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Isotopes and ideograms: Bio-archaeological and theoretical approaches to pastoralism in light of the Mari (and other) texts

*Anne Porter**

Abstract

The fundamental issue obscuring our understanding of the place of animal husbandry in the societies and economies of the ancient Near East remains definitional. The continual conflation of the term “mobile pastoralism” with politically and socially independent pastoralism has reached a point where mobile pastoralism is claimed never to have existed. Sheep and goat recovered from settlements that show evidence for foddering, or grazing on cultivated crops, is argued as proof of a lack of mobility, but this fails to take into account whether these animals are used for meat and milk, sacrifice, or their wool. It also does not distinguish between those animals kept for daily use by individual households and institutionally owned animals. The textual evidence from Mari, which describes mobile pastoralism, is argued to be anomalous. But, in fact, Mari pastoralism is not independent pastoralism either. It is, however, mobile and long-distance, as well as variable according to the particular pastoralist group under discussion. This paper argues that a wide range of textual material, of which the Mari archive is but one source, shows that multiple kinds of pastoralism coexisted in the ancient Near East, including multiple kinds of mobile, or distance, pastoralism, as distinct from independent pastoralism.

Key-Words: mobile pastoralism; independent pastoralism; Mari archive; animal sacrifice; Ebla

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Isótopos e ideogramas: perspectivas bio-arquológicas y teóricas para el estudio del pastoralismo a la luz de los textos de Mari y otros

Resumen

La cuestión fundamental que oscurece nuestra comprensión del lugar de la cría de animales en las sociedades y economías del antiguo Cercano Oriente sigue siendo definitiva. La combinación del término "pastoralismo móvil con el pastoralismo político y socialmente independiente ha llegado a un punto en el que se afirma que el pastoralismo móvil nunca existió. Las ovejas y cabras recuperadas de los asentamientos que muestran evidencia de forraje o pastoreo sobre los cultivos se toman como pruebas de la ausencia de movilidad, pero esto no considera si estos animales se usaban por su carne y leche, para sacrificio o por su lana. Tampoco se distingue entre los animales criados para uso diario por hogares individuales y animales de propiedad institucional. Se argumenta que la evidencia textual de Mari, que describe el pastoralismo móvil, es anómala. Pero, de hecho, el pastoralismo de Mari tampoco es pastoralismo independiente: se trata de un pastoralismo móvil y de larga distancia, así como variable según el grupo pastoral particular en discusión. Este artículo argumenta que una amplia gama de material textual, del cual el archivo Mari es solo una fuente, muestra que coexistieron múltiples tipos de pastoreo en el antiguo Cercano Oriente, incluidos múltiples tipos de pastoralismo móvil o a distancia, a diferencia del pastoralismo independiente.

Palabras claves: pastoralismo móvil; pastoralismo independiente; archivos de Mari; sacrificios animales; Ebla

Introduction

The recent upsurge of interest in pastoralism in the Near East and beyond (Potts 2014; Wilkinson et al. 2014; Hammer 2014; Honeychurch 2014; Makarewicz and Sealy 2015; Schou 2015; Rosen 2016; D’Anna and Palumbi 2017; Hammer and Arbuckle 2017; Makarewicz 2017; Miller and Makarewicz 2018; Chazin et al. 2019; Arbuckle and Hammer 2019; Nugent 2019) is both encouraging and somewhat disquieting. The methodological turn embraced by several of these endeavors offers tests of a topic that has long rested on theoretical constructs, circumstantial evidence and corresponding inference. This turn is obviously to be desired. The outcome of some of this work though, is to dismiss the very existence of mobile pastoralism in the ancient Near East. Closer analysis of arguments to this end shows that some interpretations of these tests rest on their own flawed assumptions as well as many of the same old atheoretical constructs as before. Moreover, as acknowledged by practitioners, techniques such as isotopic analyses are not in and of themselves a magic wand that will solve all our problems. Availability of appropriately detailed isotopic reference sets is but one problem (Miller and Makarewicz 2018). On a finer scale, it can be difficult to distinguish between human actions on water, for example, which affects Oxygen ratios, from climatic ones (Nugent 2019: 134).

Particularly disturbing though is the accompanying trend of dismissing the utility of textual evidence (e.g. Potts 2014; Rosen 2016). The result of this, especially for the purposes of this volume of *Claroescuro*, is the marginalization specifically of the Mari archive and the world it so vividly portrays (e.g. Burke 2014, 2017). At best this world is seen as belonging to a unique moment in time, the product of circumstances not duplicated either before or after the first half of the second millennium. At worst, it is simply ignored. This to me is both a failure of imagination and of understanding.

The very use of the word “imagination” will of course bring forth the obvious retort that this is what has been wrong all along: previous work was based on unscientific, largely imagined scenarios inappropriately drawn from ethnographic analogy and textual evidence. And I concur wholeheartedly. But I am also aware that if we cannot imagine multiple possibilities for any archaeological scenario, we cannot test for them. We cannot hold those possibilities up against our own assumptions to query the latter’s validity. I am struck by the fact that much of the current discussion ignores the very possibility of multiple possibilities. It has come down to an either/or situation (as for example Potts 2014; Glatz and Casana 2016: 144): did

long-distance pastoralism, defined variously as mobile or specialized, exist in the ancient world, or is all animal husbandry based in locally grazed, settlement-controlled herds? The textual evidence indicates that there is far more involved than this simplistic choice. At the very least it helps us to construct possibilities.

The Possibilities of Pastoralisms

It was the following passage from Arbuckle and Hammer (2019: 397) that initiated these reflections:

“Texts, however, are problematic since they were written from the narrow perspectives of urban elites and institutions, and it is often difficult to assess the economic and social role of pastoralism within society from these sources (Porter 2012, p. 294; Vanstiphout 2000).”

