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Special Topic on *Conflict and Warfare in the Near Eastern Neolithic*

We extend our most sincere thanks to Ofer Bar-Yosef for his keynote contribution on *Warfare in the Levantine Neolithic*, the special topic of this Neo-Lithics issue, which has attracted the intellectual company of so many colleagues. The result is a very substantial and in many parts new discussion, and the thickest issue of Neo-Lithics published to date. The keynote triggered some controversy, as we expected, and this appears to come less from the different perceptions of the warfare issue *per se* and more from the different areas in which such perceptions are gained. Indeed, it is essential that we differentiate between these two aspects. For example, in a recent discussion at a fish *mezze* to which one of us (H.G.K.G.) was invited by Mehmet Özdoğan, I learned that Neolithic warfare should not be neglected just because one's own sights are dominated by evidence from more extensive Neolithic habitats. Also personal moral and political views can considerably influence many of the sights and approaches to the topic. We extend our sincere thanks to all contributors for preparing the substratum of a broader discussion upon which we can build in the future; the diversity of arguments and approaches which our discourse has started shows that we are at the very beginning of addressing the issue of conflict and warfare.

It was a pleasure to cooperate with our guest editor, Lee Clare. We not only won the perfect colleague for this special topic, but he also brought in the patience and care for the contributions which were collected in just a few months. While we finalize works on this issue, we become confident that it is only a matter of time until we are confronted with direct evidence for warfare or coalitional aggression from one of the current excavations. This issue aims to raise awareness about such findings …

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Introduction: Conflict and Warfare in the Near Eastern Neolithic

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This edition of Neo-Lithics is dedicated to a topic that to present has received relatively little attention from scholars working in the field of the Near Eastern Neolithic. Conflict and warfare in traditional societies can range significantly in scale from minor intra-familial clashes at the level of small residence groups to large scale inter-community hostilities characterised by alliance formation and the annexation of foreign territories. Granted, *warfare* can in some instances be agent, institutionalised, and serve significant socio-economic and ritual functions, but in others, where an increase in hostilities, particularly at the regional and supra-regional level, has an external catalyst, bellicose enterprises can culminate in the breakdown of afflicted communities, migration, adjustment of vertical differentiation within social networks, and material culture change. As such, this absence of scholarly interest with respect to the Neolithic in the Near East, with a few notable exceptions, is all the more incomprehensible. This edition of Neo-Lithics seeks not to remedy directly this deficit of scholarly activity, but to provide a platform for initial discussions and deliberations in the hope that more detailed studies will duly follow.

Our volume is opened by a keynote paper by Ofer **Bar-Yosef** to which comments and contributions were invited from esteemed scholars from the fields of warfare and conflict studies and Near Eastern and European prehistory. Unfortunately, some academic disciplines are still missing in our collection of comments, for example we are lacking contributions from the spheres of physical anthropology, human ethology, evolutionary psychology, neurobiology, cognitive neurosciences and others; these areas will undoubtedly play a sigificant role in the future, i.e. in the second stage of our discourse on conflict and warfare. Topics addressed by contributors in this issue range from theoretical issues, concerned with the origins and genesis of Neolithic conflict, to more practical aspects such as the identification of markers for hostilities in the archaeological record. Indeed, this latter point would appear to constitute one of the most pressing concerns among prehistorians, at least judging by the frequency by which this topic has been broached in recent publications, and also within this present volume. Here, the observation that a lack of evidence is not necessarily tantamount to a real absence of warfare is certainly not insignificant, and some relevant lines of documentation are simply misunderstood (**LeBlanc**). On the other hand, as demonstrated by **Bernbeck** with respect to site abandonment and **Grosman** with reference to skeletal pathologies, there always remains a degree of

ambiguity concerning the correct interpretation, even of those lines of evidence frequently cited as being among the most reliable. Again, **Guilaine** and **Clare** discuss the significance of the ratio of arrowheads in lithic assemblages as an indicator of violence on Cyprus and in the southern Levant respectively. If however clear archaeological evidence for violence is unearthed, how can we ascertain the extent of bellicosity in the respective culture; could it not be that we are merely witnessing a single (otherwise infrequent) outbreak of violence (**Roksandic**)? Indeed, **Bernbeck** goes one step further and criticises the pursuit of evidence of prehistoric conflict (for conflict's sake) in a region and period to have hitherto provided comparatively little indication of its occurrence.

Returning to theoretical considerations, since the Enlightenment discussion to focus upon the origins of war has been dominated by the '*nature-nurture controversy*'. On the one hand, there are those, predominantly biologists and biological anthropologists, who regard violence as an intrinsic element of human nature, whilst on the other there are scholars, mainly cultural and social anthropologists, who argue that war is culturally bequeathed, *i.e*. nurtured. These paradigms are synonymous with two prominent philosophers from the early modern era, Thomas Hobbes (1588-1679) and Jean-Jacques Rousseau (1712-1778), also referred to as the '*philosopher of war*' and the '*philosopher of peace*' respectively (*cf*. Dawson 1996). Discussion surrounding the *nature vs. nurture* debate has never abated and still abounds today. Indeed, its influence can still be felt in practically all papers and publications to broach the topic (*cf*. Thorpe 2005). Consequently, whilst adherents of the *nature* paradigm express what are referred to as *neo-Hobbesian* views, those to advocate cultural explanations are deemed adherents of *neo-Rousseauism*. Especially in the twentieth century, it is the latter of these paradigms which proved prevalent, most predominantly in the frame of cultural ecology, due not least to the in many respects still prevailing intellectual disposition favouring the dogma of *cultural determinism*. In this volume, for instance, **Grosman** cites the apparent correlation between violent conflict and the inception of sedentary lifeways, a clearly neo-Rousseauan approach to the origins of warfare according to which conflict only emerged following the inception of agriculture, associated demographic growth, and the rise of more complex forms of social organisation, and **Warburton**, in a similar vein, posits that the origins of warfare lie not in the European Palaeolithic but in the Levantine Neolithic, as it was here that the demographic basis for sustained conflict first appeared.

Although neo-evolutionary views and ideals of self preservation (*survival of the fittest*), as suggested in works by modern sociobiologists, are not encountered in this volume, elsewhere adherents to this paradigm have referred to conflict and violence as an inherent characteristic of human life, an urge that demands manipulation of our genetic imperatives to control it, akin to resisting temptations of calorie-rich foods and casual sex (Smith 2009: 27)! This is most certainly an extreme view, but as pointed out by **Gebel**, who succeeds in combining the two grand paradigms, human aggression, although biologically anchored, is nevertheless dominated by cooperation and empathy. Further, and most intriguingly, he goes on to assert clearly neo-Rousseauan values positing that Neolithic conflict is intrinsically linked to sedentary lifeways and is dictated by the failure of related mechanisms of aggregation, commodification and innovation. This stance is echoed in the contributions by Özdoğan and **Otterbein**. For the period of the formation and dissemination of the aceramic Neolithic in the Levant and Anatolia Özdoğan concludes that this would only have been possible through the sharing of knowledge, which in itself implies high levels of inter-community cooperation in the respective regions and landscapes. The onset of conflict and violence only became reality upon the collapse of the aceramic system, an observation which would appear to be enforced by **Rollefson**'s comments that violence in the Pre-Pottery Neolithic was likely limited in scale to interpersonal vendettas and intragroup strife. **Otterbein** also stresses the significance of the absence of violence for the dispersal of Neolithic lifeways; he too comes to the same conclusion as **Özdoğan**, *i.e.* that any steps towards domestication would have been impossible had contemporary societies been racked by violent conflict. On the other hand, **Roscoe** takes a quite different approach. Focusing on the characteristic aggregation of populations in the early Neolithic he concludes (on the basis of ethnographic parallels) that the spatial occurrence of resources alone does not explain sufficiently this phenomenon; intriguingly, he sees the development of increasingly more substantial villages in the Levant in the course of the early Neolithic linked to the growing risk and threat of attack.

A further important theoretical approach, and one to feature perhaps most prominently in the keynote by **Bar-Yosef** but with a clearly visible resonance in numerous other contributions, is the Malthusian paradigm. Accordingly, in addition to disease and famine, warfare is considered one of the standard consequences of overpopulation and overstretched carrying capacities. Nevertheless, inherent deficits of the Malthusian approach are picked out as a central theme by **Clare** and **Müller-Neuhof**. These authors propose that the potential of prehistoric societies to actually engage in armed conflict should first be assessed on the basis of prevailing socio-economic factors. Consequently, in their respective contributions it is demonstrated that the Malthusian model is only conditionally applicative and that alternative solutions and coping strategies are equally capable of resolving crisis situations. Be this as it may, and in support of some of the notions put forward by **Bar-Yosef**, environmental scarcity is without doubt a widely acknowledged cause of violence. Contentions to arise from anthropogenic and climate induced environmental degradation can occur

on various scales and comprise for example conflicts incited by competition over resource access, including the effects of scarcity upon economic productivity and livelihoods, as well as migrations of afflicted communities and their infringement upon foreign territories. Thus, in many respects, sources of conflict cannot be understood without including environmental scarcity as part of its causal story (Homer-Dixon 1999).

If we were to approach our topic from the standpoint of recent discussions and considerations from the disciplines of ethnology and evolutionary psychology, we might better grasp the range of questions we ought to consider when undertaking archaeological research of warfare and conflict. Unfortunately, Joachim Bauer and Wulf Schiefenhövel were unable to contribute to this discussion owing to time constraints. Their works illustrate to what extent our discussion is dependent upon interdisciplinary efforts and the support of disciplines specialised in human conflict behaviour. In addition to introducing into the discussion an interdisciplinary established terminology and a framework of definitions for Neolithic types of warfare, conflict and aggression, we must also differentiate between the various regional ecological, social, and economic conditions of conflict in the Levant throughout the Neolithic Evolution. Why is it that our discussion somehow imagines that consultation of the many disciplines undertaking aggression research (cognitive neurosciences and neurobiology; human ethology; social biology; behavioral ecology; environmental, evolutionary, and religion psychologies; ethnology and others) is not necessary? Why is it that these disciplines did not receive our information when in desperate need of archaeological data for their study of the evolution of aggression? And, why is it that our research is hardly aware of "typical" conflict constellations, *e.g.* by "simply" reconstructing size and productivity of habitats as related to settlement sizes and pattern? Finally, regarding primary empiric bases: Don't we need a systematic search for traumata through the physical anthropological records (*cf.* for example the Basta homicide, Röhrer-Ertl *et al.* 1988)?

