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In summary, the hypothesis that the non-local individuals identified by isotopes are indicative of secondary burials cannot be sustained. Rather, several children were part of the ritual, and were of loess origin, i.e. raised at Herxheim or other classic LBK sites. They and their parents would certainly have lived for longer periods on loess and probably even in Herxheim. It is quite possible that members of former upland 'Home Communities' came to Herxheim during their lifetime. They probably died there or were an active part of the ritual along with their children who were born there. Furthermore, it has been proven several times that potential members of 'Home Communities' could also be part of regular burials in settlements and cemeteries (cf. 1.). Lastly, the diet of all individuals is a classical terrestrial-Neolithic one within the last years of life (cf. 3.) and does not represent a community with a nutrition different from LBK lifeways.

The archaeometric results of the nonlocal adult individuals should not be used to interpret primary or secondary mortuary actions, as these results do not indicate their residence before their death, but the region of their birth and early childhood.

The puzzle of LBK endings: the curious (and special) case of Herxheim

By Alasdair Whittle

Those people who lived in longhouses – the *Linearbandkeramik* (LBK) of the second half of the sixth millennium cal BC – have left us an archaeology full of challenges. A lot of it is very well known, and persistently recurrent: longhouses themselves, settlements, cemeteries, preference for good soils, pot styles widespread at first and then increasingly regionalised, cereal cultivation and domesticated animals, especially cattle. Such familiarity may lull us into thinking that we understand most of the essentials of LBK history and lifeways. But get down to the detail, and plenty of puzzles remain. The nature of households and other social groupings including descent groups, clans and sodalities, the internal structure of individual settlements and of regional groupings or cells, gender relations, and the chronological detail of the timing and pace of the initial spread and subsequent development of the LBK, are all far from well understood. The study of all these aspects of the first farmers across wide swathes of central and western Europe has been further enhanced in recent years by isotopic and now aDNA studies, revealing complex and diverse patterns of lifetime mobility among an incoming population of ultimately Aegean and Near Eastern descent. Now things do not seem so easy. And then there is the business of the demise of the LBK, perhaps straddling the 51st and 50th centuries cal BC. There they were, on the best soils in regional landscape after regional landscape, with a robust and productive subsistence economy; there was plenty of space beyond their chosen valley-edge locales in which to expand if necessary, and there were abundant valley pastures and forests in which to move their herds about. What could possibly have gone wrong? Go wrong things certainly did, because against a background of increasingly regionalised patterns of material culture, perhaps projecting more emphasis on local identities in previously broad patterns of connectivity, we now have evidence from some regions of the whole LBK distribution, especially in the Rhineland and surrounds, first, for killings not only of what might have been male raiding parties – as at Halberstadt – but also of smaller and larger social groups or communities – represented respectively by Talheim and Schöneck-Kilianstädten, and by Asparn/Schletz – and secondly, for disruption if not hiatus in some regional sequences, not least up and down the Rhine valley as a whole (DENAIRE et al. 2017; cf. MARCINIAK et al. 2022). There is evidence at Schöneck-Kilianstädten for the mutilation of some of the victims (many individual references are cited in the useful overview by FIBIGER et al. 2023).

One realistic possibility is climatic downturn, especially if pronounced enough to produce a run of bad years sufficient to outrun the buffering inherent in the agricultural system of garden cultivation and extensive animal herding. However, we still do not understand the timing, intensity and effects of climate events in the later sixth millennium cal BC. If they, perhaps in combination with too many people packed into chosen, optimal parts of the landscape following rapid and sustained population growth from the 53rd century cal BC, were the cause of the troubles seen in the late sixth millennium cal BC, why do we not see more signs of local adaptation? Could there be some further factor at work exacerbating the conditions of these troubled times? Disease is one candidate to think about (DENAIRE et al. 2017; WHITTLE 2018), though I am not aware of any specific indications for it beyond the presence of tuberculosis in LBK populations, and I believe that signs of plague are not yet dated earlier than the fourth millennium. Some kind of collective mental crisis (ZEEB-LANZ 2009) should also be kept in mind. A further important perspective is regional variability. As far as I know, Asparn is the most easterly example of an LBK communal killing (with Halberstadt beyond the Harz mountains the most northerly example), and there seems more positive support further east for continuity – and thus less disruption – between LBK and the succeeding Lengyel culture (REGENYE et al. 2020).