While it is certainly true that texts, or at least the texts thus far recovered, derive from urban contexts, they may still shed considerable light on the multiple significances, organizations and practices of pastoralism in the ancient Near East. My own discussion on the cited page was specifically in reference to *literary* texts, which I consider best approached from the perspective of literary theory rather than history. Elsewhere in *Mobile Pastoralism* (2012) I add royal inscriptions to this material because it is difficult to take at face value the self-presentation of ruling bodies who deploy history in their own interests. But there are vast numbers of texts, of different kinds, that do not suffer from such limitations. Administrative texts, legal documents, and letters are some such. From the household accounts of mid-third millennium Tell Beydar; records of the holdings of the occupants of Ebla Palace G in the later third millennium; to the extensive discussions and descriptions of pastoralist activities in the Mari letters of the second quarter of the second millennium; we have perhaps the most direct evidence from Syrian documents. The most copious southern Mesopotamian sources date primarily from the end of the third millennium (from the über-bureaucrats of the Third Dynasty of Ur) and from temple archives of the first millennium such as those of the Eanna temple at Uruk (van Driel and Nemet-Nejat 1994). They include documents as diverse as records of wool plucking to the trials for embezzlement of the manager of herding subcontractors, Gimillu (Holtz 2014). But information on textile production, and indeed the importance

of sheep, date as early as the late fourth millennium. Some of the very first tablets denote the offering of sheep to Inanna. A comprehensive list of publications on all these texts is provided by Becker et al. (2016: 112). I would not necessarily disagree that long distance pastoralism does not exist prior to the new geo-political world created by urbanism. Because my concern is with the role of texts in such discussions, I of necessity confine the following discussion to the mid-fourth millennium onwards. Information must be extracted from archaeological sources through deduction and inference, as it does from written sources, and written sources must be appropriately theorized, as archaeological sources must. Just because these documents are all from sedentary, and mostly institutional, contexts, and just because we seemingly have no texts deriving from mobile groups (although of course we do, in the Mari letters, [contra Rosen 2016: 61] see below), does not mean that they contain no information about mobile pastoralism across several millennia. To assume that this kind of documentation is inapplicable to pastoralism is to assume, still, that urban environments, both palatial and non-palatial, are completely divorced from pastoralist worlds and activities. And this is the problem. Although not specified, in many discussions, “mobile”, and especially “long-distance,” pastoralism is continually conflated with politically and socially independent, and more often than not, tribally organized, pastoralism. This is the Bedouin paradigm that we all, rightly, bemoan as inapplicable to the ancient world. And yet that is the very paradigm that is tested for (e.g. Potts’s definition of pastoral nomadism [2014: 2-3]), and its absence in settlement records is taken as proof that only locally herded systems of animal management existed.

This contrast is iterated across the chronological periods examined by Arbuckle and Hammer (2019), but just one example suffices to demonstrate some of the problems implicit in this approach.

“Although the production of wool sheep during the Uruk period is uncontroversial given textual references, there is virtually no direct evidence for Uruk pastoral mobility, and most data strongly link herding to agricultural settlements. At Sarafabad in western Iran, faunal remains from a large Uruk pit are interpreted to represent alternating layers of summer and winter deposits, confirming that herds were present in the vicinity of this Uruk “rural center” throughout the year (Wright et al. 1980). Furthermore, at indigenous LC Kurban and Haçinebi the ratio of wild seeds to cereals indicates that herds were generally foddered and/or grazed on harvested fields rather than steppe

pastures (Miller and Marston 2012). This supports the interpretation that the animals consumed at these sites were herded locally in the Euphrates Valley” (Arbuckle and Hammer 2019: 414)

There are several points here that warrant further consideration. The first is that the concept of mobility does not contain any implication of distance. It might be near or far, vertical or horizontal¹. The second is that pastoral mobility is not necessarily *un*-linked to agricultural settlements. To claim that it is, is to perpetuate the assumption that mobility equals autonomy and separation. Köhler-Rollefson’s (1992) term “tethered pastoralism” can be taken from its evolutionary, segregating, context (Arbuckle and Hammer 2019: 404) to denote the kind of kin and or ancestral relationship between settlement and pastoralist described in *Mobile Pastoralism* (Porter 2012), where distance is primarily relevant in terms of potential socio-political attenuation and the means taken to address it. This kind of relationship is attested in the Mari archive as Arbuckle and Hammer acknowledge (e.g. 2019: 423), but ultimately sideline, and is by now so thoroughly delineated (Durand 1992, 2004; Charpin 2004; Fleming 2004) that it needs little rehearsal here. Earlier descriptions of fundamentally oppositional relations between mobile and sedentary members of the Mari kingdom have been replaced by more detailed, and consequently, more nuanced, treatments. Kin relations cut across divisions of subsistence practice, class, and locality. Pastoralist movements are extensive and traditional, but responsive to changing geopolitical and environmental circumstances. And, importantly for this discussion, they are perceived as having historical depth. A well-known example that captures both historical depth and changing political circumstances is letter A2730 to Zimri-Lim from Ibal-el, a pastoralist, and not just any pastoralist, but a chief of pasture. Ibal-el writes

“Like the land of Yamhad, that of Qatna, and that of Amur-rum, are the *nighum* of the Yaminites, and in this country the Yaminites have watered and grazed their flocks, well! since the dawn of time, the *nighum* of the pastoralists² is Ida Maraṣ. What

¹I am not sure when “mobile” pastoralism became synonymous with “long distance” pastoralism. The very point of this term was indeed to allow for many different kinds of movement, in distinction to “nomadism.”

²Durand translates the word I render as pastoralists as “Bedouin”; Sasson prefers “nomads”. Both of these terms are problematic theoretically. “Pastoralist” is the most neutral of these designations.

offense have the pastoralists committed towards Ida-Maraş? The happiness of the pastoralists is that of Ida Maraş. Why did the people of Ida Maraş commit an offense against the pastoralists? They slaughtered those who exercised authority in my name. They booted young males and young women, my daughters and those who served me. They put their hands on sheep, cattle and donkeys, herds of my country. Did I commit a fault against him or did I set fire to his crops? My *nighum*, since always, it's him. Why did he do something wrong with me?" (Durand 2004: 121, my translation of the French, see also Sasson 2015: 145-6)

Hyperbolic as it may be, "the dawn of time" clearly indicates a long-term, rather than one-off, relationship. Indeed Durand (2004: 118-121) adduces extensive evidence to define *nighum* as *traditional* grazing land. Another letter, perhaps from the same person (Sasson 2015: 137) describes the actions of the chief of pasture in organizing the herds' movement to different grazing lands. Four places are on offer. Extispicies were performed to test the viability of each place – one proved to be infelicitous (Heimpel 2003: 244-5). Another may have been sanctioned by divination but required the prior agreement of Hammurabi of Kurda, not yet forthcoming.