Prehistory will not succeed in understanding warfare and conflict in the archaeological record if it does not open up to the human ethology of warfare and conflict (as this is true also for all the other findings emerging from Neolithic cognitive systems). Two positions should be mentioned here to outline possible directions: Wulf Schiefenhöfel criticises (pers. comm.) that in the humanities the idea still prevails that the *homo sapiens* is basically a harmonious and peaceful being which only became aggressive through sedentism. "As our colleague Ofer Bar-Yosef correctly explains, primates also show aggressive, even war-type behaviour, as this is known from other mammals, too." Schiefenhövel suggests that in addition to the evolutionary perspective our discussion needs to consult ethnographical findings. (*cf. e.g.* Schiefenhövel 2001). Joachim Bauer argues in several of his publications (*e.g.* Bauer 2008) against "neo-darwinistic" biologists who follow Sigmund Freud and Konrad Lorenz by postulating a human drive for aggression. Neurobiology, however, does not understand the human being as good or bad, aggressive or not, but as a being which is oriented primarily towards social acceptance and cooperation (*cf.* data in Bauer 2006).

As already mentioned, we are at the beginning of the debate for the Near Eastern Neolithic. We are in the fortunate position that we are still able to structure discussion and data in advance of the interdisciplinary contacts that we must soon seek. We hope that the collection of papers in this issue can serve as a starting point for this endeavour, from which the discussion of collective violence in the Near Eastern Neolithic can unfold and progress.

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Warfare in Levantine Early Neolithic. A Hypothesis to be Considered

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Personal conflicts are not a new mode of interaction between humans; even primates do the same. In primate and human evolution a small task group killing an individual has the same history; this pattern of behavior exemplifies inter-group physical and/or ritual conflicts. The archaeological evidence already shows that during the closing millennia of the Pleistocene and early Holocene, mass graves can be interpreted as resulting from human violence (Gulaine and Zammit 2001; Martin and Frayer 1997 and papers therein; Ferguson and Whitehead 1992 and papers therein). Mass burials have been interpreted as the results of tribal wars such as the case of Ofnet Cave, dated to the European Mesolithic (Frayer 1997). Similar Epi-Paleolithc burials with direct or indirect evidence for killing are known from cemeteries in Egyptian Nubia (Anderson 1968) and North Africa in the cave sites of Afalou Bou Rhummel, Mechta el 'Arbi, and Taforalt (Roper 1969), Hence "*war before civilization*" (Keeley 1996) is certainly no exaggeration.

Considering the wealth of information from the Levant and Anatolia, we should consider whether Levantine Early Neolithic contexts provide evidence for acts of warfare which, as most authorities agree, emanate from increasing population densities and steep inter-group competition (Keeley 1996). For example, we should ask ourselves whether Early Neolithic villages were simply abandoned every few centuries due to peaceful reasons, such as over-exploitation of soils, depletion of soil fertility owing to lack of fertilizers, the effects of salinization, abrupt climatic changes with droughts, harvest failures and famines, and diseases, or whether these abandonments actually resulted from physical conflicts between neighboring populations which could have emerged from a combination of the above mentioned difficulties (*e.g.* Clare *et al.* 2008). In order to identify the increasing frequency of physical conflicts that I believe are correlated with increasing demographic pressures, I begin the story with the impact of the demographic effects caused by the Late Glacial Maximum and proceed to the early Neolithic period.

As far as we know today, this harsh cold and dry period (also known as MOSI2, *ca.* 24/23-18 ka calBP) resulted in a discernible reduction of human populations (a genetic "bottle neck") in many regions of the Old World. But, after *ca.* 18/17 ka calBP, as temperatures increased steadily and the distribution of rainfall watered larger areas than before, humans recovered from the difficult times and the unfavorable environments conditions of the LGM. The post-LGM climatic amelioration facilitated reproductively successful hunter-gatherer societies to occupy almost every ecological habitat of the world and to disperse into the Americas. If we examine the Levant during this period, we observe the expansion of the microlithic Geometric Kebaran exploiting every ecological niche from the northern Levant to the southern mountains and the Sinai peninsula at around 16,500 -14,500 cal BP. We therefore encounter these hunter-gatherers in every vegetation belt, including the Mediterranean, Irano-Turanian, and Saharo-Arabian area. Within the Mediterranean vegetation belt semi-sedentary Geometric Kebaran sites were established.

There also existed more or less contemporary groups which competed and co-existed with these Levantine foragers. In the South these were the Mushabians and Ramonians which, according to one interpretation, originated in North Africa (Bar-Yosef and Phillips 1997), a proposal supported by the analysis of the E-M35 Y chromosome (Lancaster 2009 on line). It is also conceivable that other groups of hunter-gathers were attracted by the improving environmental conditions of the previously semi-arid belt, and moved into the Levantine area from the Syro-Arabian desert and/or the Taurus foothills (Goring-Morris 1995).

What is most intriguing and yet unclear is whether a short climatic spell (known in Europe as the Older Dryas) caused a temporary retraction of the steppic belt triggering certain groups to establish the Early Natufian hamlets (*e.g.*, Bar-Yosef and Belfer-Cohen 1989; Bar-Yosef 2002). Without discussing the available evidence, the critical point is that this initial formation of human agglomerations, combining a few families or even sub-clans, resulted from the decision to live together for reasons of security, defending their territory, either by force or by symbolic acts (see Roscoe 2008, 2009 and references therein). We once referred to this societal major change as a "point of no return" (Bar-Yosef and Belfer-Cohen 1989; Belfer-Cohen and Bar-Yosef 2000), or what we would call today the "tipping point". I suggest that we should also refer to the formation of the Early Natufian hamlets as the onset of 'history'. Undoubtedly, as the archaeological records demonstrate, the socio-economic processes from the Late Natufian in the northern Levant, and in spite of the ensuing socio-economic ups and downs, led – without forward vision by the first cultivators and herders – to the invention of writing systems from which the history of the people in the Ancient Near East is told. Needless to say, the evolution and elaboration of cosmologies in this region, expressed in artistic imageries (whether painted, sculptured, or symbolized by treated human and animal remains), gained important momentum (Cauvin 2000).

The small hamlets of the Natufian (ca.14,500

-11,700/500 calBP) were constructed from a series of brush huts built above circular stone foundations, and contain the evidence for territorial ownership, a conclusion derived from the on-site presence of cemeteries. Adopting the subdivision of the Natufian to three schematic phases (Valla 1984) the Final Natufian was a tumultuous time due to the effects of the Younger Dryas (ca. 13,000/800-11,700/500 calBP). Under these circumstances of ecological stress the options of human groups were determined by their socio-cultural concepts (*e.g.*, Bar-Yosef and Belfer-Cohen 1991; Miller-Rosen 2007) as follows:

(1) Increased mobility as characterized by the Late/ Final Natufian that resulted in particular ecological adaptations known for example as the Harifian culture in the Negev and Sinai where the Harif point – a typical arrow head – was invented (Goring-Morris 1991).

(2) Increased sedentism demonstrated in the establishment of the villages of Hallan Çemi in a tributary of the Tigris River, the Late Natufian in Mureybet and Abu Hureyra (Rosenberg and Redding 2000; Moore *et al.* 2000).

(3) Intensified hunting and gathering and part time cultivation (that may indicate increased sedentism) commenced in the foothills of the Taurus and along the middle Euphrates River Valley (Willcox *et al.* 2009).

The effects of the general decrease in resources is clearly shown by the nature of the latest occupations of Eynan in the Hula Valley (Valla *et al.* 2007), an area that was the most suitable ecological niche for sedentary communities and was hardly ever affected in a major way by abrupt climatic changes.

Early Neolithic communities, which we still label using the term PPNA instead of the affiliated cultural entities such as Khiamian, Mureybetian and Sultanian (ca. 11,700/500 – 10, 700/500 cal BP), are generally villages eight times (or more) larger than their ancestor hamlets, a reflection of rapid population growth. Levantine PPNA people, considered the direct descendants of the Natufians, spent more energy than their forefathers in constructing their houses. Circular and oval stone foundations continued to be the standard shape of the domestic unit, but their use of quarried clay and hand-molded plano-convex bricks for the walls, as well as flat roofs that required supporting posts, represent increased investment in the formation of human space. In addition, there were considerable changes in the ground stone tools, which probably signified different techniques of food preparation. The 'sudden' population growth from 30-50 (rarely up to 100) people at a Natufian site to 250-400 at an early Neolithic village within a relatively short time (two-three centuries) requires explanation. In my opinion, without the benefits of systematic cereal cultivation, which commenced in the closing one to two centuries of the Final Natufian, there is no way of explaining this rapid population growth across the Levant. PPNA villages in the Mediterranean and steppic belts, however, do not show the same crowded clustering that became the marker among later several PPNB sites, including those labeled as "mega sites" along the Jordanian plateau. This issue is worth exploring in the future. AMS calibrated radiocarbon chronologies, mostly of short-lived samples such as seeds and bones, indicate that the abandonment of almost every village, except in rare cases such as Jerf et Ahmar (Stordeur and Abbés 2002), occurred everywhere in the Levant. Even those situated adjacent to a copious springs (like Jericho) or on the bank of a river (like Mureybet) survived only for a few centuries. Not surprisingly, a similar settlement history was recorded for the following PPNB period (ca. 10,700/500-8,200 cal BP) in spite of the fast accumulating evidence that indicates better climatic conditions (*e.g.* Weninger *et al.* 2009).

The question that we need to ask is to what extent intra-group and inter-group human conflicts caused the interrupted sequences as recorded in many Early Neolithic sites. The most parsimonious interpretation would be that both intra-group fissioning and individual conflicts played a major role during the PPNA and PPNB. I draw this interpretation by formulating a model based on ethnographic and historical records, but to my best knowledge we never employ these sources to formulate 'a one to one analogy'. Under the premise that intra-group conflicts caused what is known as "scalar stress", this may have triggered the splitting of villages (*e.g.* Roscoe 2008,2009; Belfer-Cohen and Goring-Morris 2002; Kuijt 2000; Goring-Morris and Belfer-Cohen 2008). The 'breaking up' of village communities could explain, for example, why Gilgal and Netiv Hagdud are situated only 1.5 km apart. Their calibrated radiocarbon chronology indicates that the first was founded earlier, but there was a time when the two villages were apparently contemporary. Another option is that Jericho was founded earlier, and either Gilgal or Netiv Hagdud represents a budding-off PPNA community, *i.e.* when a large group moved from the original large site of Jericho. Alternatively, perhaps Jericho was founded later than Gilgal and/or Netiv Hagdud. Thus, it is not surprising that Mureybet and Abu Hureyra, which both accommodated Late/ Final Natufian communities, had a similar relationship; they are only separated by a distance of some 20 km. While we have no information about other sites, which are now inundated by the water of the Tabqa Dam, a possible interpretation that we should entertain is that the makers of the Late Natufian of Abu Hureyra joined those of Mureybet to establish this important PPNA site. For clarity I include the Khiamian with the Mureybetian in the definition of the PPNA in the northern Levant, similar to the southern Levant where the Khiamian and the Sultanian are incorporated in what we label as PPNA.

