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IN MEMORIAM

Diskussionen

New perspectives on deliberate fragmentation and bodily mobility

By John Chapman, Bisserka Gaydarska and Tina Jakob

Introduction

If anything characterises archaeological evidence, it is its fragmentary nature. Be it fragments of a pot, a house or a cemetery (a single grave), they ultimately lead to the reconstruction of a part of our past. It is no surprise, then, that, for a very long time, fragments constituted “rubbish” in archaeology, probably because of the unhelpful commonplace that archaeology is concerned with the rubbish of past generations. This perspective drastically curtailed the potential of archaeologists to construct interesting narratives based on fragments, which were fit only for disposal. Nonetheless, the efforts to deal with fragments continued.

The new research perspective of deliberate fragmentation emerged in the late 1990s (for a brief history, CHAPMAN 2022). From the outset, a key part of fragmentation methodology was re-fitting. The re-fitting studies collected in John Chapman’s “Fragmentation in archaeology” (CHAPMAN 2000) supported the notions of deliberate fragmentation and fragment curation, as well as the practical use of fragments after the break, including children’s play with fragments. This stage of the research can be summarised in what was termed the ‘Fragmentation Premise’ – namely that “objects were regularly deliberately fragmented and the resulting fragments were often re-used in an extended use-life ‘after the break’” (CHAPMAN/GAYDARSKA 2007, 2; 8–10; 18). It is revealing that, despite significant attention to the fragmentation of human remains in the burial process in the 2000 book (CHAPMAN 2000, 134–179), human bones were not included in the Premise in the subsequent fragmentation volume in 2007. Nonetheless, conceptualisation of the fragmentation of objects and human bones treated the two very different materials as homologous (*Fig. 1a*).

This research direction was addressed in two studies of mortuary remains in the Balkan Mesolithic, Neolithic and Chalcolithic (CHAPMAN 2010; CHAPMAN et al. 2013), in which J. Chapman and his colleagues emphasised the repeated occurrence of the fragmentation of the deceased’s body into parts that were then stored, curated, moved, further transformed and ultimately buried. Rosalind WALLDUCK (2013, 15–17) noted that burial of partial bodies was a much more complex mortuary process than the one-stage burial of single, complete bodies. In parallel to what was the ‘normal’ burial of individual, articulated bodies as single burials in a separate burial pit, Balkan and Carpathian communities practised five forms of ‘deviant’ burials, involving fragmentation and partial removal of bone fragments; addition of bones from another skeleton to a burial; removal of complete bones from a burial; the creation of a hybrid body through bone re-combination from two different bodies; and substitution of human bones by replacement with artefacts.

However, the use of the identity triangle as a framework for thinking about basic human relations to objects and places (*Fig. 1b*) reminds us of a fundamental incompleteness in fragmentation theory, which has recently been addressed (CHAPMAN 2022; CHAPMAN/GAYDARSKA 2022; CHAPMAN et al. in press) through the integration of landscape fragmentation into the research framework. We propose to modify the Fragmentation Premise as follows:

Places, human bodies and objects were regularly deliberately fragmented and the resulting fragments were often re-used in an extended use-life ‘after the break’.

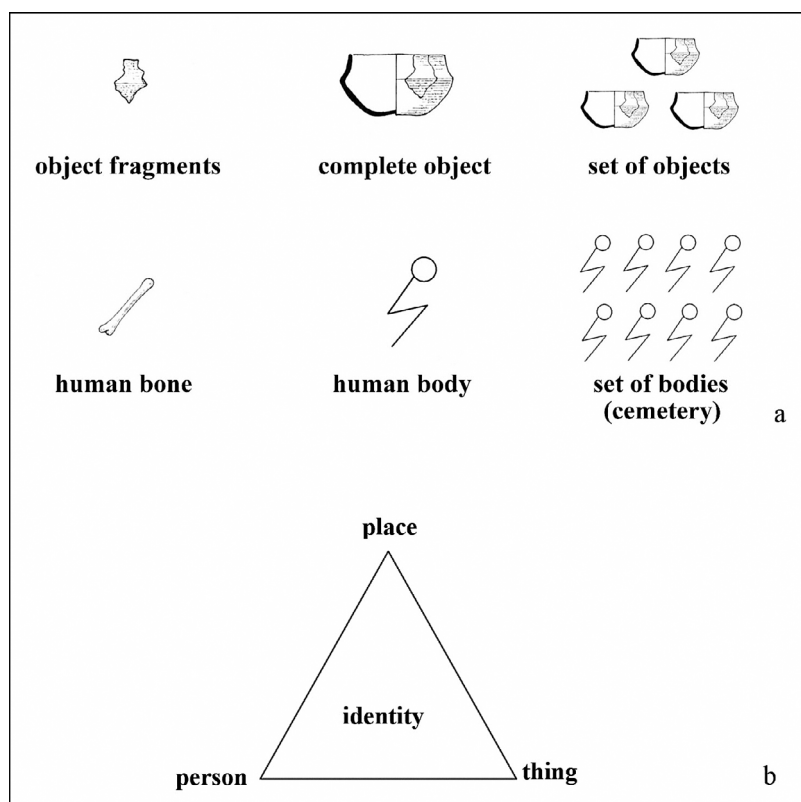


Fig. 1. a Relationship between parts, wholes and sets for objects and human bones. –
b Identity triangle.

The fragmentation of place is therefore the origin-metaphor for the general process of relating in the world – viz., enchainment. It is the link between a place and an open-ended series of other places that marks out enchainment as central to the creation and maintenance of social life. An approach focused on the itineraries of bodies and objects provides a holistic means of re-integrating places, persons and objects. In other words, the incorporation of the fragmentation of place is essential for an integrated theory of fragmentation, with its variety of operational chains – reductive, additive, and transformative (*Fig. 2*). But of equal significance are the human bodies which were not solely buried in an articulated complete manner but often fragmented to form different, partial but more dividually dynamic bodies¹.

In this article, we wish to consider the processes whereby human bodies are fragmented and moved around sites and even around the landscape. We consider examples of well-documented bodily mobility in European prehistory (*Fig. 3*) as an introduction to a proposed re-interpretation of the remarkable *Linearbandkeramik* (LBK) site of Herxheim, Rhineland-Palatinate (DE), where excavations have uncovered the deposition of thousands of incomplete human and animal bones, sherds, and stone tools (ZEEB-LANZ 2016; ZEEB-LANZ 2019a). It is our primary aim to explain the principal Herxheim puzzle that has so far defeated attempts to understand the source of the human remains.

¹ The term 'dividual' refers to an entity whose identity is composed of all the relations which that en-

tity shares with other persons, places and bodies (CHAPMAN 2000, 14–16).

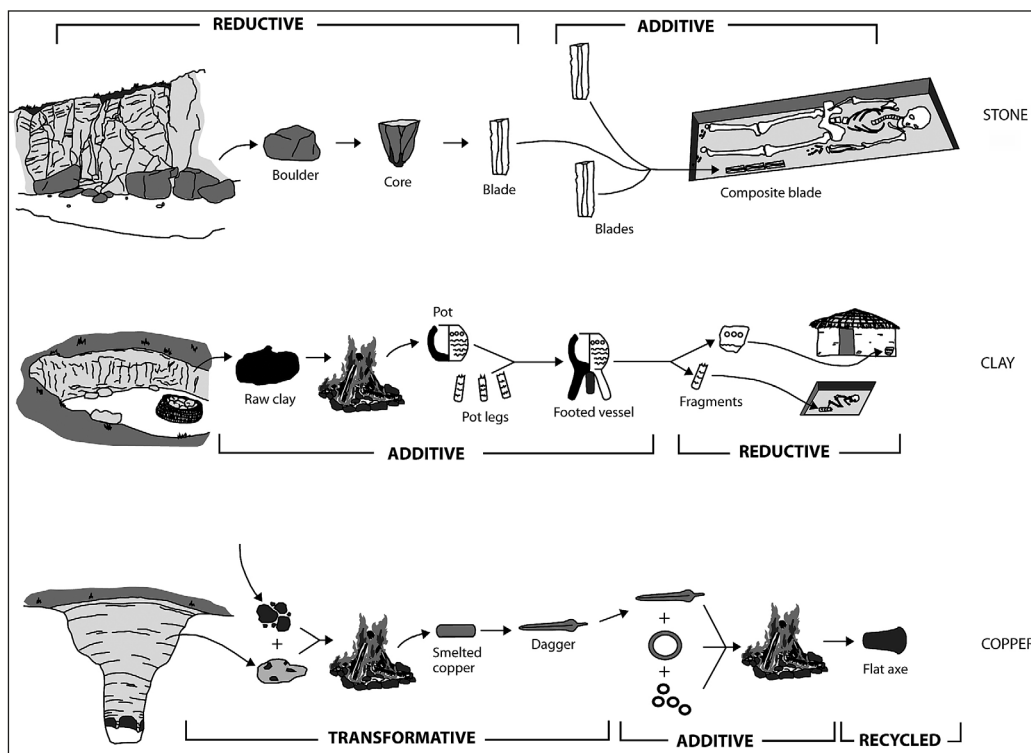


Fig. 2. Operational chain for object production: reductive, additive, transformative, recycled.

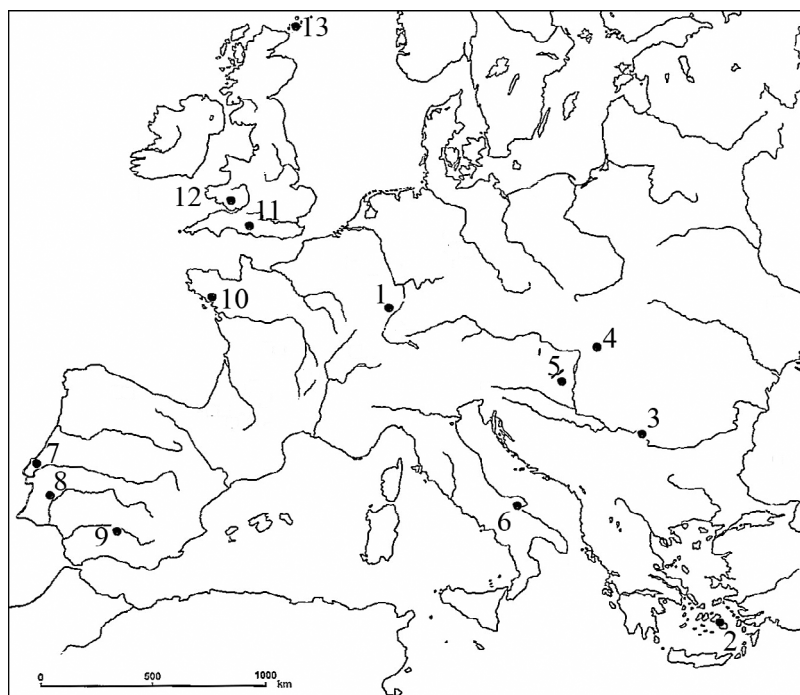


Fig. 3. Sites mentioned in the text: 1 Herxheim; 2 Kavos, Keros; 3 Lepenski Vir (Iron Gates Mesolithic); 4 Polgár-Csőszhalom; 5 Alsónyék; 6 Grotta Scaloria; 7 Bom Santo cave; 8 Perdigões; 9 Marroquíes; 10 Breton megaliths; 11 Hambledon Hill; 12 Southeast Welsh long cairns; 13 Orcadian megaliths.

Inter-site re-fits and bodily mobility

It is trivial to suggest that people moved between sites and across their landscapes in ways both simple and complex (MONTGOMERY 2010; chapters in FERNÁNDEZ-GÖTZ et al. 2023). Radiogenic and stable isotopic research has shown the places and geologically distinctive landscapes where dead people were born, often long distances from where they were buried (BENTLEY 2006; SMITS et al. 2013; LAFFOON et al. 2017). These narratives have often been confirmed by large-scale aDNA studies (OLALDE et al. 2018; MATHIESON et al. 2018). Equally, massive, long-term research effort has been expended on determining the links between objects made of specific raw materials and their sources (PÉTREQUIN et al. 2017; GEHLEN et al. 2022; MUNTONI et al. 2022). The combination of these two approaches has yielded many insights into the networks relating people, places and objects in the past (JONES 2012; LEARY 2014). The addition of a fragmentation perspective to these approaches has not been without its methodological issues (CHAPMAN / GAYDARSKA 2007, 81–85), but we can now confirm the identification of fragments of the same object deposited on two or more different sites from a wide range of time-places, including Upper Palaeolithic and Mesolithic lithics, Bronze Age swords, and Roman pottery as well as large decorated stones placed in different megalithic tombs in the Breton Neolithic and Californian chert linking a quarry and a workshop in the Chuckwallah valley over 63 km (SINGER 1984; CHAPMAN / GAYDARSKA 2007, 106–111). The large-scale re-fitting operation in the Kavos Project, Cyclades (GR), was able to demonstrate the movement of thousands of marble figurine fragments to a special pilgrimage centre on Kavos by showing the total absence of on-site re-fits for the deposited fragments and therefore thousands of ‘orphan’ fragments (RENFREW 2015; RENFREW et al. 2013; RENFREW et al. 2015). The petrographic identification of the pottery sources for a sample of the fragments showed they mostly originated from within the Keros Triangle of Naxos, Amorgos and Ios (all GR; RENFREW 2015, 94 fig. 7,20).

However, when the fragmentation perspective shifts to human remains, new questions arise. First, it is much harder to identify re-fits between fragments of human (or animal) bone, even within an on-site study (SMITH / BRICKLEY 2009, chapter 4; ROSELL et al. 2019; MORIN et al. 2021). Secondly, isotopic studies of tooth enamel are ideally required to demonstrate that the place of origin was different from the place where the deceased was buried (BENTLEY 2006; FREI / PRICE 2012; COFFIN et al. 2022). Nonetheless, a growing group of studies has identified the presence of buried individuals who grew up outside the local settlement catchment or, even more appositely, the re-burial of body parts from non-local individuals (BRÜCK / BOOTH 2022). These studies offer support for the alternative mobility scenario for the Herxheim site – a notion based upon a division of bodies for transport.

With the benefit of minimal reference to isotopic analysis (MONTGOMERY et al. 2000), Martin KING (2003) has identified the widespread dispersal of human skeletal material across the landscape in both the Mesolithic and Neolithic periods in the UK, suggesting that, at a gross level, this was a “fall-out from a dispersed, mobile occupation system” (KING 2003, 199). As part of this pattern, King identified a large number of cases of the deliberate selection of particular skeletal categories by their presence or absence (e.g. Orcadian tombs and the Hambledon Hill complex: KING 2003, 102). While the lack of isotopic information prevented King from specifying the spatial range of human skeletal mobility, more recent studies have confirmed his basic thesis. An example is Samantha Neil’s study of the strontium isotopic characteristics of disarticulated, highly fragmentary human remains from two Welsh Early Neolithic sites, which shows contrasting results from the two chamber tombs, with the Penywylrod individuals mostly ‘local’ and the Ty Isaf individuals mostly ‘non-local’ (NEIL 2022). The bioarchaeological study showed how the commingled remains

from different individuals were the “result of re-arrangements from a pool of already disarticulated bones” (WYSOCKI 2022, 174).

In a study focussing on the Iron Gates Mesolithic and Neolithic, WALLDUCK (2013) has demonstrated similar bodily mobility through the identification of many partial burials, noting that they often comprised far more complex *chaînes opératoires* than single, complete individual burials. Later studies of the Gorge showed a more complex mobility pattern, with Camille DE BECDELÈVRE et al. (2020) using a combination of other researchers’ strontium isotopic data and aDNA data to identify some locals who grew up in the gorge possessing Anatolian genomic ancestry, while one non-local from another region had a Mesolithic genomic ancestry similar to that of many Iron Gates Mesolithic individuals. These results underline the key role of Lepenski Vir (RS; 6150–5500 cal BC) as a congregation site for both high-status hunter-gatherers and farmers (RADOVANOVIĆ 1996), with grouped burials of locals and non-locals reinforcing new social relations (DE BECDELÈVRE et al. 2020).

Moving to the Mediterranean zone, Antonio Faustino CARVALHO et al.’s (2019) study of the excavations of concentrations of partial skeletons in two chambers at the Middle Neolithic Bom Santo cave in Portugal (3800–3400 cal BC) has shown how the cave formed part of a widespread, complex mortuary network linked by a chain of funerary practices, including primary deposition, exhumation, transportation and secondary deposition of parts of skeletons in the cave. This network distributed the mortuary process across the landscape in a series of different cemeteries, which were all linked to the Bom Santo cave. Comparable mortuary complexes with secondary burials of complete or partial bodies are known from the LBK and Later Neolithic from the Jungfernhöhle near Tiefenellern (SEREGÉLY 2012) and from Bronze Age Germany (e.g., the Lichtensteinhöhle, Lower Saxony; SCHILZ 2006).

Recent isotopic studies have focussed on those buried at an Italian Neolithic cave of comparable significance to Bom Santo – the late 6th millennium cal BC Grotta Scaloria in Southeast Italy (ELSTER et al. 2016). Here, in the Upper Cave, a small assemblage of highly fragmented, commingled bones including adult males, adult females and juveniles showed both a wide variety of dietary nitrogen isotopic values (TAFURI et al. 2016a, 137) and strontium isotopic values suggesting the population derived from a plurality of geological and social catchments (TAFURI et al. 2016b). These findings were interpreted to indicate the use of Grotta Scaloria not simply as a burial site for a local community but rather a gathering place for people living in the entire Gargano-Tavoliere region. The partial correlation between strontium values of ‘non-locals’ and particular body parts (e.g. femora) may indicate that “bodies from “non-local” places were brought to the cave as selected parts” (TAFURI et al. 2016a, 142).

Two isotopic studies of the burials at large enclosures found in Portugal dating to the Late Neolithic and Chalcolithic show contrasting results about the distances travelled to the final resting places. In a study of teeth from 115 different individuals buried over a period of 200 years (26th–25th centuries cal BC) at the enclosure of Marroquies, only 8% proved to be non-locals (DÍAZ-ZORITA BONILLA et al. 2018). As the research group observed, “the social life of the body did not end with death but acquired a new ontological status, transforming the deceased individual into a new kind of being who retains both agency and a capacity for action” (DÍAZ-ZORITA BONILLA et al. 2020, 347).

By contrast, the results of the isotopic study of the fragmented remains of 69 Neolithic and Chalcolithic persons from Perdigoões showed that a quarter came from beyond the local geologies characterising a 20km radius of the enclosure (VALERA et al. 2020). António Carlos Valera et al.

maintain that discussions of mobility should include assessments of the nature of the site and its social role, given the contrasting finding that all the individuals analysed from the megalithic tombs near Perdigões came from local catchments.

A final comparative study concerns the congregation site of Alsónyék in Western Hungary (BÁNFY *et al.* 2016; GAYDARSKA / CHAPMAN 2022). One of the longest-living places in European prehistory, with occupations lasting from the Early Neolithic Starčevo group, with a periodic gap, to the late Lengyel period (58th century cal BC–43rd century cal BC), Alsónyék reached the apogee of its mortuary activities in the Lengyel period, in the 46th and 45th centuries cal BC, with a modelled mortuary peak c. 4730 cal BC (BAYLISS *et al.* 2016). Consistent with the usual assumptions of Neolithic lifeways, the research team estimated a large population in the Lengyel period coeval with the vast mortuary deposition. However, reconstruction of the local palaeo-environment (DEPAERMEN-TIER *et al.* 2020) indicated a rather low land-use potential which would in no way have been capable of sustaining such a large population. Our radical alternative to the ‘normal’ picture of permanent Neolithic settlement involved moving complete bodies of deceased people from neighbouring Lengyel sites to Alsónyék to explicate the high number of burials. Although the site burials have not been published in full, there is no indication as yet (OSZTÁS *et al.* 2016) of the transport of partially dismembered human bodies to what we consider as a mortuary congregation centre. Our alternative implies the transport of complete human bodies to Alsónyék over distances of up to 20 km.

All of these studies share the same results of a widespread dispersion and movement of human remains across a variety of European landscapes, with Bom Santo and Grotta Scaloria showing the movement of fragmented bodies. While accepting that there will be a variety of different funeral ritual pathways, it is important to give proper consideration of the possibility of the movement of parts of human bodies across the landscape to special places.

There are two stages in the methodology to distinguish the mobility of human bodies or their parts: (1) the demonstration that fragmentary or complete bodies were buried at some distance from their place of childhood residence, thus enabling the differentiation of ‘locals’ from ‘non-locals’ (e.g. Welsh long cairns, Bom Santo, Grotta Scaloria, Marroquies, Perdigões, Alsónyék and the Lichtensteinhöhle); and (2) the demonstration that fragmentary or complete human bodies were moved to their place of burial from a non-local settlement rather than simply moving to near their place of burial for the last 10–20 years of their lives (e.g. Grotta Scaloria, Alsónyék).