The meticulous work of the many Mari scholars, including a new generation working in multiple languages, now puts us in a position to map these traditional territories as well as the social/political alliances that facilitate movement across space by groups of different affiliations. This work is simply not given due consideration in archaeological contexts. Indeed, the completely superseded publications of Luke (1965), Rowton (especially discussions of "dimorphic" society, such as Rowton 1976) and Matthews (1978), important as they were in their time, are still cited (e.g. Sołtysiak and Schutkowski 2015: 176; Rosen 2016: 31) when there are now English translations of many letters (Heimpel 2003; Sasson 2015) and extensive discussions of their significance (Fleming 2004; Buccellati 2008; Charpin 2004, 2011; Miglio 2014). Suffice it to say that Mari pastoralists travelled far beyond the 25 km limit to local pastoralism set by Arbuckle and Hammer (2019).

But perhaps the most important thing to take from the Mari letters is that *there is not one kind of mobile pastoralism*, be that defined socially, politically, or in terms of mobility. The Yaminites and Similates, as well as other less frequently mentioned groups such as the Numha and Yamutbal have varying internal social and political organizations, varying relations to

state and settlement, and to each other (Durand 1992; Fleming 2004; Porter 2009, 2012). And too, varying ways in where, when and how they move.

The second point to consider in Arbuckle and Hammer's approach is that systems of local animal management may certainly co-exist with systems of tethered distance management in the same settlement, and that distance management would not *necessarily* result in any measurable frequency of animal remains in those settlements, even if the managers were integral parts of the sedentary system. The assumption that they *would* is based on the idea of the "symbiotic" relationship that dominated the beginning of the revolt against notions of the divided socio-subsistence worlds of "the desert and the sown." That is, that independent pastoralists would have to participate in exchange relationships with sedentary societies to fulfill their needs for agricultural products. Thus, animals grazed in the steppe would always find their way into a settlement, primarily for their meat, obviating the need for small-scale household-based herds.

This "imagined foodscape" (Makarewicz 2018: 142) was a product of its age – the 1970s – when pastoralism was viewed as a monolithic and less than desirable subsistence practice. It also raises issues of how animals were exchanged and distributed throughout the community (Allentuck 2004; Allentuck and Greenfield 2010). Yet even when such a symbiotic relationship can be demonstrated to exist, we need to take into account the fact that the resulting presence of animal remains depends on the purpose for which the animal is exchanged. Texts reveal that not only are there are multiple uses for animals managed in multiple ways, and grazed over multiple kinds of distances, there are multiple kinds of sheep and goat used for these different purposes (eg. Fleming 1992: 135; Van Driel and Nemat Nejdlat 1994: 52; Becker et al. 2016: 117). Among the many uses to which these animals were put, in large quantities, were wool, milk, and meat, but most elements of the carcass were also employed, from hoof to horn (Becker et al. 2016: 103). Bringing animals to a settlement for meat results in a very different material outcome to bringing the hides of dead animals for their horns, or to bringing live animals to town to have their wool plucked. The latter would return to grazing lands. Where they ended up when no longer productive would be shaped by many factors.

An additional concern is that one of the key measures of whether an animal is managed under the auspices of a settlement or by independent pastoralists is based on the proportion of weeds and wild plants to cultivated crops found within settlement remains. If the premise is that animals grazed in the steppe are feeding on wild/weed taxa, while village animals

are foddered or grazing on the remains of harvested crops, which, because they are cultivated, are relatively weed free, then there are several issues. The comparative proportions of steppe-based dung to crop-based dung will, again, depend on the purpose and frequency of bringing animals to town. For example, it takes about 30 hours for goats to accomplish “maximum excretion,” and up to a week for the last little bit to leave the body (goatlink.com). For high proportions of weeds in dung found in settlements to come from animals grazed in the steppe, the animals are returning nightly or weekly to home base. This is not long distance or broad range pastoralism. It is typical of village-based transhumant herding, as demonstrated through isotopic analyses by Nugent (2019: 16) for Qizqala in Azerbaijan³, in a very different landscape to most of the Near East. It is most likely for the purposes of milk, and to a lesser extent, meat. Wool herds need not return to base on such a short term-basis, wool gathering happening infrequently. If annually, when plucking wool would take place because of the molting process (Algaze 2008: 89), then the proportions of wild/weed taxa to crop taxa will be very low – and I might add, just as likely to be found, not in the settlement, but outside it.

It should also be noted that animals fed in the immediate vicinity of the settlement, but not on fodder or field, will ingest just as many weeds as animals grazed in the steppe. It is not a matter of proportion of weeds, then, but what, exactly, are the taxa. Which raises the question: is there a marked differentiation between species growing in fallow fields and uncultivated lands immediately around a site, and those growing further into the steppe, up, say, on the escarpment rimming the Euphrates valley? Moreover, foddering is not confined to the animals husbanded by sedentary, crop cultivators. Mobile pastoralists in a variety of regimes will also fodder animals on feed collected during the summer months (Makarewicz 2018: 146), although this is usually restricted to specific, and generally adverse, situations. It should also be noted that fodder was not confined to cereals. Reeds and bran were also used (Sharlach 2004: 352-3).

Foddering and stubble-grazing are limited feeding strategies for several reasons. As year-round food sources, they cannot possibly accommodate the scale of animal husbandry that texts register and that the scale of textile production indicates must have been in existence. Yes, modern tabulations of these data are highly variable. For example, Milano (1995) estimates Ebla’s’ sheep holdings at c. 670,000 while Archi notes the palace “inspected”

³And see “A Trek through the Accursed Mountains” <https://www.theatlantic.com/video/index/603382/mountain/>

80,000 to 136,000 sheep and goat. Wilkinson et al. (2014: 58) cite Butz's (in Pettinato 1991) calculation of two million animals belonging to Ebla. Nevertheless, Waetzoldt (1972) collated textile records for seven Mesopotamian cities in the Ur III period (end of the third millennium) showed 15,000 workers involved in some aspect of textile production at Lagash alone (Waetzoldt 1987), while some 500,000 sheep/goat could be adduced as the property of Ur, which paid rations to c. 13,000 weavers (Algaze 2008: 82-3) In total we are again looking at millions of animals for southern Mesopotamia at this time, with tens of thousands of attached workers – numbers so immense they have proved hard to accept at face value. But this is as much a product of our own prejudices and ignorance as it is of the ancients. Even the lower figures have been dismissed as unrealistic – exaggerations or mistakes – and yet the accountants of Ebla and the Third dynasty of Ur were meticulous – obsessive – in their record keeping. Although we do know of the odd embezzler within the institutional system of herd management (the aforementioned Gimmillu), to assume these documents are all false is to perpetuate a colonialist approach to ancient societies that surely needs little rebuttal by now. We may not understand the recording system well but that does not mean the system is inaccurate or fanciful.

