/by/4.0/>

Response to the reviewers' comments

Reviewer #1 (Remarks to the Author):

Panyushkina et al., '663 BCE is the start date of the Miyake solar-proton event at ca. 660 BCE'

¹⁴C in tree rings can identify strong solar proton events (SPE) on annual/sub annual scale, and several SPEs have been found in the past 12500 years. Among them, the SPE at ca. 660 BC appears unusually prolonged over several years. Several studies already discussed this observation, suggesting several SPEs over three years.

This study uses trees of two specially selected sites, one of high North latitude and one of high altitude, to consider ¹⁴C dispersion in the atmosphere and tree species aspects of carbon uptake at the 660 BCE interval. In addition, four published studies of the 660 BC SPE are discussed in detail in the work. The authors assign the extension of the ¹⁴C signal in the 663 BC event to the dispersion of ¹⁴CO₂ between stratosphere and troposphere, latitudinal mixing of ¹⁴CO₂ in the troposphere, and tree-species specific uptake of CO₂ during the growing interval. Hence diverse characters of solar events would not be required from the ¹⁴C sequence of the 660 BC SPE.

The study documents well planned sample choice, and presents detailed discussion of ¹⁴CO₂ propagation in the atmosphere and uptake by photosynthesis, which is not present in previous studies. These aspects definitively will guide future research using ¹⁴C to identify SPEs. I fully recommend publication of the paper as it is. It is an important contribution to the study of SPEs, using cosmogenic isotopes.

We are thankful to the Reviewer #1 for thorough reading of the manuscript and the expert knowledge about the Miyake events and the nature of ¹⁴C tree-ring proxies.

Reviewer #2 (Remarks to the Author):

Dear Editor,

Thank you for sending me this very interesting paper by Panyushkina et al on the 660 BCE Miyake Event. It is timely and important and executed well. It certainly merits publication with minor revisions detailed below.

I do note that it might require some light language editing for grammar.

Kind regards,
The Reviewer

.....

Major Comments:

- This is a good paper and I have mainly methodological comments and comments on presentation.

- The general thrust of the paper is that the new high latitude tree samples show short events in 660 BCE, rather than the prolonged events claimed in previous literature. This is pretty exciting!

In terms of presentation, the figures could get this point across *much* more clearly with a few minor changes. It is hard to read captions or small legends to distinguish which

curves/panels correspond to the new Altai and Yamal datasets and which are from the literature

We enlarged the font size and labeled each panel with the name of dataset. We re-phrased the figure captions stressing that the Altai and Yamal datasets are from this study.

- I suggest you display two warm shades (red, orange?) for these, and cool shades (blue, purple, green...) for the literature events, or vice versa, keeping colour consistent throughout the paper to make it clear which dataset is which (eg all the way through to Fig 4).

We followed this suggestion and changed the color coding of the datasets. Also, the added names of the datasets on the plot panels help follow quickly the differences between various datasets.

- Likewise Figures 2, 3, it is unclear which panel is which dataset - titles on each panel,

Added.

- Figure 4 is very interesting and I have a number of suggestions, beyond colour and formatting noted above, about how to improve it.

First, there are no units on the top two panels (presumably astronomical year and years) and 'times' for the production PDF should really be something explicit like q_0 yr as used in Zhang et al 2022 if you are using similar outputs from ticktack, or else in appropriate units like atoms/cm² or kg production if you prefer. If this is in the same q_0 yr units as in Zhang et al, the explanation in the body text is unclear about being x "times higher than the 11 year average production" - it should properly say x times the "average annual production".

The labeling of X and Y axis were corrected as suggested and the figure caption revised:

1) changed the time scale from Astro years to BCE and labeled Y-axis "year BCE" in the Panel a); title of Y axis is revised in Panel b-c-d similar to relevant figures in Zhang et al. 2023 "Modeling cosmic radiation event in tree rings.... "

2) changed the color coding of curves and named the datasets in the legend.

3) In the caption, we added clear definition of the production rate shown on the Panel d) as the following " radiocarbon production rate in the equivalent of years with steady-state production".

- Looking at the phase PDF, I think I can see a problem: the Altai larch is cut off strictly at zero years and the Choukai early wood strictly at 11 years. This implies hard edges to the prior on phase, where you really want it to be cyclic so that 0 and 11 years wrap around. Fixing this may lead to improvements in the fits.

We use the same length of time series in the calculations. The estimate of phase is very sensitive to the length of the series and noise (outliers) that some series have. We did have a statement that the phase estimate is not reliable because the length of solar cycle may vary (< or > 11 yrs). We added encouragement for future research of the spike (see the last paragraph of the text) to develop longer time series of 14C. This will help to pinpoint the phase of solar cycle.

- Overall, in light of the comments above, I think it would be beset to try a joint fit to all 5 datasets to see if the joint fit has improved posteriors; and to display the multidimensional, covariant posteriors from the individual and joint fits as a corner plot (grid of 2d histograms) using for example chainconsumer or corner.py. This set of visualizations is likely to be very informative.