In addition to the abandonment of sites, we need to examine other aspects of social expressions that indicate 'fear and security' in the way that villages,

small or large, were constructed in a given area. Ba'ja is located in a closed valley with a narrow and difficult access passage through Wadi Musa, which bears the same idea as later Bronze Age site city gates. While the Anatolian examples of house clustering at Çatalhöyük and Aşıklı Höyük are well known, similar tight agglomerations were exposed in Bouqras (where the site is also situated on top of a hill), and other sites in northern Mesopotamia, such as Magzalia, Yarim Tepe, or in Beidha in the southern Levant. One option already known from the literature as a sign of warfare are town or city walls. This was the original interpretation given to the wall and tower in Jericho. My alternative interpretation was published long ago and I still hold this position that either full or partial early perimeter walls were erected in order to protect the site from floods (Bar-Yosef 1986). Additional examples are walls in Beidha, 'Ain Ghazal, Mezra'a Tleilat, etc. I expect other sites, and in particular the so-called "mega-sites" in Transjordan that were targets for only partial excavations, except for 'Ain Ghazal (*e.g.* Rollefson 2004), to conceal similar walls. We should also remember that houses built along the perimeter of the village provide protection with their rear walls, and a sense of security. However, this type of defense was constructed in order to deter the enemy from conquering the site. We should also remember that protruding towers, built along the outer surface of the wall, were intended to shoot people who attempted to break in by climbing on the wall. This is why the famous tower in Jericho could not have served the same purpose as it was built inside the village and within the wall intended to protect the tower. Therefore, in the evolution of human warfare we should probably look for other signs of violence during the early Neolithic.

Another reason for site abandonment recorded among PPNB sites could have been the impact of a climatic change. Those that lasted till *ca.* 8,600/400-8200 calBP were supposedly deserted during the "8200 cold event" (*e.g.* Bar-Yosef 2001; Weninger *et al.* 2006; Berger and Guilaine 2008; Weninger *et al.* 2009). During these several centuries a drier climate prevailed in the Eastern Mediterranean. Droughts were probably a recurrent phenomenon. Villagers abandoned their settlements, died of hunger and/or moved to other places by forcing their way or in agreement with locals. These are the times that the archaeological evidence for violence should increase and become visible in the excavations.

Finally, a point about the lithics should be raised. Arrowheads as tool types were the projectiles used for hunting, but in general their reported numbers in most PPNA sites in the 'sown land' are relatively small (*e.g.*, M.C Cauvin 2008). However, what S. Kozlowski has termed the Big Arrowhead Industry demonstrates that in many farming communities the frequencies are high if one considers the MNI of hunted species in relation to the abundance of arrowheads. In addition, many steppic and semi-arid sites of foragers, such as in the southern Sinai or the margins of the Transjordanian plateau, produced staggering amounts of PPNB projectiles (*e.g.*, Gopher 1994). One potential explanation for these frequencies is that the groups that employed the famous "desert kites", most of which are concentrated along the western margins of the Syro-Arabian desert, hunted and supplied animal tissues and hides to PPNB communities (probably the "mega-sites"), as part of mutual interactions (*e.g.* Bar-Yosef 1986, 2001). In addition, with the development of farming and herding, established farmers needed the skills of mobile artisans. In the absence of skilled knappers an "arms race" could have replaced local production of arrowheads by exchange or trade. Similar to other commodities that were transmitted over large geographic distances such as the obsidian and marine shells, it is conceivable that the arrowheads produced by foragers were supplied to rival farming communities.

Clearly, to test the hypothesis regarding the evolution of warfare among Neolithic societies of southwestern Asia, as was shown among the Neolithic groups in Western Europe (Guilaine and Zammit 2005), we need to find skeletal evidence of victims of violence, burned houses, portions of these skeletons buried in the rubble, and so on. To refute the hypothesis we need to look for evidence that will demonstrate that other causes were responsible for the site abandonment. It is also possible that both phenomena existed, and that sites were deserted for different circumstantial reasons including persistence of droughts, conflicts and sudden epidemic outbreaks. While we often operate under the premise of the 'noble savage,' we should be fully aware that searching for the evidence of warfare among the ancient farming communities of Southwestern Asia would be beneficial for understanding the history of the ensuing millennia in this region.

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Prehistoric Wars: A Scholastic Fallacy

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Ofer Bar-Yosef's statement on early Neolithic Levantine warfare is an interesting call to search for evidence of violence in the past. One of the problems I see with this proposal, however, is that it uses a modernist, and to my mind inappropriate language („war", „arms race"), one that is derived from violent conflicts between political entities that are centrally organized. Since we have no indication of such political units for any of the time periods discussed, I do not think that we should be talking of „war." Bar-Yosef's thesis attempts to find a phenomenon he considers historically important but hitherto lacking of evidence. Therefore, he admonishes his colleagues that „we should probably look for other signs of violence during the early Neolithic [than defense walls, R.B.]", „we need to examine other aspects of social expressions that indicate ‚fear and security'", „we need to find skeletal evidence of victims of violence, burnt houses, portions of these skeletons buried in the rubble, and so on."

Bar-Yosef makes it explicit that he is excavating an ancient conceptual conflict, that between Hobbes' "solitary, poor, nasty, brutish, and short" life in non-state societies, and the "noble savage" romanticism that came to be associated with Rousseau. Bar-Yosef promotes a Hobbesian ideology by searching for violence in a region and time where it has hitherto not been identified. On this very general level, I hesitate to agree with the ideological background of the text: it de-historicizes our current condition of permanent small scale wars by suggesting that characteristics of mass violence are timeless. More concretely, the line of argument Bar-Yosef pursues constructs cause-effect relations across different kinds of historical scales. Violence and war belong to the scale of a *histoire événementielle*. They figure as effects of other historical processes, among which he lists demography and climate change. While demography functions on the scale of Braudelian conjunctures, that is, a mid-level temporal scale, climate change is a matter of the *longue durée.* Linking historical processes across such vastly different scales is in my view a major problem. This leads to what Bourdieu (1997) called "scholastic fallacy", an approach that refrains from including past peoples' experiences and aspirations: Bar-Yosef takes a purely objectivist stance, one that is *a priori* so distanced and withdrawn from the world of real Neolithic groups' motivations to act that (subjective) reasons for past violence are not and cannot be taken into consideration. However, they are most likely one of the major factors in the explanation of any occurrence of social violence, and particularly so for violence in small-scale, non-state societies. Knauft's (1987, 1990) work on this topic is of immediate relevance here. And such internal views cannot be reduced to the sort of prehistoric dominant ideology Bar-Yosef hints at with his "cosmologies in this region, expressed in artistic imageries."

The combination of an objectivist stance and obfuscation of scales is also at the root of more practical problems with this account of early Neolithic violence. I mention only the two core evidentiary elements from his text, site abandonment and various elements of defensive settlement arrangements. For Bar-Yosef, site abandonment is a process that is *per definitionem* pressed upon a community. One does not move from one site to the next without being driven out. Other processual archaeologists, imputing instrumental thinking to people in the past, have mobilized similar arguments and explained such moves as the result of group size and associated scalar stress (Johnson 1982; Bandy 2004) or, in the case of climate, drought and hunger. This reasoning has its roots in what I have called elsewhere (Bernbeck 2008) "sedentarocentrism", namely the idea that apart from genuinely "mobile" people such as foragers and nomads, all others have a "natural" tendency to stay put where they are. However, why should there not have been people who, especially in the millennia of a very slow, multi-trajectory transition from foraging, mobile life to more sedentary urban life, took on a diversity of semi-sedentary ways of living? Why should periodic moves of whole communities in a rhythm of decades, generations, even centuries not have been part of the unquestioned lifeworld of past peoples? Current research on the Late Neolithic Halaf period has led to some agreement that mobility on a non-seasonal temporal scale must have been an important facet of late 7th to early 6th millennium life (Akkermans and Duistermaat 1997; Bernbeck, Pollock *et al.* 2003). In a cultural universe that includes a pattern of periodic moves of whole communities, logically constructed cause-effect links are inappropriate when they become the sole mode of interpretation: we rather need to think of such events as underdetermined, triggered by occurrences that can vary highly. Therefore, frequent site abandonment cannot in itself serve as an indicator of conflicts. Violence is at best a sufficient, but not a necessary condition for such abandonments.

Bar-Yosef also argues that we need to interpret site structures as signs of "fear and security", especially the dense packing of houses. Again, a direct cause-effect link is constructed: where there are signs of fear, there must be a specifiable reason for them, and that is a community's other, its "enemies". But again, the archaeological correlates of defensiveness may not be matched in a one-to-one fashion by external

conditions. Sigmund Freud made the very useful distinction between "angst" as a kind of anxiety that has no specific source, and fear, whose source is clearly defined. Village plans such as those of Bouqras or Çatal Höyük likely thrived on unspecified anxieties: the lack of evidence for frequent collective violence supports such an interpretation, which again emphasizes the often under-determined character of historical processes. Wouldn't an investigation of the social mechanisms of producing peace in the early Neolithic be a research goal at least as worthwhile as the one Bar-Yosef proposes? Peace is constantly negotiated, not a historical given.

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Pastoral Clashes: Conflict Risk and Mitigation at the Pottery Neolithic Transition in the Southern Levant

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The Pottery Neolithic (PN) transition in the southern Levant marks a turning point in the prehistory of the region: The "*mega-sites*" in the Transjordanian Highlands were eclipsing, communities were becoming ever more reliant on a different form of subsistence (pastoralism), people were more mobile, were establishing new settlements in the Mediterranean plain to the west, and their socio-economic systems were changing in accordance. At the same time, their environment was subject to abrupt and severe climatic oscillations associated with the onset of an interval of Rapid Climate Change (RCC), characterised by severe winters and arid conditions, bringing the increased likelihood of famine and epidemics. In this paper, these factors are considered in more detail and the resulting capacity for conflict among PN transitional and early PN populations is assessed. It goes without saying that this capacity is high, though archaeological evidence for fighting is low. Does this reflect a real absence of warfare or are these early *pastoral clashes* simply invisible to us in the archaeological record? In this paper the former is posited, and in so doing I hope to oust the illusion that prehistoric communities when faced with insurmountable resource failures inevitably lapsed into a violent state with frantic raiding and pillaging.

PN Transition

The PN transition as understood in this paper comprises the PPNC (Rollefson and Köhler-Rollefson 1993) and the subsequent southern Levantine PN (*cf.* Garfinkel 1999). In the past the latter of these two phases, encompassing the Yarmoukian, Jericho IX, and Nizzanim cultures, has been described as turbulent and marking an era of material and cultural decline following the affluence of the late aceramic Neolithic (PPNB). Most significantly, however, it is a period associated with the movement of substantial parts of the southern Levantine population away from so called "*mega-sites*" in the Transjordanian Highlands to smaller settlements in the lower lying Mediterranean plain to the west (*e.g.* Gebel 2002), a process which went hand in hand with an abrupt decrease in population density: Compared to the approx. 900 inhabitants estimated for a LPPNB "*mega-site*", a PN settlement would have accommodated no more than 200 to 300 individuals (perhaps up to 450 people in the case of Sha'ar Hagolan; see below) (Kuijt 2008). Further, the PN was also attended by stark regionalisation processes (*viz.* Yarmoukian, Jericho IX, Nizzanim cultures) and marked by a weakening of long-distance networks.