In the remainder of this paper, our aim is to investigate the two stages of the human body mobility scenario in the case of the Herxheim enclosure. After all, it has already been well established by the Herxheim team’s research that a mobility model can account for the exotic pottery and stone tools which were brought by some means to Herxheim. So why not extend this notion to human body parts?

The Herxheim enclosure

The Herxheim enclosure is one of the most extraordinary sites in prehistoric Europe, with a massive series of heavily fragmented deposits of human and animal bones, pottery, lithics, and single finds, placed for the most part in two incomplete ditch circuits in the latest LBK phase, some time in the last century of the 6th millennium cal BC (ZEEB-LANZ *et al.* 2007; ZEEB-LANZ 2016; 2019a).

The excavation of the Herxheim enclosure took place in two four-year operations – 1996–1999 (the ‘rescue’ excavation) and 2005–2008 (the ‘research’ excavation) (ZEEB-LANZ / HAACK 2016). The Herxheim enclosure is double-ditched but incomplete, forming a trapezoidal shape of almost

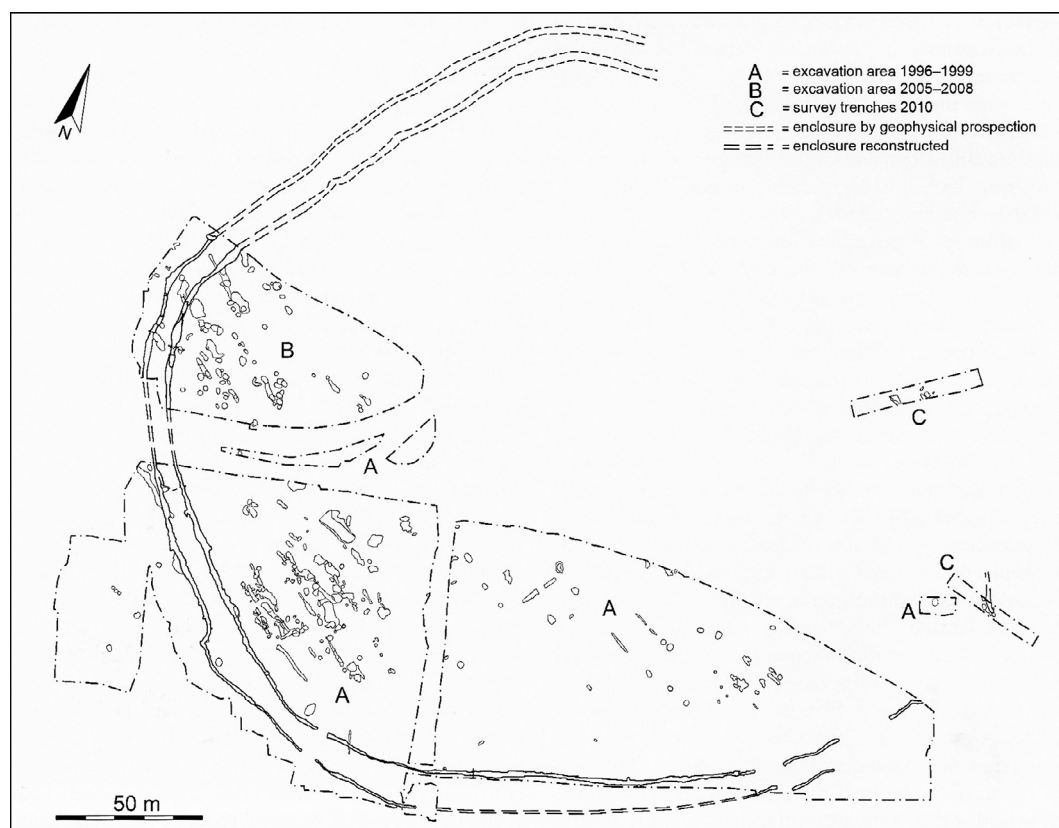


Fig. 4. Herxheim. Plan of enclosure showing location of excavations: A rescue excavation; B research excavation; C survey trenches.

6 ha (c. 270 m [north–south] x 220 m [east–west]) (Fig. 4). Long segments of ditch were missing on the east side and it is possible that only the inner ditch was present in the south-east part of the site (ZEEB-LANZ 2019b, 429). Much erosion has truncated the LBK living surface and much of the upper portions of the pits and ditches (HAACK 2016a, esp. 22–23). Pottery from Phases II–V of the Palatinate LBK sequence has been found on site but the main activity – termed ‘the ritual phase’ and including the digging of the ditches – dated to the latest LBK Phase V, with secondary incorporation of what we would term ‘ancestral’ material (viz. Phases III and IV pottery) in the bottom of some ditch segments. Settlement material inside the inner ditch included house remains and cut features (usually pits), mostly from Phase V. The basic architectural unit was the ‘long pit’ (ZEEB-LANZ 2019b, 428), with sequences of long pits comprising the inner and outer ditches. There were very few re-cuts of the ditch fill, leading Fabian HAACK (2016a, 113–115) to reject Christian JEUNESSE’s (2011) claim that Herxheim fitted the ‘Rosheim’ model of a pseudo-ditched enclosure acting as a long-term ritual centre for secondary burials lasting for centuries. Instead, Andrea Zeeb-Lanz favours a series of long pits each dug and utilised over a short period of time, with finds concentrations often covering the length of several long pits (ZEEB-LANZ 2019b, 428). While Anthony Denaire suggests that the remains of each ritual ‘event’ were placed in a midden and then thrown into the nearest long pit (DENAIRE 2019, 38), A. Zeeb-Lanz proposes the variant that piles of ritually fragmented material were kept temporarily near the open parts of the ditches, with the unintentional commingling of fragments through deposition in the ditches (ZEEB-LANZ 2019b,

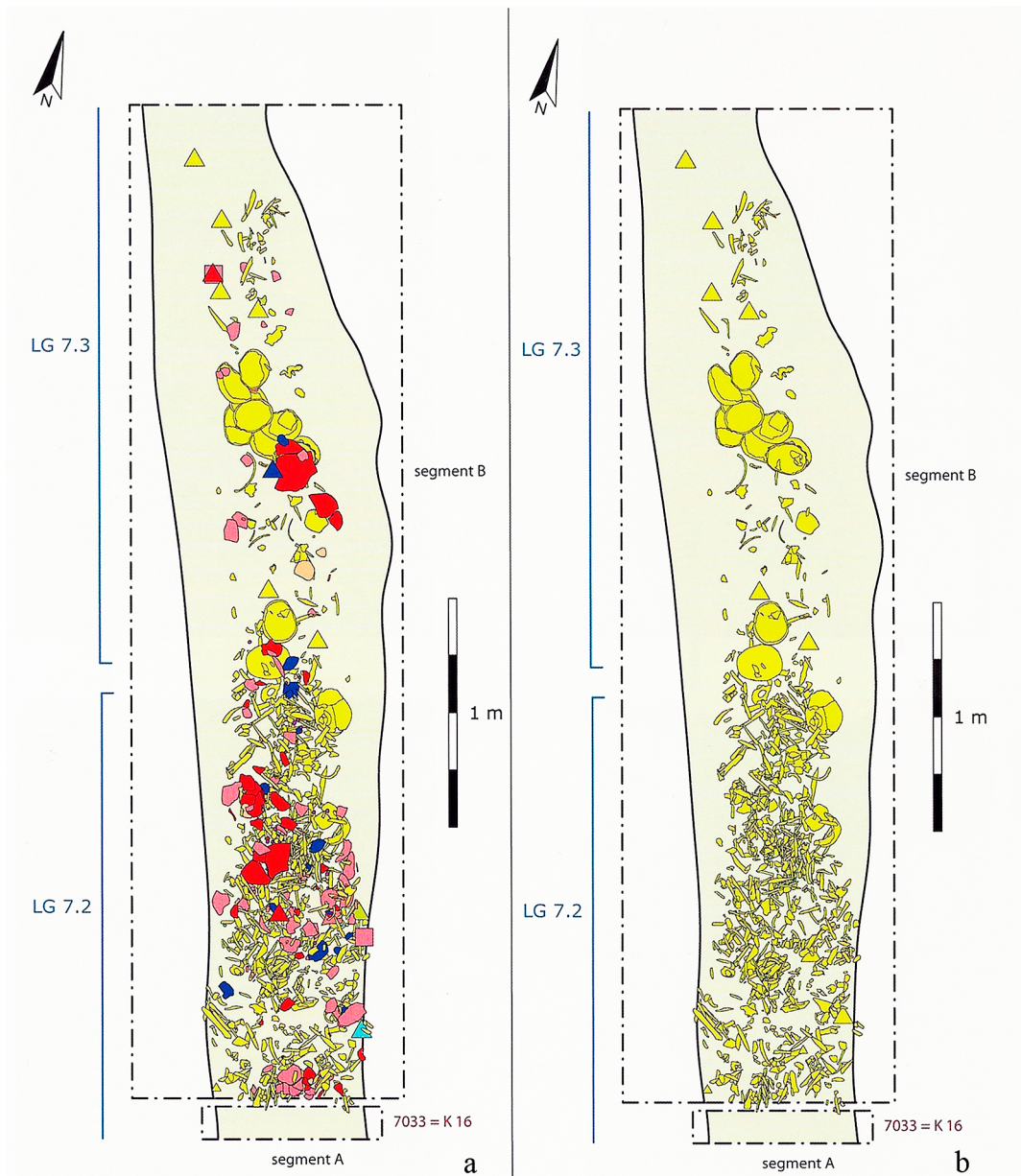


Fig. 5. Herxheim. Detail of two plans of Long Pits LG 7.2 and 7.3, inner ditch ring, showing finds horizons 5–19, at depths of 132.00–130.51 m ASL.

466). A major recurrent feature of both ditches was the presence of ‘finds concentrations’ which were normally a mixture of predominantly earth interspersed with some finds (ZEEB-LANZ et al. 2007, 266). Occasionally, as in concentration K16, a 1.50 m-thick concentration of finds (depths 132.00–130.512 m ASL) contained mostly earth (Fig. 5) (HAACK 2016b, pl. 66). Tightly packed finds clusters occurred rarely and, even then, not across the whole of a concentration (e.g. clusters were found in only part of Concentration K 9/18) (HAACK 2016a, 69–74). The discovery of finds in the earth above these concentrations has been thought to mean that the finds were deposited with earth as the final stage of the ritual, with no intention of placing all of the finds in concentra-

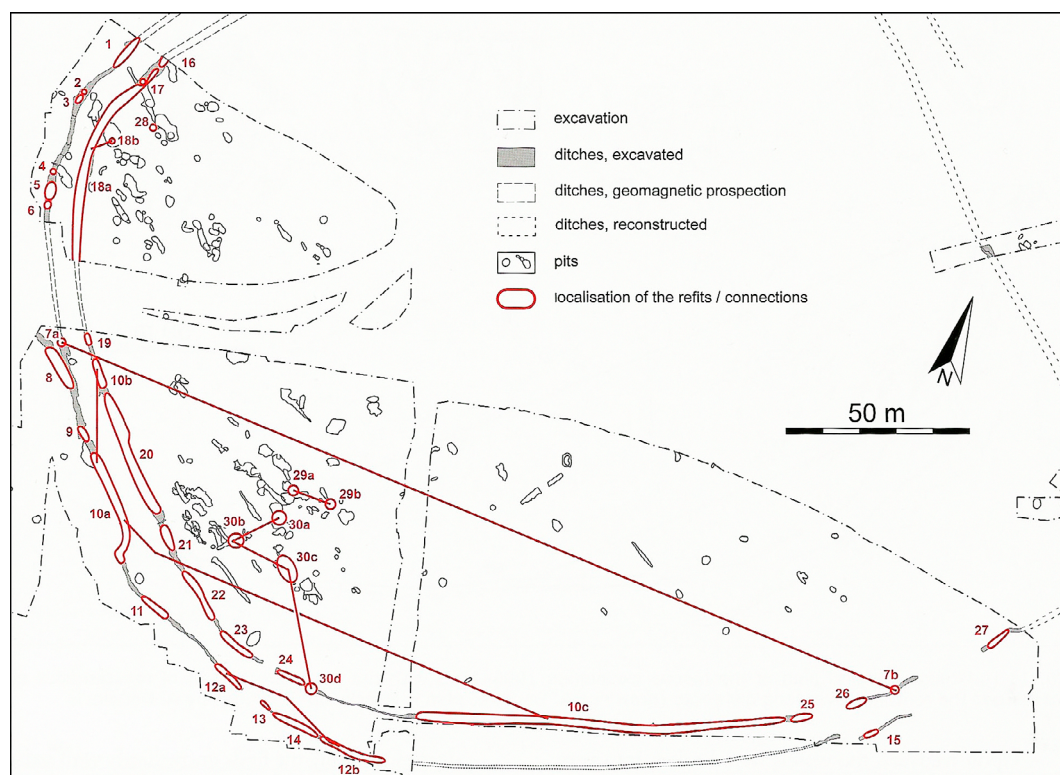


Fig. 6. Herxheim. Concentrations of reassembled sherds and long-distance sherd re-fits.

tions (HAACK 2016a, 74). Anthony Denaire has noted that the major clusters of sherd re-fits correlate well with the human bone concentrations (*Fig. 6*) (DENAIRE 2019, 34).

The most striking biosocial remains were the human skeletal remains, most of which were deposited in the inner and outer ditches (BOULESTIN / COUPEY 2015; BAUER 2019). The human bones derive from persons of all ages but with an under-representation of children younger than five years and an over-representation of juveniles and young adults (BOULESTIN / COUPEY 2015). However, this observed age representation might be caused by osteological and taphonomic factors rather than representing the 'true' demographic picture. The bodies suffered from dismemberment soon after death and then the smashing of their bones. Despite re-fitting efforts with 1891 bone fragments from eight slots in the rescue excavations (slots 282-100 to 282-107), it has never been possible to re-fit a single complete long bone from these slots (BAUER 2019, 11). Without a more comprehensive re-fitting programme, it is hard to answer the question of the location of the missing fragments – whether off-site or on-site, on the surface of the enclosure or in as yet unexcavated parts of the ditches (ZEEB-LANZ / HAACK 2020). Special treatment was afforded to cranial material, with the production of calottes (skull-caps) and – in a few limited cases – their deposition in groups. Both Bruno Boulestin / Anne-Sophie Coupey and Silja Bauer have recognised the overwhelming predominance of peri-mortem butchery and the fragmentation of 'fresh' bone – bone with the flesh still in place – through cut marks, fracture lines, and fracture profiles over the relatively few cases of post-mortem processing of 'dry' bones (BAUER 2019, 5). All analysts have also noted the low number of burnt human bone fragments, at c. 3–3.6%, with the burning often appearing on fractures, so post-dating the break (BOULESTIN / COUPEY 2015, 65; BAUER 2019, 16). A major disagreement

within the Herxheim team concerns the treatment of the human remains, with B. BOULESTIN et al. (2009) arguing for mass cannibalism and Zeeb-Lanz rehearsing lengthy and compelling arguments against this interpretation (ZEEB-LANZ 2019b, 449–454). The alternative that Zeeb-Lanz proposes is the mass sacrifice of human captives at Herxheim following raids on other villages (ZEEB-LANZ 2019b, 457–463).

The Herxheim pottery assemblage has not yet been studied in full but discussion of the ceramics has occurred regularly in the project publications (HAACK 2016a, 15–118; DENAIRE 2019, 25–40; MECKING 2019, 41–54). The assemblage comprised an estimated 15 000 sherds, with over 50 % produced in the local Palatinate style. Petrographic analysis has been conducted on over 100 sherds, including 25 imported and 19 possibly imported sherds (MECKING 2019). There was considerable variability in the pastes of sherds ‘local’ to Herxheim. On the assumption that all the ‘exotic’ sherds were made in the area where their style was used, the surprisingly high total of 37 different pastes for the 45 sherds indicates not only that the imports came from many different regions but from multiple sites within those regions (*Fig. 7a*; MECKING 2019, 51–53).

The highly fragmented faunal assemblage of over 15 000 fragments deriving from the ditches and the settlement features shows all the characteristics of butchering waste (ARBOGAST 2019), suggesting the intensive exploitation of the carcasses and bones for food – meat (GILLIS 2019), marrow, and grease (JOHNSON 2019) as well as leather, sinews, and hair. Overall, the wild animal remains in the ditch assemblages were similar to those in the settlement assemblage, with limited selection of carcass elements. Slaughter and butchery of domestic animals took place in the same areas, except for the deposition of clustered dog remains in the inner ditch, where bone re-fits of 90 bones derived from eight individuals, with remains from a further two to four dogs present (JANSSENS et al. 2019). There was both more burning and greater fragmentation of animal bones in the settlement in comparison with the ditch assemblages. The animal bones from the ditches were more mineralised than those from the settlement, either because they had been curated before deposition or because these were ‘ancestral’ bones from earlier pits cut by the Latest LBK ditches (JOHNSON 2019).

The lithic remains can be divided into chipped stone, polished stone, colouring items, and ground stone (SCHIMMELPFENNIG 2019). The chipped stone showed the tool spectrum of a ‘normal’ settlement assemblage (SCHIMMELPFENNIG 2019, 102) except for the off-site ‘destruction’ of many sickle blades which were subsequently brought into the enclosure as splintered pieces. The majority of chipped stone items was deposited in the inner ditch, with fewer in the settlement features and even fewer in the outer ditch. Most of the items were of Upper Cretaceous flint from at least 200 km away, with a small number of Jurassic cherts from the south (no more than 300 km away) and a few Bartonian flints from much further east. These inter-regional imports were considered as important and coming from many directions (*Fig. 7a*; SCHIMMELPFENNIG 2019, 91). Lower-quality local *Muschelkalk* cherts would have been available 15–20 km from Herxheim.

The polished stone fragments derived from shoe-last adzes, with several re-fits and signs of both intentional fragmentation and thus deliberate destruction. The depositional structure was the same as for the chipped stone. Sources for the amphibolites included the Bohemian Jizera Mountains, 550 km away, while pelite-quartz came from the Vosges Mountains, 190 km away.

All of the larger pieces of sandstone were manuports – unworked objects not local to the site, which had therefore been brought onto the site – mostly of Bunter sandstone, which was either local, from 15 km away, or from the Vosges Mountains. Most of the ground stone came from settlement features, less from the inner ditch and least from the outer ditch, with very varied quantities of ground stone in the concentrations. A small proportion of ground stone pieces comprised complete

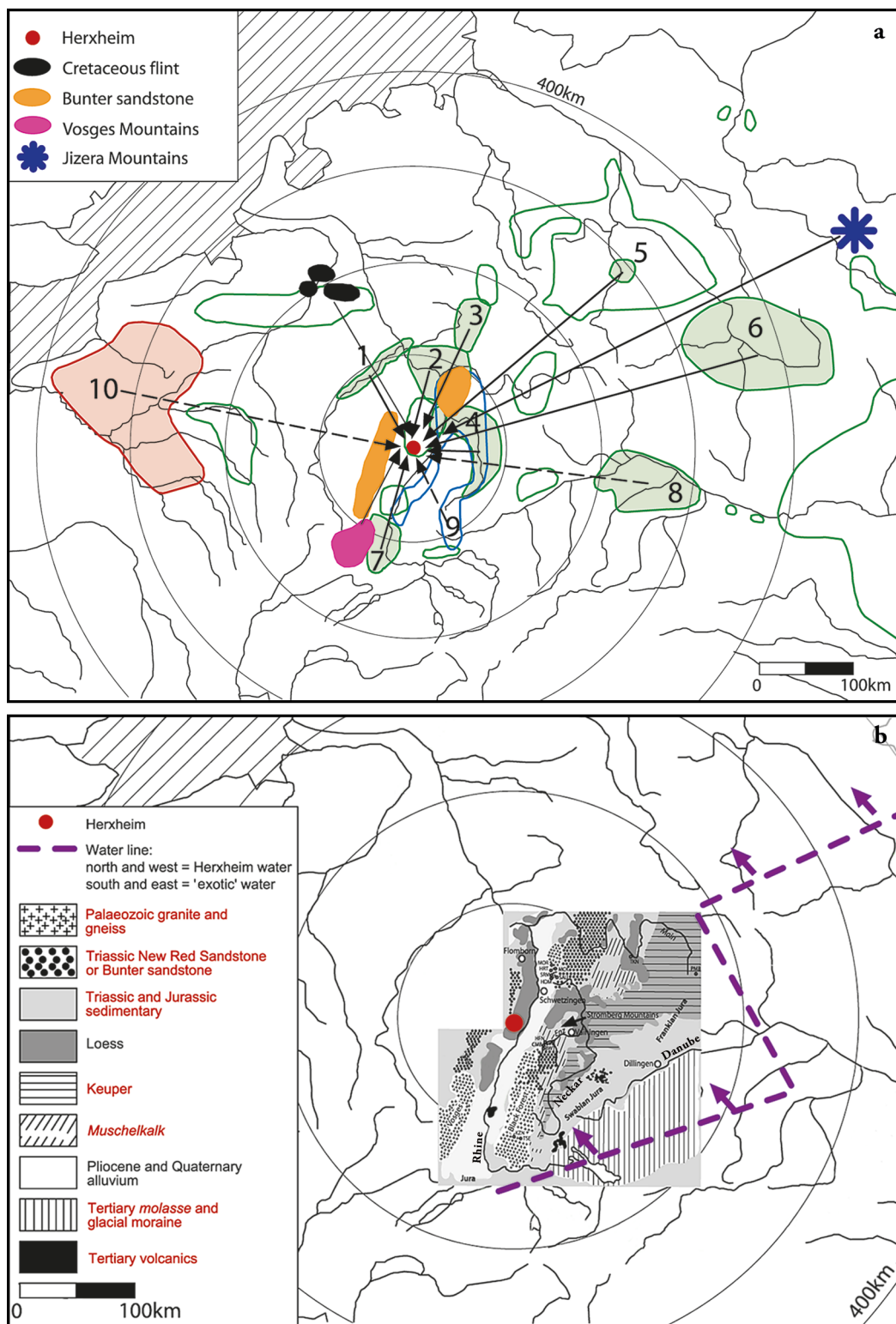


Fig. 7. a Sources of lithic and ceramic materials found deposited at Herxheim: lithics – in key; ceramic styles: 1 Rhine-Moselle; 2 Rhine-Main; 3 Northern Hesse; 4 Neckar; 5 Elster-Saale; 6 Bohemia (Šarka); 7 Upper Alsace; 8 Bavaria; 9 Hinkelstein area; 10 Blicquy. – b Water sources based upon $\delta^{18}\text{O}\text{H}_2\text{O}$ values.

objects – grinders and saddle querns. Many querns had been made brittle by being placed in fires and intentionally smashed afterwards (ZEEB-LANZ 2019b, 436).

