

Introduction: European Prehistory and Urban Studies

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The idea for this special issue arose out of a session on ‘Pre-Roman Urbanism in Eurasia’ at the conference of the European Association of Archaeologists (EAA) in Istanbul in 2014. This was preceded by an international symposium in Vienna in 2012 on proto-urbanization in Western Anatolia and neighbouring areas in the fourth millennium BC, and succeeded by two more conferences on early urbanism with special focus on Eurasia at the universities of Buffalo (April 2016) and Durham (May 2016). This healthy interest reflects an emerging research agenda inspired by exciting new (and not so new) discoveries, some of which form the focus of the following papers. It also brought a skeleton out of the closet, that of the troubled relationship between European prehistory and the emergence of urbanism, a problem with two aspects.

The first is the tacit assumption that the first impulses of urban development might be expected to follow the same Asiatic trajectory as the preceding Neolithization of Europe. Thus, the Minoan ‘first-generation secondary states’ (Parkinson and Galaty 2007, p. 118) should be considered the earliest European examples. Despite the well-argued case that the Balkans were an independent centre of innovations (Renfrew 1969)—in the case of copper metallurgy, even preceding Anatolia (Kienlin 2010)—diffusionist models affect research agendas to this day.

The second aspect of the problem stems from another deep-rooted prejudice, whereby an essentialized view of the Classical, primarily Mediterranean, town or *oppidum* denied a fair ‘urban’ hearing to any Iron Age set of evidence that apparently deviated from this norm (Moore et al. 2013; Fernández-Götz et al. 2014). One of the aims of this special issue is to question the validity of these long-held views on the basis of new evidence. Simply ignoring this evidence or branding these cases exceptions is no longer sustainable: the new straws have already broken the old camel’s back.

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The second aim of this special issue is to address the common misconception that, if a given settlement form was not sustained for long enough (and how long that is has not been clearly defined), then it probably did not contribute to the overall urbanism phenomenon. The flaw in this view has been demonstrated by the now well-documented ‘boom and bust’ pattern that existed alongside a more stable pattern during the EBA urbanization in the Fertile Crescent (Wilkinson et al. 2014). Other patterns of urbanization may involve cycles of centralization and decentralization (Fernández-Götz et al. 2014). Permanently occupied, long-term settlements were but one part of the urban narrative, albeit an important part. Looking at the wider context should reveal different trajectories of living together, even if some of these ended up in evolutionary *culs-de-sac*.

Cross-Cultural Comparison

One may wonder what links the very different sets of evidence discussed in this issue, and veteran cross-culturalists will ask ‘what is there to be compared?’ Ironically, it is exactly what Steward once saw as not worthy of comparison that merits a comparative view – the ‘unique, exotic and non-recurrent particulars’ (Steward 1955, p. 209). The case studies below present evidence that diverges from a critical contemporary settlement mass, and in the case of Trypillia, from any preceding and succeeding examples in the same area. This difference may be perceived in terms of size (e.g. the Iberian mega-sites), or landscape setting (e.g. the LIA *oppida*), but what is important is that there were underlying structuring principles behind the emergence of *different types of site*. Comparing these structuring (as opposed to historical) factors/principles is, I would argue, meaningful, since the various contemporary settlement forms co-emerge and the relationship between them is more important through time and space than the actual characteristics of a given settlement form. Those quick to see that as advancing structure over content in a structure vs. agency debate will be disappointed. Daily practices consist of a constantly re-negotiated social order, whose material expression mediates individual and communal motivations and actions.

The choice of case studies favours an emerging form of human habitation characterized by low density occupation. However, essentializing the relational framework suggested below to comprise only low-density types of occupation would be dangerous, so a single case of high-density occupation was also included for comparative purposes.

Defining the City

A widely held view is that defining the city is difficult because a definition is static, while the city is by nature dynamic. Yet not attempting a definition is not an option if you are to be taken seriously. Archaeologists have adopted two ways of dealing with the issue—either providing a short definition (e.g. M. E. Smith 2007, p. 4) or problematizing the term over several pages (e.g. M. L. Smith 2003). This raises the critical question of whether modern science has found an answer to the conundrum

posed by V. G. Childe in 1950: ‘the concept of “city” is notoriously hard to define’ (1950, p. 3). The optimists in the field of urban studies will respond positively, others, like me, will acknowledge the importance of the empirical data accumulated since 1950 but will doubt that defining the city has become any easier; indeed, the diversity of this new material makes the task even harder.

