Reply to Pierre Vermeersch, by Katie Manning and Adrian Timpson

1. Introduction

In volume 101 of Quaternary Science Reviews we published a summed probability distribution (SPD) analysis of radiocarbon data from 1011 Neolithic archaeological sites to examine the demographic response to Holocene climate change in the Sahara. Vermeersch (2014) is highly critical of our approach and makes 10 specific comments, which fall broadly into four categories: simple misunderstandings; inconsistencies in the data; our exclusion of Pharaonic samples; and criticisms of the method. We would like to thank Vermeersch for his comments and we appreciate the opportunity to provide clarity on these issues and respond in detail to all points below, with respect to these categories.

1.1. Simple misunderstandings (comments 1 and 6)

A couple of Vermeesch's comments raise questions that we feel were fully answered in the publication. We regret if our language caused any ambiguity, and hope to resolve these here.

Comment 1 questions the difference between Figure 3, which uses 3086 samples, compared to Appendix A, which has 3287 samples. Different scales of analysis, represented in the different figures, used different subsets of the data. Figure 2 illustrates the full SPD for the entire radiocarbon data set, inclusive of all 3287 samples, whilst Figure 3 illustrates the four sub-regional SPDs. Two hundred and one samples are from sites that were not included in the sub-regional analyses since they were not assigned to one of the four polygons (grey dots shown and explained in Figure 1). For example, the grey dots in West Africa, which are located predominantly to the south of the river Niger, display important social and economic differences to sites in both the Western littoral and Central Saharan polygons e.g. arable farming, technological specialisation and Sahelian pottery motifs (Manning 2008). It was therefore considered preferable to exclude them from either polygon.

Comment 6 includes a statement that we have not specified the time period of interest. We describe in Appendix A that the data '*…covers a calendar date range of 14,000-2000 yrs BP'*. This is the same time range shown on the time-axis in Figures 2, 3 and 4.

1.2. Inconsistencies with the data (comments 2, 3 and 4)

Efforts to clean large datasets (in this case thousands of radiocarbon dates) will never entirely eradicate errors, both human, and those inherited from other sources. The task therefore is always to minimise errors to acceptable levels. Even if we accept all Vermeersch's examples (n=22) this represents only 0.67% of the dataset, a rate so low that it can be considered a commendation of our data hygiene. An error rate this low will have a negligible affect on the analysis, and is dwarfed by the uncertainty associated with archaeological (or any empirical) sampling. Furthermore we do not agree with all his examples. Comment 2 notes the inclusion of 11 dates with an unknown lab code. These data were deliberately included in the analysis, having been obtained from a published source. The lack of a lab code is an obvious limitation of their provenance, which therefore increases the uncertainty associated with them, at worst adding noise to the signal. Their inclusion therefore promotes a more conservative approach.

Comment 3 and 4 criticise the inclusion of five Thermoluminescence (TL) samples, and six duplicate samples. Vermeersch is indeed correct that both TL samples and duplicates should not have been included. Nevertheless the detrimental effects of this are to some extent mitigated by our methods, in two different ways. Firstly the TL samples have extremely large standard deviations, so their probability distributions are low and diffuse across a wide period. As with comment 2 this has a conservative effect of adding some noise to the signal. Secondly, the binning algorithm (further explained in Timpson *et al.* 2014) somewhat mitigates duplicate samples, since they will by definition be assigned to the same bin, after which their combined probability distribution is normalised.

1.3. Exclusion of Pharaonic samples (comments 6,7 and 8)

This appears to be Vermeersch's primary criticism, regarding the inclusion of radiocarbon dates in the eastern Sahara polygon, and the specific culture history of that region. The key objective of our publication was to explore spatio-temporal fluctuations in "Neolithic" populations, and not the adaptive strategies of state-level societies. This is an important distinction, since the former are characterised by a less intensive economic and political system and one that is essentially restricted by an environment's carrying capacity as a consequence of relatively low technological investment. In contrast, one of the key characteristics of state-level societies is their adoption of what Stuart and Gauthier (1981) termed "Power strategies" i.e. intensive resource exploitation including arable farming, complex technology, extensive trade, social stratification and extended political and economic alliances. These socio-political developments remove much of the environmental risk faced by Neolithic populations, and hence provoke a very different set of questions relating to resilience and climatic change (Turchin 2003, Weiss and Bradley 2001). We are explicit about this in the publication, stating that our data comes from 1011 "*Neolithic sites*" and specifically for the eastern Saharan polygon, that "*This (abrupt population collapse) may be partly explained by the emergence of Pharaonic civilization along the Nile River leading to a spatial bias in our results as only Neolithic dates are included in this analysis*".