We do have these plots for each dataset but choose not use them due to the space limitation. Such plot for one dataset takes a haft of page.

- Straying a little from my expertise, I cannot comment on the correctness of the botany in the later sections but it is very interesting. I think it is easy to see how different plants in different regions would show a different amplitude event, as explained very well by the text. I struggle to see why this would change the apparent *duration* of the event though - perhaps the text could explain this more clearly.

The modeling showed that all datasets register the same duration of the event (Fig 4b) but with different degree of uncertainties, which depends on the strength of the spike signal and noise in the time series (d14C).

- A major implication of this work is that archaeological dating of events using Miyake events as tie-points (eg Dee & Pope 2016, Kuitens et al 2021) is actually not as precise as archaeologists might have hoped. If the apparent duration and start date of these events varies from tree to tree by ~ a year or two, this isn't disastrous for archaeology but is likely to be a significant impact on their methods and the confidence of their interpretations. The impact of this paper will be higher if the authors point out this problem in the text.

This is true. We stressed this point in Abstract adding the sentence " Using this ME for a single-year-dating could be problematic", and in the text (very last paragraph) " Finally, the double pulse of the 663–662 BCE ME onset and the prolonged waning of the ¹⁴C spike signal implies possible uncertainties complicating the use of this spike signal for single-year dating of archaeological timbers and occurrences. "

Minor Comments:

- Might be a bit controversial to say six Miyake events when the Bard et al 2023 event of 14.3 cal year BP is published now.

We counted the 14.3 BP event and cited Bard et al 2023. Six events include: 774 CE, 993CE, ca. 660 BCE, 5259 BCE, 7176 BCE and 12450 BCE.

- It is probably also a little too strong to say that Miyake et al citations 4 and 5 conclusively demonstrated that 774 and 993 were SPE; this wasn't really a consensus until Usoskin 2013 and then later the beryllium data, at least from memory.

The statement is not linked to citations [4] and [5] and the demonstration that 774 and 993 were SPE. This statement are related to the successful use of ^{14}C measurements in tree rings and placed next to the quote from P. Damon paper. No action is taken.

- I'm not sure why the astronomical year with a zero year is used rather than BCE in line with other publications on these data.

This is very valuable comment. Using two time scales could be very confusing.

We recalculated all data in calendar scale and re-plotted the results for all figures changing the astronomical years to BCE years.

- One possibility not addressed by the text is that a poor fit to the data could be model error - an incorrect carbon box model, in that the preindustrial carbon cycle may have different reservoirs to the Brehm 22 box model employed. There isn't much we can do about it here, but it should be noted - future work might try to directly infer preindustrial carbon box model parameters from tree-ring or other data, and this is likely to be very valuable. It is also worth noting that general circulation models to predict the detailed kinematics may explain why some regions experience different sub-annual and few-year kinematics.

Indeed, the carbon box models is far from being perfect in parametrizing the global carbon cycle and the direct inference of pre-industrial carbon from tree rings needs to be explored. Before writing up our results we experimented with two other CBM (Gutler et al 2015 and Brehm et al 2021). The calculation results varied very insignificant. We want to avoid going to the proposed argumentation and keep our study within the accepted parameters for different reservoirs.

General circulation models could be very helpful in characterization of climate variations in the studied interval but at the large scale. We believe that when diagnosing ME, it is also necessary to take into account the influence of different environments, species and tree phenology of particular tree, which experienced the impact of local weather conditions.

- The 1-month timesteps noted in the appendix on use of ticktack seem a little short when we are dealing with very detailed questions of time resolution - perhaps going a factor of a few finer in time resolution might be good for numerical robustness (though I don't think this is likely to be a major issue). It is probably also worth citing emcee (Foreman-Mackey et al) for the MCMC sampler.

Generally, the length of growing season may vary from 2 months (e.g. foremost north of tree distribution like in Taymyr and Yamal peninsula) to 6 months (e.g. in Tucson, Rincon Mountains). Thickening the cell walls (source of cellulose) occurs during the late part of the growing season. Depending on the weather from one year to another, the window of cell thickening / carbon allocation may change and shift (earlier or later) from one year to another. A one-month resolution is sufficient in our point of view since we do not have clear understandings of the intersessional and annual patterns of structural carbon allocation in stem at each of these locations back in 660 BCE. Finer time resolution may introduce a noise to the calculations.

We added citation for emcee in the Methods. Thank you.

Foreman-Mackey D., Hogg D.W, Lang D., Goodman J. 2013 mcee: The MCMC Hammer.
Astronomical Society of the Pacific 125 306, 10.1086/670067

Response to the reviewers' comments

We are thankful to the Reviewer #2 for the quick response and helpful additional comments. Our response is written in blue color and placed below the raised concern or comment.

Reviewer #2 (Remarks to the Author):

Dear Editor,

Thank you for sending me this revised manuscript. It is much improved and nearly ready for publication - there are a couple of minor points raised in the resubmission that might require additional comment.

