

Peer Review File

Manuscript Title: Single-year radiocarbon dating anchors Viking Age trade cycles in time

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1 (Remarks to the Author):

This paper provides a well-founded chronology for the important early medieval trading site of Ribe in Jutland. It establishes an original high-precision radiocarbon calibration curve for the relevant years, improving on the newly published IntCal20. It employs this curve, and Bayesian analysis based on archaeological stratigraphy, to calibrate 140 radiocarbon dates from a new excavation within the settlement. The results facilitate absolute dating of the introduction of trade goods from different sources, such as Norway, the Rhineland and the "Middle East". It is demonstrated that the regional imports arrive before the Middle Eastern ones, suggesting local rather than world-systems processes as the cause of the Viking Age. It is then argued that the chronology of increased trade can be associated with climatic cooling, and environmental causation is inferred.

The new chronology is an important contribution to knowledge. The relative dates of differing trade patterns are not entirely new, insofar as the stratigraphic relationships were previously known and phasing for this settlement has always been good for the 8th century (based on stratigraphy, artefact typology and some dendrochronological dates). Nevertheless, this paper provides an unambiguously absolute and high-precision chronology. It thus merits publication, but first a number of revisions (ranging from major to incidental) are strongly recommended.

Lines 17 and 88-90. The paper makes several claims of first usage (e.g. of annual calibration in archaeology). Such claims are very difficult to document or verify and the paper does not hinge on their veracity. It may be better to exclude them.

Lines 44-45. The authors need to explain briefly how a precise chronology will allow them to differentiate between the two hypotheses as stated (world system vs regional development).

Figure 1. The significance of this figure vis-à-vis the paper's stated goals is not made sufficiently clear. It would benefit from simplification, possibly sacrificing detail for clarity to an interdisciplinary readership. The key point must be to show, graphically, how better resolution by ^{14}C is now possible using the Aarhus calibration curve. This does not come across, especially as the size of the tree ring symbols obscures the curves in panel A. We also don't learn enough about the dated features/phases from this figure to make sense of Panel C. A new version of this panel could also be simplified by leaving out IntCal13, which is irrelevant. The only meaningful comparison is between IntCal20 and Aarhus. Moreover, it would be valuable if this figure could incorporate a highlighted illustration of the key SPEs, to show their relevance.

Lines 69 and 97. Please briefly define what is meant by more structure.

Line 78. It is not clearly explained why the Aarhus curve is bi-annual not annual.

Lines 86-87 and elsewhere. It is necessary to define where exactly is intended by the term Middle East, to say where specifically from within this region the beads are thought to come from, and on what evidence (e.g. typology, characterization)? The sources of these beads, key to the paper's main argument, are not clearly identified or justified.

Lines 102-105 presently read "A calibration with IntCal13 (34) favours a felling date before 700 CE, conflicting with the archaeological find context and artefact chronology, while IntCal20 and the Aarhus curve result in a bimodal distribution with another peak in the mid-700s CE, in agreement with the archaeological record." Please explain briefly on what bases the conflict is known to

exists, showing the reader that the logic is not circular, and that the radiocarbon work in the present paper is not redundant (i.e. if precise dating was already available).
Lines 147-148. For an interdisciplinary audience the authors need to say when the first Viking raids began at this point in the paper.

Line 151. Although the growth of Scandinavian trade to include the eastern Mediterranean is an example of globalization processes, it is not of course global. The wording ("global") thus needs to be revised here. It is also not unprecedented. Similar trade connections, for different goods, existed in the Roman Iron Age.

Lines 154-155. This statement needs clearer explanation of the calibration process that allows the authors to infer a single year. In addition to the calibration method, they need to set out clear evidence of stratigraphic integrity regarding the dated materials – that they are not intrusive or residual, and carry no residual age. It may help to note the raw material of these two samples here, to show they provide annual resolution (e.g. short-lived plant macrofossils). There also needs to be reassurance that they are in primary stratigraphic locations. Otherwise the 775 SPE identified could be displaced within the stratigraphy.

Line 159. When the authors say "the Rhine area", did they intend to say the Middle East, given that it is beads attributed to the latter source that are shown in relevant panel (C) of figure 3?

Lines 159-162. The text reads "Sub-phase F9f is a thick levelling layer that accumulated after the use of house K21 terminated and can be called the first Viking Age layer of Ribe. It is dated by AAR 32114 to 785-810 CE (posterior probability at 95.4% confidence, Table 12 in SI)." The date of this layer is attributed great significance in the paper, as the point in time when new long-range imports appear in the stratigraphy and as the basis of climate-change correlations. However, if it is a thick levelling layer, how can it be adequately dated by a single sample, and how can one be certain that this sample is not residual? Archaeological levelling layers very often represent redeposited and/or mixed material. In the supplementary material, the authors write "Layers or parts of layers with evidence for massive bioturbation were avoided as well as large homogenous layers that were interpreted as levelling layers, e.g. preparation of a plot after the demolition of a house, which could include substantial amounts of redeposited older material."

Figure 3. Panel A is really two panels (summer temperature anomaly and phase 9 calibrated dates) and should be labelled accordingly. The figure would be clearer if only the Aarhus curve results were included for the calibrated dates, with the IntCal20 comparison moved to supplementary material. It would also be helpful if the caption briefly explained the significance of showing the climate data, which has not yet been mentioned in the paper (but see further below on this issue). On a minor point, the units need to be provided for the scale of panel B.

Line 186. This is the first mention of environment in the paper, other than in Figure 3. If it is to be a major consideration it would be better if it were mentioned in the introduction and abstract. As noted further below, however, the authors may alternatively wish to rework their climate-based conclusions.