Comment 6 notes a lack of predynastic and Pharaonic samples post c. 4000 BP. Vermeersch is correct that more Pharaonic dates are available in the Nile Valley. Similarly there are many more dates available from sites associated with the Garamantian civilization, the Kingdom of Kush and other state level societies, which flourished in northern Africa during the late Holocene, and within our time range of 14000-2000 yrs BP. However, as we have explained, the objective of this paper was to explore the population history of "Neolithic" populations, and not the adaptive strategies of state-level societies. Comments 7 and 8 again lament the exclusion of Pharaonic samples, which are present in Vermeersch's own Egyptian dataset, and many of which are published in Kuper and Kröpelin (2006). The additional Pharaonic samples are irrelevant for the objectives of our analysis, as they would conflate the available Neolithic data with samples derived from Pharaonic contexts, and tell us little about the response of Neolithic populations to the AHP. Similarly the data provided in Kuper and Kröpelin (2006) is in fact included in our database, but we have simply removed the Pharaonic samples. Whilst many of the dates from the additional suggested references were included, it is possible that some were unintentionally missed in our data collection. The size of our dataset is simply the consequence of practical limits in our research budget. To what extent any empirical sample is fairly representative of the true population is a fundamental problem to be explored through appropriate statistics and inference. Hence, the MCSPD method presented in this paper, specifically tests the degree to which a population curve deviates from what would be expected if the distribution was merely a result of random sampling. In this sense, Vermeersch's description of our data set as "*...very incomplete...*" and his rhetoric " *How can the database [...] be trusted as being representative for the region?*" are unhelpful, since by definition all samples are incomplete approximations of the true population.

1.4 Criticisms of the method (comments 5, 9 and 10)

In comment 5, Vermeersch suggests the Nile valley should have been excluded from the analysis, and asserts "*…the Nile valley population was not subjected to the same environmental conditions as the Saharan population*", and the "*River Nile cannot be considered as a Saharan river*". Clearly this is a simple matter of scale. No two points are ever subjected to exactly the same environmental conditions, although we can expect closer points to have more similar conditions. As our analysis was at the sub-continental scale, we defined our region of interest, and the River Nile finds itself flowing through it. Ultimately our results suggest that at this scale, both the eastern Saharan populations (inclusive of the Nile Valley), the Central Sahara and the Atlas/Hoggar were subjected to similar climatic drivers. The inclusion of the Nile Valley data is also well justified on archaeological grounds. There are clear affinities in the material culture and economic practices of the earliest Holocene populations both in this region and subsequent settlements to the west, providing compelling evidence for the Nile Valley being the source of Neolithic expansion into the Sahara (Sutton 1974; Gabriel 1978; Haaland 1992). To exclude these samples would essentially remove a key component of Saharan population history, giving the false impression that Neolithic populations did not inhabit the Nile Valley.

Furthermore, Vermeersch plays fast and loose with the problem of variation in environmental conditions, by implying that the rest of the Sahara was ecologically homogeneous. This was clearly not the case, and there has been extensive research documenting the ecological and palaeohydrological variation of the Saharan Holocene (Francus et al. 2013; Drake et al. 2011; Hély et al. 2014), as well as the adaptive strategies adopted by local populations in response to that variation (Cancellieri and di Lernia 2013).

At the root of this lies a common issue- the incorrect view that research at different scales are in some way incongruent. Our publication is not a local scale study of regionally specific demographic histories, as Vermeersch appears to have interpreted it. Instead we have provided a broad-scale view of demographic changes at the sub-continental level. By assessing the similarity in regional population fluctuations across the entire width of Africa we were able to show strong support for a climatic cause, without needing to rely on palaeoclimate proxies, and the inherent difficulties they bring.

Comment 9 correctly notes that a more recent calibration curve has now been published, and it is of course always better to use the most up-to-date data for any analysis. Nevertheless, subsequent re-analysis using the 2013 intcal. curve, has made no difference to the inferences and conclusions and has a negligible effect on the SPD plots.

Comment 10 raises the point that the shape of the SPD will be influenced by differences in research intensity in different areas. This is certainly true, not just for this analysis but for any archaeological analysis, since we are always limited to data from where someone decided to excavate. This is one of several biases that Rick discussed as early as 1987 when he first proposed constructing summed date distributions, and has since been dealt with in detail by many authors including Timpson *et al*. 2014. Whilst the issue of sampling bias certainly adds some complexity, it falls far short of undermining the utility of the methods used.

2. Conclusions

To conclude, a few of Vermeersch's comments are readily answered in the text itself, nevertheless we hope our reply offers some additional transparency, particularly in regards to the objectives of our analysis. Other comments are correct in revealing an extremely low-level error rate in the data, and we are most grateful for being given the opportunity to amend these. However, it appears that Vermeersch fully grasped neither the objectives of the paper, nor the methods we have used. The use of radiocarbon dates both as a valid population proxy and a potential palaeoclimate proxy, rely on utilising an appropriate scale of observation in both time and space. This research examined the broad-scale fluctuations in Neolithic Saharan populations in relation to changes in climate associated with the onset and termination of the AHP. To achieve this we have deliberately disregarded much of the specific culture histories of each region, as well as the evident variation in Saharan paleoecology. Whilst this was necessary to reveal the broad-scale fluctuations in Neolithic population levels, it is not our intention to negate specific regional culture histories. On the contrary, a key objective of this research was to establish a background trend in the demographic response to Holocene climate change in the Sahara in order to provide a framework for more localised studies of demographic change and related ecosystem dynamics.

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