Dirk Schimmelpfennig emphasises the dual, complementary aspects of stone – as an integral part of everyday life and as an important part of ritual. He suggests that the fragmentation of stone items – often through burning – was precisely because of its importance in everyday practices of building, maintenance activities and subsistence (SCHIMMELPFENNIG 2019, 130–131).

The huge quantity of material has formed the basis for one of the most ambitious re-fitting exercises in the last decades, for which the researchers should be congratulated. The re-fitting operations included several thousand human bone fragments from the rescue excavations out of the total sample of 75 000, over 15 000 sherds and an undefined number of lithic items out of a total sample of 6000. Some 356 physical re-fits have been made of sherds, with over 2000 additional ‘connections’ (probable re-fits without a physical connection; DENAIRE 2019, 27–32; *Fig. 6*). The majority of re-fits occurred between pottery within the same ditch segment but inter-cluster re-fits were made, as well as ditch-to-inner pit re-fits and long-distance re-fits up to 245 m (HAACK 2016a, 47–77; DENAIRE 2019, 32–35), although it is important to note that several concentrations (5, 7 and 28) had no inter-cluster re-fits at all (DENAIRE 2019, 33). Human bone re-fits have also been demonstrated though not on the same scale as pottery refits, with re-fits between the ditch and the inner settlement pits as well as within and between concentrations and between ditches (HAACK 2016a, 62–63). D. SCHIMMELPFENNIG (2019, 96) confirms that only 2% of the chipped stone assemblage of c. 6000 items could be re-fitted, without giving specific details of the locations. He also confirms deliberate fragmentation of polished stone tools in a small assemblage of 55 fragments, with 11 fragments re-fitted to five axes (SCHIMMELPFENNIG 2019, 105). But the intentional fragmentation of ground stone querns, often using fire, is much more widespread, with 110 querns re-fitted from 271 fragments, including inter-ditch re-fits but no re-fits between ditches and inner settlement pits (SCHIMMELPFENNIG 2019, 119). The overall picture from the re-fitting studies shows a richly interconnected set of contexts showing the materialisation of enchainment relations through fragment deposition.

Analyses of aDNA, strontium and oxygen in teeth, together with carbon/nitrogen isotopes of human bone provided surprising results (TURCK 2019). While the aDNA analysis showed that all the analysed bones and teeth were indistinguishable from the standard Early Neolithic signal from central Europe, the strontium isotopic analysis showed that 90% of the sample, at least in their early years, were non-local to Herxheim, deriving from at least four other areas: 20 lowland individuals from three regions: nine local to Herxheim, the others from non-Herxheim loess or *Muschelkalk* areas; 13 individuals from *Keuper* or *Buntsandstein* areas, hills, or low mountain ranges; and 40 individuals from low mountain ranges (*Buntsandstein* and granite or slate; *Fig. 7b*). Such a conclusion is not contradicted by the oxygen isotopic analysis of varied, often non-local water sources and the carbon/nitrogen isotopic analyses of varied diets (cf. BUDD et al. 2000). This has posed the greatest dilemma for the interpretation of Herxheim, since neither coeval upland nor coeval lowland sites are currently known. Yet the aDNA of the ‘upland’ individuals with isotopic strontium results (a minimum of 22 individuals) shows that they were as much part of the typical early farming genomic signal as the lowland individuals. The most significant issue thus remains how such a large group of fragmented bones from an estimated 1000 people (TURCK 2019), of whom an estimated 900 were non-locals, came to be deposited at Herxheim.

It is difficult to summarise the Herxheim findings without oversimplifying what is clearly an enormously complex sequence of operations. There is an underlying tension in the project team publications between the significance of random behaviour at the site in contrast to the systematic,

regular, rule-bound practices which may be expected to govern such a complex operation. We shall not dwell on this contrast since we believe that variability is inevitable, given the different ways that various participating groups drew on the basic rules pertinent to the Herxheim enclosure, which necessarily included both ‘normal’ settlement practices and ‘extraordinary’ ritual practices.

The current interpretation of the Herxheim findings is most comprehensively presented in Zeeb-Lanz’s concluding synthesis of the project’s second monograph (ZEEB-LANZ 2019b), which draws on insights from other researchers (GRAMSCH 2012; HOFMANN 2012). For Zeeb-Lanz, Herxheim possessed the widest range of ritual practices in the latest LBK, with other regions connected to Herxheim through an inter-regional alliance based on traditional lineage ties. The primary aim of the Herxheim ceremonial feasting and rituals was to strengthen and deepen such ties. The Herxheim rituals were less about “violence against humans and objects” than “ritually charged transformations of humans and objects using physical force” (ZEEB-LANZ 2019b, 463). What this meant was that “the destruction of precious artefacts (to which we may add ‘precious persons’) represented a leading theme throughout the Herxheim scenario as a whole” (ZEEB-LANZ 2019b, 454). Zeeb-Lanz admits that the identity of the victims of this violence is still a mystery but insists that they came from a different ethnic group from the Herxheim residents and were either unfree serfs (slaves) or captives, brought to Herxheim by farming groups from the inter-regional alliance. These slaves/captives were ‘processed’ in a multi-stage sequence at Herxheim: (1) intentional killing; (2) dismemberment of their bodies; (3) removal of muscle tissue; (4) smashing of all bones except for the cranial material; (5) burning of some of the bones; and (6) final deposition in the ditches and, more rarely, in the settlement features. The artefacts were also ‘processed’ through various operational chains, all of which concluded in deposition as the final stage of the ritual process.

Now that we have summarised the key elements of the Herxheim publications, it is time to apply to Herxheim the insights derived from other examples of mobility (see above, *pp.* 4–6) to open up an interpretative space for an alternative narrative for the Herxheim site.

Source criticism at Herxheim

The key insight from landscape fragmentation is the way that other landscapes are enchainment to places by the materials derived from the sources and used and deposited in other places. We have shown how the vast majority of the Herxheim stone assemblage, as well as an estimated quarter of the pottery assemblage, derived from landscape fragmentation of rock outcrops and clay sources in numerous different zones (*Fig. 7a*). This means that we can demonstrate enchainment of the Herxheim residents with an exchange network of other LBK groups reaching in all directions, often up to 200 km and sometimes more. The importance of Zeeb-Lanz’s inter-regional network lies in the contrast between the ways that enchainment works over long distances (exchange of sherds) as well as over short distances (inter-long-pit re-fits at Herxheim), using the principle of *synecdoche* – the part representing the whole (CHAPMAN 2000, 67).

Andy JONES (2012, 19–20) has reminded us that enchainment is the basis for the creation and maintenance of all social relations. In the context of dismembered human bones, the placement of one fragment of a person’s body in a long pit with sherds from the Elster-Saale region linked the person to the vessel (e. g. in the inner ditch in slot 282-139), just as a second bone fragment placed in another long pit with sherds from the Rhine-Main region (e. g. in the inner ditch in slot 282-12) indicates the use of biosocial material (bones and sherds) to proclaim specific relationships at the time of a spectacular performance in a central place. The choice of a specific long pit for deposition was also an enchainment of biosocial material to the group or groups that excavated that ditch

segment. In this reading, the most disputable idea in the Herxheim reports is the claim that the fragmented remains were “nothing more than the refuse of the rituals”. It is ironic that this misleading claim was made by the specialist who re-fitted the pottery (DENAIRE 2019, 39). Just because earth, sherds, stone tools, human bones and animal bones were mixed together does not mean that there was no intentionality towards those combinations. Fragments are never only refuse but always enchain other relations, even if, in this case, we can rarely answer the fragmenterist’s question: “where are the missing fragments?” Enchained relations expressed through biosocial remains can provide a general explanation of many detailed questions raised for Herxheim, such as the excavation team’s puzzlement over fragmented bone deposits in settlement pits, which would have related the deceased to the local residents (ZEEB-LANZ 2019b, 457). But its greatest relevance is to the big questions of Herxheim – the overall motive for such large-scale fragmentation and deposition. While the excavation team has properly concentrated on the details of the depositional sequences, we wish to present here general patterns based upon a different approach.

If there is a single criticism of the published Herxheim interpretation, it is that sherd or bone re-fits have been used to support a chronological link in deposition without, or very rarely, taking into account the possibility of fragment curation. Thus, re-fits in both layers of concentration K16 are interpreted to mean that both layers were deposited at the same time (HAACK 2016a, 49), while ZEEB-LANZ (2019b, 448) claims that long-distance re-fits mean that ditch segments far apart were open at the same time. In this instance, we are not making a direct comparison of other sites (e.g. Kavos in the Cyclades: CHAPMAN et al. in press) with Herxheim but, rather, highlighting possible social practices attested at other sites which could be investigated at Herxheim.

The evidence for sherd curation at Herxheim comes from three sources – sherd surfaces, different life-histories and object itineraries, and the evidence for vessel fragmentation. The observation of spalling on many sherds, including on one sherd of the longest sherd re-fit (DENAIRE 2019, 35), has indicated exposure to the elements, even if for no more than one winter (DENAIRE 2019, 38). However, ZEEB-LANZ (2019b) objects that the variety of sediments in which the sherds were deposited could also have produced spalling. The issue remains for further scientific study. Another sign of the temporal scale of events is the important observation, not pursued by DENAIRE (2019, 27), that fragments from the same vessel had experienced different later life-histories after the break. We have examined this question in all of our re-fitting experiments (CHAPMAN / GAYDARSKA 2007, Chapters 3; 6; 7) and have demonstrated that it takes time for different life-histories to develop and this time often involves fragment curation. The important observation that many of the sherds have fresh breaks, showing fragmentation occurred shortly before deposition, neither supports nor denies the notion of pottery curation. Turning to human bone curation, the rarity of carnivore gnawing marks on the human bone fragments is not necessarily only a sign of rapid burial but could also indicate burial at a depth or curation of the body in a place inaccessible to scavengers such as dogs. As we shall see, the possibility of bone and sherd curation is an important factor at Herxheim. We now turn to a possible sequence of operations explaining Herxheim’s major dilemma.

An alternative narrative for Herxheim

In the relational approach to persons which underlies fragmentation research, the persons in the Herxheim network were both individuals with specific identities restricted to themselves (e.g. she was a daughter of Johannes and Brigitte²) and also individuals, whose relations with all the other persons, places and objects to whom they were linked contributed to their identities. This aspect of

² Not their real names!

dividuality was particularly important when objects were fragmented and re-used ‘after the break’, as in the case of the Hamangia figurines whose new fragmented identities created a change of gender (CHAPMAN/GAYDARSKA 2007, 33–70). But the Herxheim story forces us to confront the uncomfortable truth that the different parts of dismembered human bodies also took on this dividual aspect of their former identity, with smashed bones enchained to other smashed bones and other parts of the once-unified, now-fragmented body. How does this principle help us to write a different Herxheim narrative? Our answer transcends the insights of Jörg ORSCHIEDT and Miriam Noël HAIDLE (2006; 2012, 133), who proposed that the individuals were buried elsewhere, dug up again, and moved to Herxheim where their bones were subject to further manipulation and deposition. In any case, this sequence is opposed by Rouven TURCK (2019), who proposed that people gathered at Herxheim before their deaths, but he has not explained this idea in any more detail. We do not invoke the practice of secondary burial but, rather, a staged sequence of bodily manipulation that stretched spatially far beyond the Herxheim enclosure itself.

The Herxheim researchers’ attitudes to secondary burial rests on a questionable interpretation of peri-mortem cut marks on, and dismemberment of, the bones. In our view, very little evidence has been published that demonstrates conclusively that the people whose bones were deposited at Herxheim in the ‘ritual’ phase of the site also died at Herxheim. The exceptions were the few complete skeletons buried in the enclosure; even the few examples of articulated bones in the ditches could have been brought to the site. Furthermore, Zeeb-Lanz’s interpretation does not explain the large number of individuals (an estimated 1000 individuals) whose body parts were deposited at Herxheim. Zeeb-Lanz shows how there was only one Latest LBK hamlet outside the enclosure that was coeval with the ritual deposition: thus, most body parts came from outsiders who were not dwelling at Herxheim. The issue of an incomplete settlement record in both the lowlands and the uplands near Herxheim is troubling, although current revisions to the chronology of the Latest LBK settlement pattern are ongoing (pers. comm. Zeeb-Lanz). Just as there are currently no other known coeval lowland sites, there are no known upland sites – a symmetrical absence which merits further discussion. As Daniela HOFMANN (2020a, 231) concludes, “in addition to more or less standard agricultural sites which just happen to be at higher elevations, there must have been other communities in upland areas who have so far remained largely archaeologically invisible, and who perhaps set different economic priorities”.

Two of the most challenging results of fragmentation research since 2000 concern the certainty of inter-site re-fitting, whether at the landscape scale of moving parts of decorated stone blocks between megaliths in Neolithic Brittany (CHAPMAN/GAYDARSKA 2007, 106–107), or the high probability of fragment-based exchange, as seen, for example, at the Polgár-Csőszhalom tell in Hungary (CHAPMAN 2000, 64). The central paradox for Herxheim is the linkage of over three-quarters of the dismembered bodies to the hilly areas and low mountain zone lying a minimum of 20–25 km away to the east or west, or at least 75 km away to the North (*Fig. 7a*). Even if these sites have not yet been identified in the field, there is no reason to dispute the findings of the strontium and oxygen isotopic analyses. We shall make the assumption that there was a network of small, permanent, upland LBK settlements – we’ll call them ‘Home Communities’ – who maintained their position in the inter-regional Herxheim network through the provision of exchange goods, services and marriage partners. At a certain point (see below, *pp. 20–21*, for a discussion of the origins of the ritual practice at Herxheim), the community living at Herxheim – for the sake of argument, we’ll call them the ‘Herxheim Guardians’ – began a ritual intensification, inviting ‘Home Communities’ from a wide range of mostly lowland regions to contribute to the expanded rituals by spending a period of time – perhaps two weeks to a month *per annum* – at Herxheim, bringing animals to the feasting season, socialising, exchanging material valuables, dig-

ging a long pit, depositing sherds, stones and bones, and then re-filling the long pit. At another key changing point, the decision was made to bring parts of the dead ancestors of specific 'Home Communities' – those who had died since the last feasting season – to Herxheim to contribute their own ancestral enchainment links between Herxheim and the Home Community. This new practice involved a three-stage treatment of the newly-dead: curation of the newly-dead bodies until close to the time of the feasting season; dismemberment into several large pieces in preparation for travel to Herxheim; and additional, more intensified fragmentation of the bones at Herxheim (*Fig. 8*). Although they were sometimes identical, there was no necessary link between those 'Home Communities' bringing vessels to Herxheim and those 'Home Communities' who brought their own ancestral remains to the enclosure. In terms of the dismemberment of deceased humans, it may not be coincidental that animal carcasses at Herxheim were quartered into more easily treated pieces (ARBOGAST 2019, 162).

The bioarchaeological data demonstrate that the bodies were mostly 'fresh' when they were dismembered. There is little evidence for any difference between peri-mortem dissection performed six months after death or six days after death and the peri-mortem interval can potentially last indefinitely (SYMES et al. 2012). Although differences between dry and fresh bone fracture patterns can be identified, the exact timing of bone transitioning to dry bone fracture characteristics is not well understood. Data from experimental forensic studies, using pig or deer bone, have recorded different intervals, with a significant overlap of dry and fresh bone fracture patterns. Local climate and burial condition are the most important determining factors (GREEN/SCHULTZ 2017). A study of pig bones in eastern Ontario, Canada (JANJUA/ROGERS 2018) determined that bones remained fresh for as long as nine months. In addition, dry and fresh bone fracture characteristics of pig bones from Missouri, USA, were maintained for up to 141 days (WIEBERG/WESCOTT 2008). In frozen conditions or submerged environments, bone can retain moisture and would therefore present fresh fracture patterns for considerable periods of time (GALLOWAY et al. 2014). A focussed analysis of a sub-sample of bones, using microscopy to evaluate bioerosion, could potentially provide information about pre-depositional treatment of human remains (as in, e.g., BOOTH 2016).

In the upland settlements, the cold winter months would have aided preservation of the newly-dead bodies and curation of several months would have been possible until the onset of warmer weather and enhanced body odours. It is also possible that 'Home Communities' partly de-fleshed the bodies of their newly-dead, thus removing the olfactory objection. In any case, the more intensive dismemberment of the body parts and the smashing of individual bones would have taken place at Herxheim.

There are three variables in a Herxheim bodily mobility model: the number of annual Herxheim festive seasons; the total number of persons whose remains were deposited at Herxheim; and the number of upland 'Home Communities' contributing their ancestors to the enclosure.