Into this context, enter Herxheim. This extraordinary site serves to intensify our sense of a late LBK crisis, although, as I shall stress, there is much that we simply do not yet understand about it. Above all, it is the scale of the human remains deposited and the remarkable treatment meted out to them which seem to project a situation in which normal practices were profoundly altered, with emphasis on the transformation of the dead (whoever they were), the obliteration of previous identities, and accompanying violence and anger. Among the many remaining uncertainties, I would highlight on the one hand the origins and numbers of the people involved (both the living participants and those who ended up being deposited) and the duration of this activity, and on the other the detailed peri-mortem questions about natural deaths or deliberate killings, the processes of dismemberment and dissolution, the possibility of cannibalism, and the significance of the transformed skulls ('calottes'), the main remaining recognisable skeletal element surviving the onslaught on the bones of the deceased. I do not have any easy or simple solution to the problems set by Herxheim, but my instinct is that better answers will lie in continuing research on the details of the site and its deposits on the one hand, and on the wider late LBK local, regional and trans-regional context on the other.

The contribution of John Chapman, Bisserka Gaydarska and Tina Jakob to this puzzle is welcome. J. Chapman and B. Gaydarska have a distinguished record of detailed research on numerous problems in the study of the Neolithic and Copper Age of south-east Europe (as far north as Hungary), and an enviable fluency in the language of modern archaeological theory. Coming fresh to the LBK, there is the promise that they could see things overlooked by established regional and period specialists. A parallel study of the Lengyel site of Alsónyék in south-west Hungary, in a wider look at 'megasites' in European prehistory, proposed that this place was above all a regional centre for burial and attendant mortuary ritual (GAYDARSKA / CHAPMAN 2022, chapter 3). That was certainly valuable in drawing attention again to the very large numbers of burials involved (BÁNYFY et al. 2016), but downplaying the settlement dimensions of the place overlooked the presence of some 120 substantial houses, numerous large, complex pit deposits, abundant animal bones and an estimated million or so sherds of pottery (Eszter Bánffy, pers. comm.). For Herxheim, the starting point and finishing point for Chapman et al. are theoretically-inspired themes of fragmentation, enchainment, dividuality and bodily mobility. They focus on the results so far of strontium and oxygen isotope analysis which has suggested that some three quarters of those deposited at Herxheim were of non-local origin, probably from uplands some distance to the north. They follow the preliminary estimate that the total number of people deposited after the treatments noted above could

have been around or over a thousand. Following their theoretical understanding of fragmentation, enchainment and dividuality, they go on to envisage – some might say invent – an orderly and rather sanitised set of relations between upland LBK communities (for which there is virtually no empirical evidence) and the residents or regular users of Herxheim in the lowlands, the uplanders (slightly confusingly labelled as ‘Home Communities’) supplying dead people and/or parts of dead people to the Herxheim residents or ‘Guardians’ for use in annual festivals, over a shorter or longer period of time, probably in the 51st century cal BC.

I find their model hard to accept, though their article usefully draws attention again to the exceptional character of Herxheim (just as their recent chapter in their megasites book did for Alsónyék). However, I do think they are unduly swayed by their attachment to particular kinds of theory and by a fondness for generalisation and broad analogy, and in my view, they do not give enough attention to the local, regional and trans-regional contexts of the late LBK.

On the plus side, I like their emphasis on the continuity and tradition behind the creation of the important place of Herxheim. Perhaps they could have made even more of the diverse regional styles of pottery represented; only two re-fitting Šárka sherds from widely spaced contexts at the site get a detailed mention. I like their attention to the re-fitting studies, and the conclusion that we simply do not know the fate or whereabouts of substantial portions of the total human bone assemblage. Their speculation about relative numbers and timescales for the arrival of bodies or body parts from the uplands (*Tab. 1*) is informative and useful, not least because it serves to offer smaller arrivals at any one time. This breaks down the impression of mass at Herxheim, bringing it (though the authors do not pick this up) more into line with other killings in the late LBK. I also admire their questioning of the criteria for the freshness of human bone, even though it may slide into special pleading, to enable their hypothesis of extended upland–lowland interchange.

On a critical note, I lament on the one hand the authors’ attachment to generalising theory and on the other their lack of attention to other relevant studies of the LBK. On the theoretical side first, it seems clear that what drives this study above all is belief in the big ideas of fragmentation, enchainment and dividuality, allied to a general notion of bodily mobility. The article opens and closes with these themes. There is an admirably wide frame of comparative reference across the European Neolithic and Copper Age as a whole, examples cited ranging as far afield as southern Portugal and southern Italy. The specific case study of Herxheim only comes in after several pages of theorising and generalising. And we could reflect that the theory offered begs plenty of questions. I think that the level of generalisation is set too high. I am doubtful whether there are universal principles of fragmentation, as opposed to particular practices in specific historical contexts. I do not see the breaking up of a decorated menhir in Brittany as guided by the same motives as those behind the obliteration inflicted on the Herxheim human remains. Enchainment too risks being little more than truism, unless translated into the specifics of each and every context, and into the specifics of how complex relationships were created – and differed through historical sequences. I remember the anthropologist Tim Ingold remarking drily at a seminar some years ago that he had never met a dividual in his years of anthropological fieldwork.