Urban has come to mean what modern scholars want it to mean. It may have functional connotations (M. E. Smith 2007); or contain sociological empirical distinctions, highly influenced by Western societies (Wirth 1938); or be viewed from the perspective of seats of power (Liverani 2013); practice theory (Cristophersen 2015); the Weberian division into consumer, producer and merchant city (1921); or indeed Fletcher’s (1995) global model of settlement growth. This list is very long. More often than not, *urban* is associated with the cultural phenomenon of *civilization*. Despite the caution raised as early as the 1970s that urbanization and civilization are independent variables (Rouse 1972), 2003 saw the publication of a book called *Understanding early civilizations*, which refers to seven case-studies of urban development (Trigger 2003), and another book, *The social construction of ancient cities*, calling civilization a ‘disfavoured’ term (M. L. Smith 2003, p. 12). The latter publication also criticises the continual causal association between *urban*, *political authority* and *state* (see also Osborne 2007; Jennings and Earle 2016). If equating state formation with the origins of urbanism is not without its problems for some societies serving as benchmarks in urban studies (e.g. the Greek *polis*: Whitley 2001, p. 168), it remains a fatal obstacle for sites with different forms of evidence that may indicate early stages of urban formations.

The dynamic relation between complexity, size (of settled area but also population) (e.g. Feinman 2011) and urbanism (e.g. M. G. Smith 1972) is widely recognized, but cross-cultural trends (e.g. Trigger 2003) have been seen as limiting rather than helpful due to an inherent analytical scalar bias (e.g. Stone 2008). For some, however, size is an important variable and Cowgill (2004, p. 528) sees populations ‘of at least a few thousand ... [as] a necessary, if not sufficient, requirement for a settlement or a society to be urban’.

A further complication is entailed by the two competing strands that either conceptualize urban development in an evolutionary framework (e.g. Childe 1950; Yoffee 2005), or deem an ‘evolutionary typology of urban life’ impossible (Trigger 1972; A. Smith 2003).

Where do the mega-sites fit into this debate? To which of these notions should the study of newly discovered sites—and in particular mega-sites—adhere in order to build a credible case for a hitherto unrecognized urban development? European prehistory will be last to join this system of *negotiating* urban status, akin to the legal system of precedence practised in half the globe today, whereby the first judgment affects all subsequent cases whatever the specifics of the original context. In that vein, the most fruitful way to view mega-sites and their level of urban development is to follow M. L. Smith’s notion that ultimately the definition depends on the questions asked of the dataset (2003, p. 11).

If we are interested in how certain sites came to be, and how they functioned in relation to each other, then we should be more concerned with the type of relationship and how and why it was maintained, than with how we label the sites.

Where they are independently known, the names of the various settlement forms can be very meaningful for understanding the establishment of inter-settlement relations. In the past, *urban* came with its own definition, which imposed a framework within which inter-site relationships were necessarily viewed. Here we offer a bottom-up approach. We call the sites discussed here *mega-sites* because they are large both in absolute terms—e.g. some of the Iberian and Ukrainian sites are much bigger than many Greek cities; and in relative terms—they stand out from contemporary sites.

An Alternative Approach to Differentiating Sites

Sites, urban or not, do not function in isolation, in landscapes void of meaning. I would argue that a useful way to study any network of sites is within a relational framework in which different categories of site co-emerge. Historical examples include the Greek system of *polis* (*chora*), *apoikia*, *emporion* and the Aztec *altepetl*. A simple urban/non-urban division would capture the complexity of neither social organization, so do we need it? I find it redundant, and scholars before me have tried to mitigate the rigid division by defining various degrees of urbanity (e.g. Gringmuth-Dallmer 1996; Andreev 1989). The approach offered here differs from these previous attempts to distinguish between various population concentrations (M. L. Smith 2003) in that it is relational, not hierarchical (although hierarchy may underpin inter-site relationships), and does not favour either of the variables discussed below.