Bayesian modelling of the AMS dates for the Herxheim enclosure is hindered by the existence of a plateau on the calibration curve (HAJDAS 2019), rendering all dates between 5200 and 5080 cal BC more or less indistinguishable. The only Latest LBK Phase V site with a large number of AMS dates is Herxheim and it is generally considered that the LBK Phase V is poorly dated (DENAIRE et al. 2017). Equally, the paucity of AMS dates from the earliest phase of the regional Middle Neolithic (the Hinkelstein I phase) means that it is currently hard to date the end of the regional LBK. By comparison, there is a time-span of c. 200 years between the end of LBK Phase IV 5100–5040 cal BC) and the start of the Hinkelstein II group (4835–4745 cal BC) in Lower Alsace, which could partly be filled by LBK Phase V – coeval with Herxheim. In the absence of sufficient AMS dates, a reasonable duration for LBK Phase V is 100 years, of which Herxheim could make up no more

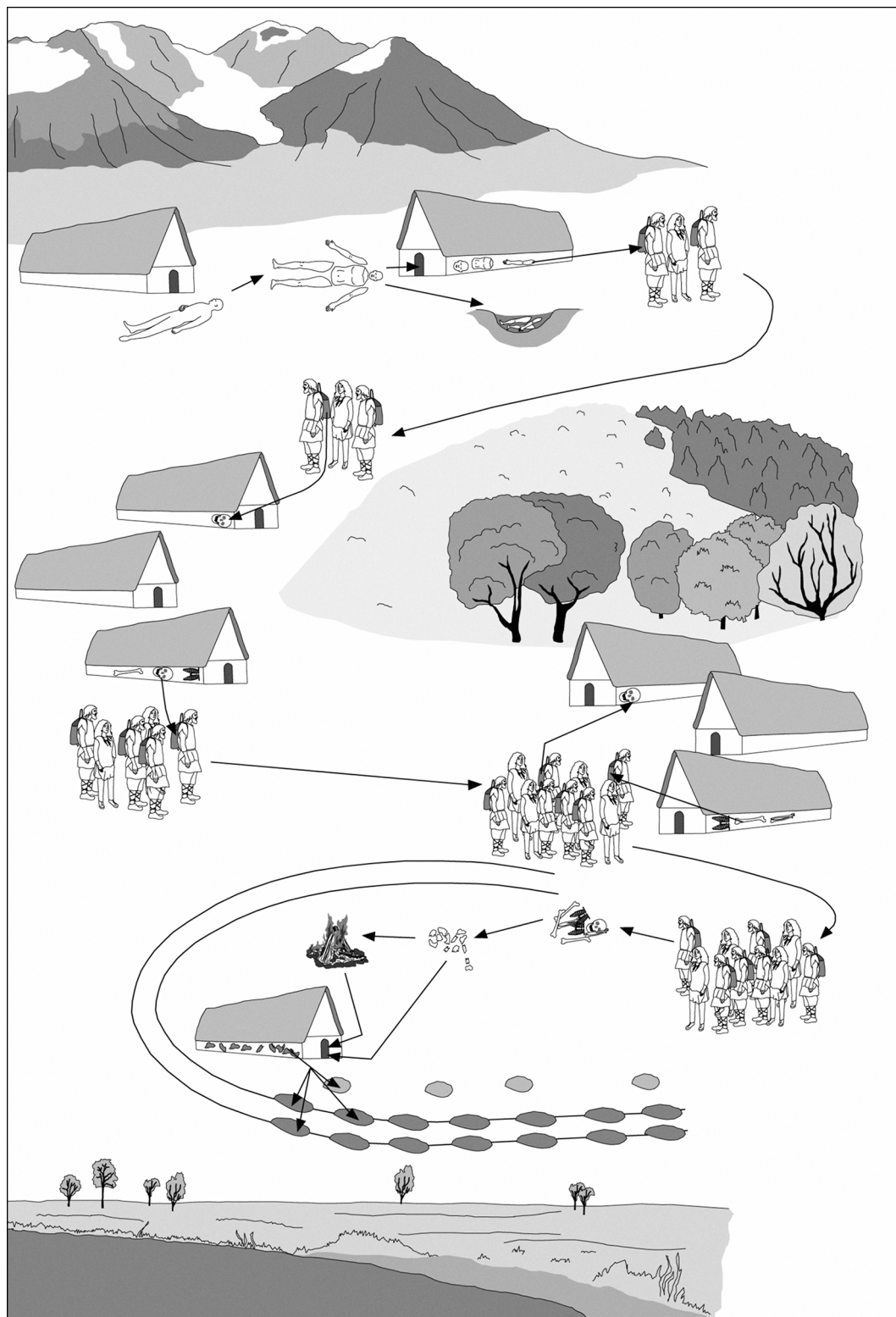


Fig. 8. Bodily mobility model for the Herxheim enclosure.

than half – for example, up to 50 years (RIEDHAMMER 2019). Karin RIEDHAMMER's (2019, 289) suggestion of a “relatively short-term set of events, perhaps spanning no more than 50 calendar years” is not based upon her Bayesian modelling but on ZEEB-LANZ et al.'s (2016) published views. In general, these insights fit poorly with Zeeb-Lanz's revised proposal of a much shorter duration for the Herxheim ‘ritual’ phase – “a narrow temporal window for the activities as a whole” – based upon a period of three, five or ten years centred on c. 5050 cal BC or 5030 cal BC (ZEEB-LANZ 2019b, 439, 448).

Peter DEMJÁN and Peter PAVÚK (2020) have developed a new method for the statistical evaluation of the clustering of AMS dates. The application of this method to the two sets of Herxheim dates – the 15 dates used by Irka HAJDAS (2019) and all of the 26 dates quoted by I. Hajdas and K. Riedhammer (RIEDHAMMER 2019) – produced different results. Testing the dates for normality (i. e. whether they are normally distributed around a single mean value) yielded a negative result for the larger dataset, meaning that the dates probably originate from two or more events. For the smaller dataset, normality could not be rejected, which may support the single-event hypothesis. Bayesian chronological models based on the most likely clustering into nine events for the larger and six events for the smaller dataset (assuming it was not a single event) show estimated time-spans of 64–261 years and 6–233 years respectively. Thus, it does not seem likely that the deaths of the buried individuals occurred over a time-span shorter than five years and it was more likely 50 or more years. Additional AMS dates would be required to estimate the number of events in the optimal clustering of dates.

The number of persons whose remains were deposited at Herxheim rests on a large sample of human bones which has not yet been completely studied. Many of the issues derive from the fact that three groups of people have analysed the remains (ORSCHIEDT/Haidle 2012; BOULESTIN/COUPEY 2015; BAUER 2019). The first two groups of researchers have made strong criticisms of each other's work, while Bauer – the Master's student of J. Orschiedt – comes to broadly similar conclusions as BOULESTIN and A.-S. COUPEY (2015). However, Boulestin and Coupey are correct in saying that there is no published quantification or analysis of the remains studied by J. Orschiedt and M. N. Haidle.

It is thus not surprising that the estimates for the minimum number of individuals (MNI) show considerable discrepancies and each estimate has been critiqued. The Boulestin and Coupey estimate of 104, based upon cranial fragments in the research excavation, has been extrapolated to a value of 1300–1400 for the whole site. The MNI of 1000 for the whole site is the estimate used by R. TURCK (2019); for the sake of modelling, we shall use Turck's estimate.

The number of upland communities from which human bones could have been transported is as difficult to substantiate as the number of coeval lowland communities. The methodologies for the production of the South Palatinate LBK settlement pattern is not known, but we suspect that it is not based upon widespread intensive, systematic fieldwalking. The chronological basis for the claim that Herxheim is the only occupied LBK Phase V site (for Latest LBK sites near Herxheim see ZEEB-LANZ 2019b, figs 2; 4) is currently undergoing revision (pers. comm. A. Zeeb-Lanz). Current data show that there is only Herxheim in the south at the latest LBK and three or four settlements in the north of the Palatinate (near Kirchheimbolanden, Donnersbergkreis, DE) which date to the Latest LBK. However, following the regional style of the pottery, the northern communities have no ties with the southern Palatinate as they belong to another style group from which not a single sherd has been found in Herxheim. ZEEB-LANZ's (2019b) figure 4 shows distinctly that, in the latest LBK, there is only the enclosure and a single homestead (one house, excavated during the rescue excavation) which obviously has direct ties to the rituals and may even belong to the Herxheim community, as the house is just 450 m outside the enclosure. There are currently no other known

Variable	Model 1	Model 2	Model 3	Model 4
Duration (years)	10	20	50	75
Estimated MNI	1,000	1,000	1,000	1,000
Estimated MNI, upland bodies	730	730	730	730
Estimated MNI, lowland bodies	270	270	270	270
No. of body part sets p.a., upland HCs	73	37	14	10
No. of body part sets p.a., lowland HCs	27	14	5	4
No. of sets x upland sites (Version 1)	10 sets x 7 sites	5 sets x 7 sites	3 sets x 5 sites	3 sets x 3 sites
No. of sets x upland sites (Version 2)	6 sets x 12 sites	3 sets x 12 sites	2 sets x 7 sites	1 set x 10 sites
No. of sets x lowland sites (Version 1)	5 sets x 6 sites	5 sets x 3 sites	6 sets x 1 site	4 sets x 1 site
No. of sets x lowland sites (Version 2)	2 sets x 13 sites	1 set x 14 sites	1 set x 5 sites	1 set x 4 sites

Tab. 1. Four bodily mobility models for the Herxheim enclosure (key: HCs – Home Communities; Versions – alternative values for each model).

nearby lowland communities at the time of the rituals. We have equally poor knowledge of upland settlement patterns, since forested areas and widespread pasture have reduced the possibility of ploughzone archaeology to a minimum (as in the Zemplén Mountains of Northeast Hungary: CHAPMAN et al. 2010).

These evidential gaps mean that it is imperative to create four models (*Tab. 1*) to provide the opportunity to examine as wide as possible a range of durations for Herxheim.

These relatively modest numbers suggest that both upland and lowland ‘Home Communities’ would have been able to produce sets of body parts on a scale compatible with the Herxheim human bone deposition for each of the four models, covering over ten to 75 years. The inverse relationship between duration and depositional intensity means that the maximum modelled number of sites in Model 1 reached 13 sites or, alternatively, the highest number of newly-dead reached ten per Home Community. Extending the temporal range of the ritual phase means a concomitant reduction in the number of sites or number of newly-dead, with values of 14 body-part sets for the model with the longest duration (75 years), deriving from between one and ten sites.

The range of estimated body-part deposition in the four models covers 14 *per annum* (75 years) to 100 *per annum* (ten years). It is worth noting the extraordinarily high rate of deposition at Herxheim in comparison with other large cemeteries in European prehistory. At the Varna I cemetery (BG), an estimated rate of one–two or two burials *per annum* was calculated for the 310 burials over a modelled duration of c. 150 years, while a higher rate of 12–24 *per annum* was modelled for the smaller Varna 3 cemetery (GAYDARSKA et al. 2021). At Durankulak (BG), Bayesian modelling of the AMS dates showed a duration of 800 years for a total of 1200 burials, or three burials every two years (HONCH et al. 2013). The standard version of the 50-year peak of Lengyel burials at Alsónyék produced modelled mortuary rates of 60 burials *per annum* (4725–4700 cal BC) and over 50 burials *per annum* (4700–4675 cal BC) (BÁNFFY et al. 2016; but see above, *p. 6*, for criticism of this model). Acceptance of the shortest duration of ten years for the use of the Herxheim enclosure means double the rate of bodily deposition than at the fastest rate of burial claimed elsewhere in European prehistory.

The likelihood of the radical incompleteness of the vast majority of the human bones deposited at Herxheim is echoed in the incompleteness of most of the bodies. This raises four possible answers to the question: “Where are the missing parts?” First, we cannot exclude that missing parts may have been deposited in the so far unexcavated parts of the site. If this were to be true, the distance between parts of the same bone indicates quite clearly a degree of intentionality in the bone dispersion and deposition. Secondly, the central point about *synecdoche* is that not all of the ancestral body was brought to Herxheim but parts of that body were retained in the Home Community to symbolise the local significance of the newly-dead person and their enchainment links to Herxheim. Thirdly, part of the ancestral body may have been taken from the Home Community but a fragment of that part was exchanged with a less remote settlement *en route* to Herxheim. And, fourthly, the body parts of the ancestor may have been brought to Herxheim but a fragment of those parts may have been exchanged with other ‘Home Communities’ during the festive season for removal to *their* Home Community. Similar scenarios may be proposed to explain why so few objects could be reconstructed to completeness at Herxheim. The important point to remember is that none of these scenarios requires a complex rationale over and above the notion of bodily dividuality and *synecdoche* – the potential to sub-divide the ancestral body to enchain the ancestor (and the living) to other persons or communities.

We suggest that the transport of parts of between one and ten bodies was feasible for upland communities, even if they had to travel for 100 km to Herxheim (*Fig. 8*). One of the authors (CHAPMAN 2020, 316) has proposed that the time taken to walk from a source site to a consumer site should be doubled to add ‘social time’ – the interactions with other communities *en route*. It is possible that communities *en route* may have joined the ‘mourners’ from the most remote Home Community in their common journey to the central place to form local ‘processions’. If this merging of mourning groups became part of an annual movement to the lowlands, it is possible to see the emergence of something not so divorced from a series of pilgrimage routes to Herxheim (for a very different form of prehistoric pilgrimage, see the Pilgrimage Model for the Trypillia megasite of Nebelivka, UA: CHAPMAN / GAYDARSKA 2019).

Origins and endings

We are very aware that the most difficult parts of explaining a complex, unique site such as Herxheim are why the ritual practices began and why they ended. It is interesting to note that neither topic has been a strong focus of research for the Herxheim research team in the last decade, with certain exceptions (ZEEB-LANZ 2009; ZEEB-LANZ et al. 2016; ZEEB-LANZ 2019b, 463–464). We can only suggest some hints at these end-points.

The origins of a central place are often connected to the history of that place, in the sense that a long-lived place builds up a cumulative place-value greater than that of a small, short-lived place (e.g. Lepenski Vir, Balkan tells, Minoan towns, and Stonehenge: CHAPMAN 2016). The history of Herxheim at the end of LBK Phase IV comprised the longest history of any of the LBK Palatinate sites, with settlement evidence for dwelling in Phases II, III and IV. It was therefore a sign of the importance of ancestral links to establish a central place at Herxheim. This decision will have been made within a wider debate on whether to create a central place at all and, if so, where it should be located. There was a major change in settlement patterns at the Phase IV/V transition in the Palatinate, as in other LBK regions (PECHTL 2020), defined by settlement contraction and/or concentration in the lowlands and settlement expansion into the uplands, at least as far as is documented by the Herxheim strontium evidence. This view is also supported by the analysis of drinking water

and by the isotopic dietary evidence, with a wider range of C and N values than those indicating a predominantly cereal-based diet (TURCK 2019, 379–381; 386–388) – i. e. a more pastoralist diet consistent with upland dwelling. This re-working of the settlement network could imply diametrically opposed views: an increased need for lowland–upland integration, especially if the novel environment and climate proved challenging to upland dwelling; or an upland expansion to rid the new ‘Home Communities’ of the undesirable political influence of an emergent lowland centre. In either case, these changes in network dynamics meant changes to traditional exchange relations, especially for the procurement of chipped stone, ground stone, axes and pottery. An emergent lowland centre such as Herxheim could well have been predicated on improved exchange networks, with seasonal meetings creating new opportunities for exchange, personal contacts and perhaps marriage. In these new socio-political conditions, ancestral relations and access to exotic goods were mutually reinforcing and would have strengthened the position of Herxheim as a central place. It would be at a later stage that the consolidation of the new upland sites would have led to more balanced relations with Herxheim, eventually leading to regular upland–lowland visits, the re-establishment of traditional lineage relations betokening shared ancestors, and the consolidation of enchainment relations through the upland communities’ most venerated objects – their own ancestors of a variety of ages. Any individual of whatever age whose body was dismembered in preparation for the visit to Herxheim had clearly been accorded a certain status as an ‘ancestor-to-be’ in an upland Home Community.

The 10-year, 20-year, 50-year or 75-year period of annual Herxheim festive seasons would have increased the renown of the Herxheim Guardians as the organisers of a key performance in the Palatinate LBK’s social calendar. The community of Guardians did not need to be very large – just a group of 20 persons living in one or two long-houses near an enclosure-to-be. The long-houses would have also acted as storage areas for festival food and drink, as well as any curated objects or relics waiting for an active role in the next festival. The Guardians would have lived for eleven months of the year as many other, smaller LBK villages had lived. But, for the one-month festive season, the Guardians took on a key role in ritual co-ordination and food supply for the festival. They would have collected sufficient tools for the digging of the long pits and amassed food for all of the participants. They would have marked out the locations of the first few long-pits in each of the two ditch segments, ready for the digging of the pits by the early arrivals. Once the ritual season had started, special skills among the Guardians would have been required for the deliberate fragmentation of the large quantities of human bones, animal bones, pottery and stonework brought onto the site or already present. A certain level of planning was required for the performances which contributed so vividly to the overall impression of the festive season, based as it was on fire, singing, dancing, story-telling, sex and violence. In addition, the labour of those visitors to Herxheim would have supplemented the work of the Herxheim Guardians.

An important feature of the Herxheim ceremonies was the cumulative repetition of the rites. With each passing year, ‘Home Communities’ created stronger enchainment links with the centre through the deposition of growing numbers of their newly-dead. These Communities also created increasingly vibrant links with other ‘Home Communities’ who visited the centre, making it increasingly difficult to resist the attractions of making the annual visit to Herxheim. In this way, the place-value of Herxheim grew at the same time as the strength of the tradition of participating in the Herxheim ceremonies. The identity of the central place became increasingly evident; Lucy Shaw EVANGELISTA and A. C. VALERA’S (2019, 64) words seem particularly evocative for Herxheim: “human bones in enclosures built the strength of each enclosure”.

One feature of the Herxheim ceremonies was the way that the ‘Home Communities’ retained power over detailed decisions on deposition. The choice of which fragment of a polished stone adze

for deposition in a long pit in the inner rather than the outer ditch and in which long pit to place fragments from the same decorated vessel (*Kumpf*) depended upon the enchainment relations which each Home Community wished to create or maintain with other groups. There was the choice of which group to help with digging 'their' long pit or filling it in later. All of these decisions meant a complex chain of events which led to highly varied depositional results. We shall not readily understand the detailed choices made by each participating group but the overall individual principles are clear enough. The two re-fitting Šárka sherds found 245 m apart in the inner and outer ditches (DENAIRE 2019, fig. 3) betokened a relationship that people from that distant area wished to create with another group. When the time came to go back home after the festive season, the 'Home Communities' would have kept some exotics to maintain their connections with the new contacts they had made at Herxheim.

The causes of a slowdown in the Herxheim performances included falling frequencies of exotic goods, and an increase in disputes, both of which could have prompted a decrease in the number of upland participants. While the dispute level is hard to estimate, an increase in the proportion of ditch segments without any deposited finds may indicate a downturn. Any decrease in the intensity of deposition may have been perceived as a decline in performance – a less impressive spectacle compared to the good old days. Serious disputes leading to homicides would certainly have led to problems – assassinations did wonders for the stay-at-home faction. For the participation of an upland settlement in the Herxheim festive season was not necessarily a straightforward decision. The curation of the newly-dead, their partial dismemberment and the long journey transporting their remains to the lowlands would have led to resistance from some members of the community. Any increase in problems at Herxheim could have led to the strengthening of these factions.

These are some of the issues for any group seeking to maintain a long-term ritual centre. One additional factor which could have created a tipping-point in favour of moving away from Herxheim was the creation of a rival, or simply another, central place (in a comparative example, the abandonment of the Nebelivka centre may well have been related to the founding of two relatively close 'rival' centres – Taljanki and Majdanetske, both UA: GAYDARSKA 2020). Any negative aspects of the Herxheim festive season over its entire history could have been exploited by the Guardians of a new centre, leading to further issues for the Herxheim community. There was no doubt of the potential at the Herxheim site to increase the number of long pits on the eastern side, leading to an extension of both ditch circuits. Even though ditch segments were infilled soon after deposits had been made, there would have been physical traces of the lines of the ditches, reinforced as they were with the cultural memory of the intensive rituals. The fact that extensions to the ditches did not happen may not relate to an original planning decision on the form of the enclosure but rather the dwindling interest in the ritual centre. The possibility of the future discovery of sites with similar complex mobility remains for further discussion.

In summary, the possibility of a greater upland contribution to Herxheim may be considered as our relational response to the central dilemma of the Herxheim site – the absence of upland settlement evidence in the Latest LBK when the strontium isotopic signals indicate an upland origin for three-quarters of the persons whose bones were deposited at Herxheim. The partible LBK body (ZEEB-LANZ 2019c; HOFMANN 2020a) suggests a way out of this dilemma, in which parts of the newly-dead from upland settlements were brought to Herxheim for further dismemberment and smashing of their bones prior to deposition. The principle of *synecdoche* was widely utilised to enchain human bones, sherds and fragmentary stonework in the complex depositional practices of this key site. Hofmann's case for widespread, if not ubiquitous, mobility within the entire duration of the LBK (HOFMANN 2020a) fits well with this proposed explanation of the Herxheim phenomenon.

Conclusions

In this article, we consider the fragmentation of all of the three poles of the identity triangle – persons, places and objects. Without the incorporation of places and bodies, the Fragmentation Premise remains damagingly incomplete. Conversely, the integration of bodies and places into fragmentation research provides a rich opportunity to address some major conundra in European prehistory.

The alternative model for Herxheim which we present is based upon principles and practices well-known to fragmentation research – enchainment, *synecdoche*, presencing, and curation. All of these provide ways to explain small-scale inter-household practices just as well as unusually large concentrations of fragmented remains and at a variety of spatial scales, from intra-ditch to wide-ranging exchange networks. The most important concerns enchainment, which operates at the on-site level in several ways, relating fragments of deposited objects or humans to each other, to other fragments of the same object or person, to other objects and humans deposited nearby at the same time and to the people who dug the ditch segment where the deposition occurred. The principle of *synecdoche*, or *pars pro toto*, provides a dynamic logic for fragmentation, in that the parts which are broken and removed, to whatever place/time, remain linked to the other fragment. *Synecdoche* is a powerful expression of the effect of presencing, which brings or keeps absent fragments of objects or persons in relation with another part of the same object or person. The process of object or human bone curation differs from the other three notions in that it assures temporal continuity between an initial act of breakage and the deposition or removal of (some of) the resulting fragments. Curation gives a sense of planning in the operational chain of deposition. Our claim is that we have demonstrated the important role of all four notions in the proposed explanation of the Herxheim deposits. The Herxheim Team has produced compelling evidence of intra-site re-fits, especially of decorated sherds, while *synecdoche* and presencing are embodied in the many fragments of ceramic and lithic objects from a wide range of different sources, up to and beyond 200 km. Sherd curation is evident at Herxheim from wear traces, separate biographies ‘after the break’ and high frequencies of missing object or body parts, while body curation is fundamental to the alternative explanation.