Kind regards,

The reviewer

Main Comments:

- In the authors' response, it seems like they may have misunderstood my comment about the phase PDF. I am not concerned about the length of the time-series - but about the boundary conditions used in the prior over the phase of the solar cycle. It appears that this is a uniform prior in (0, 11) years, but it needs to be cyclic appropriately - the PDF for the Altai larch seems to be running up against 0 in a way that would indicate it is being cut off. This is nothing to do with the length of the time series but only to do with the choice of prior - and this is my most important comment - if this is not fixed, the results of that fit are definitely going to be wrong.

We used a fixed duration of 12-year solar cycle, and the bounding values of solar cycle phase are 0 and 11. Although, we doubt that the Altai larch series was cut-off in the modeled result, we re-calculated the posteriors probabilities for all time-series and each parameter. The PDF peak of the Altai series has moved away from the solar maximum ("0"), but the Yamal time series curve got closer to the zero. This probably happened because we adjusted the time scale of these two series relatively each other due to a resolved discrepancy in tree-ring cross-dating between the Yamal and Altai master chronologies (this could be seen in Fig 1a).

Action taken: The Fig 4 was updated. We added this sentence in the Fig. 4 caption: "The bounding values of solar cycle phase are 0 and 11".

- If the authors have a joint fit to all datasets, which has been excluded for space reasons, I think this is important to include as an appendix/supplementary online material/in whatever form is seen appropriate by the authors and the editor - but it is certainly important that this is available somewhere. The phase relative to solar maximum may be better constrained there.

We added cornerplots in the Supplementary Materials. The cornerplots show posterior distribution of the ^{14}C spike-signal parameters from the MCMC modeling for the individual time series. The Altai time series plots show no cut-off.

- Likewise, the marginal posteriors in Fig 4 are interesting, but it is important (even if only in an appendix) that the authors display the full covariant posteriors with corner plots, such as from chainconsumer or pairplots.jl.

Included in the Supplemental Materials, thank you for the suggestion.

- I would still do finer timesteps than 1 month in the use of ticktack, for numerical stability of the solver - it's not about whether finer time resolutions are irrelevant because of the trees, but whether the numerics of the solver are correct.

We re-run the inverse-solver code and got the same results. 1000 iterations. No difference is found with the previous runs. We use different approaches for cross-examination and parameterization of the spike signal in our new time series. Our two new datasets from the Altai and Yamal show the enhancement of ^{14}C production during 664-663 BCE more than 3 times over the global average rate. This result is consistent (not lesser) with the parametric estimate of the production rate from these datasets.

- Figure 2 I maybe don't quite understand - this is a step plot, which is the format produced by the inverse-solver in ticktack, but the caption says this is with MCMC and control points - which interpolates a smooth function with a Gaussian Process. There is a mismatch between the caption and the figure that needs to be clarified.

We corrected the caption and change from the MCMC fitting to Gaussian fitting. Thank you for noticing it. The same was applied to the sentence referring to Fig. 2. [Nonparametric Gaussian fitting uses direct $\Delta^{14}\text{C}$ measurements and estimates an average rate of 8 atoms $\text{cm}^{-2}\text{s}^{-1}$ over one growing season (Fig. 2)].

Minor Comments:

- Re having six events - I see that where I had misunderstood that the authors excluded Bard, I realize it was the 5410 BCE event. It merits comment as to why this is excluded.

Here we were setting the background for our main questions: When exactly the ca. 600 BCE event occurred. We meant ca. 660 BCE event, since its onset has not clearly been determined in the tree rings nor in the ice core. To avoid possible confusion by other readers, we re-phrased the beginning of this sentence. Now it sounds " Six these events are clearly tied to solar cosmic-ray events, as ^{10}Be has been independently measured at these times in ice cores ..."

- The authors argue that the statement about Miyake "conclusively" demonstrating "The statement is not linked to citations [4] and [5] and the demonstration that 774 and 993 were SPE. This statement are related to the successful use of ^{14}C measurements in tree rings and placed

next to the quote from P. Damon paper. No action is taken." The plain language reading of the text in the paper is that it links to citations 4 and 5, and there is ongoing discussion about whether these events were SPE. I strongly recommend removing the word "conclusively", if only that word.

We removed the word "conclusively" and slightly modified the sentence, which now sounds as the following: "Miyake et al. [4, 5] were the first to demonstrate the rapid excursions at 774–775 CE and 993–994 CE in ^{14}C tree-ring content, which were consequences of energetic solar proton events or extreme solar radiation storms."

- Why does the domain of the duration PDF in Fig 4 extend to negative values?

The German oak series is noisy, and the spike (pulse) signal is weak here. This series demonstrates a weak prolonged increase of ^{14}C between 664 BCE to 660 BCE (not sharp enhancement of the ^{14}C content as in other time series). We think the negative value is a statistical artefact generated by the Gaussian function. The duration parameter derived from this time series is near 0 meaning that the Gaussian bell curve is centered near "0", so the early part of the bell curve went to minus values. We would be very concerned if this happened to our other times series but not to the German oak series.