Two factors are identified whose structuring effect has important implications for delineating sites in a relational framework. The first one is centrality. Central places are understood variably in archaeology: as properties of geographically-inspired Thiessen polygons (Christaller 1966); as meaningful nodes in the landscape (Schmidt 2012); as the dominant part of a core–periphery model (Kardulias 1999); or as a gateway community integrating a centre with its hinterland (Hirth 1978). There is a strong tendency to assign a hierarchical position to such central places.

The understanding of central places advocated here is defined by their role in ensuring and negotiating the social reproduction of more or less dispersed human networks in their hinterland through the provision of *different* opportunities for interaction—whether ritual or related to production or consumption. A crucial characteristic is the evidence for residence, not necessarily permanent but sufficiently recurrent to allow a differentiation between seasonal gatherings or ceremonial assemblies and permanent occupation by a core of population. The different relations formed between a centre and its hinterland in the process of population nucleation will affect the form and trajectory of both parts of the settlement network.

The second factor is intensification of what Cowgill (2004, p. 543) calls variables: the economic basis (production, distribution, consumption); ideology (political, ritual, civic, the ‘Big Other’, and the ways in which values are negotiated); investment projects (stone architecture, ditches, ramparts); exchange networks (staple goods, exotic goods, prestige goods); inter-personal relations and

social power (relational, heterarchical, hierarchical); conflict (competition, warfare); utilization of social space (at site level and at landscape level, including the use and ownership of land); and cultural memory and representation (mnemonics, writing, performance).

Both centrality and intensification are necessarily broad umbrella terms, since the relationships underpinning social change were many and varied. Not all societies will attribute equal importance to all of these variables. The dynamic interplay between cross-cutting social practices related to these basic principles results in the emergence, maintenance and abandonment of specific settlement forms. In a cross-cultural study such as this special issue presents, it is important to establish the time-depth of the central place—the length of occupation, the time it took to reach its peak, and the time of its decline.

How are we to measure the degrees of centrality and intensification? Just as there were different kinds of central places, so there were stages of intensification. We need to establish the boundaries/thresholds between such stages and decide what we mean by low, medium and high intensity. To do this, representative high-resolution data are crucial (see below). Secondly, measuring intensification is contextual and relative to meaningful markers. There are no absolute numbers, but qualitative and quantitative pointers that will make sense in a given culture, region and period. Let me give an example using just one variable—investment projects—referring to one of the case-studies discussed below. GIS spatial analysis combining several data sources (e.g. satellite imagery, published gazetteers, etc.) of Trypillia sites dated to the ceramic phase BII in three sample regions has established three broad categories of site size: up to 10 ha (76% of all sites); up to 100 ha (19%); and above 100 ha (5%) (Nebbia 2017). So the norm (the critical mass) of human occupation appears to be the smaller nucleated settlement, whose building and maintenance would hardly require outside help, or only by a number of outsiders equivalent to the number of residents. The next two categories, however, would require co-operation in the order of 5–25 times more energy. The intensity of inter-personal contacts, labour planning and co-operation, raw material procurement, logistics, infrastructure, etc., would differ significantly between building a small settlement and building a 50 ha site, with a further leap to a 250 ha site. It seems inconceivable that participants (and their descendants) in these very different collaborative projects would conceptualize them in the same way. I would argue that different meanings emerged through these different experiences and these influenced how dwelling at these places was perceived by residents and outsiders alike. The construction of a central place would have affected the worldview of everyone connected to the process.

Where are the urban settlements in this scheme? I personally find the concept of categories of site (not to be mixed with categories of city) more helpful, and where they are known from written and ethnographic sources, I would always favour them. However, if archaeological evidence is to be compatible with sociology, history and geography, and requires common terminology, then *urban* will connote settlements whose residential centrality is underpinned by high-intensity social practices relative to contemporary and previous sites of any of the above variables or combinations of any of them. To conclude with definitions: *urbanism* is a culturally

specific process within which an urban way of life is conceived, while *urbanization* concerns the proliferation and sustainability of urban settlement forms.

The Evidential Basis, or Developing Adequate Methodologies

A lot of research energy has been expended on defining the city but less on the methodologies of studying this complex phenomenon. This is hardly surprising, given the enormous variety of field evidence currently counting as urban and the variability of theoretical approaches outlined above.