The central, as yet unexplained, issue at Herxheim is the attribution of the body parts of an estimated 900 persons to upland ‘Home Communities’ even though no, or very few, sites are known from nearby uplands. The proposed bodily mobility model makes the assumption of an upland–lowland settlement network, with the curation of the newly-dead in upland and lowland ‘Home Communities’ until the season of the annual Herxheim festival, at which point parts of the newly-dead’s bodies were moved to Herxheim for further treatment and ultimate deposition. Four models of bodily mobility are presented to cope with data uncertainties surrounding the duration of the Herxheim deposition, the number of persons represented in the deposits and the number of upland and lowland sites contributing to that deposition. P. Demján and P. Pavúk’s new method for a statistical evaluation of the clustering of AMS dates shows a longer duration of > 50 years to be more likely than a shorter duration of < five years but the model allows a range of durations, from ten years to 75 years. While the number of people represented is taken, *pace* Turck, as 1000, the number of contributing sites varies up to a maximum value of 13 sites. There is an inverse relation between the deposition of body parts and the duration of the deposition, with outlying ranges of 14 body parts *per annum* over 75 years to 100 body parts *per annum* over ten years. All of these estimates are well within the range of possibilities for small-scale lowland-upland LBK networks.

We suggest that the choice of Herxheim for a regional, if not inter-regional, congregation place is related to two factors – the cumulative place-value which derived from the longest LBK occupa-

tion in the region prior to the construction of the enclosure and the new opportunities for exchange offered by the site once chosen. Repetition of the same routes to Herxheim would have led to a formalisation of the movement into ‘processions’, in turn formalising the ritual practices at Herxheim into a form of pilgrimage. The importance of seasonal or annual repetition built the special deposits through growing place-value and expanding cultural memory, creating a deeper attachment to the history of this place. Decreasing exchange and the creation of an alternative (difficult to identify in the declining settlement network) congregation centre could have played a role in the decline of the Herxheim centre.

While bodily mobility is widespread in European prehistory, this practice has not been frequently invoked in the LBK, with its current total of over 3000 known burials, often in small groups in settlements and cemeteries. The alternative Herxheim model which we propose is ultimately grounded on two bodily practices – bodily dividuality and bodily *synecdoche*. Herxheim shows the massive effects of these two initially simple practices when worked through in a consistent, cumulative, and concentrated manner.

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Herxheim unchained: a reply to Chapman et al.

By Andrea Zeeb-Lanz and Alexander Gramsch

Following the publication of three important books about Herxheim in recent years (BOULESTIN / COUPEY 2015; ZEEB-LANZ 2016; 2019a), the discussion of the function and interpretation of this enigmatic Early Neolithic site has significantly died down. It is therefore very welcome that John Chapman, Bisserka Gaydarska, and Tina Jakob have undertaken a new attempt at deciphering the complex scenario at Herxheim. Based on their previous research on fragmentation and enchainment as a social mechanism in prehistory and recent debates on mobility of both living and dead bodies, they take up the notion of the “division of dead bodies for transport” (*Chapman / Gaydarska / Jakob p. 4*) to create a sequence of actions resulting in the archaeological and anthropological findings from Herxheim that is challenging, stimulating, and good food for thought.

The new narrative can perhaps be boiled down as follows: people from contemporary – though still undiscovered – *Linearbandkeramik* (LBK) sites (so-called ‘Home Communities’) chose body parts of their deceased, which they had been curating up until the “festival season” at Herxheim, and then carried them to Herxheim. Along the way, they probably stopped at other yet undiscovered

ered ‘Home Communities’ to exchange or donate body parts. Evidently, more upland communities brought ancestral body parts to Herxheim than lowland communities (cf. *Tab. 1*); the latter took fewer body parts, but ceramic vessels to Herxheim. There, these different groups met for larger feasts and mixed and deposited their souvenirs to enchain their various communities. This mixture of “biosocial material” was deposited in a double ring ditch system, the parts of which were excavated by the incomers. As stated above, this narrative integrates a number of elements for which archaeological evidence is missing, but which help to create a coherent picture (cf. *Fig. 8*) and a complete storyline with actors, requisites, and settings. A film is created in our minds, the plot of which is unfamiliar but not completely unthinkable. However, the rich data from the research of Herxheim provide evidence for many actions which cannot be easily integrated into this drama, if at all. In the following, we want to summarise some of these elements – some of them missing, others misinterpreted – that make the picture far less coherent.

Let us start with the central act – the treatment of the dead bodies. The most important and markedly over-represented skeleton parts at Herxheim, the calottes, are mentioned (“Special treatment was afforded to cranial material, with the production of calottes [skull-caps] and – in a few limited cases – their deposition in groups”, *Chapman/Gaydarska/Jakob, p. 9*), but not discussed in detail, although they certainly played an important and special role in the body-transformative practices. The narrative does not propose any explanation for the fashioning of these outstanding “artefacts” but treats them like all the other human bone fragments, despite the fact that there is a wealth of anthropological literature on the particular significance of head and / or skull in practices related to the transformation of bodies (be they ancestral, or enemy, or other) (e.g. BONOGOFSKY 2011; ROSENDAHL/WIECZOREK 2011; cf. BARLEY 1981 and below). Moreover, cutmarks are understood as aiming for the dissection of the dead body in order to facilitate transport and enchainment. However, many cutmarks all over the body and on the skulls in particular cannot be explained this way (e.g. cuts on a child’s clavicle, s. GRAMSCH/GROSSKOPF 2023, 109 fig. 5). Fewer and simpler cuttings would have been sufficient to remove the skull from the postcranial part of the body, to detach the scalp from the skull, and to dissect the postcranial body. The complexity of body-related practices is not at all reflected in the new model.

There is both a kind of standardisation of body treatment and a high degree of variation in the way bodies are transformed, leading to this complexity of body-transforming practices. This includes the observation that many bodies were dissected to such a degree that, as the authors rightly state, Silja Bauer, in her analysis of the remains from eight depositions which were uncovered in the rescue excavation, was unable “to re-fit a single complete long bone from these slots” (*Chapman/Gaydarska/Jakob, p. 9*) (BAUER 2019, 11), while an – albeit restricted – number of other deceased are still distinguishable as individuals, preserved as torsos, or almost complete skeletons. While the authors express that “even the few examples of articulated bones in the ditches could have been brought to the site” (*Chapman/Gaydarska/Jakob, p. 15*), we think it is very unlikely that these articulated parts – such as whole spinal columns, long bones from legs, and arms with feet and hands still attached to them – could retain their original anatomical association during transport from the ‘Home Communities’ on the long way to Herxheim (*Fig. 9*). Returning to the calottes, a closer look shows that quite a number of them have one, or even both, temporal scales (a thin bone formed like a scale that is very fragile and linked to the skull with only minimal connection) preserved. The temporal scales would never survive a journey from a ‘Home Community’ still adherent to the skull when the calottes were prepared weeks before their arrival at Herxheim. Therefore, these skull caps must have been produced on site. In our opinion, this suggests that the individuals in fact died in Herxheim (*contra Chapman/Gaydarska/Jakob, p. 15f.*).



Fig. 9. Herxheim. Central part of concentration K 6 (rescue excavation) with long bones intact and an arm with part of the hand in original connection.

Another striking element in the transformation of (dead) bodies is the age distribution of the excavated individuals. Chapman et al. refer to the strong over-representation of juveniles and young adults and the lack of infants, which is highly untypical for normal death communities of the time, as already established by Bruno Boulestin (BOULESTIN / COUPEY 2015, 104–114; esp. 114). They suggest this distribution “might be caused by osteological and taphonomic factors rather than representing the ‘true’ demographic picture” (Chapman / Gaydarska / Jakob, p. 9). However, they do not cite a single example of taphonomic processes leading to a significant loss of bones. And indeed, there is neither evidence of gnawing, nor are there animal traces in the ditches. The lime-rich loess at Herxheim definitely prevented decomposition of the bones, and there is even so much lime in the soil that it covered the majority of the bones with thin layers of lime or even sometimes thick and solid sinter concretions. Additionally, what kind of “osteological factors” should exist that lead to the disappearance of the bones of elder people and young children? The striking overhang of juveniles and young adults is one of the elements missing in or even contradicting the narrative of Chapman et al., whereas we consider it a clear indication of intentional killing of members from communities of the living.

It seems to us, then, that the first act outlined in the new plot for Herxheim does not correspond to the body-transformative practices that can actually be reconstructed from the evidence. But what about the second act, which is the mixing of body parts and pottery to enchain upland and loess communities? Chapman et al. selected inner ditch slot 282-139 as an example where allegedly “the placement of one fragment of a person’s body in a long pit with sherds from the Elster-Saale region linked the person to the vessel” (Chapman / Gaydarska / Jakob, p. 13). However, this slot comprises a total of 42 fragments of human bone and two calottes commingled with 74 sherds (18 decorated and 56 undecorated, none of which shows Elster-Saale-decoration). Again, the evidence suggests a far more complex series of actions that led to the archaeological record, rather than a specific enchainment as recognised by Chapman et al. Directly in the middle of a pottery sherd concentration in this slot we identified two sherds with early-LBK Flomborn decoration and two further ones dating from the middle to younger LBK. Fabian Haack figured correctly that the older sherds, which were found in various concentrations or singly in the backfill, were mixed into the concentra-

tions along with the soil in which the finds had been interspersed before deposition, as this was the original filling of the ditches which sometimes cut older pits (e. g. HAACK 2016b, 83; 94; 113). But if all the material in the concentrations stems from deliberate enchainment actions, it becomes very difficult to explain the chronologically much older pottery in quite a number of the concentrations; Chapman et al. conveniently omit these facts.

Their second example, the enchainment of “another bone fragment placed in another long pit with sherds from the Rhein-Main region” in slot 282-12 of the inner ditch (*Chapman/Gaydarska/Jakob, p. 13f.*) is not convincing either: The excavation database shows that in the vicinity of the sherds present that have Rhine-Main-hatching style decoration – which are not lying together – there is a huge number of various bone fragments, mandibulae, and more than 100 pottery sherds and stone implement fragments. There is even an intact skeleton in the classical flexed position in this slot, lying on the same level as quite a number of bone fragments and a huge variety of sherds with different decoration styles. It seems unlikely to us that so many dead individuals and so many ceramic vessels could have been enchainment in a single act, let alone the specific enchainment of a Rhine-Main vessel with an upland individual. Moreover, it remains open how the intact skeleton fits in the enchainment narrative. Again, by omission, Chapman et al. avoid evidence that is very difficult to work into their plot.

This example leads us to the matter of scale: Some “biosocial material clusters” are small and consist of fragments of only a few vessels and of body parts from 1–2 individuals, but the majority of the clusters comprised a huge amount of pottery and human bones mixed with soil, animal bones, stone tool fragments, and bone tools. For example, Concentration K 16 of the research excavation includes, in addition to animal bones in larger quantities, 4000 human bone fragments (including 13 skull caps), about 500 pottery sherds, and a smaller number of grinding stone and stone adze fragments (*Fig. 10*). It is quite impossible to imagine how many different ‘Home Communities’ must have been involved in order to create this set of enchainment landscape, pottery, and human individuals. Moreover, this and similar clusters are obviously not the result of a single depositional act: the commingled mass of soil, bone fragments, pottery sherds, and other artefacts shows that no clear-cut, closed “enchainment sets” were deposited in the ditches, but rather they were the result of a series of actions in which bones, sherds etc. were scattered, reassembled, mixed with soil, and spread into open sections of the ditches.

It has to be noted that a huge number of single sherds and bones were found in the backfill layers above and below the main concentrations of “biosocial” material. These numerous, but often individually lying sherds and bone fragments do not fit into the enchainment model either – or only if we assume that the enchainment consisted merely of a selection of smashed human bones and fragmented vessels – and the rest was simply discarded with the backfill material. The huge amount of material that eludes any kind of enchainment is ignored in the narrative by Chapman et al.

All in all, there are around 80 000 human bone fragments (including around 500 calottes), ca. 25 000 vessel units (many consisting of more than ten sherds), several thousand pieces of stone implements, hundreds of bone tools and ornaments, as well as a lot of animal bones. Imagine the number of people – several thousand probably – necessary to achieve the goal of enchainment all these objects with each other! The composition of the concentrations and the backfill layers do not suggest a repeated use of one and the same spot for deposition in subsequent years or at subsequent occasions – and the many pottery re-fits between freshly broken sherds lying in neighbouring concentrations suggest longer, simultaneously open ditch sections.

Aside from remains from butchered domestic and some wild animals, the concentrations in the ditch fillings also comprised animal remains such as the wing bones from a large wading bird, a

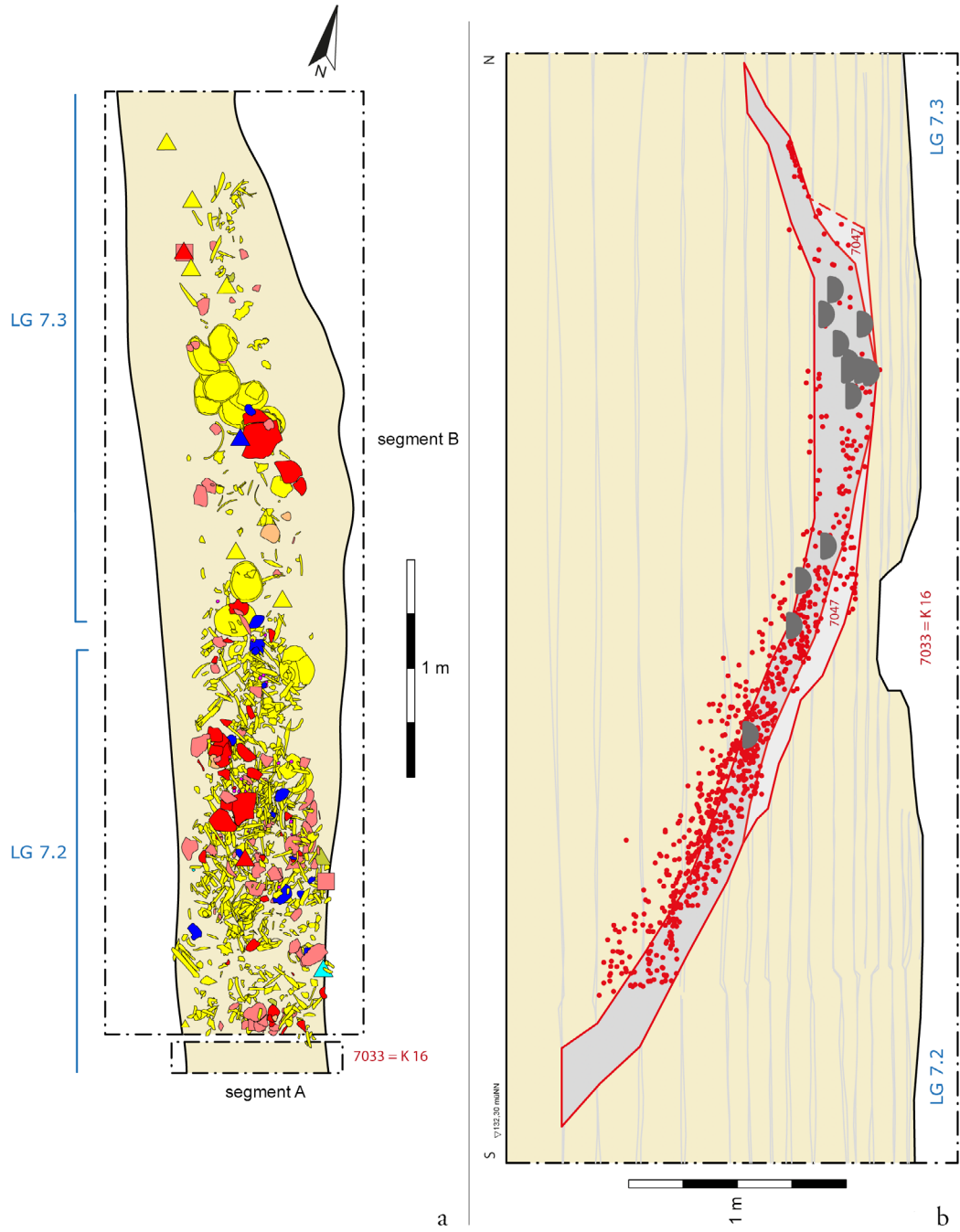


Fig. 10. Herxheim. Inner ditch ring, finds concentration K 16. a Planum 5 with finds horizons 5–19. b Longitudinal profile with finds horizons 5–19.

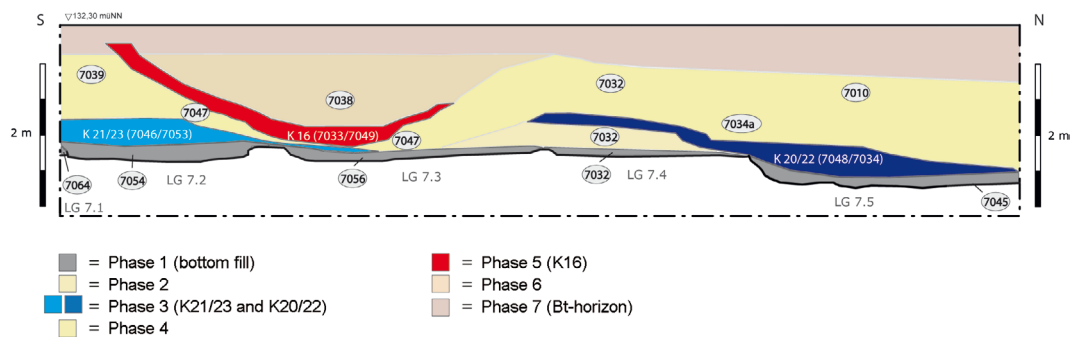


Fig. 11. Herxheim. Longitudinal profile from the east with schematic reconstruction of the backfilling phases in the area of the concentrations K 21/23, K 16 and K 20/22.

number of bucrania and aigicrania, as well as around 40 mandibles (mostly cut in half and sprinkled with ochre) from small carnivores like marten, polecat, or wild cat. These special and elaborately treated animal bones add to the complexity of the practices resulting in the deposition of “biosocial” clusters as well as to the symbolic dimensions of the site, neither of which is accounted for by Chapman et al.

And what about the final act outlined in this plot, the digging of pit sections for depositing mixed “biosocial material”? Chapman et al. sketch a picture where each ‘Home Community’ digs a special pit for their enchainment material and speak of “the choice of a specific long pit for deposition” (*Chapman/Gaydarska/Jakob, p. 13*). This directly contradicts the results of the analysis of the filling history of the ditches of Herxheim and the placement of the find concentrations. The ditch system has undergone a thorough, meticulous examination based on the enormous data set resulting from both the research excavation and the previous rescue excavation (HAACK 2016b). On the basis of his detailed and conclusive research on the double ditch, Fabian Haack concludes that although a long pit is the primary architectural unit for both ditches, several long pits were always open at the same time and formed long, open ditch segments. This is proven by the fact that the concentrations and other backfill layers can usually be traced over the length of several of the long pits (*Fig. 11*), the latter being recognisable only via the steps in the ditch bed, as evidenced by the large number of existent long profiles. This fact, cemented by the meticulous presentation of plans and profiles in Haack’s dissertation, much of which was published in the first Herxheim volume (HAACK 2016b), vitiates the vision of specific areas, namely distinct long pits, into which each group of uplanders deposits the enchained bones and sherds of their specific bonds with selected lowland communities.

We thus conclude that the plot sketched by Chapman et al. excludes much of the evidence, reduces the complexity of the practices, and cannot account for what really led to the various transformations and mixing of human and animal bodies, pottery, and other material (*Fig. 12*). Additionally, it seems to us that the plot itself is contradictory; it lacks an explanation for why upland communities sent human remains but no artefacts (or at least evidence is lacking for upland produced artefacts), while loess communities sent pottery but only very few body parts (a small amount of bones comes from other lowland regions or even from Herxheim; see *comment by Turck and TURCK 2019, 364, 370–375 with fig. 40*). It seems unlikely that communities would never pick up practices of such a high symbolic value which were practised by other enchained communities. Furthermore, there is a lack of evidence for a Herxheim-based network that existed before the use of body parts and that involved upland communities in large-scale trans-regional feasting.



Fig. 12. Herxheim. Detail of the huge concentration K 9/18 with lots of highly fragmented human bones, calottes, and other finds.