- Agree with the author comments about CBMs vs GCMs - all I mean is that it would strengthen the future work section of the paper to say something about it there, even something more or less verbatim like the rebuttal letter.

Response to the reviewers' comments

We are thankful to the Reviewer #2 for the quick response and helpful additional comments. Our response is written in blue color and placed below the raised concern or comment.

Reviewer #2 (Remarks to the Author):

Dear Editor,

Thank you for sending me this revised manuscript. It is much improved and nearly ready for publication - there are a couple of minor points raised in the resubmission that might require additional comment.

Kind regards,

The reviewer

Main Comments:

- In the authors' response, it seems like they may have misunderstood my comment about the phase PDF. I am not concerned about the length of the time-series - but about the boundary conditions used in the prior over the phase of the solar cycle. It appears that this is a uniform prior in (0, 11) years, but it needs to be cyclic appropriately - the PDF for the Altai larch seems to be running up against 0 in a way that would indicate it is being cut off. This is nothing to do with the length of the time series but only to do with the choice of prior - and this is my most important comment - if this is not fixed, the results of that fit are definitely going to be wrong.

We used a fixed duration of 12-year solar cycle, and the bounding values of solar cycle phase are 0 and 11. Although, we doubt that the Altai larch series was cut-off in the modeled result, we re-calculated the posteriors probabilities for all time-series and each parameter. The PDF peak of the Altai series has moved away from the solar maximum ("0"), but the Yamal time series curve got closer to the zero. This probably happened because we adjusted the time scale of these two series relatively each other due to a resolved discrepancy in tree-ring cross-dating between the Yamal and Altai master chronologies (this could be seen in Fig 1a).

Action taken: The Fig 4 was updated. We added this sentence in the Fig. 4 caption: "The bounding values of solar cycle phase are 0 and 11".

- If the authors have a joint fit to all datasets, which has been excluded for space reasons, I think this is important to include as an appendix/supplementary online material/in whatever form is seen appropriate by the authors and the editor - but it is certainly important that this is available somewhere. The phase relative to solar maximum may be better constrained there.

We added cornerplots in the Supplementary Materials. The cornerplots show posterior distribution of the ^{14}C spike-signal parameters from the MCMC modeling for the individual time series. The Altai time series plots show no cut-off.

- Likewise, the marginal posteriors in Fig 4 are interesting, but it is important (even if only in an appendix) that the authors display the full covariant posteriors with corner plots, such as from chainconsumer or pairplots.jl.

Included in the Supplemental Materials, thank you for the suggestion.

- I would still do finer timesteps than 1 month in the use of ticktack, for numerical stability of the solver - it's not about whether finer time resolutions are irrelevant because of the trees, but whether the numerics of the solver are correct.

We re-run the inverse-solver code and got the same results. 1000 iterations. No difference is found with the previous runs. We use different approaches for cross-examination and parameterization of the spike signal in our new time series. Our two new datasets from the Altai and Yamal show the enhancement of ^{14}C production during 664-663 BCE more than 3 times over the global average rate. This result is consistent (not lesser) with the parametric estimate of the production rate from these datasets.

- Figure 2 I maybe don't quite understand - this is a step plot, which is the format produced by the inverse-solver in ticktack, but the caption says this is with MCMC and control points - which interpolates a smooth function with a Gaussian Process. There is a mismatch between the caption and the figure that needs to be clarified.

We corrected the caption and change from the MCMC fitting to Gaussian fitting. Thank you for noticing it. The same was applied to the sentence referring to Fig. 2. [Nonparametric Gaussian fitting uses direct $\Delta^{14}\text{C}$ measurements and estimates an average rate of 8 atoms $\text{cm}^{-2}\text{s}^{-1}$ over one growing season (Fig. 2)].

Minor Comments:

- Re having six events - I see that where I had misunderstood that the authors excluded Bard, I realize it was the 5410 BCE event. It merits comment as to why this is excluded.

Here we were setting the background for our main questions: When exactly the ca. 600 BCE event occurred. We meant ca. 660 BCE event, since its onset has not clearly been determined in the tree rings nor in the ice core. To avoid possible confusion by other readers, we re-phrased the beginning of this sentence. Now it sounds " Six these events are clearly tied to solar cosmic-ray events, as ^{10}Be has been independently measured at these times in ice cores ..."

- The authors argue that the statement about Miyake "conclusively" demonstrating "The statement is not linked to citations [4] and [5] and the demonstration that 774 and 993 were SPE. This statement are related to the successful use of ^{14}C measurements in tree rings and placed

next to the quote from P. Damon paper. No action is taken." The plain language reading of the text in the paper is that it links to citations 4 and 5, and there is ongoing discussion about whether these events were SPE. I strongly recommend removing the word "conclusively", if only that word.