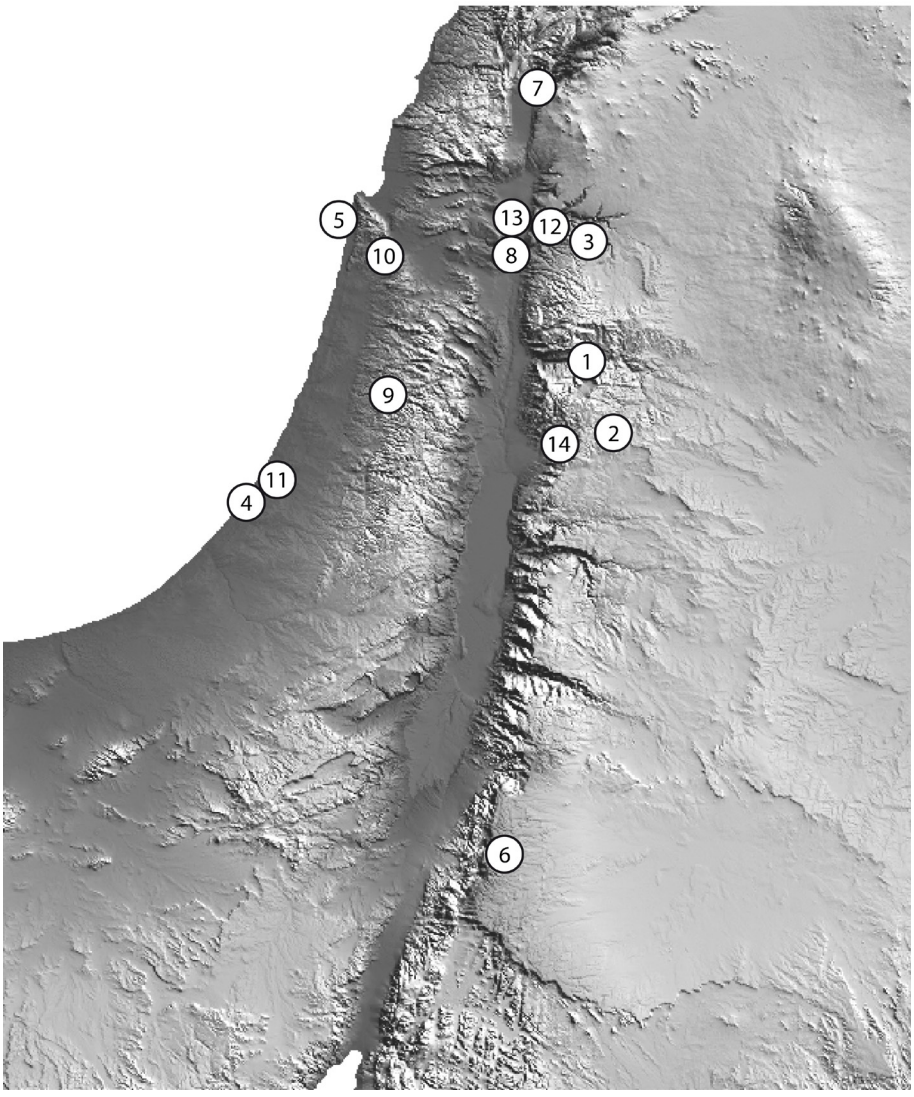

Fig. 1 PN-Transitional sites mentioned in the text: 1. Abu Thawwab; 2. 'Ain Ghazal; 3. 'Ain Rahub; 4. Ashkelon; 5. Atlit-Yam; 6. Basta; 7. Hagoshrim; 8. Munhata; 9. Nahal Qanah Cave; 10. Nahal Zehora II; 11. Nizzanim; 12. Sha'ar Hagolan; 13. Tel Ali; 14. Wadi Shu'eib.

This weakening is illustrated, for example, by a decline in the occurrence of obsidian from Anatolia, as noted for example at Yarmoukian culture sites (Garfinkel and Miller 2002b: 4). Notwithstanding, perhaps the most important development of the PN-transition is related to subsistence practices; both the PPNC and PN are traditionally acknowledged as *the* period in which pastoral lifeways first became widespread in the region.

Early Pastoralists

In the course of the last decade the significance of pastoralism for PN transitional societies has been illustrated by studies of faunal remains from numerous sites: Sha'ar Hagolan (Hesse 2002), Hagoshrim (Haber and Dayan 2004), 'Ain Rahub (al-Shiyab 1997), 'Ain Ghazal (Köhler-Rollefson *et al.* 1988; von den Driesch and Wodtke 1997; Wasse 1997), and Abu Thawwab (Köhler-Rollefson 2001). These (agro-)pastoral societies relied mainly on animal husbandry with domesticated ruminants, primarily sheep, smaller numbers of cattle, and possibly pig. Horticulture played a less significant role. Remarkable in this context is the documented (abrupt?) rise to dominance of the sheep in the PPNC, as demonstrated for 'Ain Ghazal by Wasse (1997). Indeed, this evidence, together with related observations, has led to discussions concerning the appearance of (nomadic) pastoralism in the southern Levant in the late PPN, and its role in alleviating resource conflicts and worsening economic conditions in the central settlements ("*mega-sites*") in the Jordanian highlands at this time (*e.g.* Rollefson and Köhler-Rollefson 1993; Quintero *et al.* 2004). Interestingly, this interpretation echoes earlier claims that the rise of nomadic lifeways, in whatever form, is primarily influenced by the deterioration of regional environmental conditions or by human induced factors such as warfare, overhunting, overgrazing, and the overdevelopment of human settlement (Berque 1954: 482). *Ergo*, not only times of surplus but also periods of intense scarcity can lead to fundamental technological developments (*cf.* "*innovation thesis*" after Gebel 2002; Gebel in press).

It follows that by the PN, pastoral practices involving secondary products (milk, wool and hair harvesting) had become common place in the southern Levant. Significantly, the arrival of pastoralism went hand in hand with a marked decrease in hunting. For example, at 'Ain Ghazal the ratio of quarry, particularly the previously ubiquitous gazelle, drops abruptly in the Yarmoukian, with a similarly low ratio (3.6 %) of this animal recorded in the faunal assemblage of the (newly founded?) Yarmoukian settlement at Sha'ar Hagolan (albeit that this may reflect location and habitat). Accordingly, at PPNC and Yarmoukian 'Ain Ghazal less than 10% of meat was secured by hunting (Rollefson and Köhler-Rollefson 1993: 35). In addition to this trend, which signals the collapse of broad spectrum and intense hunting, a change in the spectrum of hunted animals at the site can also be noted; compared to earlier phases increased numbers of solidungulates occur in Yarmoukian levels, thus indicative of a shift to quarry that was better adapted to higher aridity (Köhler-Rollefson *et al.* 1988; for the significance of this observation see below). Supplementary evidence for the diminishing significance of hunting is found, for example, in a decrease in the ratio of arrowheads in PN lithic tool assemblages (Gopher 1994b; see below) and by an accompanying decline in numbers of zoomorphic figurines at contemporary sites (Freikman and Garfinkel 2009).

In spite of the aforementioned data, it would be wrong to portray PPNC communities, and particularly PN (Yarmoukian) groups, purely as nomadic pastoralists lacking substantial and permanent settlements. In recent years, no site has done more to discredit this assumption than Sha'ar Hagolan (Garfinkel and Miller 2002). Excavations at this site have completely transformed previous perceptions of PN-lifeways. Covering an area of some 20 ha, featuring monumental building complexes with large courtyard houses separated by well-planned streets, Sha'ar Hagolan is the epitome of a vast and sedentary Yarmoukian village. A similar picture of settlement is also attested for the PPNC occupation phase at 'Ain Ghazal (Rollefson and Köhler-Rollefson 1993). The existence of such large villages has significant implications for the reconstruction of prevailing social and hierarchical systems, themselves of considerable importance when assessing the vulnerability of contemporary communities to hazard and associated conflict risk (see below). Especially, it gives cause to question the relationship between sedentary populations on the one hand and nomadic groups on the other. Are we dealing with members of the same communities, *i.e.* does the archaeological evidence attest to a system of transhumance, or were these two groups, with their contrasting lifeways, distinct from one another, perhaps to the extent that territorial disputes might have occurred?

Be this as it may, all the aforementioned developments (reduced range of wild species, increased reliance on domesticates, rise to eminence of sheep, and the appearance of pastoralist subsistence techniques) are innovations that are often mistakenly associated solely with the onset of the Pottery Neolithic. Notwithstanding, and it should be stressed, these innovations had already become established in the preceding PPNC and continued to flourish in the subsequent period. This realisation is especially significant since ^{14}C ages indicate a temporal overlap between PPNC and an interval of Rapid Climate Change (RCC) commencing at 8.600 calBP (*cf.* Weninger *et al.* 2009), thus suggestive of a causal relationship between climate and subsistence change. Further, absolute radiocarbon ages for the PN show that this period is contemporary with the entire RCC, including the last two to three centuries of the ninth millennium calBP when RCC reached its apex under the added impact of the Hudson Bay outflow (Fig. 2 and below).

Environmental Hazards (RCC)

On the basis of data from a range of palaeoclimate proxies from both the Eastern Mediterranean and the North Atlantic it has been demonstrated that RCC intervals (originally defined by Mayewski *et al.* 2004) have occurred on no fewer than five separate occasions during the Holocene: 10.2 ka calBP, 8.6-8.0 ka calBP, 6.5-5.8 ka calBP, 3.5-2.8 ka calBP, and the recent "*Little Ice Age*" (LIA; *c.* 1500-1900 calAD) (Weninger *et al.* 2009). RCC intervals are associated with a variety of meteorological impacts, ranging from increased frequencies and severities of drought, sporadically interrupted by the occurrence of intense precipitation events (downpours), to the enhanced likelihood of harsh winters and late wintery outbreaks with severe frosts. These impacts are causally related (*inter alia*) to the frequent recurrence of intense high pressure over Siberia in the winter months. Naturally, this is a highly simplified narrative of RCC and it is stressed that a plethora of different (*e.g.* physiographical and meteorological) factors would have determined RCC conditions at the local and micro-regional level.

Consequently, any attempt to reconstruct the rate and intensity of RCC in a given landscape is difficult, not least due to an acute deficiency of local, adequately high resolution and chronologically secure palaeoclimate proxies. Notwithstanding, especially for the southern Levantine interior, the water line of the Dead Sea represents a significant gauge for prehistoric aridity levels (Fig. 2). Remarkably, the onset of the 8.6-8.0 ka calBP RCC interval is marked by an unprecedented drop in the water level of the Dead Sea. For the first time in the Holocene the water level lay below the sill separating the northern from the southern Dead Sea basin (Migowski *et al.* 2006). Independent confirmation for the prevalence of arid conditions in this period is found in the aforementioned appearance in PPNC levels at 'Ain Ghazal of desert species such as wild onager and Desert Monitor lizard (Köhler-Rollefson *et al.* 1988: 429) coupled with only scarce remains of water-reliant (wild) pig at this site (von den Driesch and Wodtke 1997: 528) and at Abu Thawwab (Köhler-Rollefson 2001: 212).