Moreover, Chapman et al. rightly point at the social efficacy of practices that transform and transfer bodies, but if we give these aspects a different weight, we may understand the practices performed at Herxheim (and possibly elsewhere before) as aiming for the dissolution of (most, but not all) individuals – no matter where they previously lived. Similar to secondary burial practices as described by Nigel Barley for the Dowayo of Cameroon, the disintegration of individual bodies, the special treatment of skulls, and their final deposition together with other skulls and other singularised body parts “have a common theme – the change from differentiation to undifferentiation” (BARLEY 1981, 157), from individual to community. The manifold practices of manipulation of the human body visible at Herxheim (whole skeletons in regular burial posture, torsi of rump and partly treated skull, extremities with hands and feet, spine column parts, whole skulls, skull caps...) that transform the bodies into new entities or dissolve them completely can indeed be understood as a means to create community or “social bonding”, e.g. in a scenario of “extreme processing” (e.g. OSTERHOLTZ 2018) – but the question is: What are the groups that bond here? Like Chapman et al., we think that the actors at Herxheim (or perhaps at other places involved prior to the depositions in the double ditch) are groups from various regions of the LBK world, visible through the different regional pottery decoration styles. These groups have ancient ties through clan-bonds or old lineages. However, the nature of such ties and such bonding practices is much less peaceful, and social relations are far from being as harmonious and conflict-free as the new narrative suggests. Whoever engaged in the practices discussed here, it is unlikely that all social and / or regional groups at any time had the same communal goal, namely the creation and maintenance of enchainment. Feasting very often is not just integrative, but has a competitive nature (e.g. DIETLER 2011, 180). Feasting aims not only to create or maintain social bonds, but also to negotiate, challenge, and change positions of individuals and groups; it is a central mechanism to negotiate or establish

emerging hierarchies, especially in societies where these are rather weak, or which undergo fundamental social change (BENZ/GRAMSCH 2006, 425 f.). We think that this also applies to late LBK society, as it is both in transition and low in hierarchy; however, enchainment here is described not only as free of conflict, but as a very technical, sober, matter-of-fact, processual procedure – without regard to the probably involved emotions such as grief or irritation, or to ambitions such as wanting to outplay other feasting participants.

We appreciate the attempt of Chapman et al. to revive the discussion about Herxheim and to merge at least some of the evidence into a new narrative. We agree with them that it “is difficult to summarise the Herxheim findings without oversimplifying what is clearly an enormously complex sequence of operations” (*Chapman/Gaydarska/Jakob*, p. 12). One certainly positive implication of the new narrative is the demonstration that peri- or post-mortal treatment of human bodies and their disintegration does not necessarily need to be considered negative, derogatory, or punitive (cf. GRAMSCH/GROSSKOPF 2023, 107) – one might even go further and state that the killing of individuals can be positively connoted, such as in the instance of self-sacrifice. We support their relational approach which considers persons as “both individuals with specific identities restricted to themselves ... but also individuals, whose relations with all the other persons, places and objects to whom they were linked contributed to their identities” (*Chapman/Gaydarska/Jakob*, p. 14). However, more attention needs to be paid to the question of whether and how body-transformative practices are related to personal identities (GRAMSCH/GROSSKOPF 2023). We suggest that the active disintegration of bodies as well as of lavishly produced pottery visible in Herxheim is part of a physical, as well as social, process of dissolution of individuals and of social transformation in a society in transition. We also welcome the strong focus on practices, as Herxheim provides a lot of evidence for these. However, the new model integrates the evidence only where it blends well into the narrative. In their plot, every action at the site is meaningful and fits together with the other actions – but there are many more practices involved here than are taken into account and many elements such as the sheer amount of material involved, as well as the unusual age distribution of the dead, which contradict the narrative of John Chapman, Bisserka Gaydarska, and Tina Jakob.

An archaeometrical perspective on “New perspectives on deliberate fragmentation and bodily mobility” by John Chapman, Bisserka Gaydarska and Tina Jakob

By Rouven Turck

In their discussion paper, John Chapman, Bisserka Gaydarska and Tina Jakob present a new perspective on the treatment of human remains at Herxheim at the end of the *Linearbandkeramik* (LBK). How these human remains came to the late LBK settlement and what happened to them prior to their deposition in the double ring ditch is still a matter of debate. Based on their own numerous studies on fragmentation and enchainment as a means of establishing social interregional ties, the authors look for evidence that allows the fragmented dead from Herxheim to appear in a secondary burial context as already discussed by Jörg ORSCHIEDT and Miriam Noël HAIDLE (2006).

A central argument of the authors is the regional origin of the dead. By means of strontium and oxygen isotope analyses, very high proportions of the individuals were identified as of nonlocal origin (TURCK 2019). These individuals mostly originate from geological formations that clearly deviate from the settlement landscapes on loess postulated so far for the LBK. The assumption that

nonlocal individuals lived at sites not located in loess regions, died, and then were posthumously transported to Herxheim after a primary burial is at first compelling.

From an archaeometric perspective, the following remarks should be made:

1. In principle, quite a few studies have been done for the LBK in which a high number of individuals from other sites can be detected to originate from areas other than loess regions (e.g. the contributions in BICKLE/WHITTLE 2013). These same studies have also demonstrated that individuals from non-loess geological regions were buried in regular LBK cemeteries (BICKLE/WHITTLE 2013) or even in settlement contexts (NEHLICH et al. 2009).
2. The data obtained essentially from the first molars of the individuals from Herxheim clearly demonstrate that most individuals did not grow up on loess (TURCK 2019, figs 39–40). However, these data do not reflect the last months, or even years, of life of the juvenile, adult, or older individuals. Thus, it is left completely open where these older individuals had lived during the period before their deaths. That they appear “foreign” refers to their origin, i.e. the place of their birth and childhood, and not to the residence prior to death. For a detailed description of tooth mineralization and supplementary methodology, see TURCK 2019, 439–445.
3. The statement by Chapman et al. concerning the carbon and nitrogen isotopes of the individuals from Herxheim is not correct; the analyses reflect a terrestrial Neolithic diet that does not differ from other LBK sites. On the contrary, the range of the data is small (*Fig. 13*). With respect to nutrition strategies, I concluded that, compared to other studies, “there is no indication that the Herxheim individuals or their diet were in any way remarkable” (TURCK 2019, 388). Moreover, the study by HÜJİÇ (2009) should be considered in which the morphological tooth examination resulted in the detection of unusual wear traces.

Following Chapman et al.’s assumption that the individuals may never have lived on loess in Herxheim but were brought there as the result of secondary burials from upland LBK settlements, which the authors refer to as ‘Home Communities’, the consideration of the analyses of children, especially *infans I* and neonates, is helpful: their life span was short, and their isotopic values indeed reflect their place of living during the first and last months of life, during which they took in isotopes from the geological bedrock.

It is clear from *Figure 14* that all four analysed children (*infans I*) belong to ‘local individuals’ which lived on loess, i.e. they clearly spent their short lives in classic LBK settlements. It is informative that the three neonates have slightly elevated strontium values of about 0.71–0.715. These data represent the stay of their mothers who had actually not spent their entire lives on loess but had lived at sites on more crystalline bedrock. However, since these values do not prove to be particularly high compared to the other non-local individuals at Herxheim, it could be possible that the values around 0.71 represent a “mixed value” of crystalline and loess flatland, which could thus indicate migration of the mothers to lowlands during their lifetime. This hypothesis cannot be carried out further by the available data, but we can conclude that at least some of the individuals’ remains which were deposited in the Herxheim ditches – the children with their mothers and perhaps even further relatives – had lived there for several months or years prior to their death, dismemberment, and deposition.

These analyses based on only seven young individuals (three *neonates* and four *infans I*) are certainly not statistically significant. However, the fact that all individuals of the age group *infans I* lived on loess and that the neonates did not belong to the group of highly crystalline individuals is striking: these individuals were clearly part of the ritual and were definitely not brought from upland sites; they did not live off-site or at least not in regions unusual for the LBK; and they

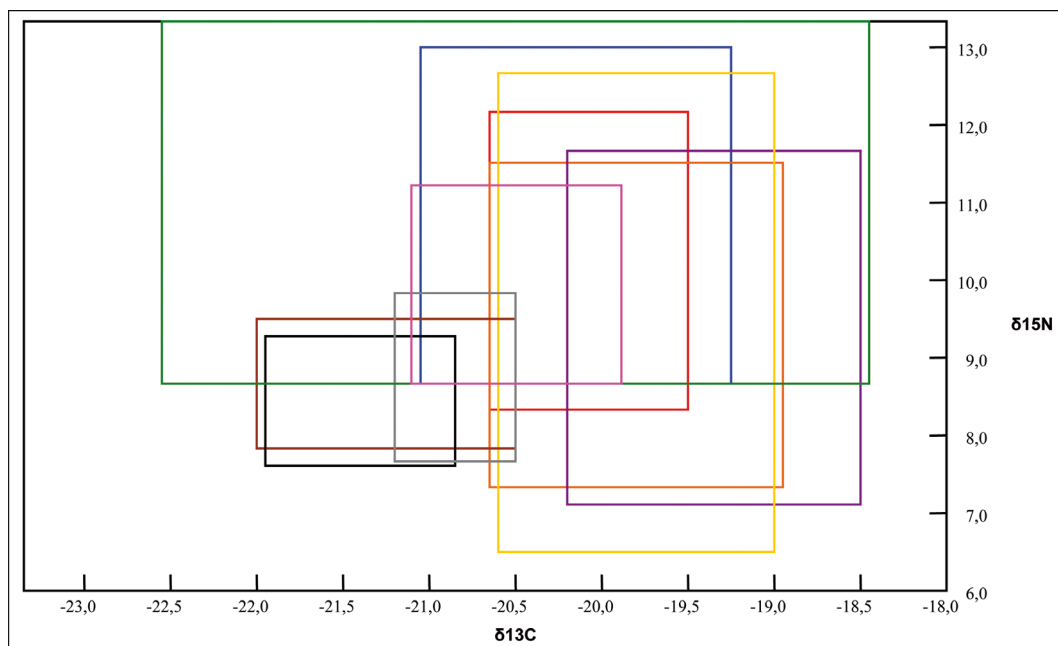


Fig. 13. LBK nutrition: Carbon and nitrogen isotope values from *Linearbandkeramik* sites. Red = Herxheim; blue = Nieder-Mörlen; orange = Derenburg; purple = Halberstadt; yellow = Karsdorf; brown = Essenbach; black = Sengkofen; grey = Dillingen; pink = Aiterhofen; green = Vedrovice.

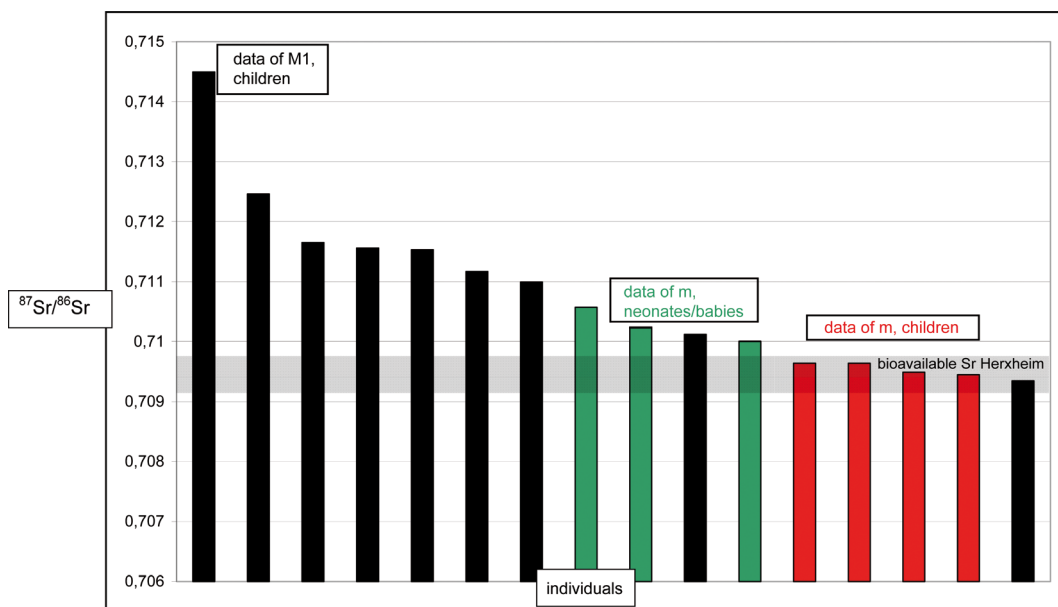


Fig. 14. Differentiation of sampled children by age groups. Green = deciduous teeth (m) of *neonatus*; red = deciduous teeth (m) of *infans* I; black = permanent molars (M1) of *infans* II.

could not have been members of upland ‘Home Communities’. Since it can be assumed that these children grew up in social associations with relatives, it cannot be excluded that migrated adult individuals, who appear to be non-locals in the analysis of first molars, had lived for several months or even years in Herxheim and had founded families there.

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 (BÁNFFY 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.

The idea that the remains of already deceased individuals were brought to Herxheim is certainly interesting and creative, as the sheer number of people buried at the site has always been an interpretative problem. Post-mortem mobility has also been raised in quite a different context by ENSOR (2021, 17–30), who suggests that the dead were returned to their natal communities to be buried with their respective birth paternal (or maternal) lineage group, thereby critiquing the widespread interpretation of patrilocality offered on the basis of genetic and isotopic studies of cemeteries. If something like this was the case here, the many diverse birth locations represented in the isotopic values would suggest a spatially dispersed lineage, much more so than on contemporary cemetery sites. This is interesting, and could for example be investigated further with additional aDNA work aimed at revealing biological relatedness. In contrast, Chapman/Gaydarska/Jakob here put forward a model whereby different settlement communities bring “their” dead to Herxheim, in other words it is still co-residence-based units that would be relevant for defining identities. Still, it is argued that these units are now more dispersed than before, and require a central place. Again, this is a suggestion worthy of further investigation. Are there always central ritual sites when LBK communities expand? How does a central place like Herxheim compare with the proposed central sites of the earliest LBK phase, such as Bad Nauheim-Nieder-Mörlen (e.g. SCHADE-LINDIG 2002; SCHADE-LINDIG/SCHWITALLA 2003; NEHLICH et al. 2009)? Or do we now, at the end of the LBK, see the first intimations of a pattern that was to become more common later in the Neolithic, with rondel enclosures or with the causewayed monuments for instance of the Michelsberg culture, which can be read as fixed centres around which a more mobile community moves?

Clearly, this opens up avenues of additional research. What is more, this new model for Herxheim contributes to the overdue de-sedentarisation of the European Neolithic. Not every site needs to be settled long-term by all of its residents, and not all kinds of mobility are due to crisis. With a view to the Alpine foreland, one can suggest many other models in which a resilient system of landscape exploitation can be maintained through mobility at some social scales (e.g. EBERSBACH et al. 2017 for a summary). The question is when such systems were developed, and perhaps the transition at the end of the LBK is one likely point.

One key issue, as Chapman/Gaydarska/Jakob rightly stress, is dating – still very imperfectly understood for the latest LBK. Relaxing the constraints of the model proposed in the Herxheim site monograph, as has been attempted here, opens the possibility that Herxheim could have been “fed” with the normally occurring dead. We would not require long-distance raids abducting hundreds of people whose ‘Home Communities’ were somehow unable to defend themselves, nor mass sacrifices. This is one of the key contributions of this article, and the different possibilities proposed should be taken seriously and tested further. In particular, we now also need more research in the uplands to try and identify when they were first visited by LBK communities, and how intensively they were ever settled. Building on the work of Sandra FETSCH (2012), situating Herxheim in its wider regional context remains a priority.

This leaves us with the problem of the extraordinary violence, possibly including cannibalism for some individuals, meted out on the human remains, which has always been stressed as unique to Herxheim. Given the rivalling conclusions of the various anthropological studies, there may be more variation here than is generally acknowledged, and there may be room for more than one scenario. All this opens once again the question of LBK personhood, in how far fragmentation may have been a part of the post-mortem biography of (most?) people and objects. Was the character of persons and objects changed by fragmentation, or did fragments remain fundamentally tied to previous identities? Did it matter how extensively and even violently fragmentation was carried out, can we distinguish care of kin from violence against enemies, or is this hopeless from our modern-

day perspective? These are questions of a largely philosophical nature, but in as far as they open new readings, and help to integrate Herxheim with what is known from other mortuary practices, they are worth pursuing.

So far, then, I am in agreement that our perspectives on Herxheim can fruitfully be widened, and that this helps us address broader questions about life and death in the LBK. But what about the idea that the remains of specific pots and specific people were deliberately combined to create a specific alliance? Here I must confess myself a sceptic. The sheer quantity of remains, increased if one takes into account the ditch fills and the enclosure interior lost to erosion, would make it a virtually impossible task to keep track of each fragment, especially if there was a longer timescale to the site. The level of administrative work for the group of Guardians (thought to be small) would be immense, indeed it would tax the capabilities of most modern-day administrators, even in Germany. In addition, most remains (other than calottes and a few bundles of bone tools, for example, see HAACK 2020) are not carefully combined with anything, they have quite evidently been tipped into ditch segments in one fell swoop. This creates a general link between people, objects, and the places they came from, but it seems to suggest rather an effacement of the specifics, of the individual person and the individual piece. Fragmentation – in the form of theatrically charged breakage – is a powerful agent here, but one of transformation, in which old identities and associations are dissolved (see also HOFMANN 2020b).

The depositional model proposed by A. ZEEB-LANZ (2019b) thus seems to provide the more convincing explanation. If, as Chapman / Gaydarska / Jakob suggest, Herxheim was in use for longer, and there were more gatherings, then above-ground deposition of parts of the material (at least for a time) can explain the different post-depositional histories of re-fitting fragments, while the incorporation of material into the ditches could have been periodic, and not necessarily marked with great formality, explaining the appearance of the spreads or “concentrations”. Moreover, we do not yet have any counterparts for Herxheim – for example the sites hundreds of kilometres away where many sherds of the Palatinate style would turn up – and that could support the proposal of sherd exchange made by the authors here. This does not detract from the fact that associations may have been created, cemented or challenged at Herxheim, and the “ritual midden” substance created by fragmentation and admixture may not have been thought of as powerfully charged. But this does not rely on the small-scale management and tracking of every single piece.

Indeed, while the elaboration of a quite detailed alternative model is thought-provoking, it also perhaps tries to challenge too much at once, turning everything on its head: from ritual rubbish we go to careful management of each single fragment, from a large population to a small group of Guardians, from victims to ancestors, from sedentism to mobility, and from a short-term event to recurring pilgrimages. In the pursuit of this general reckoning, some nuances are also lost. For example, by *Chapman / Gaydarska / Jakob* p. 15–16 it reads as though the term ‘Home Communities’ relates to both people in the uplands relatively near Herxheim and those several hundreds of kilometres away in Czechia or Bavaria, which may unhelpfully blur different sorts of relations. Also, it is far from “clear” (as stated on p. 21 by *Chapman / Gaydarska / Jakob*) that all the remains deposited at Herxheim were somehow “ancestral”, a term that suggests specific genealogical ideas and would require further investigation and comparison with other sites, in particular given the skewed demographic profile. It is not explained how the Guardians, few in number, could pull in quite so many resources and maintain an unparalleled long-distance network when they had no economic resources of their own, nor why they would go homicidal once this no longer worked. The supposed correlation between declining frequency of imports and declining density of deposition could, however, be tested statistically. In sum, many elements of this detailed scenario are per-

haps deliberately provocative. After all, the data as they stand are not clear-cut enough to absolutely support any single position: they remain blurry at the edges. This thoughtful article thus challenges researchers to look at the material afresh, rather than providing the final word. I hope that the challenge will be taken up.

The problem, if we want to call it that, is that Herxheim is so unique. In spite of recognisable elements, no other enclosure is like it, few sites can begin to rival the level of imports, and no depositional event reaches this level of complexity. No other site has therefore caused a similar level of controversy. The number of people interred remains difficult for any interpretation, as does the quantity and quality of material, and the fact that in spite of the incredible amount of work put down by a large team only parts of it could be studied and published. The severe erosion that has impacted this site, like so many others, will also always be a limitation for the understanding of depositional processes.

Chapman / Gaydarska / Jakob have also grappled with this uniqueness and these limitations, and like others before them have picked on different aspects from which to attack the problem, and reached conclusions written from this necessarily partial perspective. As they themselves state, any narrative of Herxheim will be a simplification. Their main success is the gauntlet they throw down for future research. From their model, even the more provocative parts of it, one can generate propositions for further research that can then inform on LBK social dynamics more broadly. For example, previous isotopic and aDNA research has tended to privilege whole interments – but do fragmented remains everywhere have a greater variability of origins? Do they require their own sampling, recording and interpretative strategies? Is there room for more statistical interpretation of recurrent depositional associations, at Herxheim and beyond? Who was in the uplands when? Even more crucially, the authors have provided us with a lively account that sees LBK people as political agents, rather than standard entities pushed around by climatic and demographic events beyond their control.