We removed the word "conclusively" and slightly modified the sentence, which now sounds as the following: "Miyake et al. [4, 5] were the first to demonstrate the rapid excursions at 774–775 CE and 993–994 CE in ^{14}C tree-ring content, which were consequences of energetic solar proton events or extreme solar radiation storms."

- Why does the domain of the duration PDF in Fig 4 extend to negative values?

The German oak series is noisy, and the spike (pulse) signal is weak here. This series demonstrates a weak prolonged increase of ^{14}C between 664 BCE to 660 BCE (not sharp enhancement of the ^{14}C content as in other time series). We think the negative value is a statistical artefact generated by the Gaussian function. The duration parameter derived from this time series is near 0 meaning that the Gaussian bell curve is centered near "0", so the early part of the bell curve went to minus values. We would be very concerned if this happened to our other times series but not to the German oak series.

- Agree with the author comments about CBMs vs GCMs - all I mean is that it would strengthen the future work section of the paper to say something about it there, even something more or less verbatim like the rebuttal letter.

Reply to Reviewer #2

We thank the reviewer for their suggestions. We would like to clarify the following issues using step-by-step response. Our response is given in black ink below red coloured comment.

> Nevertheless, Figure 4 does not resemble the Gaussian Process outputs from ticktack, but does resemble the Inverse Solver, which is not based on a GP but actually a direct inverse of the forward model. It is essential that the authors are clear and correct about what they did here, as these are all quite different things. It could be helpful to link to a GitHub (or other provider) repository with a Jupyter Notebook that they used for the analysis.

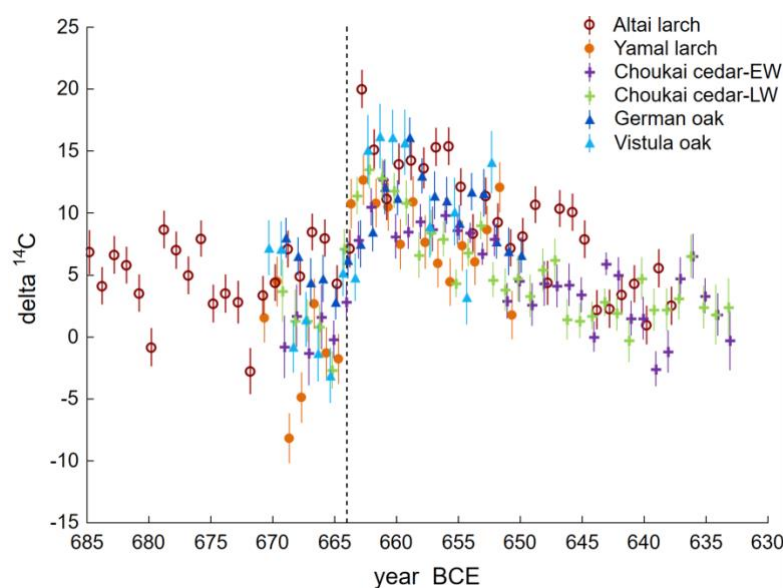
The output data plotted in Figure 4 is obtained with the single fitter py-code of Ticktack (not the notebooks). The output data was plotted separately, because the TickTack package cannot run under Windows. The py-code used to obtain output data is the single fitter provided in the TickTack package. The fact that the reviewer does not recognise the format of Fig.4 is due to the multiple issues with the package that requires external tools for visualising its output.

Because our goal is to obtain research results rather than improve the TickTack package, we have implemented multiple amendments in the code to make it at least work and provide numeric output.

> Re the issue with the solar cycle phase: in the recent response the authors note "We used a fixed duration of 12-year solar cycle, and the bounding values of solar cycle phase are 0 and 11." This is not the issue (though I note that if you are using a 12 year solar cycle period, the phase should extend from 0 to 12 not 0 to 11). The issue is that this is a hard edged prior, but if the peak is close to 0 it should appropriately know that phase = -1 is actually the same as phase = (max phase - 1). This is important and the paper cannot be accepted while there is clearly still a problem here.

The message of our paper is the accurate timing of the event in the new data. This is a novel important result, which is good to confirm by some confidence plots, even if the reviewer does not like our plotting style or discussion of the Gaussian processes. The bounds used were 0 to 11 years, which reflects the solar cycle. We did not use 12 years.

In fact, it can be seen even by naked eye, without any software, that our data uncovers the correct timing: the event happens earlier than previously thought, in 663BC rather than in 660BC, and this is the main message of our paper.



> If the authors do not wish to implement a cyclic prior themselves, they could raise an issue at <https://github.com/SharmaLlama/ticktack/issues>; or there is an even easier way. Where the posterior peak is close to the bound, just move the bounds and recalculate; eg if it's close to 0 or 12 and getting cut off, you could set the bounds to be (-6, 6) so that they are now centred on 0.

To satisfy the criticism about Figure 4, we removed the two panels of Figure 4 (duration and solar cycle phase) that cause the issues, as this is a minor piece of analysis and can be omitted.