On the other hand, the littoral plains of the Eastern Mediterranean (*e.g.* Thessaly, Cilicia, Gaza) may have been more frequently subjected to increased levels of precipitation. Analogous conditions are recorded, for example, in historical documentation relating to the LIA (Xoplaki *et al.* 2001; Tabak 2008) and at Atlit-Yam archaeobotanical analyses have recently shown that conditions were colder and more humid during the PPNC (Kislev *et al.* 2004). In this respect, particular note should be made of a further proxy, from Soreq, a karstic cave on the western flank of the Judean Hills where $\delta^{13}C$ concentrations in speleothems are (cautiously) interpreted as a proxy for flash-flood intensi-

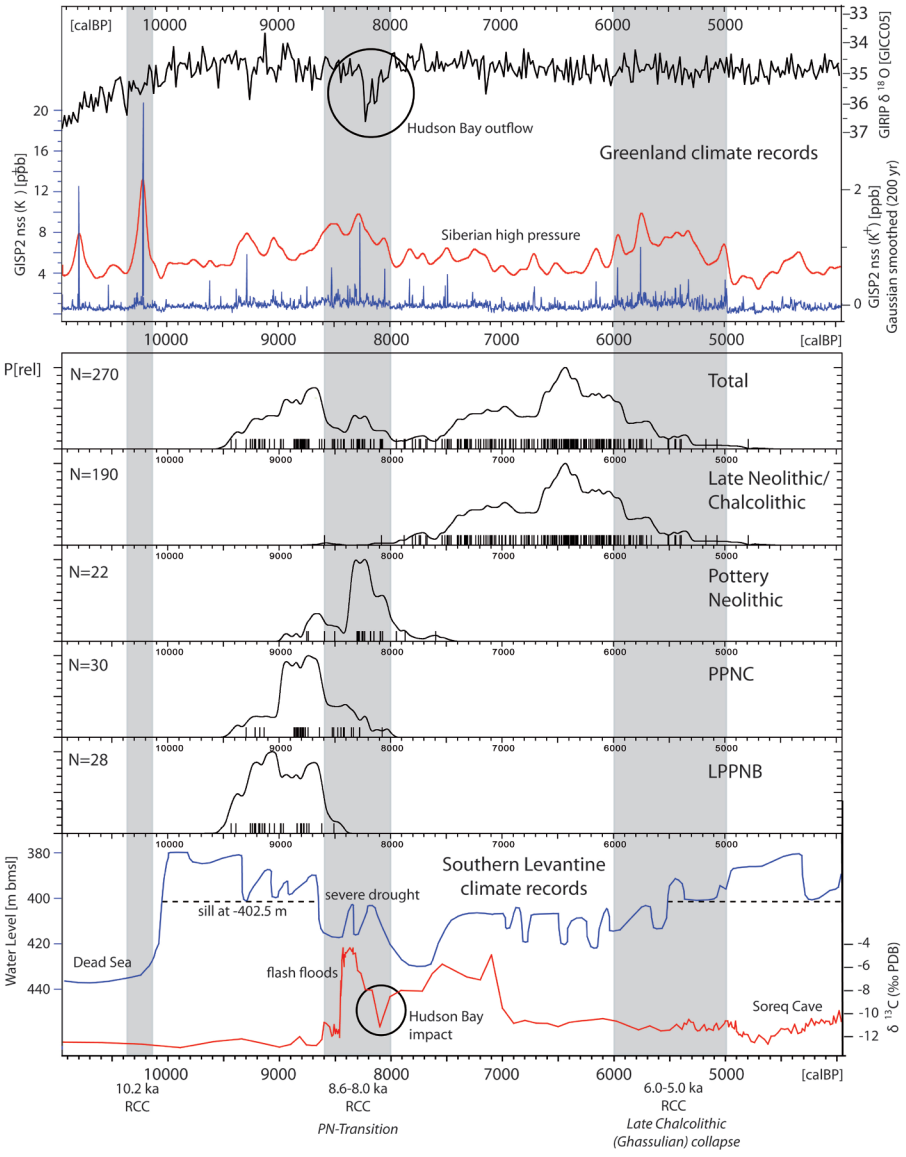

Fig. 2 Radiocarbon data from LPPNB, PPNC, PN, and Late Neolithic/Chalcolithic (LN/Ch) occupations in the southern Levant compared to palaeoclimate proxy data (cf. Tables 1-3). Top: Greenland GRIP (GICC05-age model) ice core stable oxygen isotopes δ18O (Grootes et al. 1993) and Greenland GISP2 ice core NSS [K+] chemical ions as marker for (Siberian high pressure) Rapid Climate Change (RCC). Bottom: Dead Sea levels as proxy for Holocene precipitation (Migowski et al. 2006) and Soreq Cave δ13C for flash flood intensity (Bar-Matthews et al. 2003). Grey columns denote Rapid Climate Change (RCC) intervals after Weninger et al. (2009). Whereas the 8.6-8.0 ka calBP RCC correlates with the 'PN-transition', the latter 6.0 ka calBP RCC corresponds to the 'Late Chalcolithic (Ghassulian) collapse' prior to the Early Bronze Age.

ty (Bar-Matthews *et al.* 2000; Weninger *et al.* 2009). These data show that whereas the initial centuries of the RCC interval were characterised by high values, *i.e.* high flash-flood frequency and intensity, at around 8.200 calBP there occurs a sharp reversal, perhaps causally related to more arid conditions associated with the 8.2 ka calBP (Hudson Bay outflow) event (Rohling and Pälike 2005). Significantly, these flash-floods have been linked to the genesis of *Yarmoukian rubble layers* observed covering numerous late aceramic (PPN) sites in the southern Levant (see contributions in Neo-Lithics 1/09). These rubble inundation events represent yet a further potential hazard to which contemporary populations were exposed; for example, PN transitional architecture at Basta has been found embedded within rubble layers (Gebel 2009). Thus, the question remains, was RCC contributory, or in any way catalyst, to the final abandonment of "*mega-sites*" at the end of the LPPNB?

RCC and Innovation: Absolute Dating Evidence[1]

Returning to the absolute dating evidence presented in figure 2, this requires further elaboration, particularly with regard to specific aspects of the three cultural transitions located within or adjacent to the 8.6-8.0 ka calBP (RCC) time frame. These transitions are: LPPNB to PPNC, PPNC to PN, and PN to Late Neolithic/Chalcolithic. All radiocarbon data to feature in figure 2 are filtered: Only data from securely established cultural contexts with standard deviations of less than 100 BP ($\pm 1\sigma$) have been considered, and all extreme outliers have been omitted. Even so, the remaining 270 ^{14}C ages cannot be taken at face value. As with all radiocarbon data, the context of each single ^{14}C age must be carefully scrutinized and due consideration must also be given to distortion caused by such factors as "*old wood*".

The most problematic data to feature in figure 2 stem from the PPNC. These data are from six sites: 'Ain Ghazal, Ashkelon, Atlit-Yam, Hagoshrim, Tel Ali and Tel Ramad (Table 2). Some of the most precarious data are ^{14}C ages from 'Ain Ghazal, though this is not unexpected. PPNC accumulations at this site are noted to have been particularly affected by "*stratigraphic interference*", *i.e.* admixture of older (LPPNB) material through ancient (PPNC) cutting (Rollefson and Köhler-Rollefson 1993: 34). This is particularly apparent in three ages (AA-5201, AA-5202, AA-5203) that are clearly too old; all are from the same sample, probably a piece of old wood (Rollefson and Köhler-Rollefson 1993: footnote 9). Other ^{14}C ages for PPNC occupation at 'Ain Ghazal are, however, substantiated by data from Atlit-Yam, as well as by a handful of ages from Ashkelon, Hagoshrim, Tel Ali and Tel Ramad. These suggest that the transition from LPPNB to PPNC be generously dated to around 8900-8600 calBP, *i.e.* prior to the onset of RCC. Notwithstanding, considering that all these sites (with the exception of Ashkelon and Atlit-Yam) also feature underlying PPNB levels, thus with the increased risk of admixture of older materials, this transition could yet prove younger. This is supported by the observation that most ^{14}C ages are made on samples of wood, charcoal and ash ("*old wood*"), with only very few measurements on short-lived materials, *e.g.* seeds, grain and twigs. Additional substantiation for a younger date for the LPPNB to PPNC transition is provided by the dispersal of ^{14}C ages from the LPPNB (Table 1) which is characterised by an abrupt break at around 8600 calBP.

Similar issues also apply to the subsequent cultural transition (PPNC to PN). Of the 22 available ^{14}C ages for the PN only two are measurements made on short-lived samples (OxA-9417 and HV-8509) and these are again among the youngest in this data set (*cf.* Table 2). The contemporaneousness of the southern Levantine PN and the duration of RCC is particularly remarkable, and gives reason to suppose that cultural, social, and economic developments of this period may reflect adaptation to fluctuating climatic and environmental conditions. In this respect it is especially fascinating to note the limited temporal duration of the Sha'ar Hagolan settlement. Although occupation of this site may reach back to PPNC times, ^{14}C ages made on materials, primarily from the upper (latest) occupation level of the site, indicate that abandonment was likely contemporary with the termination of RCC. Sha'ar Hagolan was probably abandoned around 8000 calBP.

The earliest reliable ^{14}C ages for the LN/Chalcolithic transition date this process to the early centuries of the eighth millennium calBP. It seems likely, however, that the two oldest ^{14}C ages (TO-1407, RT-1360) are outliers, and the third oldest age (Pta-3652) from Megadim, which stems from clay below the site, does not date cultural deposits but serves solely as *terminus post-quem* for occupation at this site (Table 3). A disregard of these measurements, although perhaps not suggestive of a hiatus in occupation between the PN and LN/Ch, does allure to a period of 'non-intense' settlement activity at this time. Interestingly, and perhaps not insignificantly, this time frame also correlates with the lowest observed water levels in the Dead Sea in the entire Early Holocene, *i.e.* it marks a time of extreme drought and/or high evaporation levels.