How do you compare the representativity of the excavated 1% of a total area of 300 ha (e.g. Erliou: Liu 2006) to a fully-exposed site such as Pompeii? What about sites with long occupations, and the issue of the palimpsest? How do we study the hinterlands around central places with sufficiently high resolution to enable meaningful comparison? The tension between the different levels of data resolution remains, despite increasing awareness at the level of both site (e.g. McMahon 2013, pp. 32–33) and hinterland (e.g. Whitelaw 2013). I would argue that mitigating the differences between the value of various datasets, whether a surface pottery scatter or a monumental building or 10,000 entries on a GIS platform, should become a primary theoretical and methodological aim of any future work trying to support inferences through adequate evidential reasoning. Employment of high-tech methods is argued on a case by case basis [e.g. LiDAR for Angkor Watt (Evans et al. 2013); AMS in the Trypillia case (Millard et al. in prep.); and GIS and geophysics for Mashkan-shapir (Stone 2008)]. The next step is to turn the accumulation of comparable high-resolution data into a prerequisite for any meaningful discussion of the criteria for urban centres.

Less than two decades into the 21st century, we are in a privileged position not only because of technological advances in archaeological practice but also because we can benefit from 150 years of investigations of the ‘city’. There is now no excuse to use low-resolution, patchy data without critical evaluation of what it can actually tell us. We need to utilise modern technology, but only if supported by a well-argued and theoretically informed methodology aimed at asking questions that the dataset is fit to answer. Returning to the example of intensification of investment projects at Trypillia, it would have been impossible to address this issue without the quality control of site size that has been provided by aerial images, coupled with a robust chronology (provided by ceramic phases in published gazetteers, although ideally this would have been supported by a chronometric dating programme). Equally, it would be unwise to use this information to ascertain whether the appearance of large sites is an indication of urbanization processes without further quality control of the data.

From Çatalhöyük to Bagendon—A Voyage of (Re)Discovery

In the history of urban studies, there is hardly a more contentious site than Çatalhöyük, and depending on the research question asked (Gaydarska 2016), the academic community will continue to be divided as to the most important

characteristics of this fascinating site. Instead of getting bogged down in deconstruction of old views, Der and Issavi take the more positive route of contextualizing Çatalhöyük in time and space. Among the many valuable insights yielded by this approach, two stand out in terms of wider implications for cross-cultural research, and in particular, a comparison to Trypillian mega-sites. The first is that the mega-site—that is, an aggregation of a large number of people in terms of the relative norm of the specific time/space pattern—is a far more common settlement formation than modern scholars have recognized. This lack of recognition results from the fragmentary and incoherent nature of the study of mega-sites, which has been either locked in regional debates, or focused on the relationship between complexity and size (usually of population rather than settlements). A fresh cross-cultural re-assessment of large settlements may help to reveal the motivations behind and sustainability of these sites. The second important implication is the lack of materialized hierarchy. A widely-held view in archaeology is that hierarchy requires proof, while egalitarianism does not. Since more often than not hierarchy is linked to ‘urban’ formation and egalitarian organization is seen as not very compatible with urban formations, seemingly egalitarian evidence, such as that from Çatalhöyük and the Trypillia mega-sites (see below), is used as an argument against a possible urban character of these sites. An alternative way to interpret the intensification of various social practices, compellingly argued by Der and Issavi, is as an attempt to diffuse social tension and mask social differentiation.

The next two papers deal with a phenomenon that inspired the above-mentioned EAA session, followed by an EAA monograph (Müller et al. 2016). They capture the controversy surrounding the sites known as the Trypillia mega-sites and argue for different readings of the evidence. The debate is initiated here by Diachenko and Menotti, who present the widely-held view of these sites. The brief history of research as presented is revealing in terms of the rhetoric which frames the interpretation of these sites. Interestingly, both the proponents and opponents of the proto-urban hypothesis explain the sustainability of this settlement form with reference to migration. Diachenko and Menotti neither distance themselves from nor engage with the prevailing Childean agenda of Trypillia research. Instead, they refer to the results of formal demographic modelling, suggesting a dynamic interplay between settlement size and density of occupation (here measured by the density of structures). A general trend is recognised of decreasing density of structures with increasing settlement size. In the next paper Chapman questions the contemporary occupation of all structures at mega-sites that is assumed in such models. Diachenko and Menotti’s conclusion that the Trypillian mega-sites are ‘low-density settlements’ rather than ‘low-density urban sites’ corresponds to the prevailing Ukrainian view of these sites as ‘settlement-giants’ that paradoxically acknowledges their size but fails to recognize its implications.