The upshot is that vast areas of grazing are required to sustain even the lower numbers above. Wilkinson et al. (2014: 58) estimate that at least a radius of 100 km around the city of Ebla would have to be utilized to sustain over half a million sheep. This distance puts us well beyond the arbitrary 25km limit provided by Arbuckle and Hammer (2019: 393) to distinguish local from long distance or “mobile” pastoralism. That radius lengthens, the higher the proportion of goat to sheep. Animals can only feed on the remains of harvested crops at very specific, and for very limited, periods. It takes little time for a sheep/goat to strip a field bare. This means that animals without weeds or wild plants in their dung must have been foddered most of the year – a very expensive and time-consuming undertaking, unlikely to be the default method of animal husbandry. Thousands of animals were foddered under the complex Ur III *bala* system (Sharlach [2004: 352-3] tabulates some monthly foddering texts), and this was very costly, in terms of labor at least. In one text (Gomo-Sato 536), 24 grass carriers were employed to feed 360 sheep at Umma (Sharlach 2004: 32), and these animals were destined for temple sacrifices.

This information raises a critical factor in the interpretation of wild/weed/-crop ratios, and as well, of kill patterns. Evidence for extensive foddering at various points in the archaeological record, in conjunction with information

from the textual record, suggests that as much as the small-scale holdings of private households, we should also be thinking about animals destined for special situations, those of whom excellent condition was desired. An interesting example comes from Mari, where female equids kept for breeding are foddered year-round, but not every day, whereas male equids were not foddered at all (Sallaberger 2014a: 347).

Surveys of the documents describing the constant rituals and festivals every city undertook demonstrate that the single largest venue for the consumption of meat, in any time and place, was “sacrifice.” Although somewhat fuzzy, this term denotes both feeding the gods (and certain others) on a daily basis; the celebration of specific festivals, including those regularly and irregularly enacted; and the practice of divination (Pongratz-Leisten 2012). Where ritual calendars and schedules of sacrifice have been reconstructed (Sallaberger 1993; Viganò 1995; Fleming 2000; Pardee 2002; Sharlach 2004; Archi 2017), it is clear that this is a virtually endless activity involving huge numbers of animals.

At Ebla, not only are sacrifices offered to gods in their respective temples and to ancestors in the palace (Archi 2017), including the ancestors of others than the Ebla dynasty (such as Armi, [Archi 1982: 205-6]), meals are also provided for the royal household and its “elders” – some 40 to 50 of them (Archi 2017: 304). While there is no doubt that sacrificial offerings were redistributed among many human dependents (eg Fleming 1992: 134; Sallaberger 2014a; Sasson 2015: 249, 5.3.f.i.1), often serving thus as food at the same time, Archi (1982) calculated on the basis of the Ebla accounts a monthly total of as many as 4500 sheep used for cultic purposes. That this level of sacrifice was considered a financial burden is evident in a letter from Mari. Samsi-Addu writes to his son Yasmaḥ-Addu (letter A.3609):

“Why would you commission the making of six gods? These gods that you plan to make require one month of festival (sacrifices). What!? – where are the oxen and sheep you must keep providing for sacrifice at festivals? Here, you keep writing to me about oxen and sheep, saying “I have no sheep or lambs!” Yet you would still fill the town with gods here, when however many sheep now available hardly suffice for sacrifice to them. . . Mari is full of gods. No other city is as full of gods as is Mari. . . even Assur.” (Sasson 2015: 250, 5.4.a.i)

At Emar in the later second millennium, directions for, or records of, special events as well as the regular cycle of offerings, also show the large quantities

of meat involved, as well as who should provide what animal or animal part, and to whom it is offered (see for example Fleming 1992). At the Zukru festival, the entire town leaves the city to participate with the gods in a feast (Fleming 2000). In addition to the food provided for that event, sacrifices are offered by the king and the city to the participating gods before and after the feast. By my count, the king alone offers one calf and ten lambs to 21 gods and other entities, all listed separately, five lambs to 16 additional entities, and two lambs to another 49 entities for a total (excluding the cattle) of 368 sheep. And this seems to be repeated each day for seven days, resulting in the sacrifice of 2576 sheep (Fleming 2000: 243-47). Another 70 lambs are offered on the fourteenth day of the festival (Fleming 2000: 249-50).

Emar is a small town, and its ritual offerings no doubt insignificant compared to those of the Third Dynasty of Ur as evidenced both in the latter's richly documented cultic calendar (Sallaberger 1993), and in the expenditures of the bala system (Sharlach 2004). These somewhat random examples, drawn from the mid- to late third millennium and the late second millennium, serve to demonstrate the extraordinary scale of ritual animal slaughter that must have taken place if we extrapolate to all the rituals in all the temples and palaces of all the major settlements in greater Mesopotamia. While specific numbers of course will fluctuate over time, it is quite clear that basic sacrificial practices did not. And this is in addition to the quantities of animals needed to sustain the textile output of major institutions. Of course some of these animals might perform one and the same function, with no-longer-productive wool animals killed in ritual contexts. But the vast majority of animals designated for sacrifice are lambs.

In sum, the number of animals required simply cannot be accommodated by management practices local to settlements without such severe and rapid degradation of the immediate landscape that those settlements would soon collapse. But my point, in fact, is not this. It is rather that failure to consider the different uses to which animals are put results in a failure to consider both the different forms of management those uses may assume, and their different outcomes in the archeological record⁴. The majority of foddered animals are most likely destined to be sacrificed, and sacrificed animals are more likely to be young, male animals, although older, and female, animals are sometimes specified. To suggest then that animal bones

⁴Although the scope of this paper does not allow for this, all these issues can be addressed through further studies such as Waetzold 1972. See Lev-Tov and McGeogh 2007. I look forward to future work from Topoi (Becker et al. 2016).

recovered from settlements that do not show the morphology or isotopes of long distance movement disproves the existence of distance pastoralism, is to misunderstand the many kinds of choices people may have made. When isotopic and other archaeometric analyses defy expectation it is not always the expectations that are wrong. It may also be the interpretation of the analyses.