Mine is therefore a middle of the road position – there is much here that I found inspiring and important for future research, notably concerning various aspects of mobility, and much that I found less convincing, in particular regarding the micro-management of depositional processes. In this case, the middle of the road, half-way between slave-raiding blood-thirsty raiders and homicidal ritual Guardians angry at falling import levels, may be a dangerous place to be. Yet we are some way from a resolution, and will never be able to understand Herxheim completely. Undoubtedly, the excavation and study of this site have lastingly expanded our horizons of what it is possible to think and write about the LBK. Chapman / Gaydarska / Jakob take us one step further along the road, but there is still some way to go.

After the break? Against parts and wholes

By Stefan Schreiber

John Chapman's, Bissierka Gaydarska's, and Tina Jakob's article makes us pause and reflect on many levels. I agree with the authors on a lot of aspects. However, I am happy to contribute a few more thoughts from a New Materialist point of view. As I am not an expert in the archaeology of *Linearbandkeramik* (LBK) myself, I would like to focus on the theoretical part, even if it takes up a rather small part of the article.

The fragmentation and incompleteness of archaeological finds represent a commonplace. While much of the archaeological debate circles around taphonomic processes of relocation and deposition, it is surprising that past fragmentation practices receive little theoretical attention. Although there are a large number of individual studies on artefact categories or find sites, for example, on the intentional destruction of weapons in burials, there are hardly any coherent theoretical discussions. A welcome exception since the end of the 1990s have been the numerous works by J. Chapman and B. Gaydarska on enchainment and fragmentation (cf. CHAPMAN 1996; 2000; CHAPMAN / GAYDARSKA 2007; GAYDARSKA / CHAPMAN 2009). Together with T. Jakob, they now bring these gradually more elaborated approaches into connection with one of the most interesting *Linearbandkeramik* sites – Herxheim in Rhineland-Palatinate.

At the outset, the authors provide an exemplary and reflective account of the genesis of their fragmentation theory. From the observation of re-fitting and enchainment, they arrive at the well-known ‘Fragmentation Premise’. This is extended to include the fragmentation of persons and places alongside the fragmentation of objects. Ultimately, they combine these aspects into a so-called identity triangle (*Fig. 1*). Their extended premise at this stage of the research can be summarised as: “Places, human bodies and objects were regularly deliberately fragmented and the resulting fragments were often re-used in an extended use-life ‘after the break’” (*Chapman / Gaydarska / Jakob, p. 1*).

First of all, I would like to emphasise that the fragmentation theory has one positive feature that can hardly be topped – it has been obtained empirically. Starting with the inductive archaeological observation of the re-fitting of objects, the subsequent study of fragmentation practices, the expansion to include persons and places, and finally the development of an (alternative) historical interpretation of the Herxheim site including a very good (and very German-style?) source criticism, all the registers of scientific work are exercised here in an exemplary manner. In contrast to the majority of other theories, one can justifiably speak here of an *Empiriegeladenheit der Theorie* (“empirical loading of the theory” (HIRSCHAUER 2008)). As a ready-to-hand theory, fragmentation theory can be quickly checked for plausibility with and integrated into archaeological-empirical research because it is itself based on archaeological-empirical research. In the best sense, it mediates as a middle-range theory between empirical phenomena and grand theories (MERTON 1968), and at the same time, fragmentation theory is highly connectable in a communicative sense. This is well illustrated by the chosen example of Herxheim.

In my opinion, however, this is also its greatest disadvantage. Archaeological research – so it would seem – does not need translation into theoretical terms. Most of the concepts used (objects, human bodies, bones, places, identity, fragmentation, operational chains) suggest a common sense that could come from any excavation report or archaeological evaluation (only ‘enchainment’ and ‘itineraries’ need more preconditions). In fact, however, they are not concepts, but merely deductive *ad hoc*-theories. Yet it is precisely these concepts that are being operated with which should be questioned as preconditions, because it is here that the origin and benefit of the approach become visible.

First and foremost, this concerns the concept of the object itself. As an *ad hoc*-theory, it marks the anchoring in Cartesian thought; thus, ‘after the break’ does not include the rupture of the object-subject dichotomy. Instead, it is exclusively the object that is broken, fragmented, and subjugated. In an instrumental relationship, the human determines both when the object breaks and how continued ‘use’ cements the continuity of the object ‘as object’. At the same time, the concept of objects determine what parts and wholes are. The extension to include human bodies – actually, almost exclusively human bones are meant here (in the triangle diagram, significantly, not marked

as bodies, but as persons) – also follows this logic. These can also be fragmented, as they have a ‘use-life’ after the break. Interestingly, the fragmentation of place follows a different logic. Places are fragmented from territorial units into networks, itineraries, and routes. But here, too, a logic of parts and wholes is applied.

At the same time, objects, human bodies, and places are described as stable, essentialist phenomena. They can be broken and thus fragmented, but at the same time the very concept of the fragment always refers to the whole. The rupture is thus transformed from an anthropocentric practice to the ontological dualism of parts and wholes, which, following Manuel DeLanda’s assemblage theory (DELANDA 2006; cf. JERVIS 2019), is referred to as thinking in totalities. While the focus is on fragmentation, it has the status of a rupture, an intervention in totality (whether it is an object, a body, or a place). Thus, fragmentation remains the *explanans*, while wholeness does not appear to need explanation.

It is surprising that despite the intense debate on the ‘flow of material’ (cf. INGOLD 2011) over the last 15 years, it has not received any attention here. It is precisely the constant transformation and mutability of the material that becomes the agenda in New Materialism (cf. HARRIS 2014; WITMORE 2014; GOVIER/STEEL 2021). Here, ruptures would be a kind of rupture of relations, not of material. In 2010, Marcus Brittain and Oliver J. T. Harris attempted to initiate a debate on how to think about fragmentation and enchainment in a consistently relational and New Materialist way (BRITTAİN/HARRIS 2010). Unfortunately, this attempt has so far been met with little response, although important points of criticism were made here following Marilyn Strathern (which I do not wish to repeat here in detail; for the details see BRITTAİN/HARRIS 2010). In my view, it would have been desirable to link fragmentation theory more closely to this debate. It is precisely the mutability, travellings, distributedness, rhizomatic, and relational transformations of the material (cf. JOYCE/GILLESPIE 2015; STEEL 2022) that could give fragmentation theory a perspective that goes beyond the narrow anthropocentric focus. Ultimately, I experimented with fragmentation theory myself in my dissertation, but then abandoned it in favour of M. DeLanda’s assemblage theory because of its limitations (cf. SCHREIBER 2018).

What is already true for the object becomes even more obvious with the inclusion of human bodies. The reference in M. BRITTAİN and O. J. T. HARRIS 2010 to modes of relational personhood (FOWLER 2004; 2008) already points to a long debate that cannot be reduced to the distinction between individuals and dividuals in Marilyn Strathern’s *Gender of the Gift* (STRATHERN 1988). Studies on distributed bodies and trans-corporeal entanglements (ALAIMO 2010; FREDEN-GREN 2021; SCHREIBER/ROTERMUND 2023) show how complexly the enacting of human and non-human bodies mesh (not to mention fragmented, distributed practices). Moreover, it is not only human bodies that are fragmented and entangled. Subjects can also be distributed, transindividual, and porous (SMITH 2012; ANDERMANN 2021). What Chris Fowler calls personhood is thus itself already an assemblage of different modes of corporeal and subjective enactment and existence. These can only be understood in a very simplified way as ‘human bones’ and their symbolic meaning. For an interpretation, as the authors suggest for the Herxheim site, the formula as “person = fragmented and reassembled bones + identity” (I am of course exaggerating here) falls considerably short. Ultimately, both the distinctions of body/person and object/body cement a basic anthropocentric attitude without making it explicit.

The fragmentation and enchainment of places also follow this logic, albeit in a slightly different way. The addition of places or landscapes to the fragmentation theory does not mean that they too are fragmented and enchained. Instead, according to the authors, they are the enchainment itself: “The fragmentation of place is therefore the origin-metaphor for the general process of relating in

the world – viz., enchainment” (*Chapman/Gaydarska/Jakob, p. 2*). It is true that a relational approach shines through here. But this remains, first and foremost, limited to the relations of people to objects. These can be located and linked as ‘metadata’, so to speak. However, they are not granted any logic of their own. Thus, the addition of places ultimately remains trapped in a transportation logic, as criticised by Tim INGOLD (2011, 149–150).

With these three aspects of the triangle, the authors offer an alternative interpretation following the presentation of previous research results on Herxheim. Despite my criticisms above, this can be considered a very successful interpretation. I found myself smiling again and again while reading it and putting positive exclamation marks on the text. Even if I do not share the preconditions myself, the interpretations are comprehensible and plausible. It shows what clout the fragmentation theory, or perhaps better enchainment theory, has for interpreting archaeological contexts. Only the tendency towards the ‘obsession with wholeness’, which can often be observed among archaeologists, seems to me to be a little too predominant. Why do places, objects, and people in the past have to be fragmented only to be re-unified? The *synecdoche* as a type of interpretation suggests a *pars pro toto* wholeness, but what if the fragments do not refer to a whole, but rather enact precisely that incompleteness, imperfection, and distributedness that already exists in a non-representational way?

Finally, I would like to put forward a thesis: Fragmentation theory is too connectable. At the outset, I stated that fragmentation theory is a middle-range theory. This is true both because of the theory’s design and its scale. It remains easy to understand without getting lost in too abstract explanations and assumptions. Unfortunately, at no point does it become clear between which practices and grand theories it actually wants to mediate. Here, I have tried to read it from the perspective of New Materialism and posthumanism. This was only partially successful, as it is too strongly rooted in Cartesian concepts. At the same time, however, it uses relational practices and can therefore be understood as a further archaeological advancement of practice theories. Moreover it is not based on an explicit theory of practice. It is preceded by ‘fragmentation as a practice’ of the ‘fragmentedness’ of the world. Further developments such as the distributed nature of practice and agency are not integrated (cf. SCHREIBER 2022; VELING 2022). It also operates with a strong premise reminiscent of the hypotheses or so-called Mickey Mouse Laws of Processual Archaeology. This is quite positive, both for the alternative perspectives to Herxheim and for integration into quantitative as well as qualitative research. Ultimately, fragmentation theory is welcome everywhere, but at home nowhere. It is itself fragmentary and enchainment. But it does not work as a middle-range theory. Rather, it serves as a bridge between paradigms. In this way, however, it primarily fulfils communicative rather than analytical tasks. However, this cannot be valued highly enough. In the end, it is not only separating theories that are needed, but also connecting ones. Even ‘after the break’.

Fragmenting and moving the parts: a reply

By John Chapman, Bisserka Gaydarska and Tina Jakob

At the outset, we should like to thank the *Germania* editorial team for agreeing to a feature on the Herxheim site and also thank the six colleagues who took the time to read and comment on our article. Since we wrote the article for *Germania*, two of us (B. Gaydarska, J. Chapman) have had the opportunity to visit Herxheim – both site and museum – in the company of Andrea Zeeb-Lanz, who continued to voice the established views of the Herxheim Team. It is these views, together with other external perspectives, that have been summarised here in the form of short critiques of our article.

It is hard to believe that nine archaeologists in the same room can agree on most things about a complex and controversial site but it seems to us that we can all agree on the following six points about Herxheim:

1. Herxheim is a unique site. There may be other ‘killing fields’ (Talheim, Halberstadt or Asparn / Schletz) but the numbers of dead amount to fewer than 70, **not** hundreds or thousands of persons.
2. Although there are many thousands of sherds, human bone fragments, etc., there were very few **complete** entities, whether human bodies, whole pots or even individual human bones. The Herxheim Team’s heroic re-fitting exercises demonstrated the significance of deliberate fragmentation of human bones as well as objects. Despite the large areas which remain unexcavated, there is a strong presumption that much of this material is missing – viz., not deposited at Herxheim.
3. While a high proportion of the individuals analysed for strontium isotopes were born in the uplands, it is still unclear where these persons lived in the years, months and weeks before their deaths. Depending on the tooth type that has been used, the strontium analysis covers the ages of 3.5–14.5 ± 6 months for permanent teeth, based on complete tooth crowns and under a year if using deciduous teeth (BEAUMONT / MONTGOMERY 2015, tab. 1). Isotopic studies are currently methodologically unable to place a single person as residing at Herxheim *pre-mortem*, even the children with “loess-signals” who could come from other loessic areas and not necessarily from Herxheim.
4. Owing to extensive site erosion, there were very few preserved houses or pits at Herxheim in LBK Phase V, so it is difficult to estimate the size of the local resident community. But it is highly improbable that this local community could have accounted for the density of human remains at the site. Equally, very few Phase V sites are known from the lowlands close to Herxheim.
5. The presence of a significant quantity and diversity of non-local objects (viz., not locally made at Herxheim) – whether pottery, ground stone, polished stone, chipped stone or shell – supports the inference from the isotopic studies that there was considerable mobility across the landscape at the local, regional and inter-regional scales. The Herxheim Team have successfully demonstrated that a mobility model can account for the exotic pottery and stone tools which were brought by some means to Herxheim.
6. So there is a conundrum with Herxheim – and none of the published ‘explanations’ can account for **all** of the points raised above.

Unsurprisingly, at this point the unanimity dissolves rather rapidly. The five commentaries in this special issue, as well as the *Germania* editors’ comments, cover the theoretical approach, isotopic studies, site studies and more general considerations on the LBK³. It is important to underline that, although some of the points made by A. Zeeb-Lanz / Alexander Gramsch and Daniela Hofmann do indeed challenge our approach directly, none of the comments from Alasdair Whittle, Stefan Schreiber and Rouven Turck put our overall approach in any jeopardy. On this occasion, we had hoped to detour around the well-trodden route of responders to critiques with the claim that our views have been misunderstood or misrepresented. Sadly, this road cannot be avoided. Two fre-

³ The surprising absence of a commentary from a bioarchaeologist has been explained to us by the editorial team: the only bioarchaeologist who read

the text stated that, since the original bioarchaeological data were used without commentary or criticism, there was nothing on which to comment.

quent characteristics of the replies to our model are the accusation of omissions which we have, in point of fact, actually discussed; and the inclusion as criticisms of points which actually do not contradict our model.

We begin with our response to Alasdair Whittle. We accept his summary of the detailed perimortem 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'). We have never argued that the people who died at other sites but part of whose remains were deposited at Herxheim did not die natural deaths. Likewise, the evidence for cannibalism has been reviewed in our paper and rejected (see *Chapman/Gaydarska/Jakob, p. 10*). The nests of calottes are also strong evidence for the deliberate practices which typified Herxheim. While Whittle complains that the relations between upland communities and Herxheim were overly orderly and sanitised, we make it abundantly clear that this was a very messy, probably smelly and unpleasant task to move bodies or body parts from site to site. If by 'orderly', it is meant that we are dealing with a repeated practice, which may have become so repetitive that it became more formalised – then we concur. But 'formalised' voyages do not make them any less unpleasant. While Whittle fails to identify our vision of the fate of missing bones – a criticism echoed by *Zeeb-Lanz/Gramsch (p. 26)* –, we submit that our model discusses in detail how persons in upland sites died, how their bodies were (sometimes) curated, how some parts were left in their 'Home Communities' and how parts of the bodies were taken on a trip to Herxheim, which included overnight stop-overs and possibly also depositing parts of bodies on these sites *en route*. What more detail can we provide? We submit that no other explanation of Herxheim comes anywhere close to matching our vision of the fate of the missing bones.

Turning to D. Hofmann, she observes that the longer use we posit for Herxheim means that above-ground deposition of parts of the material (at least for a time) can explain the different post-depositional histories of re-fitting fragments, while the incorporation of material into the ditches could have been periodic, and not necessarily marked with great formality, explaining the appearance of the spreads or "concentrations". However, there is a difference between post-fragmentation and post-depositional lives of sherds from the same vessel; post-fragmentation differences could have occurred before deposition, with some curated in one way, others in different ways, and others not at all. Periodic deposition in ditches does not contradict our model but the greater the sense of an overall mortuary congregation centre, the more probable it was to have formal 'events', involving the local Guardians and also the visitors. In response to Hofmann questioning whether all Herxheim remains were 'ancestral', we readily concede that not all of those who died at sites linked to Herxheim (upland or lowland) were selected for special transport to Herxheim. Not all of the deceased on any site became ancestors – they had to be deliberately selected and it seems probable to us that only those selected for the Herxheim deposition were in the same way selected as ancestors.

Zeeb-Lanz/Gramsch conclude that the striking over-representation of juveniles and young adults is one of the elements missing in, or even contradicting, our narrative, while interpreting this as an indication of intentional killing of members from communities of the living. One explanation could be based on methodological issues: age estimation of children and young adults is comparatively easy since it relies on dental development and eruption and fusion of epiphyses. Unerupted or partially erupted teeth can still be observed in fragmentary remains and provide fairly narrow age ranges especially for children and older individuals up to their early 20s (VODANOVIĆ et al. 2011, 15). There is also an inherent preservational bias favouring bones of young adults which are more mineralised compared to bones from very young and very old individuals (WALKER et al. 1988). But why would juveniles and young adults not have been selected as 'ancestors' at other sites, especially

if they came from significant families and/or achieved something important in their lives? Zeeb-Lanz / Gramsch's interpretation that this was a sign of intentional killing of people merely confirms our argument that there was a selective process going on in the 'Home Communities'. Their puzzlement at the inclusion of 'ancestral' material in the Herxheim ditches (Zeeb-Lanz / Gramsch, p. 26) is readily explained by the myriad cases where older material was often highly valued and therefore often curated for later deposition. There are clear parallels from the mortuary domain at Varna in the Copper Age of the East Balkans (e.g., the 'antique' *Spondylus* bracelet curated for generations before deposition in Grave 43 at Varna: CHAPMAN 2023, 296 fig. 18,3).

Zeeb-Lanz / Gramsch's discussion of special and elaborately treated animal bones, which added to the complexity of the practices, is a classic example of interesting details which not only do not contradict our model but actually reinforce it. The special animal remains in the ditches included the wing bones from a large wading bird, a number of *bucrania* and *aigicrania* as well as around 40 mandibles (mostly cut in half and sprinkled with ochre) from small carnivores like marten, polecat or wild cat. We cannot comprehend why Zeeb-Lanz / Gramsch think that symbolic deposition of animal parts is somehow contrary to our model, which is centred on enchainment and symbolic practices. The first enchainment link is between the hunter and the wild animal or bird; the second links the hunter, the prey and the person(s) preparing the deposit; and the third links any aspects of the depositional practice (e.g., red ochre, division into halves) with other deposits where similar aspects appeared. The most obvious relationship to our approach is that all of the 'wild' deposits represent the **parts** of birds or animals – especially prominent in the case of precise halves! Where were the missing parts? Has anyone tried to re-fit two separate halves to make an 'entire' mandible? We thank the commentators for adding to the complexity of our fragmentation story.

Zeeb-Lanz / Gramsch make several points about the complexities of the depositional processes at Herxheim. There is a straightforward rebuttal of two of their observations: that many pottery re-fits between freshly broken sherds lying in neighbouring concentrations speak for longer parts of the ditches standing open simultaneously and that it was not possible to have specific areas, namely distinct long pits, into which each group of uplanders deposited the enchainment bones and sherds of their specific bonds with selected lowland communities. These observations do not vitiate our depositional model, since they ignore the practice of sherd curation, which, in any case, the commentators confirm in the case of the Flomborn decorated Early LBK and Middle-Late LBK sherds. We have never denied that there may have been two or more long pits open at the same time. Our suggestion that there were distinct long pits for deposition by specific social groups may work in certain parts of the site – after all, why did Fabian Haack define long pits as the primary architectural units in the first place? – but all that you need to do is to substitute for the phrase 'specific long pits' the phrase 'specific sectors of long pits' and a similar effect is created. The final criticism levelled at our approach concerns the material deposited both below and above finds 'concentrations'. It remains unclear why Zeeb-Lanz / Gramsch assume that this huge amount of material (a) eludes any kind of enchainment and therefore (b) is ignored in our narrative. In point of fact, we do not ignore the material in the layers stratified above and below the 'concentrations'. Such a large quantity of material clearly must be incorporated into our model and indeed the human remains explicitly form part of the modelling calculations while the objects are implicitly part of the story of the enchainment processes at the heart of the Herxheim phenomenon.