In summary, ^{14}C ages from the southern Levant indicate that the onset of RCC at 8600 calBP was contemporary with the collapse of the LPPNB. Further, there is a positive temporal correlation between RCC and the genesis and temporal extent of the PN in the region. However, the transition from LPPNB to PN (via the PPNC) remains one of the most urgent issues facing present Neolithic research in the southern Levant (Gebel 2002: 41); particularly the age and duration of the PPNC is still proving difficult to pin down. Finally, the close of the PN shows a high temporal coincidence with the end of RCC at around 8000 calBP. The relative paucity of ^{14}C ages for the PN to LN/Ch transition may be indicative of a temporal hiatus or decrease in occupation activity. Consequently, on the basis of absolute dating evidence, it can be assumed that there is indeed

a positive relationship between climate, cultural transition, and societal and technological innovation in the southern Levant in the ninth millennium calBP. This is further substantiated by a parallel observation which sees the positive temporal correlation between the onset of a renewed interval of RCC at 6.0 ka calBP and the collapse of Late Chalcolithic (Ghassulian) systems.

Assessing Conflict Risk

Numerous recent contributions have highlighted correlations between episodes of climate change and the occurrence of armed conflict (*e.g.* Zhang *et al.* 2007; Burke *et al.* 2009 and citations therein). Among these studies, analyses undertaken by Zhang *et al.* (2007) for the "*Little Ice Age*" (LIA) are particularly enlightening. Adhering to a Malthusian approach, whereby a decline in land-carrying capacity is linked to temperature fluctuations, these in turn effecting a decrease in food supplies and promoting migrations, famines and armed conflicts, Zhang *et al.* present sound empirical evidence that climate change and warfare frequency are significantly correlated. This correlation is proven to exist in all landscapes, irrelevant of geographical location. Significantly, however, highest correlations are noted for arid regions, *i.e.* North Africa, the Levant, Anatolia, and central parts of Asia (Zhang *et al.* 2007: 19216).

In accepting the evidence presented by Zhang *et al.*, a positive correlation between climate, failing resources and an escalation of violence is acknowledged. Although a rational inference, and empirically substantiated elsewhere (*e.g.* Ember and Ember 1992), this approach does little to highlight the precise background of conflict and gives no credit to the adaptive and mediating capacities of human systems and individuals. Indeed, a straight forward Darwinian "*natural selection*" scenario is implied (*cf.* Bauer 2009). In contrast, it should be stressed that warfare is not born simply of a scarcity of victuals, though this can be a consequential addition, but that it can constitute an intrinsic element of prevailing economic and social systems. Especially in traditional societies warfare can serve distinctly symbolic social functions, irrelevant of prevailing resource affluence or dearth (*e.g.* Fadiman 1982). Additionally, there exist numerous other alternatives to warfare which can be implemented to cope with resource shortages (migration, trade, reciprocity *etc.*) and there are certainly social mechanisms and circumstances which make armed conflict an undesirable option.

Therefore, in order to more accurately assess the level of conflict risk among PN-transitional communities a firm understanding of their social and economic systems and particularly their capacity to counter RCC impacts is required. Using methods developed in the modern scientific discipline of risk management (*e.g.* Blaikie *et al.* 1994) the "*vulnerability*" of societies to natural hazards, such as prolonged drought, recurrent severe winters, storms *etc.*, can be analysed. This then provides a fundamental insight into the level of conflict potential and the likelihood of inter-group violence during RCC. In other words, capacity for conflict in PN-transitional systems would have been determined not only by the nature of the afflicting hazard but also by the characteristics of prevailing societal structures to be affected. Accordingly, assessment of conflict capacity must take into account not only the scale, severity, frequency and longevity of the natural hazard and the susceptibility of animals, crops and other resources thereto ("*biophysical vulnerability*") but also the efficiency of available buffering strategies, prevailing levels of societal stability and hierarchical systems, and local traditions governing resource access ("*social vulnerability*"). Hence, "*social vulnerability*" is particularly informative when attempting to identify characteristics of social systems that might have favoured sudden outbursts of inter-group conflict in lieu of other (less violent) strategies. For a more in depth discussion of "*vulnerability*", in particular in relation to RCC, see Clare and Weninger (in press). Finally, one last aspect that should not be overlooked is the increased vulnerability experienced by societies in cultural transition. We will return to a discussion of these points further below.

Archaeological Evidence for Warfare at the PN-Transition

Archaeological evidence for warfare at the PN-transition is slight and, if real, illustrates quite succinctly why any hypothesis to posit the existence of an over-simplified autogenetic mechanism linking environmental stress and warfare is wrong. On the other hand, and as rightly indicated in many other studies, a lack of evidence cannot be inexorably equated with an absence of intergroup fighting. One way to progress, therefore, is to consider in more detail the potential character of combat as might be expected for the late ninth millennium calBP.

Concerning tactics, ambush and surprise attacks are by far the most frequent form of warfare among non-centralised communities, they causing the highest proportion of war related casualties (Otterbein 2009). Further, in areas with low population densities, as would have been the case in the southern Levant during the PN-transition, head-hunting and other forms of conspicuous cruelty can be instrumental in terrorising and expelling an enemy from an area (Helbling 2006). In the case of the latter of these tactics, it is not likely that headhunting would have left any great impression in the archaeological record. Only the preoccupation with the human cranium in the antecedent PPNB might indirectly allude to such practices (*cf.* Guilaine, this volume). Ambush, on the other hand, has the potential to leave behind more substantial material evidence. In this respect, more specific to pastoral societies of the PN-transition might have been the rustling and robbing of sheep and goats rather than the sacking of settlements, which might have left behind burned layers in settlements, for which there is incidentally no evidence. Sur-

prise attacks can be launched both from a distance, using long-range weapons, as well as at close quarters. The latter is by far the most dangerous option and culminates in the most casualties. On the basis of these insights a range of objects and features might be expected to occur in the material of bellicose PN-transitional sites. These include sling missiles and arrowheads (long-range weapons); axes, adzes, mace heads and daggers (for fighting at close quarters); body armour and shields; as well as, but perhaps to a lesser extent, fortification structures surrounding settlements. Naturally, this list is by no means exhaustive and must be supplemented by countless other lines of evidence to be gleaned from the archaeological record (*cf.* LeBlanc, this volume). Further, not all these features need be present for warfare to have occurred. In the following I provide a brief outline of the most accessible archaeological evidence (fortifications, short-range and long-range weapons). It goes without saying that more in depth studies are necessary. Nevertheless, I believe the following to be representative.

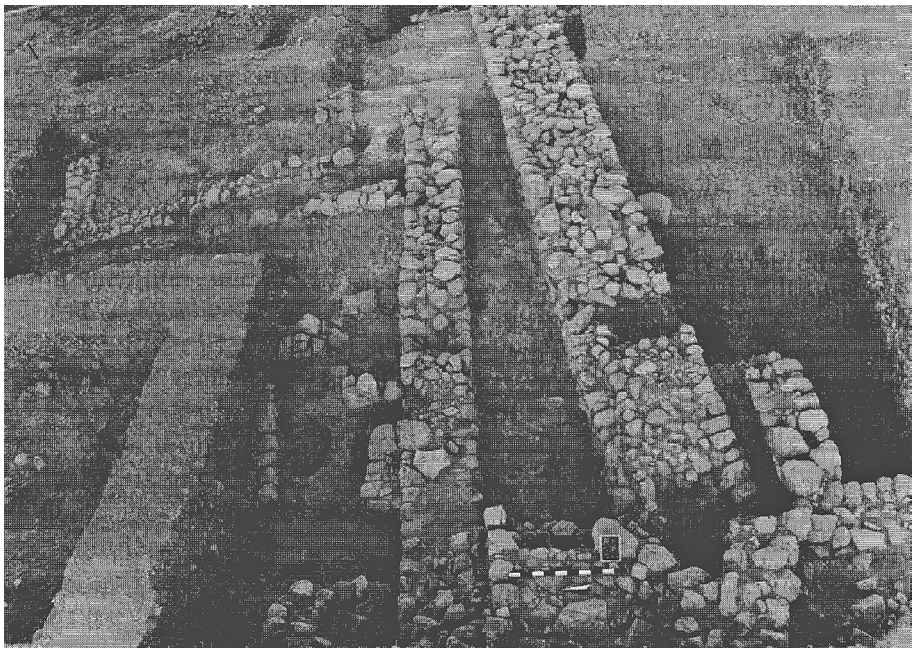

Fig. 3 The "Great Wall" at 'Ain Ghazal (Rollefson and Köhler-Rollefson 1993: fig. 3).

Fortifications

Defensive structures erected around settlements are one clear line of evidence for inter-group conflict in the prehistoric record. Concerning the Neolithic in the southern Levant the PPNA tower and walls at Jericho are certainly among the most prominent of such structures; whereas their function as a fortification was formerly questioned by Bar-Yosef (1986), LeBlanc (this volume) has commendably reopened this discussion. In the subsequent PPNB the agglomerated (Pueblo) building style observed at the Transjordanian "*mega-sites*" could also be discussed with respect to its advantages as a means of fortification. However, for the PN-transition, the focus of this paper, no obvious fortification structures are known, and structures which might be discussed in this context are, to say the least, ambiguous. Possibly the most curious of these is the "*Great Wall*" at 'Ain Ghazal (Rollefson *et al.* 1991: 108-109; Rollefson and Köhler-Rollefson 1993). This structure was preserved to a height of *c.* 60 cm, was 1.40 m wide and uncovered along a length of some 11 metres (Fig. 3). Although not excavated in its entirety – its south-eastern end was destroyed by bulldozers and in the north-west it continued into unexcavated sediments – its dimensions are certainly impressive. Indeed, its magnitude is all the more astounding given the scale of formal domestic architecture and the flimsy nature of some of certain structures erected in the Yarmoukian. The wall itself was probably erected in the LPPNB but was maintained throughout the PPNC and into the Yarmoukian period. At one time (LPPNB/PPNC) there also appears to have been a narrow gateway (c. 1,00 m across) which was filled in during the Yarmoukian (pers. comm. G. Rollefson, June 2010). Curiously, on both sides of the wall the ground surfaces were covered with a coating of ersatz plaster ("*huwwar*", a mixture of ground-up chalk and mud). "*Huwwar*" is characte-

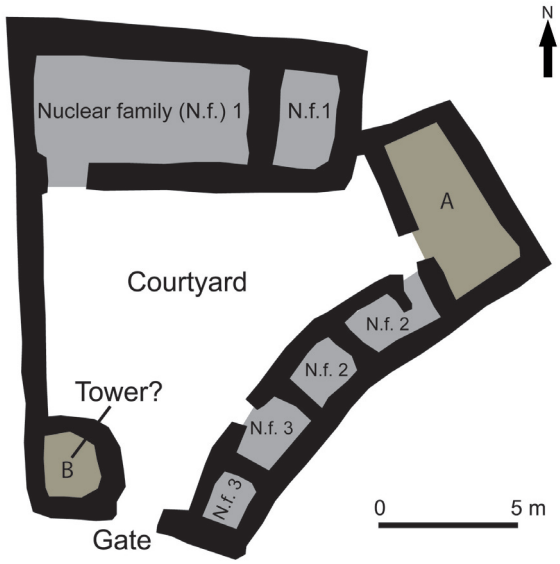

Fig. 4 Sha'ar Hagolan. Building complex 1, Area E (adapted from Garfinkel 2002b: fig. 19.3). Living quarters of three nuclear families (N.f.1, N.f. 2, N.f. 3) in a courtyard setting, with Rooms A and B serving members of the extended family (Garfinkel 2002b). In this paper an alternative interpretation of Room B is posited; both its proximity to the narrow courtyard entrance and its roughly circular ground plan convey a more defensive function.

ristic of the PPNC and Yarmoukian. Remarkably, no cross walls or corners were observed, and this might speak in favour of this structure having being erected as a fortification element. Strangely, the wall was found to separate a number of family courtyards, or possibly one part of the settlement from another (Rollefson and Köhler-Rollefson 1993: 38), but this is certainly not a function which sufficiently explains its overwhelming scale.