This is picked up by Chapman. His starting point is the uncritical adoption of the Neolithic package as a major characteristic of such vast sites. He develops an internal discussion of what he calls the *maximalist* versus the *minimalist* approach, whereby arguments for both are put forward based upon the same lines of evidence. This shifts the focus from the so-far futile terminological debate between *settlement-giants* and *proto-urban* sites to the sustainability of each individual mega-site, as

well as of this particular settlement form. Interestingly, some of the factors accounting for such sustainability are well rooted in ancestral traditions, while others are more readily associated with urban formations. This is not some slow evolutionary change but rather points to a process whereby ‘pioneering’ cities share more characteristics with preceding sites in the area than with their ‘developed’ successors (Gaydarska 2016). While further modelling will clarify the plausibility of the maximalist, minimalist or middle way, the arguments emerging from this debate have much wider resonance in terms of sites and landscapes as palimpsests and/or managed environments, contemporary occupation of large settlement units, and the social implications of nested spatial analysis (overall settlement, quarters, neighbourhoods).

The next contribution by García Sanjuán et al. demonstrates once again how the same set of evidence can be read differently by scholars (see below) following the anthropological notion of human development (Scarre 2013) and those advocating a more relational approach (see above). At first sight, the special status of Valencina de la Concepción seems to be underpinned by its enormous size (450 ha), but a closer look reveals a complex web of socio-economic relations facilitating the convergence of very diverse social practices at one and the same place. This vast site is most unlike those sites commonly associated with the Iberian Copper Age. It is neither small and fortified, nor neatly divided into domestic and burial areas (like Los Millares). What García Sanjuán et al. call a ‘check-list for primitive urbanism’ can be seen as the sum total, a microcosm of what was important, of what mattered for Iberian Copper Age communities. So much so that what was perhaps initially a small burial ground, for 900 years has been gathering place-value by the accommodation of high-intensity social practices materialized in feasting deposits, ‘domestication’ of exotic material (Chapman 2008), and the construction of impressive megalithic tombs. While I concur with the authors that trajectories to complexity should not be conflated with trajectories to urbanism, where we differ is on the idea that there is an ‘ideal’ urban trajectory here characterised by 4th millennium Mesopotamia. In the relational framework suggested above, Valencina de la Concepción is a central place in a network of interrelated sites, at which place-value was re-iterated through high-intensity social practices, thereby marking it out as different from the remaining critical mass. What we should call this difference remains contentious.

A common characteristic of many deconstructing studies of influential concepts or paradigms is that they throw the baby out with the bath water. This is certainly not the case with Fernández-Götz and Ralston’s re-assessment of Early Iron Age urbanism in temperate Europe. Focusing on various models in urban studies and in Early Iron Age Europe in particular, they explore the possibility of reconciling an ever-increasing body of evidence with these models. Their analysis reveals not so much the inadequacy of the models as a lack of appropriate engagement with the new data. Fernández-Götz and Ralston choose to argue for an ‘ephemeral urban phenomenon’ within the current interpretative framework of the European Iron Age, and the urban phenomenon more broadly, demonstrating the unexplored potential of these debates to accommodate a much wider set of evidence. They refer to heterogeneity as a characteristic of Iron Age Europe, a point echoed in the next

contribution, thus urging the re-conceptualization of the current European Iron Age narrative to account for diversity and fragility, alongside the usual rhetoric of power and hierarchy.