Line 187. Not exclusively maritime connectivity –also riverine, and overland trade routes are relevant for the Middle Eastern beads.

Lines 191-194. Previous publications regarding the chronology of Norwegian 'slate' (Baug et al. 2018) and reindeer antler (Ashby et al. 2015) at Ribe refer to dendrochronology. The authors thus need to clarify in what ways these absolute dates are provided "for the first time"?

Lines 199-201. Precedence in time makes causation possible, but does not prove causation. What is the mechanism that is inferred to have linked this trade to later raiding?

Line 202. There has not yet been any meaningful discussion of genomic evidence in the paper. What is its relevance when mentioned here without explanation?

Lines 202-210. Climate may be very relevant, but is considered here without any consideration of alternative and potentially mutually reinforcing variables. Thus it is not a tested hypothesis. The authors would need to elaborate considerably or to remove this assertion, which is correlation equals causation as written.

Line 210. Is there any evidence that early Viking-age raiding was in search of improved subsistence – as opposed to luxuries and power?

Lines 210-213. Sea ice was not a meaningful factor in Fennoscandia south of Svalbard, and the latter was not discovered until the 16th century. Sea ice was relevant to Iceland and Greenland, but the latter were not settled until the 870s and 980s respectively, well after c.800 CE (and earlier), the chronology discussed in this paper as the period of causation.

Referee #2 (Remarks to the Author):

The manuscript by Philippsen et al. reports on the first use of single-year radiocarbon calibration for an archaeological stratigraphical sequence to pinpoint the 774-75 CE solar proton event at an important Danish trading emporium and thus precisely secure the date of 790 ± 10 CE for the start of the Viking Age. The authors make use of the IntCal20 annual tree-ring data and supplement it where annual data is lacking in this portion of the calibration curve with their own high quality radiocarbon measurements of annual tree-rings from Denmark. The extensive radiocarbon and dendrochronological dates and detailed stratigraphy from the excavated site allow them to build a Bayesian model that, together with a massive number of artefacts, document the expanding trade from early evidence of trade with western Europe expanding to wider-ranging seafaring to trade with Scandinavia and finally to the arrival of Middle Eastern trade beads.

The manuscript is novel and will set the bar high for building robust chronologies for future archaeological studies. It will be also be of interest to researchers in disciplines such as history and social geography, as well as anyone interested in the Viking Age. The tree-ring ^{14}C measurements will no doubt be a welcome addition to the next update of the IntCal calibration curves and thus benefit all researchers who use radiocarbon dating in this time frame. The manuscript provides extensive supplemental information including artefact numbers, radiocarbon and dendrochronological dates and the OxCal models used to provide statistical sound dates and uncertainties. The document is clearly written (with the exception of a few minor points given below). The abstract is appropriate. The introduction provides sufficient background information and conclusions valid and reliable.

Specific comments:

Lines 61-72 Figure 1 caption.

What is the white square in Fig 1a? In Fig 1b, the y-axis is labelled IntCal13- annual curve whereas the caption says IntCal20 – annual curve. Fig 1c. It might be better to separate the wiggle match into Fig 1d and provide some explanation the nature of the samples in the sequence. How is straw related (I see in Suppl that it is embedded in outer bark)? What was dated in the wormhole - undetermined organic material? What are the numbers in front of the tree-ring samples e.g. '15. Treering'? If this means ring 15 then perhaps it would be more clear if this was written as 'Treering 15'.

Line 101-102: 'case of a wiggle-matched 15-year old wood sample (Fig. 1c). A calibration with IntCal13 (34) favours a felling date before 700 CE'. Here the '15-year old' wood sample could be confused with '15. Treering in Figure 1c' so it might be best to say 'outermost

ring' since that is what is important for a felling date.

Line 104: 'result in a bimodal distribution'. All the probability distributions shown are bimodal so you need to explain that there are higher probabilities in the later range with IntCal20 and Aarhus curve

Line 116, Fig 2 caption 'with trade routes (orange lines) to different regions'. Is there a references for the trade routes?

Line 154-155: The shift is marked by samples that calibrate to either side of the 775 SPE (SI, "Phase F9 subdivisions"). It's confusing to reference SI here since Fig 3a in the main document has the plot that shows this whereas SI "Phase F9 subdivisions" is general information about Phase F9.

Line 170-174. Figure 3a 'Shown is also a 25 year running mean of the annual reconstructed mean temperature as well as the mean and standard deviation of the reconstructed annual temperature for the periods 620 – 700 CE, 700 – 800 CE, 800 – 900 CE, 900 – 1000 CE and 1000 – 1100 CE, yielding temperature anomalies of 0.1 ± 0.3 °C, 0.1 ± 0.4 °C, 0.1 ± 0.4 °C, 0.1 ± 0.4 °C and 0.3 ± 0.3 °C.' The 100 year mean anomalies are all well within the standard deviation so don't tell us anything. I would suggest leaving those out. The 25 year running mean shows the temperature decline at the onset of the Viking Age more clearly and Figure 3b gives a visual reconstruction for the century-long intervals.

Lines 205-206: 'At the onset of the Viking Age, summer temperatures are at lowest (-0.4 °C)'. Is this from the 25-year mean summer temperatures? If so state that and qualify when, e.g. 'at the lowest (-0.4 °C) in the last x hundred years' or whatever timing is appropriate.

Supplemental information:

p. 18, paragraph 2: 'Samples for dendrochronology were only preserved in phases F4 to F9'. I think it would more appropriate to say 'Samples suitable for dendrochronological dating'

p. 18, paragraph 3: "For wiggle match-ing, several year rings of a wood or charcoal sample are radiocarbon dated. As the year rings between the dated samples are counted'. The term 'year rings' is confusing. Do you mean several single rings were analysed separately or several rings were combined for one analysis?

p. 26, paragraph 1: 'Until phase F12, it was possible to treat the dates from pot 1 and plot 2 in the'

'Pot 1' should be Plot 1.