At Sha'ar Hagolan fortification walls have not been discovered, though this may be due to excavations having been limited to central areas of the site. The lack of attention paid to site peripheries is a frequently noted factor when debating archaeological evidence for warfare (or rather the absence thereof). Even so, a lack of obvious physical defences such as settlement walls and towers should not be considered synonymous with the non-occurrence of inter-group conflict with settlement raiding (*cf.* Roscoe 2008 and this volume). However, at Sha'ar Hagolan domestic architecture itself might be considered in the context of fortification. At this settlement it is remarkable that each domestic unit (courtyard house) is enclosed within a substantial stone-based wall. This wall features not only a narrow, easily obstructed and defendable gateway but, in the case of the most completely excavated unit, there are also observed the foundations of a tower-like structure in direct proximity to the entrance (Fig. 4). For a settlement comprising '*fortified*' domestic units an external wall surrounding the settlement might even have been superfluous. Warning of trespassers and additional protection against intruders might also have been provided by guard dogs (*cf.* LeBlanc, this volume).

Short-Range Weapons

Axes, adzes, and knives were not only important accessories of the Neolithic toolkit but were undoubtedly the foremost weapons for close-quarters combat. Remarkably, this dual-functionality applies to practically all conceivable forms of Neolithic weapons, including the bow, and to a lesser extent the sling (see below). Accordingly, these objects have recently been referred to as "*tool-weapons*" (Chapman 2004). Significantly, at Abu Thawwab knives constitute 13.6% of tools (in assemblages from 1984/1985 excavations) making them the third most numerous tool type after sickle blades and retouched flakes (Wada 2001: 119-120, Table 2). Also from this site is the fragment of a small basalt axe (Wada 2001: fig. 12.2) and an adze (Wada 2001: fig. 10.3). Slightly further north, at Sha'ar Hagolan, excavations by Stekelis also provided a number of objects of interest in the context of close-quarters fighting. These include fragments of pressure retouched blades and knives (Stekelis 1972: 20-21, plate 24) as well as significant numbers of axes, adzes and picks (Stekelis 1972: 12). At 'Ain Ghazal, however, there is a trend which sees a reduction in the ratio of knives observed in Neolithic assemblages from the PPNB (16.9%) to the PPNC (9.4%) and Yarmoukian (7.7%) (Rollefson *et al.* 1991: table 4). If representative, this development is analogue to the aforementioned decrease in the ratio of arrowheads at PN-transitional sites which has been correlated with the reduced emphasis placed on hunting. Significantly, the parallel reduction in arrowheads and knives might even suggest that we are also witnessing a decline in inter-group violence.

Moving on, a select number of objects fail to fulfil the criteria of "*tool-weapons*" instead warranting an interpretation as "*prestige-weapons*". The most notable of these is the mace but might also extend to the aforementioned pressure retouched knives (daggers) from Sha'ar Hagolan. In this respect, discussion is required as to whether "*prestige-weapons*" (still) actually functioned as weapons. As pointed out by LeBlanc (this volume) the British monarch also carries a mace, though enemies and adversaries of the Crown are no longer bludgeoned. However, due to its earlier bellicose function the mace is symbolically charged; it is a sign of power, imparting fear and intimidating antagonists, albeit that it is no longer retains its former violent function. Three possible mace heads were discovered at Abu Thawwab; the first is only partly preserved, made of basalt and with a reconstructed diameter of about 6 cm, whilst the second is an unfinished example made from a marble-like material (Wada 2001: 178, fig. 10.2, fig. 10.5). At Sha'ar Hagolan objects interpreted as "*drilled weights*" are particularly common (Garfinkel 2002a) and one wonders whether or not at least some of these pieces were also maces.

Finally, considering preservation it is certain that not all Neolithic weapons have remained preserved in the archaeological record. This would have been the case had early pastoralists carried the typical short-range weapon of Palestinian shepherds as reported in historical accounts by early Twentieth Century eye-witnesses: "*they all carry* [...] *massive clubs* [...] *of oak, formidable weapons which grow into a lump of knotted wood at the extremity*" (Rendall 1909: 899).

Long-Range Weapons

In spite of the frequently stated reduction in the ratio of arrowheads in the PPNC and PN these artefacts are still a persistent feature of lithic assemblages at most PN-transitional sites. At 'Ain Ghazal, for example, ratios of arrowheads remain relatively stable in all occupation phases, ranging from 6% to 7% in M/LPPNB to around 5% in the PN-transitional period (Rollefson and Köhler-Rollefson 1993: 35). This consistency is interpreted as reflecting a surplus production for use by herders residing in the steppe and desert where gazelle and other arid species would have been hunted in preference to slaughtering goats and sheep from herds. Alternatively, however, this production could equally be attributed to the protection of herds from assailants bent on rustling animals or asserting control over pastures and grazing rights. In PPNC levels at Tel Ali arrowheads constitute

5.5% (N=24) of tools (Garfinkel 1994: table 6, 555) and at Abu Thawwab they are the fifth most common tool class (7.6%; N=9) (Kafafi 1993). Whereas the ratio of points from Wadi Shu'eib decreases from LPPNB (5.6%; N=8) to PPNC (3.3%; N=56), in the subsequent Yarmoukian there is a marked increase, they becoming the fourth most frequent tool type (7.4%; N=28) (Simmons *et al.* 2001: table 2).

Among the characteristic arrowhead types in the final centuries of the ninth millennium calBP are Ha-Parsa, Nizzanim and Herzliya points (Gopher 1994a). These are made by bifacial pressure flaking of both surfaces. Although morphologically similar to earlier types they differ in their relatively small dimensions (usually less than 4 cm in length) and in the quality of preparation and finish. Arrowheads from the PN are also characterised by evident changes in other attributes, mainly the shaping of tangs and barbs, as well as retouch amount and location. These changes have been attributed to variations in hunting methods and equipment (Gopher 1994a: 564), thus possibly even marking artefact adaptation to/from warfare. Arrowheads disappear in Wadi Rabah assemblages almost entirely (Gopher 1994a: 564), a development that may be associated with the diffusion of the sling from Anatolia and the northern Levant at this time.

In central and south-western Anatolia the mid-ninth millennium calBP is associated with the ascent of the sling as the most widespread long range weapon (Clare *et al.* 2008). In fact, bi-conical clay sling projectiles become such a common feature at this time that they are meanwhile regarded as a fundamental new addition to the Neolithic package that was acquired in the course of its westward diffusion from the northern Levant through the Anatolian peninsula (Çilingiroğlu 2005; Özdoğan 2008). Remarkably, the increased frequency of clay (and occasionally stone) sling projectiles in Anatolian assemblages runs parallel to a reduction in the number of arrowheads. This development mirrors the aforementioned trend in the southern Levant, albeit that PPNC and PN assemblages register no influx of sling projectiles. This is by no means a recent observation but was already discernible in Korfmann's seminal study from nearly four decades ago (Korfmann 1972). Indeed, it is not until the Wadi Rabah culture (LN/ECh) that sling projectiles appear in any significant number in the southern Levant, and even then not at all sites (Rosenberg in press: fig. 1). Earlier finds from PPN and PN contexts in the region are few and far between, *e.g.* isolated examples are known from Beidha (PPNB), and, of particular relevance to this paper, from a PPNC locus at Hagoshrim and in a Lodian / Jericho IX context at Nahal Zehora II (Rosenberg in press). Significantly, however, even in the Wadi Rabah culture numbers are still exceedingly low and projectiles are fashioned solely from stone, primarily limestone, and not clay as in the north (Rosenberg in press). Ironically, the paucity of evidence for the sling in agro-pastoral communities in the southern Levant could lend support to its interpretation primarily as a weapon of aggression further north in Syria and Anatolia. Had the sling been a mere "*shepherd's implement*" (*e.g.* Perlès 2001: 229-231) it would surely have been of equal use to herdsmen in both regions, unless of course there was a conscious decision against this "*tool-weapon*" (Chapman 2004) in the South. In combination with the reduction of arrowheads and the ambiguous evidence for fortification structures, harmonious times for the southern Levant might even be suggested, at least during the PN, and this is indeed the picture that is beginning to emerge.

Discussion

In line with the Malthusian-Darwinian approach ('*survival of the fittest*'), an absence of warfare might be interpreted as indicative of the absence of any serious deficiencies in prevailing carrying capacities during the PN transition in the southern Levant. However, there is ample archaeological, geomorphological, archaeobotanical, archaeozoological and archaeoentomological evidence that contemporary communities must certainly were affected by both RCC-related meteorological impacts and anthropogenic induced environmental degradation. These lines of evidence include, for example, severe effects of anthropogenic activities upon landscapes surrounding 'Ain Ghazal (Rollefson and Köhler-Rollefson 1989), the numerous documented accounts of inundation of (parts of) late PPN settlements by "*Yarmoukian rubble slides*", and the earliest evidence of pest beetle (*S. granarius*) in stored grain due to colder and more humid conditions (Kislev *et al.* 2004). Not only this, but the transition to pastoral regimes is also widely considered to be a clear indicator for adaptation to harsher environmental conditions and increasingly limited carrying capacities. On the other hand, specialisation on sheep rather than the hardier goat, as for example at 'Ain Ghazal (see above), would have rendered communities more vulnerable to impacts of drought (increased "*biophysical vulnerability*). Indeed, there is archaeological evidence that even goat herds may have dwindled during RCC. At 'Ain Ghazal a size increase in goats during the PPNC has been attributed to a return to the hunting of wild goats, or the capture thereof, in order to replenish diminished domesticated stocks following the "*general crisis*" to have affected the site during this period (von den Driesch and Wodtke 1997: 519). Finally, although not a direct source of evidence, the aforementioned absolute chronological data indicate major cultural shifts in the second half of the ninth millennium calBP, marking not only the end of the LPPNB, but also the peak and subsequent termination of the Pottery Neolithic (Fig. 2) which could attest to adaptation to climate and environmental change.

Through the absence of evidence for armed conflict it is implied that resource shortfalls, or indeed crises, were successfully managed by buffering mechanisms and coping strategies other than violence. This realisation must have implications for our comprehension and reconstruction of PN-transitional social systems.