> The authors note about the bug in allowing negative durations in Figure 4 that "We think the negative value is a statistical artefact generated by the Gaussian function. The duration parameter derived from this time series is near 0 meaning that the Gaussian bell curve is centered near "0", so the early part of the bell curve went to minus values." This is not true. There should be no Gaussian function or bell curve here. This is an MCMC output and no Gaussian function is used. It should be impossible to get a duration less than 0 and this indicates that there is a problem, whether in the analysis code or the plotting. As the corner plots in supplementary material don't show this, I suggest it is the plotting code; perhaps the authors have missed that duration is done in logarithmic units? If the authors wish to get support for their use of their code, the development team are available at <https://github.com/SharmaLlama/ticktack/issues> and responsive to issues.

We would like to publish the new data with accurate timing, and We would like to do this without further delay, rather than help in software development of the code. However, also noted in our reply to the editor, we would accept an independent third review.

Response to Reviewer #3

July 11, 2024

Dear Dr. Heaton,

We greatly appreciate your insights on the TickTack and the review of our manuscript. We have incorporated our response to the reviewer's comments below, marked with blue ink. The manuscript has been revised based on these suggestions, and we are confident in the improvements made.

Thank you,
Irina Panyushkina

REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

Review of:

664-663 BCE is the exact timing of the Miyake ca. 660 BCE solar-proton event

T J Heaton

Summary:

This paper introduces two new time series of ^{14}C measurements in annual tree-rings over the ca. 660BCE Miyake event. They investigate the timing and size of the event in different locations. This is done visually and also using the ticktack software.

It seems there is some disagreement between reviewer 2 and the authors – specifically on ticktack. My reading is that some of this disagreement is due to a misunderstanding by the authors of what the reviewer is actually suggesting/requesting.

It seems like the reviewer is not particularly asking for more work to be done, just clarity and further explanation on what is actually being presented. I tend to agree with the reviewer that the way the ticktack various models were fitted to these trees is hard to follow for a reader and needs more explanation (but critically, not more actual work).

On the other hand, I think reviewer 2 might be getting a little too focussed on the details of ticktack itself regarding the queries about the phase parameter for one tree (I am guessing that they might be involved in that software in some way). I highly doubt anything requested by reviewer 2 on this point will change any of the conclusion – most of the conclusions are directly evident simply from the $\Delta^{14}\text{C}$ plots of the trees (Fig 1 and 3) and I would be very dubious about the reliability of ticktack if the application of that software changed them substantially)

View on TickTack

I should add that I have not used the ticktack software. My assumption is that perhaps reviewer 2 is an author of that software so would somewhat defer to their expertise with the specifics of

using that package.

My general view of ticktack is that it has some really nice ideas, but I do not believe that all the complexity it introduces is critical to inference for many aspects of these events, and some of the complexity (incorporating strong other modelling assumptions such as the spin-up and uniformity of 11yr solar cycles) probably introduces considerable biases in the estimates in other unforeseen ways.

The main use of ticktack in the original ticktack paper, is to assume that the atmospheric signal is part of a large carbon cycle. They then assume that the production of ^{14}C is a combination of:

- a mean trend an 11yr periodic cycle (of entirely fixed period and amplitude)
- a sharp ESPE-type spike (which they model as some kind of super-Gaussian)

They then try and separate out these three solar production components from v. short tree-ring signals. This includes some nice physical components that may offer inference improvements but one could equally argue that the solar cycles aren't uniform in reality, the spin-up of their carbon cycle could easily be wrong, the carbon cycle isn't linear,

Ultimately, I am doubtful that one has to use ticktack in order to understand in which year the spikes in ^{14}C occurred (this is simply evident from the plots); or generally how big the production spikes were (again I think this is likely just a function of the size of the jump from one year to the next). **Indeed, I would be quite dubious if the inferences were not fairly robust to many of the elements of ticktack.**

Specifics of Paper – Figure 2 (What Does it Show?)

The description in this 600BCE paper of how ticktack was fitted (pg 5-7) to their tree rings is very unclear. I think that much of the disagreement between reviewer 2 and the authors would disappear if the authors simply explained more clearly the specific models that they fitted (and perhaps just gave the equations).

The description contains a lot of jargon which doesn't tell the reader much. There are lots of statistical words but they don't actually provide sufficient detail to specify the exact model - tell the reader exactly model you actually fitted with an equation.

The jargon comes from Zhang et al 2022 paper that defines the TickTack python code with the wording "parametric" (SingleFitter class object) and "non-parametric" (InverseSolver class object) Bayesian inference of ^{14}C production rate modeled with the 22-box carbon model (CBM) from the delta ^{14}C tree-ring data. In [Zhang et al 2022], the model equation of the production rate (Eq.2.4, ODE) is called "parametric model", and by "non-parametric model", they mean the results of applying inverse solver to the ODE of the production rate (Eq.2.7). We follow this terminology because we use their software package in analyzing our data.