Two examples suffice here. Arbuckle and Hammer (2019: 423) suggest that a decrease in grazing around Umm al Marra in the second millennium (Miller 1996) indicates pastoralism was not as extensive as Mari texts have been taken to show. In fact, a decrease of grazing around Umm al Marra shows only that pastoralists are not exploiting this area at this time. They may, however, be grazing but a few kilometers away. Pastoralists perforce move. It might also show that consumption practices have changed as a result of, or causing, this shift, and both possibilities may be a result of political changes – clearly manifest in the other kinds of remains at Umm al Marra from the third to second millennia (Schwartz 2013). Similarly, Soltysiak and Schutkowski (2015: 176) “hypothesize that the relative proportion of animal-derived food is higher in times when mobile herders dominate and the size of the sedentary agricultural population decreases”, and further (p. 183). “it should be expected that mobile herders who, according to historical sources, dominated the region relied more on animal-related food and, moreover, fed their flocks in areas more abundant in C4 plants.” This was not the case at the expected time, the first half of the second millennium. But to think that it would be is to assume that those herders died and were buried at Tell Barri, the source of the materials analyzed. If derived from primary burials, then those herders died on site. If from secondary burials, then they died elsewhere and were brought to the site for re-interment at a later time. Thus the key issue is the nature of the burial.

The animals used for wool, milk, and meat, or only for breeding, could be foddered and/or pastured; pastures might be located in the immediate vicinity of the site; locally; further away; long distance, and the same animal might graze in more than one of these places in the course of its life. Quotidian meat animals – those whose bones would be most readily recovered – may well be locally grazed; sacrificial animals may well be foddered; wool animals might be driven from a distance to be plucked by their temple owners, and then taken back to pasture, to be eliminated there at the end of their useful life. Herds ranging from the few to the thousands could be managed by: sedentary families (especially utilizing children); mobile families; or professional herders. Institutionally owned herds were managed very

differently from household herds, and temple herds were managed differently to ones owned by palaces, and all of these elements vary over time and place (see also Wilkinson et al. 2014: 56). Mesopotamian evidence shows that temple animals could be outsourced on small and large scales, while wool is supplied to Ebla through a complex network of relationships (for more on this see below), but animals belonging directly to the Ebla palace are distributed among family members located in the countryside (Archi 1990, 1992).

If owned by an institution, the carcass of a dead sheep or goat would then be brought back to prove that the animal had not been misappropriated (Liverani and Heimpel 1995) – because pilfering certainly could be a problem (Holtz 2014: 147-71). Contra Adams (2006: 153-4), there is no reason to assume that returning carcasses could only happen if flocks were pastured nearby the temple. Although the following reference is not related to this issue, an Ur III document (ITT3 6128) mentions a boat loaded with hides sent to Nippur (Dahl 2006: 84). Bulk collection and shipment of cured hides of these dead animals is certainly a feasible option, and depending on accounting practices, could be sent at specified times of the year. Or the wool might be plucked in the field, and transported in the covered wagons that were in existence in the third millennium (Porter 2015). Milk animals might well be locally grazed, but it is also possible that milk was turned into dried lumps of yoghurt or cheese and transported across considerable distances.

Such possibilities have to be imagined before they can be tested. Potts (2014) and Arbuckle and Hammer (2019) are specifically examining sites that have previously been assumed to be related to pastoralist activities on the grounds of location or general architectural scrappiness (and for further discussion see Potts 2014: 42-3). In addition to their own claim that the animal remains from these sites do not come from “mobile” pastoralists, there are two other ways of considering the evidence. One is that indeed, these are not the sites of pastoralists, but that certainly does not mean that there are no pastoralist sites! In arguing that we should find the settlements/encampments of mobile pastoralists, the ground-breaking work of Robert Cribb (1991) is cited (Arbuckle and Hammer 2019: 396). But Cribb’s study is based on the observation of contemporary pastoralists in Anatolia, an ethnoarchaeological approach that the authors have elsewhere decried. There is little other substantive discussion of what constitutes a pastoralist camp (for an exception see Rosen 2016). We cannot therefore know if ancient distance pastoralists did leave recoverable remains, or if we

are looking in the right places for the right kind of remains. As Wilkinson (2003: 50) notes, “it is still extremely difficult to recognize, estimate, and date these [pastoralist sites] with any precision” (and cited in both Potts 2014: 7 and Burke 2017: 275 along with a quotation from Bacon 1954: 44). But we can think about the many possibilities such questions raise.

The second consideration is that many of these sites may be, in one way or another, integrated with, or linked to larger polities, for whom they are providing foddered animals, or indeed, practicing their own cultic sacrifices. And models for this second scenario are to be found in the Mari texts. My own knowledge of the Mari texts is insufficient to know whether details of herd management are just not recorded – or just not published. I suspect the former because they are hardly the concern of the correspondents, whose interests are mainly political rather than economic. But the movements of pastoralists within and beyond the Mari kingdom *is* a concern, and within these discussions some valuable information is revealed. While the geography of mobility in the Mari kingdom is registered by intersections with various kingdoms, far humbler places in the landscape are also demarcated by names such as “encampment” “well” “place of the tombs” and “ruin” (Charpin 2003; Porter 2009). Humble some of these places may be, but they are nevertheless integral to the state.

There seem to be four places called Der (encampment) attested in the Mari archives (Durand 2004: 125-8), all related to Simalite pasturage (Porter 2009: 202-8). Simalites, as the people of the last king of Mari, have particular, but complex, ties to the state. The two Ders that concern us here are: one, Der south of Mari; and two, a place presumably much further north of Mari, called by Mari scholars “Balikh” Der⁵. There is no evidence that either were much more than their name suggests,⁶ although they were permanent places. But they were central to the definition of the state, if state is vested in palace and king. Both were assembly points for the mobile component of the Mari kingdom, and were places where a range of rituals were performed. Some of these rituals required the attendance of tribal leaders and kings related, or subordinate, to the kings of Mari from Yaggid-Lim to Zimri-Lim. Donkey sacrifices establishing or renewing socially constructed

⁵But not actually on that river (Durand 2004: 126). Durand (2011: 160) acknowledges the possibility that the Mari Der and Balikh Der are one and the same place. I consider this unlikely for a number of reasons, but it is beyond the scope of the paper to address this issue.

⁶Notwithstanding Heimpel’s (2003: 376) reconstruction of the word “palace” in a letter mentioning work done at Der (letter 26 455).

kinship between these rulers were performed (Durand and Guichard 1997: 39-41; Porter 2009: 205-9)⁷. Some involved the attendance of the king to the kingdom's goddess, not coincidentally named Deritum; some required commemoration of the dead; some were for mobile peoples alone.