A skilful illustration of how the current ‘urban’ concept can be limiting rather than having comprehensive explanatory power is provided by the last contribution by Moore. As with the Trypillia mega-sites, the long-running debate about the urban vs. non-urban character of European *oppida* has failed to tackle their heterogeneity. The reason for Neves’ (2013) plea to ban the use of *complex society* and *social complexity* for the next ten years in archaeology is very well exposed by Moore’s deconstruction, not necessarily of the terms themselves but of their widespread misuse, whereby the term *complex society* stands in for a critical assessment of what constitutes social complexity. The combination of terms such as *low-density urbanism*, *assembly places* and *powerscapes* is not readily associated with European *oppida*; however, Moore convincingly brings them together in a coherent new approach to social transformation at the end of the first millennium BC. European *oppida* are not simply ‘towns’, they are social responses to socio-political and economic challenges that differentiate them from preceding and contemporary settlements but also from idealized urban centres. For Moore, understanding *oppida* does not involve dropping the ‘urban lens’ altogether but re-focusing it to cover a much wider set of questions.

Concluding Remarks

These usually come at the end of a special issue such as this, so the reader is welcome to stop here and return later to this point. The aim of this issue was to challenge prevailing views of evolutionary urban development, a phenomenon believed to have emerged for the first time in late 4th millennium Mesopotamia, while arguing for a more flexible, less prescriptive and less linear framework of distributed settlement trajectories, within which what may qualify as ‘urban’ is not defined by its ‘success’ in modern classificatory systems (Gaydarska 2016). While I admit that the current perception of ‘urban’ is compromised and not very helpful, it remains very difficult to replace it with anything that will ensure comparability between archaeology and other disciplines dealing with urban phenomena. We have no choice but to update and constantly re-visit our analytical constructs, aiming to embrace variability as best as we can (e.g. Christophersen 2015; Ortman et al. 2014). A cross-cultural collection of case-studies was thought to be the best way to deconstruct the canonical view of urbanism. While each of the contributions offers an assessment (or re-assessment) of well- or little-known archaeological cases, not all of the authors engage critically with the possible implications of these data beyond the interpretative frameworks currently in operation. Insofar as two of the contributions to this special issue (Der and Issavi 2017; Fernández-Götz and Ralston 2017) feel more comfortable with the ‘traditional’ reading of urbanism, neither the Trypillia nor the Iberian mega-sites are perceived as urban. The other four contributions approach from different angles the core premise of this special issue

(that there are different trajectories to/of urban inception and urbanization), with each reaching regionally specific, yet globally important, conclusions.

The special issue raises more questions than it answers, which I hope will inspire future research. At least three areas are highlighted for further attention. The first is methodological and involves the development of ‘measurement’ in archaeology (cf. that suggested for the social sciences: Cartwright and Runhardt 2014) that will enable the identification of relational rather than fixed categories of sites. The second concerns the conceptual vacuum in European prehistory with regard to low-density and temporary/seasonal sites, both generally, and specifically in terms of urbanization processes. While the Late Iron Age *oppida* are elegantly discussed in these terms (Moore), the Ukrainian and Iberian mega-sites seem to have become embedded in the conceptual problem of accepting that less permanent sites could be urban, despite historical analogues found in the mobile capitals of Medieval Ethiopia (Pankhurst 1979). The third critical point is the extent to which the lack of visible institutions and settlement hierarchization can be accepted as secure evidence for the lack of social stratification (Birch 2013). This approach leads in one direction which we have hardly touched upon as yet—can there be egalitarian cities? The existence of materialized hierarchy is crucial for archaeological arguments used to support socio-historical narratives of human development. But the inference that a social hierarchy which is not conspicuously materialized thereby does not exist is ontologically flawed, even if to some degree epistemologically correct (you cannot study what is not there). One way around this problem is the critical assessment of the archaeological evidence for practices aimed at solving social tensions and regulating corporate, communal and household interrelations. In this way, the communities of Catalhöyük, Nebelivka and Valencina de la Concepción might have been much more stratified than we are currently led to believe.

In 2007, Robin Osborne claimed that ‘urbanization has become a somewhat unfashionable topic’, unaware that, within a decade, there would be a surge of renewed interest in the field. If we are to transcend the mere accumulation of new data about new kinds of cities—low- or high-density, industrial or even hunter-gatherer—we need to make some hard theoretical choices about the direction of travel. This selection of papers suggests that the relational route holds out strong hopes for progress in future urban studies.

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