TickTack package offers these two approaches to estimate the production rate with the Bayesian statistics. **The SingleFitter class applies MCMC for sampling from the posteriors. The InverseSolver class uses a Matérn–3/2 Gaussian process (Williams & Rasmussen 2006, we added this reference).** Overall, we observe that the parametric approach to modeling production rate estimates it more accurately. The non-parametric approach produces more coarse estimates from the reconstructed posterior probabilities of $\Delta^{14}\text{C}$. Zhang et al 2022 suggest using one or another approaches and considers the non-parametric fitting as an alternative one. **We use both fitting approaches** because the complexity of the spike signal and

the diversity of geography and tree species of the studied tree ring data. Both fittings employ the 22-box carbon model as described in Buntgen et al 2018 (available in the TickTack package). The results of these two fittings are slightly different (specifically for cedar earlywood), which we discuss.

- We included two equations in the Methods that define the parametric and non-parametric fittings from Zhang et al 2022.
- We clarified and defined better what is here called parametric and non-parametric fitting following the TickTack developers terminology minus jargon (like InverseSolver class object and SingleFitter class object).

I agree with reviewer 2 that the plot in Figure 2 is not what the non-parametric version of ticktack normally seems to produce. My guess (as reviewer 2 also suggests) is that this is NOT the non-parametric fitting procedure as described in the ticktack paper (that using a Gaussian process). Instead, I am assuming (like reviewer 2) that it is the production rate that would be required in any single year to interpolate the raw data (obtained by inverting the carbon cycle model). I (and I am guessing reviewer 2) think this inverted annual production estimate is absolutely fine/valid to show (actually my view is that it's probably the most useful thing to show as an initial exploratory plot) – it is simply the accompanying explanation which needs to be corrected.

Yes, this is correct understanding. The plot was produced with the alternative fitting of the 22-box carbon model using TickTack's Inverse Solver code (Myinversesolver.ipynb). The code output includes a box plot of production rate estimated from the reconstructed series, and plot of measured and reconstructed $\delta^{14}\text{C}$ series.

In our Figure 2 we show only the box plot of production rate calculated with InverseSolver. The second type of plots of $\delta^{14}\text{C}$ are omitted due to redundancy, they very closely follow the measured $\delta^{14}\text{C}$ shown in Figure 1.

- We revised the caption of Figure 2, included the fitting equation (1.2) in the Methods, and named the code (InverseSolver) for this box plot, and removed the wording referring to the TickTack's Control points code (Mycontrols.ipynb), which was mistakenly left from the earlier version of the manuscript.

I do not understand the response from the authors here – they say it isn't the above, that it is a Gaussian process, but I am still not convinced as I don't know how they would have got from a Gaussian process (smooth and continuous) to a set of annual boxes.

The box plot visualizes the mean and the standard deviation of the chain. It looks coarse but suits for the comparison.

Suggestion to Authors: Provide a much clearer explanation of the two models that were fitted, perhaps split into two sections. These would be much better presented using equations (which for the parametric model are explicitly given in the ticktack paper) rather than statistical jargon. Provide real clarity on what the output of Figure 2 is.

Phase of Solar Cycle in Figure 4b

Here, I have more sympathy for the authors if they have simply applied the default ticktack. I don't think it is reasonable to expect them to be able to edit and improve the software (although they do need to fully understand what it does and describe it). Also, my feeling is that, ultimately the main results are really not reliant upon this (it seems minutiae of the method).

Firstly, I am assuming reviewer 1 actually means the Yamal larch (in yellow) as it is this which very sharply drops to zero as though it has been cut-off. The Altai larch (red) has no such cutoff. I entirely agree that the underlying method should recognise the cyclicity of the phase parameter. However, this doesn't seem to be an issue with the prior – it's surely to do with the proposal distribution and the mixing within the MCMC? In this specific case:

1) It isn't actually clear whether there is a problem with the mixing and there should be a wraparound mass to 11 in this specific case. It's not a totally cut-off drop at 0 but rather a very sharp drop and there does appear to be some wrap-around/multimodality from the purple (Choukai) tree.

Perhaps the plotted yellow Yamal posterior is correct (and it definitely is 0 – 1 yrs after the maximum), and the MCMC mixing is working fine and it really is that concentrated in this case
2) Alternatively this is an issue with the proposal distribution in ticktack (or the mixing) and, in my view, it's not reasonable for the authors to be expected to fix and rewrite the software.

I don't know how the original ticktack authors implemented their method, but I am assuming the method includes a recognition that a proposal to move the phase parameter can wrap around (rather than considering it on the whole line and then taking a modulus afterwards). If so, then certainly a prior $U[0,11]$ is the right prior and should allow wrap-around if ticktack mixes correctly.