The two Ders are both likely located in what Wilkinson et al. (2014) call "the zone of uncertainty". Given the state of our current knowledge of these areas, there is little that distinguishes them as "cities" (Durand 2011: 160, n. 16), for they have not yet been identified archaeologically. From the information at hand, it is possible that the Balikh Der is well within the Western Habur, and while it might once have been a *Kranzhugel* of the third millennium, we should not forget Amûd-Pâ-El's description of the city of Asnakkum (in letter A.2434): "I keep an empty city, a (simple) bivouac" (Guichard 2009). Guichard goes on to comment: "the image of the 'camp' also reveals that the walls abandoned by the townspeople only served to protect the Bedouins passing through." The second Der is near Mari but south of it. This area becomes increasingly arid, with an increasingly narrow river valley of limited potential for agricultural production to sustain a second, large, settlement.

That significant rituals could be performed in insubstantial places is reinforced by reference to the storm god Addu designated as *ša maḥanim*, a deity significant enough for Zimri-lim to register a year-name after him. *Maḥanim* is understood by Durand (2011; see also Sasson 2015: 249, n. 45) also to mean camp, such as letter A.1191, "All the pastoralists met in the *mahanim*". Addu *ša maḥanim* would therefore be the storm god of the mobile camp, such as letter A.1005, "the next day, the pastoralists will make the sacrifice in the *maḥanim* to the god of the Storm (Addu)." (Durand 2011: 159). Furthermore, letter A. 808, from Ibâl-pî-El to Zimri-Lim states: "I have read the tablet that my lord made me carry; he wrote to me to offer to the encampment the throne of the Storm God" (Durand 2011: 160)⁸. There is no indication of the number of animals deployed in the political and religious ceremonies of the two Ders or the *maḥanim*, but it is impossible to imagine that quantities of sheep and goat were not consumed

⁷It is interesting too that the meat and other materials from the sacrifices of one place are shared with the kings of other places (Durand and Guichard 1997: see Sasson 2015: 249 for English translations of relevant letters). I have discussed the significance of sharing food in many contexts. For the most recent, see Porter 2016.

⁸A precious object made in the Mari workshops (Durand 2011: 160). This was a common practice of Zimri-Lim (Sasson 2015: 252). Any misunderstandings in these translations of the French are my own.

in feasts and sacrifices accompanying the various rituals that integrated the constituents of the kingdom and its networks of political allies providing collateral territorial access.

If nothing else, the Mari letters indicate the paucity of archaeological reconstructions that rely on monumentalism as an equivalence for significance, for significance is vested in *what* people do as much as it is in *where* they do it and in what kind of built environments they do it in. And those are, indeed, variable. They also reveal the complexity and diversity of social relationships, practices of mobility, and the political and ritual strategies that arise around them.

Contextualizing Mari pastoralism

It remains then to query whether the situation at Mari does in fact represent a unique moment in the cultural and political history of the Near East as is the current trend. One school of thought argues that Mari pastoralism was created at the end of the third millennium because climate change forced sedentary farmers into an alternate mode of existence (Weiss 2014). Another argues that Mari pastoralism was a purely marginal and local phenomenon that was little changed from previous practices (Burke 2017). Both positions are entangled in the Amorite question, where the perspective on pastoralism is a tool to argue for who the Amorites were and from whence they came. It is therefore difficult to find any depth of understanding about pastoralism itself, largely because few writers actually pay any attention to the Mari evidence. But what these discussions do reveal is the same monolithic understanding of pastoralism, and the same conflation of distance pastoralism with independent pastoralism, as outlined above.

If proving the existence of mobile pastoralism outside the kingdom of Mari is to be dependent on finding identical social and political relationships and practices of mobility, as revealed in the Mari letters, then yes, there is no mobile pastoralism outside this time and place. But that of course is a ludicrous position, if only because we have no other source remotely like the Mari letters from any other time and place. It goes beyond our written sources however, and our archaeological evidence. This is a failure, not just of imagination, but of theoretical position. It is a failure to recognize that “mobile” pastoralism takes more than one form. This I think is in large part due to the power of the ethnographic example set so thoroughly by anthropology in the 1970s and 80s (e.g. Salzman 1972; Dyson Hudson and

Dysin-Hudson 1980; Spooner 1973) and dominated by studies of Iranian pastoralists. An essentialist view of pastoralism often pervades the very work of those that decry it.

But more than this, it is a failure to understand the nature of contingency. Existence is in a constant state of flux and fragility (see also Rosen 2016: 27). Political relationships are fragile, environment and climate are fragile and inconstant. Agency is, by very definition, variable. Potential and constraint shift all the time. Decades of deriving single-path explanations for topics such as origin of city and state have accustomed us to assume all polities in areas defined by academic boundaries are essentially the same, having come into being the same way. This they are not (Porter 2010). At any given moment, the specific combination of politics, climate, agency, and myriad other factors, will vary. Third millennium Ebla is not the same as third millennium Mari, even though they were in constant interaction; third millennium Mari is not the same as second millennium Mari – and they cannot be.

So instead each situation must be examined for what it is; rational parameters for what exactly is the target, must be set. And that target is simply distance pastoralism. Distance pastoralism might or might not be independent – I have argued, rather extensively, that independent pastoralism is *not* a feature of the ancient Near East but that distance pastoralism is (Porter 2012) – and distances as well as strategies will vary. Nor is defining kinds of animal management by specific distance allocations likely to reflect the way ancient pastoralists thought about, and acted on, their choices. Time – a day, more than one day, many days – type – fields, scrub, mountains – and place – defined by kin group or kingdom – are all relevant considerations. In the geo-political world of the Mari texts, it is undoubtedly place that prevails. But the geopolitical world of Mari is unique, even if the distances pastoralists travel in that world are not.

If seeking to understand “the rich diversity in pastoralist subsistence strategies and mobility [that] are key to understanding broader processes associated with the emergence and evolution of pastoralist social organization, ideological systems, and political formations” (Miller and Makarewicz 2018: 1) without getting entangled in endless definitions, then the question should be expressed very simply. Does the kind of pastoralism that does not return to settlement on a short-term basis exist outside the Mari letters? Both direct and indirect evidence tells us it does. Because I have discussed much of this evidence in detail elsewhere, as well as theoretical approaches to it (Porter 2009, 2012), I will but summarize a few salient points. My

focus is the north, rather than the south, for the simple reason that this is where maximum and optimal grazing lands can be found.