Referee #3 (Remarks to the Author):

This reviewer has expert knowledge in Viking Age Archaeology, and is familiar with radiocarbon science only as a user of dating results received from labs and presented in publications. Thus, this review address the archaeological application of the methodological advances and the refined radiocarbon calibration curve presented in the paper as well as their significance for archaeological scholarship. This review does therefore not assess the validity of the methodological advances and the calibration curve, but is based on the assumption that they are valid.

The paper supplements the recent radiocarbon calibration curve based in annual tree-ring datings, IntCal20, with the authors' own biannual datings in periods where the IntCal20 curve is weak. This improved curve is applied to samples from a recent excavation of a well-documented site with complex stratigraphy and well-preserved organic materials, including wood. The site lies centrally in Ribe, western Jutland, which is a key site for exploring 8th–9th-century trade networks in the North Sea area and in north-western Scandinavia. By identifying the Solar Particle Event in 775 CE in samples from the site and combining radiocarbon dates with dendrochronology in a Bayesian

model, the paper anchors the site's relative chronology in calendar years, thus allowing high-precision datings of significant shifts in trade networks in the mid-late-8th and early-9th centuries. These shifts have been pivotal in explanations of the period's economic upsurge in Francia and the taking up of Viking raiding in the 790s. The paper dates these shifts much more precisely than has hitherto been possible, and the results appear to favour regional explanations to these phenomena over world-system explanations.

The paper presents novel and original results of major significance for scholarship on Ribe, on the early Viking Age, on the period's economy and trade in north-western Europe, and on the archaeological use of radiocarbon datings. Through the refinement of the calibration curve and by conceiving the highly innovative combination of calibration data, targeted sampling strategies, site stratigraphy, and dendrochronological data, the authors have developed a potent package of methods that will have great significance for dating sites with preserved burnt or unburnt wood.

This reviewer finds the paper to be of a very high quality and of outstanding interest to the scholarly communities worldwide, not only within Viking Age archaeology. The text is lucid and clear, and relevant references to previous research are included. With the caveats described below, conclusions are robust and reliable. Given that the authors deal adequately with the issues raised below, this reviewer highly recommends the paper for publication.

Suggested improvements:

1. While the paper's use of statistics and treatment of uncertainties in general seems cautious and balanced, the artefact counts in Figures 2d–h and 3c appear to be based in a biased sample that, potentially, does not provide sufficient basis for some of the paper's conclusions (lines 140–162). The excavation that has provided evidence for this study covers little more than one plot in the town. Therefore, the crafts, artisanal production, and other activities that left artefacts on this specific plot, for instance glass-bead production, crafts that demanded the use of whetstones, or storing and trade in imported beads, will potentially have had a huge impact on the number and types of finds. This means that the finds on this plot are not necessarily representative for the town or, in turn, for the shifting trade connections that the artefacts' distribution is used to identify and date (Figures 2h and 3c). For example, it seems a possible scenario that the consumption of Norwegian whetstones in phases F8 and F9a–e was much greater on plots where shoemakers and combmakers left their residue. The authors should comment on this uncertainty and, if possible, make amends to limit its significance.
2. Since the duration of the phases varies, it seems somewhat misleading to provide the frequency of the selected artefact types as counts rather than artefacts pr. year or decade (Figures 2d–h and 3c), which would allow realistic comparisons of their frequencies in the various phases.
3. In the text (line 146), the date of phase F7 is given as 733–745 CE. This is not quite the same span as indicated in Figure 2c (731/742–743/752). The authors should explain how they have arrived at the more precise dating of the phases, and also consider presenting the resulting dates for all phases, not only F7. Is 785–810 CE (lines 161, 189) the date range of phase F9?
4. The 'clear causal link' between trade and raiding asserted in lines 199–200 does not seem obvious to this reviewer. The link should be fleshed out or the issue should be left out altogether.
5. At the very end of the discussion section, the authors include climatic data (Figure 3d, lines 168–174 and 204–213). They suggest that the slight drop in summer temperatures in the late 700s and early 800s contributed to "the expansion of maritime raiding and trade in search for improved subsistence". While this argument has been used for more than a century to explain the Viking settlements overseas, which probably started in the mid-830s, they present no argument as to how raiding and trading might have contributed to subsistence in the earlier period, when raiding and trading were summer activities. Are they of the opinion that raiders and traders set out to obtain foodstuffs? This should be clarified, and the authors need to substantiate underlying

claim that subsistence production in Scandinavia around 800 was so weak that the reduction in summer temperature was a real threat to subsistence production.

6. The authors end the paper by suggesting that the low extent of sea ice around 800 provided “easier access to the northernmost areas of the Viking world” (lines 210–213). This claim is hard to assess, since they do not present information on the extent of sea ice at that time. However, current data (<https://www.npolar.no/en/themes/the-marginal-ice-zone/>) do not suggest that sea ice will have had any influence on the opportunities for accessing the northernmost waters known to have been navigated by Scandinavians at this time. Coastal sailing was the norm then, and during the sailing season (May–September) the southern edge of the marginal ice zone is found 300–600 kilometres north of the Scandinavian Peninsula. From July to September, one may sail unconstrained by sea ice eastwards to Novaja Semlja, which lies well beyond the easternmost Viking destinations evidenced in the late 800s. The authors need to substantiate why they consider sea ice significant for access to the northernmost areas.