As a reviewer, I can't really comment on whether the authors have correctly implemented ticktack. I am willing to take the authors at their word here (if they say they have implemented the parametric ticktack properly) and the responsibility lies with them to have done so correctly. Importantly, whether the phase for the ESPE in this tree is actually 0.5 or 10.5, I do not believe it makes any substantial difference to the main results in the paper about the timing or the size of the ESPE (and if it did then I would have very considerable doubt about the reliability of ticktack as an approach/package). This perhaps seems to be a case that reviewer 2 has become a little too focussed on a technical aspect of a model which doesn't affect the bigger picture/inference (especially as in reality solar cycles aren't uniform as in ticktack).

Suggestion to Authors:

Again, I think this is resolved just by:

A) Being clearer about the method (using equations to specify both the production model and the specific priors) when fitting the parametric model.

B) Providing the code on, e.g., Github, so people can reproduce the results (and choose different priors if they so wish)

I don't think it is reasonable to expect the authors to go in and edit ticktack and address/investigate potential coding/mixing problems in the MCMC within it.

We have updated the caption of Figure 4 to ensure the designation of the parametric Bayesian inference for the estimated parameters (start date and production rate) as outlined in the Methods section. The figure provides a summary of these two parameters derived from the analysis of the four time series related to this event.

The authors did not produce any original calculation codes – those are obtained using TickTack that the authors have acknowledged. The [plotting codes](#) were prepared in Matlab due to local software setup limitations. They are not novel and serve visualization only, taking the [calculation output from TickTack](#).

Overall, I think the authors really need to:

- A) Improve the clarity of exposition of the methods on pg5-7 (to remove jargon, add an equation or two to formally specify their model for solar production rates and priors).
- B) To upload their code to Github (or some other repository) so that the analysis can be repeated (and if reviewer 2 thinks their phase analysis is wrong and makes a big difference then they can check/redo it)

Importantly, I believe that their main conclusions (about the varying timing of the ESPE in the different trees) are highly robust to the specifics of ticktack phasing as ultimately they are evident from the first plot in Fig 1a.

My other general comments:

More Major

- 1) Very confusing notation for time periods when using – to denote interval. Sometimes, e.g. 993 – 4, it appears to be used to represent/imply a single year. However, in other places, e.g. 664 – 663 in the abstract it is supposed to denote two years.

We checked and corrected the two-year abbreviation of the ME dates including 993-994 CE, 774-775 CE and 664-663 BCE.

- 2) Where is the data/analysis from the Vistula Oak?

This Vistula oak dataset is short (<20yrs) time series with few gaps and was measured at two different AMS facilities. To avoid possible laboratory bias and keep the 20-yr common interval for the CMB modeling, we removed this series from the modeling, but showed in the Figure 1 where all available ^{14}C delta data of this event presented. This was briefly explained in the caption of Figure 1. To ensure the understanding, we added a sentence reasoning about the removal of the Vistula oak series from the CBM modeling to the last paragraph of the Introduction.

- 3) I think it is worth noting how poorly the Altai larch is fitted (Fig 3a). This seems much more critical whether the phase of Yamal in comparison to a solar cycle is off by 0.5 yrs

This was noted in the Discussion. Additionally, we include the following sentence in the second paragraph of the Results where the Figure 3 is presented. "Despite the Altai larch fit being affected by the sharp rise in the second year of the spike pulse (663 BCE)."

Minor/Typos:

1) In Figure 3, if possible it would be useful to have a consistent and common $\Delta^{14}\text{C}$ scale in the y-axis (so you can compare the sizes of the events to one another more easily)

Unfortunately, changing the scale would necessitate recalculating everything. Therefore, we have decided to disregard the suggestion.

2) My understanding is that you are somewhat inconsistent in your definitions of ME and ESPEs compared with the wider literature. I was under the impression that a Miyake Event is used to refer to a sharp jump in ^{14}C (above a particular threshold). It becomes an ESPE if it is possible to identify the was caused by a solar particle event.

Unfortunately, a clear definition of a Miyake Event is currently lacking. We define Miyake Event as a ^{14}C spike in tree rings which nature associated with a solar energetic particle event through other cosmogenic isotopes of ice cores. Therefore, we believe that not every ^{14}C spike observed in tree-ring content qualifies as a Miyake event. The nature of ^{14}C spikes is much broader than the impact of solar proton events on the atmosphere's chemistry.

3 3) Pg 11 – you mean 993/4 CE event (not 934 CE).

Yes, we corrected the typo.

Finally, I would say that it is actually not the case that the 660 BCE event is the only event which seems to have differential timing in the ^{14}C spike in different locations/trees. The 993 CE event also has similar properties as discussed in an upcoming perspective article:

Extreme Solar Storms and the Quest for Exact Dating with Radiocarbon Heaton, Bard, Bayliss, Blauuw, Bronk Ramsey, Reimer, Turney & Usoskin (Nature, to appear)
Not sure when this perspective will be out (hopefully next month or so).

We look forward to the publication! Expanding the application of ^{14}C spike to precise single year dating as much important as defying the limitations and possible pitfalls of this approach.