R. Turck's isotopic data provide important but also limiting information about the Herxheim dead. Although Turck can claim that at least some of the individuals the remains of which were deposited in the Herxheim ditches – the children with their mothers and perhaps even further relatives – had lived there for several months or years prior to their death, dismemberment and deposi-

tion, we have never claimed that all of the persons whose bodies or bodily parts were deposited at Herxheim were all from exotic places – whether in the uplands or the lowlands. But 80% of the isotopic sourcing shows an upland component in childhood and there is as yet no evidence that they did not spend much of their lives in the uplands. Equally for neonates and *infans I* children, Turck remarks that all individuals of the age group *infans I* lived on loess and that the neonates did not belong to the group of ‘individuals growing up on highly crystalline bedrock’. He draws three conclusions: these individuals were clearly part of the ritual and definitely were not brought from upland sites; they did not live off-site or at least not in regions unusual for the LBK; and they could not have been members of upland ‘Home Communities’. However, Turck cannot demonstrate that the large sample of **three** neonates and **four** *infans I* babies were born at Herxheim – only that they were born on an area of loessic substrate. This could be a loessic area at Herxheim, any one of the many loessic areas near Herxheim or even at some distance from Herxheim. There is **no** evidence that the neonates did not live off-site – only not in areas of geology different from that of the loessic substrate. By contrast, Turck confirms that their mothers lived part of their lives on or near crystalline rocks – another confirmation of the Neolithic mobility which Hofmann so correctly supports. Moreover, Turck’s statement that those individuals with upland signatures gained them in their childhood implies that there *were* indeed upland settlements where these young people ate the food that gave them such signatures – indeed for up to 15 years of their lives. Turck’s data do not remove the problem of upland settlement – they actually confirm it.

All of these detailed claims and counter-claims share a pattern of inference that their core observations somehow falsify our narrative. We find it ironic that we can utilise so many of these points in **support** of our narrative.

It is now time to turn to more general issues of criticism rather than to wade through the mass of site detail only to discover that these data reveal a wide range of complex practices which led to such an extraordinary deposition of human bone fragments. Zeeb-Lanz/Gramsche pinpoint three alleged failings in our research: (a) the downplaying of the sheer scale of the operations – a criticism also made by Hofmann; (b) the over-simplification of such practices and (c) the exclusion of much material because it does not fit our favoured general practice of enchainment. However, none of the strictures of Zeeb-Lanz/Gramsche provides a conclusive argument against the movement of bodies or body parts to Herxheim from other sites. What we sought to demonstrate for Herxheim was a pattern found at many other European Neolithic and Chalcolithic sites – the movement of parts of human bodies across the landscape to special places.

In response to point (a), in our bodily mobility model, we have discussed the movement of up to 1,000 bodies to Herxheim – twice the number of persons which Zeeb-Lanz/Gramsche infer from the total number of skulls (*viz.*, 500). There is an underlying tension in the project team publications between the significance of random behaviour at the site in contrast to the systematic, regular, rule-bound practices which may be expected to govern such a complex operation. This tension underlies Hofmann’s critique of our implied micro-management of depositional processes through small-scale management and tracking of every piece of human bone and sherd (*Chapman/Gaydarska/Jakob*, p. 21–22). But Hofmann overlooks the actually modest number of human body part sets brought to Herxheim each year – varying from a minimal four body part sets to a much larger 73 (*Tab. 1*). There is thus no need for a Teutonic bureaucracy, or masses of Guardians’ resources, to distribute the bone fragments to different parts of the long pits or different long pits. The addition of stone objects and pottery would admittedly have increased the organisational load but the increase in body part sets comes with a concurrent growth in the number of ‘Home Communities’, each of whom would have contributed to the organisation of the enchainment process.

Zeeb-Lanz / Gramsch show a sad lack of imagination when they note “the impossibility of imagining how many ‘Home Communities’ should have been involved to create” the Herxheim remains. If they had studied our model more closely (see *Tab. 1*), they would have learnt that, **at any one visit**, representatives from between one and 14 ‘Home Communities’ would have come to Herxheim. These critiques stem from a lack of understanding of the temporality of the Herxheim deposition.

Moreover, there is a second answer to the criticism of ‘micro-management’ and the sheer scale of material deposited. The response relates to the scale of fragmentation – a point which we emphasised in the first Fragmentation book (CHAPMAN 2000, 7; fig. 1,4). While the whole may become two or more fragments, many complete objects but, critically, also **many fragments** can form a set. At Herxheim, instead of hundreds of sherds and human bones being treated as individual items, their identities were subsumed under the identity of a set. The set was then treated in a new way, in parallel to that of another set. These different sets were deposited in different parts of the enclosure ditches; a new ‘ensemble’ of many sets was created. The selection of material to include in any specific set was far less of a bureaucratic burden than would have been the selection of every sherd and each human bone fragment but, crucially, would have still permitted the creation of enchainment relationships both within and between the sets.

In response to point (b), the novel idea we propose as a ‘solution’ to the Herxheim conundrum is the movement of corpses or parts of corpses to Herxheim from other sites in the uplands and / or the lowlands. What happened to the bodies/body parts at Herxheim was a vital but secondary concern to us⁴ and became fundamental as part of our approach based upon deliberate fragmentation and the creation and maintenance of kinship and ancestral relationships through enchainment. While Hofmann questions whether all of the Herxheim body parts were ‘ancestral’, it is a term which we used as a generalising term to include all persons related by kin of whatever generation, whether they were newly-dead or ancestors for a generation or more. Much recent anthropological research on ancestors (HELMS 1998; MCANANY 2014) highlights the deliberate selection of only small numbers of the deceased in any given community as the ‘ancestors’ of that group. Movement of a small number of deceased from other settlements to Herxheim implies that these persons were chosen as ‘ancestors’ – as representatives of their Home Community.

In response to point (c), it is worth reminding readers that, far from simplifying the treatment of dead bodies at Herxheim, we quoted with approval Zeeb-Lanz’s *chaîne opératoire* of bodily treatment at Herxheim: (1) intentional killing; (2) dismemberment of their bodies; (3) removal of muscle tissue; (4) smashing of all bones except for the cranial material; (5) burning of some of the bones; and (6) final deposition in the ditches and, more rarely, in the settlement features (ZEEB-LANZ 2019b, 431–432). The artefacts were also ‘processed’ through various operational chains, all of which concluded in deposition as the final stage of the ritual process. As we stated in our article, we do not see an objection to these six stages forming parts of enchainment relations between the living and the dead. Part of these practices would have included curation. The likelihood of human and animal bone and sherd curation is an important factor at Herxheim and helps to explain the freshly broken sherds in different long pits that re-fitted, rather than necessitating the simultaneous opening of different long pits. Sherd curation is also a good explanation for the incorporation of so-called ‘ancestral’ material in later deposits.

Several reviewers seem to have a problem with ‘enchainment’. For example, Zeeb-Lanz / Gramsch envisage enchainment as described in our account as not only free of conflict, but as a very techni-

⁴ An analogy can be found in Tom Lehrer’s song about Werner von Braun: “Once the rockets are

up, who cares where they come down? That’s not my department!” says W. von Braun.

cal, sober, matter-of-fact, processual procedure – without regard to probably involved emotions such as grief or irritation or to ambitions such as wanting to outplay other feasting participants. As we have stressed from our earliest publications on enchainment, there are specific ways in which enchainment relations between persons, objects and places developed in a variety of contexts and it is our job, as fragmenterists, to define more closely the kind of enchainment that was most likely to have occurred in any particular context. I doubt that many archaeologists would accept that the death of a relative or friend, their bodily fragmentation and the movement of their bodily parts across the landscape to a central mortuary congregation site would have been devoid of strong and often negative emotions. But part of the journey, involving overnight stop-overs, moderated the pain through communal feasting and recollections of the dead persons. Although the approach to the mortuary congregation place of final deposition of these lovingly carried bodily parts must have been emotionally challenging, the act of further dismemberment and deposition with other material culture would have brought some measure of closure – again, enveloped in a communal atmosphere of shared grief and emotion.

It is worth re-iterating Andy JONES's (2012, 19–20) point that enchainment is the basis for the creation and maintenance of **all** social relations. We have always stressed that enchainment is an 'umbrella' term which requires more precise definition in individual contexts (CHAPMAN/GAYDARSKA 2007, 196–201; *contra Whittle, here p. 36*). The Herxheim case is a classic version of a very specific kind of enchainment of smashed human bones and broken human bodies. The deeply emotional aspects of bodily transport and ultimate dismemberment would certainly have been a key aspect of the Herxheim experience but there is nothing in the nature of enchainment or enchainment relations that excludes that possibility. Part of the inestimable value of the Herxheim database concerns the extensive re-fitting studies, which show a richly interconnected set of contexts underlining the materialisation of enchainment relations through fragment deposition.

On the theoretical basis of the fragmentation premise, Stefan Schreiber is supportive of the use of what he terms fragmentation/enchainment theory at Herxheim but he remains critical of the Cartesian basis of the theory. By this, he means that 'after the break' does not include the rupture of the object-subject dichotomy. Instead, it is exclusively the object that is broken, fragmented and subjugated. In an instrumental relationship, the human determines both when the object breaks and how continued 'use' cements the continuity of the object 'as object'. At the same time, the concept of objects determines what parts and wholes are.

We dispute what Schreiber terms the proposed Cartesian basis of fragmentation theory on two levels. First, there is always a recursive relationship between persons, places and things, such that they are mutually constitutive and this is an essential (but not essentialising) part of fragmentation theory. Enchainment relations are carried forward as much by the object as by the person – it is the fragment in another context which does the work of sustaining the human-to-human relationship. Secondly, we never posit a fixed, essentialist relationship between wholes and parts – indeed, in our most recent publications, we are finding it increasingly difficult to separate the two. What seem to be parts on one level have soon, in another time-frame, become wholes.

Schreiber also argues that most of the concepts used (objects, human bodies, bones, places, identity, fragmentation, operational chains) suggest a common sense that could come from any excavation report or archaeological evaluation (only 'enchainment' and 'itineraries' need more preconditions). In fact, however, he insists that they are not concepts, but merely deductive *ad hoc*-theories. And he warns that it is precisely these concepts that are operated with that should be questioned as preconditions, because it is here that the origin and benefit of the approach become visible. However, we dispute the interpretation of notions of 'fragmentation', 'operational chains' and 'identity'

as in any way co-terminous with objects, human bodies, bones and places. The former group include theoretical assumptions and methodological considerations in quite different ways from those used to consider the second group. Moreover, our discussion of personhood includes a far wider range of concepts than the commentator assumes.

Schreiber also poses the question: “Why do places, objects and people in the past have to be fragmented only to be re-unified?” We suggest that the objects that we discuss are generally manufactured as whole objects *qua* whole objects (e.g., vessels rather than fired clay images of feet or heads = a complete representation of a part of the body). The importance of operational chains of making (additive (e.g., clay) vs. reductive (e.g., stone) vs. transformational (e.g., metals)) shows the manufacturing relationships between materials and objects. The materials have a strong influence not only on how they are transformed into objects, but also on how they were broken. Our *Figure 2* gives an example of these operational chains. There is never a sense that re-unification is a central aspect of our research; re-fitting studies are conducted to demonstrate the means by which enchainment relations are materialised. When Schreiber suggests that the *synecdoche* as a type of interpretation implies a *pars pro toto* wholeness, he seems to assume that we do not accept a relational aspect to parts and wholes. The priority of wholes can be seen in the natural world (landscapes fragmented into quarries) or in the world of objects or human bodies. There is nothing inevitable about fragmentation but, wherever and whenever it occurs, it offers people the opportunity to use the resultant fragments in interesting social ways. We take the robust position that fragments can be used positively ‘after the break’ to sustain enchainment relations – so what was preserved was the relationship rather than the complete object. In the same vein, Hofmann emphasises only one aspect of fragmentation – that which dissolves relationships and identities – but overlooks the other, more creative aspect of fragments which leads to enchainment and the persistence of relationships and even, as in the case of the Hamangia figurine fragmentation, to changing sexual identities. In some cases, then, as at Herxheim, fragmentation and enchainment go hand in hand as relational strategies. Nonetheless, despite some contested ground which we explore here, we welcome Schreiber’s several suggestions as to the future development of fragmentation theory.

A major disagreement within the Herxheim team concerns the treatment of the human remains, with Bruno BOULESTIN et al. (2009) arguing for mass cannibalism and Zeeb-Lanz rehearsing lengthy and compelling arguments against this interpretation (ZEEB-LANZ 2019a, 449–454). The alternative that Zeeb-Lanz proposes is the mass sacrifice of human captives at Herxheim following raids on other villages (ZEEB-LANZ 2019a, 457–463). For Zeeb-Lanz, Herxheim possessed the widest range of ritual practices in the latest LBK, with other regions connected to Herxheim through an inter-regional alliance based on traditional lineage ties. The primary aim of the Herxheim ceremonial feasting and rituals was to strengthen and deepen such enchainment ties. This much, we can agree with wholeheartedly. However, we part company with the claim that the Herxheim rituals were less about “violence against humans and objects” than “ritually charged transformations of humans and objects using physical force.” (ZEEB-LANZ 2019a, 463). We are not sure whether those captured and killed would have agreed. What this meant was that “the destruction of precious artefacts (to which we may add ‘precious persons’) represented a leading theme throughout the Herxheim scenario as a whole” (ZEEB-LANZ 2019a, 454). Zeeb-Lanz admits that the identity of the victims of this violence is still a mystery but insists that they came from a different ethnic group from the Herxheim residents and were either unfree serfs (slaves) or captives, brought to Herxheim by farming groups from the inter-regional alliance. But this is a claim hardly supported by the aDNA analysis of a small number of Herxheim physical remains (BLÖCHER et al. 2019). In addition, and in contrast to the other LBK mass graves of mainly complete individuals, there were relatively few signs of trauma as a cause of death, even on the skulls. Moreover, the large number of ‘victims’ in the short timescale

proposed by Zeeb-Lanz makes impossible demands on the regional settlement network, which is thin to the point of disappearance in the Southern and Northern Palatinate in LBK Phase V and consists of one house outside the enclosure at the site itself. The disjunction between the scale of raiding and capture in the Palatinate and its minimal settlement evidence mean that the Zeeb-Lanz 'model' of inter-communal violence is fatally flawed. Moreover, this model has no explanation of the fragment re-fits of many objects deposited at Herxheim, often in different long pits. It is imperative to reach a new understanding of such an important site as Herxheim.

In their views of Herxheim, both Hofmann and Zeeb-Lanz / Gramsch emphasise the dissolution of human individuals and social transformation – the way that bodies change from differentiated to undifferentiated in “an active disintegration of persons”. The ontological basis of these views is the Western assumption of the primacy of the complete, undivided, separate body – an assumption now widely questioned and also implicitly by Schreiber in his reply. To put the Herxheim model in the context of dividuality, we offer a commentary in the *chaîne opératoire* of dividuals in LBK Phase V who died somewhere other than Herxheim. The death of a member of their community is, first, traumatic and, secondly, opening up the possibility of change. But the kinship and exchange relationships between community members and dividuals in other settlements meant that one death affected the whole region; this was, after all, what was meant by a social network. Once a regional mortuary centre had been established at Herxheim, the Home Community made a decision whether the newly-dead were to be moved to the centre or buried locally. If death occurred in the snowy, winter season, there would have been body curation, which already created a spatial and emotional distance between the living community and the deceased. Bodies/body parts could also have been curated submerged in water (rivers/streams or ponds) or other types of anorexic environments (e.g., bogs), which, in addition, would have reduced the unpleasant odour of the dead (BRÜCK / BOOTH 2022). Partial body dismemberment may also have started at that time. Each further step towards Herxheim would have increased that physical and emotional distance between the living and the dead, as well as the likelihood of body fragmentation. Observations that the temporal bone was still articulating with the parietal bones in some of the skull caps should not be used to argue against a dismemberment / production of skull caps prior to the journey to Herxheim from other communities. Although the squamous suture is less strong compared to other cranial sutures, examples from albeit much more recent (19th century) craniotomies have shown that these bones can still remain attached to each other. Nevertheless, simply wrapping the newly produced skulls caps in fabric or leather would prevent the cranial bones from becoming detached from each other. The exchange of body parts with other settlements *en route* for Herxheim further emphasised the dividuality of the corpse, while strengthening the links with the other communities. We could hypothesise that many of the body parts started out as partial limbs that were broken down into small component parts further along the journey to Herxheim. This, and the presence of soft tissue, could explain the abundance of cut marks. The few still articulated body parts might have come from a place closer to Herxheim with fewer opportunities of leaving parts of the body with other communities. Alternatively, these articulated remains might have had better soft tissue preservation, including ligaments and tendons, that allowed the bones to remain articulated. By the time the mourners had reached Herxheim, their social relations had been maintained but the bodily remains of their dead kin may well have shrunk. Above all, Herxheim was a liminal place – a place of transformation from the physical remains of the newly-dead – already probably incomplete – to an even more dispersed, dissolved dividual. But the purpose of smashing the bones still further was not only the dissolution of the physical body – a process that had been going on for some time prior – but the sustaining of enchainned links between the ancestors who had already been buried at Herxheim, the Guardian community and any other Home Community represented at the time. Anyone who

has tried to dissect an animal carcass will know how hard it is to break animal bone – and the same would have been true with human bones. But the Herxheim Guardians looked after the process of the final incorporation of the much reduced bodily remains into as many places as was necessary. It was only then that the last voyage of the newly-dead could be considered concluded – and that *dividual* could become an ancestor in the full sense of a member of a much wider ancestral community, representing themselves, their own community and those other communities who benefited from the exchange of their bodily parts.

The possibility of a greater external contribution to Herxheim – from upland and/or lowland settlements – may be considered as our relational response to the central dilemma of the Herxheim site – the absence of settlement evidence in the Latest LBK when the strontium isotopic signals indicate a non-local origin for the early childhood of three-quarters of the persons whose bones were deposited at Herxheim. A natal age for the first molars sampled is improbable, unless the very tip of the tooth was sampled (BEAUMONT/MONTGOMERY 2015). It is surely instructive to note that none of the commentators has made any substantive objections to our model of bodily mobility, whether in the details of the varying numbers of body part sets brought to Herxheim or in the basic enchainment principle which underlies this proposed explanation.

We wish to offer three final comments on the debate which our article has engendered, thanks to the *Germania* editorial team and the six commentators: on future research at and around Herxheim; on the widespread evidence for the body mobility model in European prehistory; and on the significance of *dividuality*.

This debate has underlined the vital need for new research on strontium isotopic measurements and lowland and upland site prospection in the Palatinate. Turck's isotopic studies have unfortunately lacked the transverse micro-sampling of enamel across the tooth which would have allowed not only the signal at birth but also into adolescence (c. 15 years) depending on the type of tooth being sampled to identify potential changes in residency during childhood (BEAUMONT/MONTGOMERY 2015; BOETHIUS et al. 2022). Such new analyses would reduce the uncertainty of the regions in which the future 'occupants' of Herxheim had lived, which could also be clarified through oxygen and sulphur isotopic analysis.

The more difficult research goal is the extension and intensification of site prospection in both the uplands and the lowlands. In the lowland areas, standard methodology is available for intensive, systematic fieldwalking but, although vital for establishing the precise settlement context of Herxheim, it is time-consuming and expensive. Upland site prospection methods, including LIDAR and drone photography or thermography (CALASTRENC et al. 2020), may well miss flimsy site remains such as short-term Early Neolithic settlements, so any development potential, such as the excavation and construction of new roads, railway lines, service trenches or pipelines would be important targets for fieldwork.

Our recent research on Alsónyék as well as Herxheim has demonstrated the likelihood of body transport across the landscape at the local and maybe regional level. What we cannot understand is the almost instantaneous rejection of such models when they are absolutely standard for many areas of Europe in the Neolithic. The most obvious examples concern the dispersed settlement pattern associated with megalithic burials, with their own shifting territories within which movement of complete corpses or body parts was normal across the full area (WHITTLE 1996, 244). We suspect there may be a perception gap here that we shall seek to bridge in future research.

The other obvious perception gap is how archaeologists conceive of '*dividuals*' or partible persons. At one point (here, p. 42), a commentator seems to think that a '*dividual*' equates to an

incomplete body! Despite the inelegant term, there is strong ontological support for such entities, matched by increasing quantities of archaeological data, often but not necessarily involving deliberate fragmentation and enchainment relations. But there still seems to be an issue with thinking through what it meant to be a dividual in the Neolithic – how identity lived on through the person's fragmented body parts and how object fragments associated with that dividual can have the same transformative effect in the establishment and maintenance of relations. Many of the negative comments by our six commentators are related to this issue. We are apparently still in thrall to Western modernist assumptions about the 'individual'. Unless and until we free ourselves from these questionable ontological assumptions, we shall always struggle for a deeper comprehension of social relationships in the Neolithic and beyond.

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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).