7. This reviewer finds the introduction of climatic data at the very end of the paper, as well as the two applications of the data (above, nos. 5–6), to be the weakest part of the paper. Neither the abstract nor the introduction indicates that the paper aims at explaining why the Viking trading and raiding started, which is what the discussion of the climatic data attempts to do. This reviewer would suggest that the climatic data is left out and that instead, the authors return to the overall question that they pose in the introduction (lines 34–43), and which their results appear highly relevant for, namely the question whether the start of Viking trading and raiding is best explained in regional or world-system contexts.

Author Rebuttals to Initial Comments:

REVIEWER 1

This paper provides a well-founded chronology for the important early medieval trading site of Ribe in Jutland. It establishes an original high-precision radiocarbon calibration curve for the relevant years, improving on the newly published IntCal20. It employs this curve, and Bayesian analysis based on archaeological stratigraphy, to calibrate 140 radiocarbon dates from a new excavation within the settlement. The results facilitate absolute dating of the introduction of trade goods from different sources, such as Norway, the Rhineland and the “Middle East”. It is demonstrated that the regional imports arrive before the Middle Eastern ones, suggesting local rather than world-systems processes as the cause of the Viking Age. It is then argued that the chronology of increased trade can be associated with climatic cooling, and environmental causation is inferred.

The new chronology is an important contribution to knowledge. The relative dates of differing trade patterns are not entirely new, insofar as the stratigraphic relationships were previously known and phasing for this settlement has always been good for the 8th century (based on stratigraphy, artefact typology and some dendrochronological dates). Nevertheless, this paper provides an unambiguously absolute and high-precision chronology. It thus merits publication, but first a number of revisions (ranging from major to incidental) are strongly recommended.

REPLY:

We thank the reviewer for the very useful comments. We have revised the paper in accordance with the recommendations, cf. answers below.

Lines 17 and 88-90. The paper makes several claims of first usage (e.g. of annual calibration in archaeology). Such claims are very difficult to document or verify and the paper does not hinge on their veracity. It may be better to exclude them.

REPLY:

We have excluded most of the claims of “first usage”, and only left those that are verified.

Lines 44-45. The authors need to explain briefly how a precise chronology will allow them to differentiate between the two hypotheses as stated (world system vs regional development).

REPLY:

We have clarified how a precise chronology can differentiate between the two hypotheses in lines 34-43.

Figure 1. The significance of this figure vis-à-vis the paper’s stated goals is not made sufficiently clear. It would benefit from simplification, possibly sacrificing detail for clarity to an interdisciplinary readership. The key point must be to show, graphically, how better resolution by 14C is now possible using the Aarhus calibration curve. This does not come across, especially as the size of the tree ring symbols obscures the curves in panel A. We also don’t learn enough about the dated features/phases from this figure to make sense of Panel C. A new version of this panel could also be simplified by leaving out IntCal13, which is irrelevant. The only meaningful comparison is between IntCal20 and Aarhus. Moreover, it would be valuable if this figure could incorporate a highlighted illustration of the key SPEs, to show their relevance.

REPLY:

We have revised Figure 1 according to the reviewer’s suggestions. We have indicated the SPE that occurred within the timeframe of the figure (other SPEs were out of the time range). We have also considered the suggestion to leave out the comparison with IntCal13. However, we chose to keep it, as one of the strengths of the study is to demonstrate the significance of annual calibration as pioneered in IntCal20 and supplemented for selected periods by the baseline data published in the present study.

We have made sure that the curves are no longer obscured by the tree ring symbols. Generally, we have simplified the figure by moving panels b and c to the Supplementary Information.

Lines 69 and 97. Please briefly define what is meant by more structure.

REPLY:

This sentence has been clarified.

Line 78. It is not clearly explained why the Aarhus curve is bi-annual not annual.

REPLY:

This has now been clarified.

Lines 86-87 and elsewhere. It is necessary to define where exactly is intended by the term Middle East, to say where specifically from within this region the beads are thought to come from, and on what evidence (e.g. typology, characterization)? The sources of these beads, key to the paper's main argument, are not clearly identified or justified

REPLY:

We have specified this in the main text. Furthermore, we have included more details about the beads in the supplementary text. The provenance is based on the chemical composition and geographical distribution of the beads, as established by the cited studies. Unfortunately, it is not presently possible to identify the exact site where the beads were produced within the Middle East.

Lines 102-105 presently read "A calibration with IntCal13 (34) favours a felling date before 700 CE, conflicting with the archaeological find context and artefact chronology, while IntCal20 and the Aarhus curve result in a bimodal distribution with another peak in the mid-700s CE, in agreement with the archaeological record." Please explain briefly on what bases the conflict is known to exist, showing the reader that the logic is not circular, and that the radiocarbon work in the present paper is not redundant (i.e. if precise dating was already available).

REPLY: We have revised the text accordingly.

Lines 147-148. For an interdisciplinary audience the authors need to say when the first Viking raids began at this point in the paper.

REPLY: We have specified this in the paper.

Line 151. Although the growth of Scandinavian trade to include the eastern Mediterranean is an example of globalization processes, it is not of course global. The wording (“global”) thus needs to be revised here. It is also not unprecedented. Similar trade connections, for different goods, existed in the Roman Iron Age.

REPLY: We have revised the text accordingly.

Lines 154-155. This statement needs clearer explanation of the calibration process that allows the authors to infer a single year. In addition to the calibration method, they need to set out clear evidence of stratigraphic integrity regarding the dated materials – that they are not intrusive or residual, and carry no residual age. It may help to note the raw material of these two samples here, to show they provide annual resolution (e.g. short-lived plant macrofossils). There also needs to be reassurance that they are in primary stratigraphic locations. Otherwise the 775 SPE identified could be displaced within the stratigraphy.