Further, despite the admirable detail on many aspects of the enclosure, there is not enough attention given to wider considerations generated by the archaeology of the LBK and especially the late LBK. For all the plausibility of the meta-narrative about upland communities and their relations with Herxheim, there is virtually no exploration of the evidence – largely its absence – for upland use. A lot more could have been done with the results of isotopic studies, for lifetime movements within the LBK, and for animals and people alike, and perhaps with stone tool sources in remote locations. It is also surprising, to say the least, that a discussion of Herxheim should exclude refer-

ence to other late LBK killings, and indeed also to late fifth millennium killings in Lower Alsace (LEFRANC et al. 2021); and the stresses, strains and fissions of LBK communal life could have been explored through the example of Vaihingen, not far from the Rhine valley (BOGAARD et al. 2011; BOGAARD et al. 2016). That is not to say that the dead represented at Herxheim were definitely killed on the spot, and the discussion of bone freshness noted above should be remembered, but given what we know of other LBK massacres, that is certainly plausible. I am inclined to follow Andrea ZEEB-LANZ (2019b) here in her proposal of mass sacrifice in the context of troubled times following raids and attacks. The authors here stress their inferred movement of bodies or body parts into Herxheim, but despite their emphasis on fragmentation and bodily mobility, they do not state explicitly what their vision is of the fate of the remains unaccounted for.

Given all the uncertainties remaining, it is foolish to be dogmatic about what really went on at Herxheim. I hope that continuing research can realistically address the problems still remaining. I would want to worry about the numbers of the dead involved. Can we be sure that the current extrapolated figure is robust? Could there be 'sidedness' in the patterns of deposition around the enclosure? It would be good to see further isotopic and aDNA analyses of the human remains, to try to pin down the origins of the mortuary population even further. I am not familiar with the statistical clustering method for analysis of the radiocarbon dates, as discussed by the authors. I would advocate a renewed attempt, suitably funded, at multiple radiocarbon dates to be interrogated in a Bayesian chronological framework, with robust priors to work around the calibration plateau. It would be desirable to see continuing detailed research up and down the Rhine valley, so that the state of settlement in the latest LBK and in its aftermath could be compared in detail from region to region (cf. DENAIRE et al. 2017). In these ways, a better understanding of the context and history of Herxheim may emerge in due course. I do not think that the authors here have come up with the best answers, but their stimulating paper may become part of the motivation for continuing and renewed research on this remarkable site.

Modelling the unique: fragmentation, bodily mobility and the case of Herxheim

By Daniela Hofmann

This is a thought-provoking piece, in which the authors use a coherent theoretical approach to provide an alternative model for one of the most controversial, but also one of the most well-studied sites of the European Neolithic. Given the huge amount of information available and the many conflicting viewpoints, this is no easy task, but it has borne fruit: John Chapman, Bisserka Gaydarska and Tina Jakob treat us to new ideas of why the unique body-transforming practices at Herxheim may have begun or ended when they did, provide new arguments for moving the study of the *Linearbandkeramik* (LBK) away from a limiting sedentist paradigm, and offer a concerted interpretative effort concerning the modalities of deposition at the site. Basically, they have opened a whole lot of cans of worms at once, and it is hard to know how to do this justice in the framework of such a short comment.

Am I convinced about the reading? Well (perhaps fittingly for Herxheim), only in parts. The argument that is being framed here boils down to two axes of criticism of the model published by Andrea ZEEB-LANZ (2019a): the question of who the people were who ended up at Herxheim and how they got there; and the question of how the depositional process worked. These two issues are not necessarily linked, and I am in far greater agreement with the authors on one than on the other.

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References of figures

Fig. 1: J. Chapman/B. Gaydarska/T. Jakob. – *Fig. 2:* re-drawn by L. Woodard from authors' multiple sources. – *Fig. 3:* B. Gaydarska. – *Fig. 4:* ZEEB-LANZ/HAACK 2016, fig. 2. – *Fig. 5:* HAACK 2016a, pl. 66. – *Fig. 6:* DENAIRE 2019, fig. 10. – *Fig. 7a:* re-drawn by L. Woodard from ZEEB-LANZ 2019b, fig. 6. – *Fig. 7b:* re-drawn by L. Woodard from TURCK 2019, fig. 56, modified by L. Hies (RGK). – *Fig. 8:* re-drawn by L. Woodard from original by J. Chapman, B. Gaydarska and T. Jakob. – *Fig. 9:* A. Häußler, GDKE Außenstelle Speyer. – *Fig. 10:* HAACK 2016b, pl. 66,1; 67,2. – *Fig. 11:* HAACK 2016b, pl. 71,3. – *Fig. 12:* A. Zeeb-Lanz, GDKE Außenstelle Speyer. – *Fig. 13:* TURCK 2019, fig. 59. – *Fig. 14:* TURCK 2019, fig. 51. – *Tab. 1:* J. Chapman/B. Gaydarska/T. Jakob, layout: L. Hies (RGK).