To date, the argument for distance pastoralism in the fourth millennium remains circumstantial. Wool is already a temple industry, and a seemingly large-scale one, in Southern Mesopotamia, by the mid to late fourth millennium (Algaze 2008: 82-92). Cylinder seals from this period show animals as an integral part of the world controlled by the temple, and people presumed to be slaves engaged in textile production (Jarmer Scott 2018). Other seals portray a specially-garbed individual, referred to as the priest-king, caring for sheep and cattle (Pittman 2013). While these last images might be assumed to represent foddering, it should be noted that the figure is often depicted extending not grain, but branches of a tree or bush to the animals. Indeed, the written and visual portrayal of kingship is firmly wrapped in the image of the “shepherd” from earliest times. That this imagery appears at this time is hardly coincidental. It does not speak to the extent of animal husbandry of course, but it does speak to its ideological significance, a significance firmly tied to the state. At the same time, the first written documents appear. These were often simple records of animal offerings to the gods – UDU AN INANNA – indicating that such contributions were sufficiently numerous and/or important that they warranted this new means of registration.

Industrializing wool production, institutionalizing religious practices requiring frequent sacrifices, in conjunction with expanding settlement numbers and sizes, and expanding cultivation to support those burgeoning settlements, entailed a concomitant expansion of herds and their grazing lands. We know little of this process. Different herd sizes, with animals of varying purpose, could use different kinds of spaces, travelling over varying distances. That distance was no impediment to southern Mesopotamian economic activities, one way or another, is universally accepted. There is nothing to suggest that the people who managed these herds were independent of the new polities of southern Mesopotamia.

Beginning at this time, and continuing into the first half of the third millennium, complex and often large-scale settlements were established in areas ill-suited for extensive cereal cultivation. In the earlier period, settlements such as Habuba Kabira were located in the Middle Euphrates immediately adjacent to vast areas of steppe grazing, with enough river valley to provide agriculture for subsistence. At the beginning of the third millennium, after the end of these settlements, Tell Chuera was established in the east of the Balikh, a region far less agriculturally productive than the river valley, even

if substantially different climatic conditions prevailed at that time. Whatever the climate, the main affordance of this area was pasture (Wilkinson et al 2014: 53).

Yet neither site shows any evidence of a *direct* connection to pastoralism whatsoever. This does not mean, however, that there was no connection. Both were located where they would serve as “sites of attraction” (Porter 2020, cf. Castel and Peltenburg 2007: 612), providing social connections as well as services, religious and economic, to mobile pastoralists engaged in the proliferating wool industry. In this scenario pastoralists would bring animals for fleece or wool to the town at appropriate intervals, leaving behind little trace of animals pastured at a distance. In the fourth millennium, Habuba Kabira would then ship the materials via downstream river transport to its southern consumers; in the third, Tell Chuera might move the product – raw wool or woven (Sallaberger 2014b) – through any number of exchange systems by means of the covered wagons (represented in models found at that site). At the same time, the inhabitants of these settlements, and their like, would maintain their own small flocks for milk, wool, dung and, occasionally meat, while cultic centers would maintain foddered herds of animals for sacrificial purposes, their care no doubt outsourced to local farmers and, too, mobile herders.

There is little indication of what the movements of these putative pastoralists would have been like. But written evidence from different periods tells us that they were not necessarily bound by rivers and mountain ranges, and were often dictated not just by available pasturage, carrying capacity, and climate, as we all too often assume, but also by personal and political relationships. Political relationships determined, and maintained, the supply of animals and their products to Tell Brak, ancient Nagar, as the Habur region became quite densely settled in the mid-third millennium. Tablets from Tell Beydar, ancient Nabada, attest to a settlement-based system of comparatively small-scale herds that was part of a devolved system of control stemming from Nagar (Sallaberger 2004). Relations between the two sites were reinforced by royal progressions from Nagar to Nabada to attend the god of animals there. Since these documents stem from Nabada, and not Nagar, they represent just one component of what had to be a much larger system involving equids, cattle, sheep and goat. Approximately 4000 Nabada sheep/goat are calculable from these texts (Porter 2012: 247). This raises questions of the spatial requirements for grazing that would suggest the interstices of settlements in this area would be insufficient. This is especially since Nabada itself managed smaller settlements around it, all of which

would have maintained their own herds. Exploitation of adjacent steppic environments (Ur and Wilkinson 2008) and beyond was necessary. It should be noted that this situation was not predictable on archaeological grounds. It does not fit classic models of circular site sustaining areas because here the realities of landscape come into play, in conjunction with the historical location of other settlements.

Multiple sources from the second half of the third millennium bear witness to a combination of patterns extending across wide swathes of central and northern Syria and beyond. The written evidence comes from Ebla. The size of herds and necessary sustaining areas at Ebla have already been discussed. In addition to the palace's own herds, sheep came to the treasury of Ebla from the Harran, and textiles were sent back and forth from Carchemish. But it is the wool obtained from Armi (Wilkinson et al. 2014: 58), the polity whose ancestors shared the daily offerings with the ancestors of Ebla, that is truly suggestive. There are no ancestors from any other polity registered as present in the Ebla palace. This is a signal distinction. Although Ebla's relationship with Armi/Armanum is complex, with enmity attested as often as alliance, the co-status of Ebla's and Armi's ancestors points to the kind of relationship seen between the kings of Mari, and their Simalite kin. We do not know where Armi is (see Otto 2006; Archi 2011). It is certainly a significant sedentary settlement (it is not Banat/Bazi), but as a polity is not confined to that settlement, and like Mari might be the, or a, tether for mobile relations who traversed the space between the two cities.

Despite decades of sometimes vehement rejection of the notion that pastoralism had anything to do with the kingdom of Ebla, the pendulum has swung – too far perhaps – to the other side. Since excavation of the site of al-Rawda (Castel and Peltenburg 2007), and the discovery of “the long wall” attributed to the late third millennium (Geyer et al. 2010), attention has been focused on the Shamiya, the steppe region south east of Ebla, and the line of circular cities that seems to parallel that wall. Morphological similarities between the circular cities of the Shamiya and Tell Chuera (and similar sites in the Habur) have raised questions about potential socio-political relationships between the two regions, along with developing narratives about a direct connection between the Shamiya and Ebla (see collected papers in Castel et al. 2020). The idea that the abandonment of settlement in the southern Levant is related to Ebla pastoralism is also gaining traction (Wilkinson et al. 2014: 92; Schloen 2017; Greenfield 2017: 47-8), although it should not (D'Andrea nd).