REPLY: The text is revised and a statement on the integrity of the stratigraphy has been added. Samples are taken from a sequence with distinctive layers. We are however not suggesting a single year calibration, but merely noting that the modelled calibrated probability distributions fall on either side of the 775 SPE event. As seen from the figure each sample, though small, still has an uncertainty of about 10 years. Hence, the dates given here were the results of the entire age model, not of individual samples. We have corrected the text accordingly. The dated materials and their intrinsic ages are given in the Supplementary Information. Sample materials include charcoal, grains, nutshells, bones, antler and wiggle matched wood. Some of these materials may have a residual age, which we take into account by applying an outlier model as described in the Supplementary Information.

Line 159. When the authors say “the Rhine area”, did they intend to say the Middle East, given that it is beads attributed to the latter source that are shown in relevant panel (C) of figure 3?

REPLY: We have clarified this in the manuscript.

Lines 159-162. The text reads “Sub-phase F9f is a thick levelling layer that accumulated after the use of house K21 terminated and can be called the first Viking Age layer of Ribe. It is dated by AAR 32114 to 785-810 CE (posterior probability at 95.4% confidence, Table 12 in SI).” The date of this layer is attributed great significance in the paper, as the point in time when new long-range imports appear in the stratigraphy and as the basis of climate-change correlations. However, if it is a thick levelling layer, how can it be adequately dated by a single sample, and how can one be certain that this sample is not residual? Archaeological levelling layers very often represent redeposited and/or mixed material. In the supplementary material, the authors write “Layers or parts of layers with evidence for massive bioturbation were avoided as well as large homogenous layers that were interpreted as levelling layers, e.g. preparation of a plot after the demolition of a house, which could include substantial amounts of redeposited older material.”

REPLY: The layer in question is not composed of redeposited materials. Studies by soil micromorphology have demonstrated that it consists of a floor layer, which was exposed to outdoor conditions for a few years after the temporary abandonment of the building. We have revised the text and explained the points further in the Supplementary Information.

Figure 3. Panel A is really two panels (summer temperature anomaly and phase 9 calibrated dates) and should be labelled accordingly. The figure would be clearer if only the Aarhus curve results were included for the calibrated dates, with the IntCal20 comparison moved to supplementary material. It would also be helpful if the caption briefly explained the significance of showing the climate data, which has not yet been mentioned in the paper (but see further below on this issue). On a minor point, the units need to be provided for the scale of panel B.

REPLY:

The labelling of the panels has been made more clear. The original panel B has been removed completely. The IntCal20 data are removed in order to enhance the clarity. Further we have added a sentence about climate to the abstract. Also this paragraph is completely revised with emphasis on highlighting the possibility of linking with high resolution climate records when the archaeological records in itself is high resolution.

Line 186. This is the first mention of environment in the paper, other than in Figure 3. If it is to be a major consideration it would be better if it were mentioned in the introduction and abstract. As noted further below, however, the authors may alternatively wish to rework their climate-based conclusions.

REPLY: We have reworked the manuscript, especially the section about relations to climate/environment, according to this comment.

Line 187. Not exclusively maritime connectivity –also riverine, and overland trade routes are relevant for the Middle Eastern beads.

REPLY: Revised to 'long-distance connectivity'.

Lines 191-194. Previous publications regarding the chronology of Norwegian 'slate' (Baug et al. 2018) and reindeer antler (Ashby et al. 2015) at Ribe refer to dendrochronology. The authors thus need to clarify in what ways these absolute dates are provided "for the first time"?

REPLY:

The dendrodates referred to in these studies exist only for the period c. 705-741. We have now clarified above that "from the 740s CE onwards, no absolute dates were available" and that the dates quoted previously were "based on estimation".

Lines 199-201. Precedence in time makes causation possible, but does not prove causation. What is the mechanism that is inferred to have linked this trade to later raiding?

REPLY:

We meant only to suggest that the new chronology allows for causation, not that it is thereby proven. Until now the problem was a lack of tangible evidence for previous connections. We revised the wording thus "...in the decades prior to the escalation of raiding, providing the case for a connection previously sought for"

Line 202. There has not yet been any meaningful discussion of genomic evidence in the paper. What is its relevance when mentioned here without explanation?

REPLY: The link between the cultural development in Ribe and the Viking Age to the genomic evidence is not a main objective of this paper. However, we want to stress the point that a precise absolute chronology of a cultural development enables us to link this development to other absolutely dated records, such as climate, environment or genomic data. We hope that our motivation for including such data is clearer in the revised text.

Lines 202-210. Climate may be very relevant, but is considered here without any consideration of alternative and potentially mutually reinforcing variables. Thus it is not a

tested hypothesis. The authors would need to elaborate considerably or to remove this assertion, which is correlation equals causation as written.

REPLY:

Our objective was not to make a causal link to climate, but to highlight the need for high resolution archaeological records in order to compare or link with high resolution climate records. We have clarified this point in the revised text.

Line 210. Is there any evidence that early Viking-age raiding was in search of improved subsistence – as opposed to luxuries and power?

REPLY:

We can only speculate about the individual motivation behind Viking-age raiding, and this discussion extends beyond the scope of this paper. We have revised the text accordingly.

Lines 210-213. Sea ice was not a meaningful factor in Fennoscandia south of Svalbard, and the latter was not discovered until the 16th century. Sea ice was relevant to Iceland and Greenland, but the latter were not settled until the 870s and 980s respectively, well after c.800 CE (and earlier), the chronology discussed in this paper as the period of causation.

