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Whenever I read an abstract that decries the under theorization of anything and states its intention to enhance debate, I mentally prepare myself for a heavy dose of criticism of existing scholarship without much contribution towards new models. While there are a few ‘straw men’ lurking in Gaydarska’s article, her points of emphasis are important, not just for her struggle to understand the settlement and society of the Cucuteni-Trypillia but much more broadly. I am reacting through the lens of early urbanism (if I dare use that term) in Mesopotamia, because it is implicated as one of the ‘pioneering’ urban traditions, but also because it is the case study which I am most qualified to discuss. I believe that Gaydarska’s critique applies not only to ‘non-classic’ urban traditions such as her Trypillia case, but, on closer inspection, to core ones like early Mesopotamia.

I do not see the field to be in such dire straits as does Gaydarska, however. The trait-based approaches that she critiques (e.g. Childe 1950) have largely been abandoned, even in the scholarly traditions where they appear to successfully describe early cities (although Childe was really describing early states, despite the title). Scholars do dwell on features such as ‘monumentality’, but use it as the empirical basis for approaching social structure via questions of labour management, consensus and coercion (see, e.g., Osborne 2014). I wholly agree with Gaydarska on the utility of George

Cowgill’s multivariate approach (2004) to urban sites, and that it should be more widely employed. I do not regard it as ‘overlooked’, however. Google Scholar shows almost 100 citations, which is high by the standards of archaeological publications. Gaydarska is also overly sceptical of archaeological survey methods. I understand that the interpretative leaps between surface artefacts and ancient settlements can be large, but I know of few landscape archaeologists who do not give serious consideration to the patterns of production, consumption, discard and taphonomy that intervene. These interpretative issues are indeed serious, but they pertain to any archaeological assemblage, whether from survey or from excavation. After all, nearly all artefacts begin their journey to us as objects on the surface. Finally, I am uncertain why ‘big data’ (better called data science) represents a point of concern; it is rare to read arguments for smaller datasets in archaeological research. Part of Gaydarska’s concern may stem from the nascent interpretative frameworks (‘under-theorization’) for massive datasets, but I suspect that resistance might also come from the fact that this new approach is being applied to the ‘traditional’ regions for which large datasets are available (e.g. Menze and Ur 2012, Lawrence and Wilkinson 2015), instead of emerging regions like Gaydarska’s Romanian-Ukrainian case. If survey was the great methodological innovation of the late 20th century (Redman

1982), data science is poised to be the great leap of the early 21st (Bevan 2015).

We are indeed struggling against the semantic challenges imposed on us by language, especially by the academic prevalence of English, and I concur with Gaydarska's interest in indigenous terminology as a window into emic views of urban life. In the Near East, for example, the close study of household terminology in Sumerian, Akkadian and other cuneiform languages has provided insight into social structures in cities, with surprising divergence from the bureaucratic language that modern scholars use to describe these early 'states' (Schloen 2001, Ur 2014). In other cases, this emic approach is less satisfying. The Sumerian term *uru* and the Akkadian term *ālum* are both frequently translated as 'city', but they carried no connotation about size to their speakers; *ālum* could refer to a simple farmstead or to the Assyrian imperial capital of Nineveh (Van de Mieroop 1997, p. 10). Does this mean that all human settlements were conceptually equivalent in the minds of the speakers of these languages? Clearly, some places were so significant to the Sumerians, Babylonians and Assyrians that they felt compelled to stay in them, generation after generation, despite the economic, health and probably social drawbacks to residency. Sumerian and Akkadian terminology in itself can tell us nothing about why one settlement grew into Uruk, but its immediate neighbour suffered repeated fission and never exceeded a dozen families.

Although I am more optimistic about the state of archaeological research on cities than is Gaydarska, her critique is well applied to what she calls 'pioneering' cities. Our models are challenged by these phases of initial population aggregation, before there were other known templates of large-scale communal living beyond the mountains, or on the other side of the sea, distant but not beyond knowledge. Not all of these early experiments were successful, although we are blinded by the few durable successes that reproduced themselves. In fact, I find these 'failures' (if one can use such a term to describe settlements that persisted for

generations) to be just as interesting as the successes. Çatal Höyük was certainly one early experiment that did not result in a subsequent Central Anatolian tradition of urban life. Recent field research in Mesopotamia is now finding such early experimentation in the heartland of one of the great early urban traditions. One site, Khirbat al-Fakhar, is a vast scatter of artefacts and low mounding over some 300 ha in northeastern Syria (Al Quntar *et al.* 2011). In the late 5th millennium, it was roughly contemporary with the earliest Trypillia mega-sites, but predated Uruk by almost a millennium. It may have persisted for a century or two. Slightly later and only 80 kilometres away, at the start of the fourth millennium BC, people began to immigrate to the site of Tell Brak. Some moved into the core of the settlement, but satellite communities formed simultaneously at distances of 300–500 m, deliberately leaving space between them and the earlier core. In aggregate, these communities amounted to some 55 hectares at a time when none of their contemporaries exceeded 5 ha (Ur *et al.* 2007). Both of these 'proto-urban' settlements are very different in form (and probably social structure) from later Mesopotamian cities, but they were also different from each other. They do share one common feature, a low-density pattern of occupation in which vacant space may have delineated social boundaries (Ur 2010, pp. 393–395). In other words, some motivation was drawing communities together before they shared a social institution capable of ameliorating the social conflict that comes with dense aggregation. Five centuries later, such institutions had come into being, and the classic high-density 'Mesopotamian city' emerged at Uruk. Five millennia after that event, Childe penned his 'urban revolution' study (1950), and Uruk, which we now know to be at the end of a millennium of urban experimentation, became the canonical form of the early city globally, much to our current frustration.

We must be very careful, therefore, not to use the forms and functions of mature cities, in some cases millennia after their appearance, to impute the motivations of their creators (here I

think I am following Gaydarska's somewhat cryptic call for 'different agencies responsible for the emergence of pioneering cities and cities that appear and function in a realm where people creatively refer to memory, primary knowledge or a recurrent experience of an urban way of life'). How can potential urban residents choose to immigrate to a nascent city for functional reasons, when cities have yet to grow into those functions? This sort of teleological thinking pervades scholarship on pioneering cities. To avoid this trap, it is helpful to think about how the agency of these migrants might have inadvertently resulted in the settlement forms that we call cities. Within an interpretative framework taken from the sciences of complexity (Kohler 2012), and particularly the concept of emergence (Goldstein 1999), I have tried to explain how a simple and decidedly non-revolutionary shift in the structure of the early Mesopotamian household could result in an unintended population aggregation where previously none had existed (Ur 2014). This 'initial urbanism by accident' approach emphasizes the cumulative decision making of all residents, rather than one or a few Gilgameshes, who were able somehow to gaze into the future to recognize the social and economic advantages of a then-nonexistent settlement form. But if these early aggregations did not yet have the functions that we associate with mature urbanism, are we correct to call them 'cities'? With this issue, I struggle as much with the supposed classical models of the first cities in Mesopotamia, as does Gaydarska with the Trypillia phenomena.

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