Climate change lies behind most explanations now of what has long been

understood as a major and global diminishment of settlement toward the end of the third millennium. If settlement is shrinking in the east, it seems to be expanding in the west, and again, in areas at the very least marginal for cereal cultivation. Burke (2017: 275), Arbuckle and Hammer (2019: 422) and Sołtysiak and Schutkowski (2015: 176) accept the assumption that deteriorating climatic conditions would adversely affect pastoralists as much as cereal farmers. Therefore claims of a widespread turn to pastoralism at this time, especially as a response to climate change, are invalidated. While I too do not accept the argument that there is an expansion, or newly instituted, pastoralism at the end of the third and beginning of the second millennia, just a shift in orientation, it must be pointed out that such arguments as above both fail to take into account variable responses and adaptations that consideration of agency brings into the discussion, and are also undermined by archaeological evidence. Research currently undertaken for the end of the *second* millennium, and not yet for the third, reveals the effects of climate change to fluctuate, and responses to be local and inconsistent. Some places are successful at withstanding drought, others nearby are not (Riehl et al. 2014). Outcomes are not inevitable, then, but rather contingent. Environmental affordances, geo-political situations, willingness to experiment in subsistence strategies, all come into play. It is not therefore possible to draw a blanket picture of what would or would not happen under such circumstances.

As to the archaeological evidence, the extraordinary quantities of remains in the region of Palmyra dated to the late third millennium, although difficult to confirm, would seem to demonstrate that pastoralists were already equipped to exploit highly adverse conditions. Thousands of stone enclosures, tumuli and kites recorded through land-based and satellite surveys in the Palmyrene (Schou 2015) cannot in any way be related to sedentary farming, yet attest to intense use of this area. Moreover, the same kind of structures are found throughout the Shamiya, raising several possibilities, one of which is that the same pastoralists moved between these places. It has been demonstrated that the circular enclosures, rather than animal pens, were consistently located on the sloping sides of rivulets and wadis in order to collect moisture and soil at the bottom, allowing for the growth of limited crops or vegetables (Schou 2015: 185). Moreover, the Palmyrene seems not to have suffered the de-vegetation created by ever-extending cultivation regimes around settlements, at least, not until modern times (Schou 2015: 337-8), so that drought would have had a less drastic impact here than elsewhere.

In short, in the fourth millennium circumstances suggest the possibility of multiple ways of managing animals: locally-based herds of varying size kept for daily domestic needs such as milk and meat; specialized herds destined for cultic use; and a tethered wool pastoralism managed over different distances and mediated by sites of attraction. A little later, a similar pattern is actually evidenced in the eastern steppe, in the Habur, from Tell Chuera to Tell Beydar, while in western Syria, around Ebla, complexes of animal husbandry were closely tied to the identity as well as economy of the polity and, too, its broader political relationships. At the same time, as our exploration of areas previously ignored by archaeologists expands, it is becoming clearer that vast steppe areas, in different regions, are already utilized in the early to mid-third millennium, and utilized within intricate geo-political constructs we are only just beginning to glimpse. There is then precedent for distance pastoralism, a millennium before the Mari evidence, and intimations that at least some of that pastoralism was configured in not dissimilar ways to that of Mari (see Porter 2012 for further detail, esp. p.239-40).

Conclusion

Makarewicz (2018: 149) states that

“pastoralist movement patterns vary considerably, ranging from highly repetitive and predictable moves following fixed pathways to habitually visited locales according to a well-established seasonal round to more loosely scheduled moves that are more variable in itinerary, distance, and scheduling.”

It should not be supposed that in any one time and place in the ancient Near East this variability was inoperable. Nor should any one kind of movement be assumed associated with any one kind of socio-political pastoralist – or sedentarist – organization. Many writers recognize the notion that there are multiple ways pastoralism can be practiced, but few attempt to engage with those ways. In terms of individual research, this is simply the way scholarship works. We each grapple with a specific, and hopefully manageable, problem. *Mobile Pastoralism* (Porter 2012) for example, was not a study of mobile pastoralists, as the very first paragraph makes clear. It was not an attempt to explain changing settlement patterns from Anatolia to southern Mesopotamia to the Southern Levant at the end of third millennium. It was,

rather, the investigation of the prejudices of scholarship as seen through a very specific relationship in a specific region. This focus was a way to make a larger theoretical point: that an innate belief in the superiority of sedentary farming long preceded any actual examination of the material and written evidence and continues to do so in a myriad of ways. If, for example, settlement evidence is ambiguous, and might be explained as sedentary or as mobile, why, then, is the default choice, notwithstanding valid criticisms, the one Potts makes (2014: 45)?

“Occam’s razor forbids us from assuming its [nomadic pastoralism] existence. Indeed we should do just the opposite and presume that, until such time as evidence to the contrary emerges, nomadism, as opposed to transhumance or herdsman husbandry, was not practiced in Iran during the prehistoric era or the Bronze Age”

It is when the totality of that research in the end is focused on a single, poorly defined concept – that of politically and socially autonomous, or “independent” pastoralism – that the problem arises. Transhumance is still mobile pastoralism. It may be conducted over long distances, and for entire seasons. Highly mobile – nomadic even – pastoralism may be conducted under the auspices of political control, remaining tethered to settled communities. The permutations and combinations of all these variations do not fit neatly into the kind of classifications so popular in the 1980s and 1990s. There are no rules of behavior, only contingent situations. Nor is it a matter of “either/or”. Many different regimes may be in operation within the same society and at the same time. A key factor in the nature of the regime lies in the use to which the animal is put, and the animal’s socio-political context. And this information can be gleaned from texts.

So in this deliberately unscientific survey of possibilities, undertaking exactly the kind of analysis excoriated by some among the methodological turn, I have highlighted not only the limitations of an either/or understanding of mobile pastoralism, but the relevance of the Mari documents and other textual materials for understanding the possibilities of multiple kinds of pastoralism and the ways they may work. While texts, and for that matter ethnographies, do not in and of themselves “prove” anything, they bring to light certain realities, some of which are consistent across time and space. One such is the sheer magnitude of animal husbandry. Texts raise as well many social and political issues in the practice of pastoralism (beyond the tired refrain of antipathy) that we should consider in addition to environ-

mental possibilities and constraints. I concur therefore, with Arbuckle and Hammer (2019: 391) when they conclude “that pastoralism was a diverse, flexible, and dynamic adaptation in the ancient Near East”, and join their “call for a reinvigorated and empirically based archaeology of pastoralism in Southwest Asia”. But it must be one much more thoroughly nuanced by information we have at hand. I am quite open to the possibility that the voices denying the existence of “mobile pastoralism” are right. But they are not right yet.

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