REPLY: We have revised the discussion about the influence of climate on the beginning of the Viking Age.

REVIEWER 2

The manuscript by Philippsen et al. reports on the first use of single-year radiocarbon calibration for an archaeological stratigraphical sequence to pinpoint the 774-75 CE solar proton event at an important Danish trading emporium and thus precisely secure the date of 790 ± 10 CE for the start of the Viking Age. The authors make use of the IntCal20 annual tree-ring data and supplement it where annual data is lacking in this portion of the calibration curve with their own high quality radiocarbon measurements of annual tree-rings from Denmark. The extensive radiocarbon and dendrochronological dates and detailed stratigraphy from the excavated site allow them to build a Bayesian model that, together with a massive number of artefacts, document the expanding trade from early evidence of trade with western Europe expanding to wider-ranging seafaring to trade with Scandinavia and finally to the arrival of Middle Eastern trade beads.

The manuscript is novel and will set the bar high for building robust chronologies for future archaeological studies. It will be also be of interest to researchers in disciplines such as history and social geography, as well as anyone interested in the Viking Age. The tree-ring ^{14}C measurements will no doubt be a welcome addition to the next update of the IntCal calibration curves and thus benefit all researchers who use radiocarbon dating in this time frame. The manuscript provides extensive supplemental information including artefact numbers, radiocarbon and dendrochronological dates and the OxCal models used to provide statistical sound dates and uncertainties. The document is clearly written (with the exception of a few minor points given below). The abstract is appropriate. The introduction provides sufficient background information and conclusions valid and reliable.

REPLY:

We thank the reviewer for the very useful comments. We have revised the paper in accordance with the recommendations, cf. answers below.

Lines 61-72 Figure 1 caption.

What is the white square in Fig 1a? In Fig 1b, the y-axis is labelled IntCal13- annual curve whereas the caption says IntCal20 – annual curve. Fig 1c. It might be better to separate the wiggle match into Fig 1d and provide some explanation the nature of the samples in the sequence. How is straw related (I see in Suppl that it is embedded in outer bark)? What was dated in the wormhole - undetermined organic material? What are the numbers in front of the tree-ring samples e.g. '15. Treering'? If this means ring 15 then perhaps it would be more clear if this was written as 'Treering 15'.

REPLY:

The label of Fig 1b is corrected to IntCal20 and this panel is moved to the supplementary information. Likewise we chose to move panel c,d,e to the supplementary information. The labeling and numbering of the sample have been clarified. See also comments by reviewer 1 on Figure 1.

Line 101-102: 'case of a wiggle-matched 15-year old wood sample (Fig. 1c). A calibration with IntCal13 (34) favours a felling date before 700 CE'. Here the '15-year old' wood sample could be confused with '15. Treering in Figure 1c' so it might be best to say 'outermost ring' since that is what is important for a felling date.

Line 104: 'result in a bimodal distribution'. All the probability distributions shown are bimodal so you need to explain that there are higher probabilities in the later range with IntCal20 and Aarhus curve

REPLY: We have revised Figure 1 and specified the text accordingly. Panel 1c has been removed from Fig 1 and has been moved to the Supplementary Information. We have corrected the names of the treering samples on the figure.

Line 116, Fig 2 caption 'with trade routes (orange lines) to different regions'. Is there a references for the trade routes?

Reference added:

Hall, Richard. 2007. *Exploring the world of the Vikings*. London: Thames and Hudson, p 29.

Line 154-155: The shift is marked by samples that calibrate to either side of the 775 SPE (SI, "Phase F9 subdivisions").' It's confusing to reference SI here since Fig 3a in the main document has the plot that shows this whereas SI "Phase F9 subdivisions" is general information about Phase F9.

REPLY: We have revised Figure 3 and the text according to this reviewer's comments and the comments of reviewer 1 (see above).

Line 170-174. Figure 3a 'Shown is also a 25 year running mean of the annual reconstructed mean temperature as well as the mean and standard deviation of the reconstructed annual temperature for the periods 620 – 700 CE, 700 – 800 CE, 800 – 900 CE, 900 – 1000 CE and 1000 – 1100 CE, yielding temperature anomalies of 0.1 ± 0.3 °C, 0.1 ± 0.4 °C, 0.1 ± 0.4 °C, 0.1 ± 0.4 °C and 0.3 ± 0.3 °C.' The 100 year mean anomalies are all well within the standard deviation so don't tell us anything. I would suggest leaving those out. The 25 year running mean shows the temperature decline at the onset of the Viking Age more clearly and Figure 3b gives a visual reconstruction for the century-long intervals.

REPLY:

We acknowledge that the division of the temperature record into time intervals does not add extra value and hence we deleted the time intervals from the figure as well as in the main text.

Lines 205-206: 'At the onset of the Viking Age, summer temperatures are at lowest (-0.4°C)'. Is this from the 25-year mean summer temperatures? If so state that and qualify when, e.g. 'at the lowest (-0.4°C) in the last x hundred years' or whatever timing is appropriate.

REPLY:

We have corrected the sentence.

Supplemental information:

p. 18, paragraph 2: 'Samples for dendrochronology were only preserved in phases F4 to F9'. I think it would more appropriate to say 'Samples suitable for dendrochronological dating'

REPLY: We have revised the text accordingly.

Supplemental information:

p. 18, paragraph 3: "For wiggle match-ing, several year rings of a wood or charcoal sample are radiocarbon dated. As the year rings between the dated samples are counted'. The term 'year rings' is confusing. Do you mean several single rings were analysed separately or several rings were combined for one analysis?

REPLY: We have specified this in the text.

Supplemental information:

p. 26, paragraph 1: 'Until phase F12, it was possible to treat the dates from pot 1 and plot 2 in the'

'Pot 1' should be Plot 1.

REPLY: We have corrected this.

REVIEWER 3

This reviewer has expert knowledge in Viking Age Archaeology, and is familiar with radiocarbon science only as a user of dating results received from labs and presented in publications. Thus, this review address the archaeological application of the methodological advances and the refined radiocarbon calibration curve presented in the paper as well as their significance for archaeological scholarship. This review does therefore not assess the validity of the methodological advances and the calibration curve, but is based on the assumption that they are valid.

The paper supplements the recent radiocarbon calibration curve based in annual tree-ring datings, IntCal20, with the authors' own biannual datings in periods where the IntCal20 curve is weak. This improved curve is applied to samples from a recent excavation of a well-documented site with complex stratigraphy and well-preserved organic materials, including wood. The site lies centrally in Ribe, western Jutland, which is a key site for exploring 8th–9th-century trade networks in the North Sea area and in north-western Scandinavia. By identifying the Solar Particle Event in 775 CE in samples from the site and combining radiocarbon dates with dendrochronology in a Bayesian model, the paper anchors the site's relative chronology in calendar years, thus allowing high-precision datings of significant shifts in trade networks in the mid–late-8th and early-9th centuries. These shifts have been pivotal in explanations of the period's economic upsurge in Francia and the taking up

of Viking raiding in the 790s. The paper dates these shifts much more precisely than has hitherto been possible, and the results appear to favour regional explanations to these phenomena over world-system explanations.

The paper presents novel and original results of major significance for scholarship on Ribe, on the early Viking Age, on the period's economy and trade in north-western Europe, and on the archaeological use of radiocarbon datings. Through the refinement of the calibration curve and by conceiving the highly innovative combination of calibration data, targeted sampling strategies, site stratigraphy, and dendrochronological data, the authors have developed a potent package of methods that will have great significance for dating sites with preserved burnt or unburnt wood.

This reviewer finds the paper to be of a very high quality and of outstanding interest to the scholarly communities worldwide, not only within Viking Age archaeology. The text is lucid and clear, and relevant references to previous research are included. With the caveats described below, conclusions are robust and reliable. Given that the authors deal

adequately with the issues raised below, this reviewer highly recommends the paper for publication.

REPLY:

We thank the reviewer for the very useful comments. We have revised the paper in accordance with the recommendations, cf. answers below.

1. While the paper's use of statistics and treatment of uncertainties in general seems cautious and balanced, the artefact counts in Figures 2d–h and 3c appear to be based in a biased sample that, potentially, does not provide sufficient basis for some of the paper's conclusions (lines 140–162). The excavation that has provided evidence for this study covers little more than one plot in the town. Therefore, the crafts, artisanal production, and other activities that left artefacts on this specific plot, for instance glass-bead production, crafts that demanded the use of whetstones, or storing and trade in imported beads, will potentially have had a huge impact on the number and types of finds. This means that the finds on this plot are not necessarily representative for the town or, in turn, for the shifting trade connections that the artefacts' distribution is used to identify and date (Figures 2h and 3c). For example, it seems a possible scenario that the consumption

of Norwegian whetstones in phases F8 and F9a–e was much greater on plots where shoemakers and combmakers left their residue. The authors should comment on this uncertainty and, if possible, make amends to limit its significance.

REPLY: This is an important point, and we have clarified this in the text: while there are local fluctuations (such as the abundance of whetstones in F9), the chronological patterns of artefact distribution observed in the SJM3 excavation closely mirrors that seen in previous excavations in Ribe. Thus we can argue strongly that the absolute dates added by this study are applicable to the wider patterns of circulation reflected by the emporium.

We have added a chapter to the Supplementary Information, containing a plan of the SJM3 excavation and the neighbouring excavation ASR9, where parts of seven settlement plots had been excavated in 1990–91. We also include new tables with artefact counts for the different phases of ASR9 in the supplementary material.

2. Since the duration of the phases varies, it seems somewhat misleading to provide the frequency of the selected artefact types as counts rather than artefacts pr. year or decade (Figures 2d–h and 3c), which would allow realistic comparisons of their frequencies in the various phases.

REPLY: It would have been ideal to have counts per year. However, there are still significant uncertainties in the durations of the phases. Within these uncertainties the phases are of approximately equal length. Therefore, we chose to keep the counts per phases.

3. In the text (line 146), the date of phase F7 is given as 733–745 CE. This is not quite the same span as indicated in Figure 2c (731/742–743/752). The authors should explain how they have arrived at the more precise dating of the phases, and also consider presenting the resulting dates for all phases, not only F7. Is 785–810 CE (lines 161, 189) the date range of phase F9?

REPLY: We agree that it is not a good idea to provide precise dates for one phase only. For better readability, we have chosen not to provide precise dates for any of the phases within the text, and refer to the figures and tables for this information. Furthermore, we made sure to clearly differentiate between approximate dates and the 95.4% ranges of phase transitions from the age model.

4. The 'clear causal link' between trade and raiding asserted in lines 199–200 does not seem obvious to this reviewer. The link should be fleshed out or the issue should be left out altogether.

REPLY: We agree that the wording is unprecise. What we meant to imply was that we provide the evidence for a connection between the emporia and the regions in Scandinavia where raiding is believed to have originated. We have revised to “providing the case for a connection”

5. At the very end of the discussion section, the authors include climatic data (Figure 3d, lines 168–174 and 204–213). They suggest that the slight drop in summer temperatures in the late 700s and early 800s contributed to “the expansion of maritime raiding and trade in search for improved subsistence”. While this argument has been used for more than a century to explain the Viking settlements overseas, which probably started in the mid-830s, they present no argument as to how raiding and trading might have contributed to subsistence in the earlier period, when raiding and trading were summer activities. Are they of the opinion that raiders and traders set out to obtain foodstuffs? This should be clarified, and the authors need to substantiate underlying claim that subsistence production in Scandinavia around 800 was so weak that the reduction in summer temperature was a real threat to subsistence production.

REPLY: We have reworked the discussion about climate and have clarified the text concerning the potential links between climate and raids/trading patterns.

6. The authors end the paper by suggesting that the low extent of sea ice around 800 provided “easier access to the northernmost areas of the Viking world” (lines 210–213). This claim is hard to assess, since they do not present information on the extent of sea ice at that time. However, current data (<https://www.npolar.no/en/themes/the-marginal-ice-zone/>) do not suggest that sea ice will have had any influence on the opportunities for accessing the northernmost waters known to have been navigated by Scandinavians at this time. Coastal sailing was the norm then, and during the sailing season (May–September) the southern edge of the marginal ice zone is found 300–600 kilometres north of the Scandinavian Peninsula. From July to September, one may sail unconstrained by sea ice eastwards to Novaja Semlja, which lies well beyond the easternmost Viking destinations evidenced in the late 800s. The authors need to substantiate why they consider sea ice significant for access to

the northernmost areas.

REPLY:

We agree with the reviewer that this sentence may sound too speculative. However the paper C. Kinnard et al., Reconstructed changes in Arctic sea ice over the past 1,450 years. *Nature* **479**(7374), 509–512 (2011), does present evidence of lower sea ice cover around 800 CE with an increasing trend towards 1000 CE. Still, it is difficult to infer the geographical extent of this increase. We have therefore deleted the sentence.

7. This reviewer finds the introduction of climatic data at the very end of the paper, as well as the two applications of the data (above, nos. 5–6), to be the weakest part of the paper. Neither the abstract nor the introduction indicates that the paper aims at explaining why the Viking trading and raiding started, which is what the discussion of the climatic data attempts to do. This reviewer would suggest that the climatic data is left out and that instead, the authors return to the overall question that they pose in the introduction (lines 34–43), and which their results appear highly relevant for, namely the question whether the start of Viking trading and raiding is best explained in regional or world-system contexts.

REPLY: The link between the cultural development in Ribe and the Viking Age to climate or environment is not the main objective of this paper. However, we want to stress the point that a precise absolute chronology of a cultural development enables us to link this development to other absolutely dated records, such as climate, environment or genomic data. Therefore, we chose not to delete the mention of climate data entirely, but to rework it, include different climate data and references, and to rewrite the text. We hope that our motivation for including climate data now is clearer for the reader.

Reviewer Reports on the First Revision:

Referee #1 (Remarks to the Author):

The authors have addressed the general and specific issues raised in my initial review. The manuscript highly merits publication. The authors may nevertheless agree that the revised text would first benefit from two very minor additional edits:

Lines 20-21: The abstract states that the paper charts “the expansion of long-distance trade from Arctic Norway to the Middle East”. This phrasing implies a misleading claim. The trade discussed in the paper is from Norway (central Norway in fact - the main source of the relevant slate hones) to Ribe, and (via intermediaries) from the “Middle East” to Ribe. i.e. it is not from Arctic Norway to the Middle East.

Lines 95-99: The explanation provided regarding the 30 14C year offset for the period 792-902 CE is unclear. The relevant sentences would benefit from rephrasing.

Referee #2 (Remarks to the Author):

I am satisfied that the revisions have answered all my questions/concerns about the manuscript.

Referee #3 (Remarks to the Author):

I find that the authors have adequately addressed the issues I raised in my review. The paper is now a significant and well-researched contribution.

However, I regret that they maintain the rather speculative link, although modified in the new version, between the start of Viking raiding and climatic change. I find information on droughts, storms, and snowfalls from Irish annals to have dubious relevance for Scandinavia, not to mention for the start of overseas raiding. Why should an increase in storm frequency lead to the start of open-sea voyages? The single point they make in that discussion, that the more precise chronology “now enable us to test such links”, is probably the weakest point in the paper, not deserving of its prominent place at the very end.

As noted in my review, the questions raised in the introduction concerning the relation between regional and world-system explanations of the beginning of the Viking Age seem of much greater significance, not only for Viking studies but for studies of cultural and societal change globally. Contributing to that discussion would make the paper relevant for a larger and more diverse readership than merely adding a paper to the growing mass that make weakly founded assertions concerning connections between climatic and sociocultural changes.

I therefore maintain my advice that the climatic data are taken out and the final paragraph is dedicated to developing some general points concerning the relation between regional and world-system explanations.

Author Rebuttals to First Revision:

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We have clarified this sentence: “long-distance trade as far as Arctic Norway and the Middle East”. We chose not to delete “Arctic”, though, as we are not only referring to slate hones from central Norway, but also to reindeer antlers used in comb-making.

Lines 95-99: The explanation provided regarding the 30 14C year offset for the period 792-902 CE is unclear. The relevant sentences would benefit from rephrasing.

We have clarified this section.

Referee #3 (Remarks to the Author):

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I therefore maintain my advice that the climatic data are taken out and the final paragraph is dedicated to developing some general points concerning the relation between regional and world-system explanations.

We have revised the final paragraph of the paper and hope that it is now a better reflection of our intention: that the new absolute chronologies enable us to test chronological, but of course not causal, links between different cultural, social, environmental or climatic developments, without implying that increases in storms triggered the beginning of maritime trade or similar, in addition to being able to compare regional with world-wide developments and archaeological with historical records.