Previously, comparatively little attention has been paid to this area, though Simmons (2000: 223) has noted, as I have done in this paper, the high potential for intergroup stress during the PPNC due to increasingly difficult farming and herding conditions. This, he states, would have led to the increased risk of violence, for which he also accentuates the acute lack of data. Accordingly, this absence is attributed by him to processes of social fragmentation and a return to tribal society. In these situations emphasis would have lain increasingly on nuclear families (*cf.* Gebel 2002). Pastoralism was oriented toward smaller group sizes, meaning that the elaborate social controls of the LPPNB were no longer required. Further, the rise to dominance of animal husbandry, a traditional male activity, might have effected a reduction in the role of women, and a parallel ascension in the status of men, as might be reflected in an increase in the frequency of male figurines (Simmons 2000: 224-225); for a converse view, positing an at least partial return to gender egalitarianism during periods of higher mobility, see Gebel (in press).

"Big Men" at the PN-Transition?

Considering that PN-transitional polities might have been composed of an array of semi-mobile and sedentary tribal groups headed by competing "*Big Men*" (Sahlins 1963), the apparent lack of material evidence for warfare and associated paraphernalia proves all the more confounding. Although the presence of "*Big Men*" at the PN-transition cannot be confirmed, this could be due to the general absence of burials from PN (Yarmoukian) contexts. Notwithstanding, for the PPNC a comparatively large number of burials are known, albeit from just two sites. Particularly outstanding are the 27 PPNC interments excavated at 'Ain Ghazal, where a "*distinction among people*" is posited on the grounds of two different burial traditions ("*primary/courtyard*" and "*secondary/structure*" burials) (Rollefson and Köhler-Rollefson 1993: 38-39), and at Atlit-Yam on the Carmel Coast, where a total of 46 PPNC burials (61 individuals) were recently uncovered (Galili *et al.* 2005). Common for the burials at both these sites are the dominance of primary interments, the occurrence of multiple burials, and the decline in the tradition of skull removal, all traits that diverge from former PPNB burial customs. At Atlit-Yam grave goods (flint artefacts, bone tools *etc*.) were found deposited mainly in primary burials of both males and females, with no clear spatial pattern in the dispersal of these gift–bearing graves on the site. Flint axes, however, were only observed in association with the skeletons of males and children; might this be indicative of male dominated societal systems with a bent for objects of status?

For the subsequent Yarmoukian, however, only very few burials are known, one is noted from 'Ain Ghazal (Banning 2009) and at Sha'ar Hagolan two inhumations from this culture have been documented (Garfinkel 2002b: 261). Unfortunately, in these latter cases there is no clear material indication for the existence of social hierarchies. Therefore, in spite of the two-tier hierarchical system implied by the PPNC evidence from 'Ain Ghazal and the minor gender related deviations at Atlit-Yam, we are still a far cry away from confirmation of the "*Big Men*"-scenario. Turning instead to ethnographic and historical data, we are well informed as to the various types of social systems that can occur in pastoral societies, ranging from autonomous kinship groups to highly centralised polities (*e.g.* the Mongols of the thirteenth century calAD). Plainly, it is the former of these, *i.e.* autonomous kinship groups, that surely best reflect southern Levantine systems in the mid- to late ninth millennium calBP.

Nuclear Families

Autonomous kinship groups would have been by far the best adapted to deal with the environmental and societal stress at the PN-transition. Indeed, they would have been by far the best suited to cope with the aforementioned processes of societal fragmentation following the LPPNB, processes which placed greater emphasis particularly on the role of the nuclear family. Significantly, social systems in which nuclear families are fundamental constituents are more common in situations with an increased emphasis on shared communal resources where control is not essential and where rights to graze and water are commonly held by local kin groups (Fratkin 2003: 8; and below). Further, small households are more effective at passing any hereditary resources from generation to generation, *i.e.* with an inherently lower conflict risk. Finally, small households are also the best suited to systems with a high degree of mobility or where there is linear scheduling of spatially restricted resources (Byrd 2000: 90 and citations therein). In this context, storage is also a key factor and warrants due consideration. If centrally organised storage is correlated with conflict risk, it is not insignificant that less physical space within PN settlements was dedicated to storage purposes (Kuijt 2008: 308). Yet, a reduction in the scale and nature of storage at this time may not surprise, especially given that large portions of subsistence assets were probably kept on the hoof.

"Resource Corporate Groups"

The advantages of nuclear families at times of extreme environmental stress become more apparent when compared to difficulties experienced by "*residential corporate groups*" (Hayden and Cannon 1982). "*Residential corporate groups*" are collectives of two or more nuclear families which exhibit a recognisable degree of residential coherency. "*Residential corporate groups*" are closed units whose genesis is intrinsically linked to conditions of mild economic or environmental pressures. They exert a pervasive influence on all aspects of individuals' lives, including marriage, post-

marital residence, economic production, as well as feasting and celebrations. Most remarkably, "*residental corporate groups*" have been found to be non-adaptive both under conditions of extreme resource abundance and scarcity. Therefore, although shown to emerge at times of moderate shortage, during which these groups (or lineages) can exert control over given resources, situations with abundance and scarcity are consistent with loss of control, thus forcing groups to disband into their component nuclear families (Hayden and Cannon 1982: 149-152 and citations therein). Significantly, the social system posited for the larger Yarmoukian centre at Sha'ar Hagolan is one of "*resident corporate groups*": On the grounds of architecture and layout of this settlement, Garfinkel (2002b) has proposed a societal model centred on extended families comprising three or more nuclear families residing in closed courtyard dwelling structures (Fig. 4). Interestingly, the ^{14}C ages available for this settlement suggest that it was abandoned at the close of the PN period. The reason for this abandonment might then be sought in the shortcomings of "*resident corporate groups*" which disbanded when overwhelmed by situations of extreme resource pressures or surplus. Such scenarios might have been either connected with a period of pronounced scarcity induced by the combination of RCC and impacts of the 8.2 ka calBP Hudson Bay event (see above) or linked to an increase in reliable pasture and farmland following the abatement of RCC in the early eighth millennium calBP. Whereas acute resource scarcity would have led to increased competition for agricultural land and pastures, resource affluence would have provided increased access to land and grazing, thus neutralising any advantages held. Although warfare might then be expected at times of group disbandment, any return to social structures centring on nuclear families would have rapidly mitigated conflict risk.

Reconstruction of PN-Transitional Society

Social structures of uncentralised societies determine not only the extent to which violence occurs but also against whom it is directed, whether at others within the same society, at outsiders, or in both directions. Factors such as the organisation of interest groups, exogamous marriage, and the state of cross-cutting ties among local communities of the same society are all important in shaping violence (Ross 1986). On the basis of these observations and the lack of evidence for armed conflict a tentative reconstruction of PN-transitional social systems can be made. Accordingly, perhaps with the exception of large PN centres such as Sha'ar Hagolan, PN-transitional societies were based on small units (nuclear families) and characterised by low cross-cutting ties, *i.e.* limited links between different members of the same community and different communities in the same society; conversely, strong cross-cutting ties result in an increase in external warfare in uncentralised societies. On the other hand, it is notable that low cross-cutting ties are also synonymous with higher frequencies of internal fighting. PN-transitional societies would most likely not have known endogamy, strong local marriage, which is also associated with higher rates of external warfare in uncentralised societies; intercommunity marriage would have paved the way for stronger links between communities and at the same time reduced the risk of inter-group conflict (Ross 1986: 453-454).

Finally, PN-transitional societies would have employed strategies other than raiding, rustling, murder and massacre to overcome environmental stress and resource shortages. These strategies would have included, for example, resource distribution, reciprocity, trade, mobility and migration, as well as economic change and subsistence innovation. Therefore, it is posited that the absence of conflict and warfare in the southern Levant at this time must lie in the nature of PN-transitional communities themselves and, most significantly, in the commitment of pastoralists to their livestock. Indeed, this is a complex relationship which dictates much of the character of pastoral society. Pastoralists must organise household production to suit the needs of the animals, and these must be herded over wide areas to ensure adequate pasture. This alone requires a social organisation that not only emphasises household autonomy, mutual cooperation and defence, but also the maintenance of good social ties over a wide geographic area (Fratkin 2003). These are all factors which must have prevailed among southern Levantine PN-transitional agro-pastoral populations and which were instrumental in the mitigation of conflict between these communities. Consequently, Bar-Yosef's assumption (this volume) that archaeological evidence for violence should increase during the 8.2 ka calBP event cannot be substantiated, at least for the southern Levant.

Conclusions

If we follow the still widely propagated hypothesis that warfare first became endemic upon the onset of sedentary lifeways (concerning "*pacification of the past*" see LeBlanc, this volume), it should follow that any return to semi-sedentary, more mobile and nomadic traditions, as at the PN transition, must also mark a restoration of more peaceful and harmonious times. However, this general assumption (sedentary lifeways → warfare) has found considerable criticism in recent years, with numerous authors providing anthropological, historical and archaeological evidence for the widespread occurrence of fighting in traditional, mobile and semi-mobile hunter-gatherer communities (*cf.* LeBlanc, this volume). Notwithstanding, the current debate on prehistoric warfare is still dominated by the Malthus paradigm and the assumption that a combination of a reduction in carrying capacity and resource shortages generates an auto-catalytic process: armed conflict. Perhaps on account of this, the philosophical trend can be observed to have swayed in another, more disturbing, indeed

dangerous, direction, a direction which regards organised violence as an inherent constituent of human nature that can be traced back to our uncivilized primate past, thus providing the ultimate justification for any rally to arms, be it in prehistory, history, the present or in the future, *i.e.* much akin to the Darwinian scenarios of "*natural selection*" and "*survival of the fittest*".

Instead, in this paper I hope to have adequately stressed that warfare is not a direct consequence of resource shortages but is just one in a whole repertoire of possible buffering mechanisms employed in times of environmental stress. More generally, warfare can serve distinctly symbolic economic and social functions, irrelevant of prevailing resource affluence. Nevertheless, PN-transitional communities were visibly vulnerable, both biophysically and socially, to the impacts of climate change and the consequences of environmental degradation at their own hands (or those of their forebears). Adaptation processes leading to the formation of agro-pastoral lifeways in the course of the PPNC and PN were likely tantamount to their survival, as were inter-group cooperation and collaboration. On the grounds of available absolute dating evidence, the end of the LPPNB can be dated to approximately 8600 calBP, *i.e.* the onset of Rapid Climate Change (RCC). Although the exact timing for the initial PPNC is difficult to identify, due to such factors as "*old wood*", it seems fair to speculate that RCC impacts, if not causal, were certainly catalyst in the rise of agro-pastoralism as the dominant subsistence form in the southern Levant. Finally, it was the genesis of the latter, and the rise of adapted socio-economic systems, that just might have resulted in a less bellicose character of contemporary communities. Nevertheless, conflict and violence can certainly not be ruled out, albeit that the motivation for such actions should not be sought solely in the sphere of resource shortfalls.

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Notes

1 Archaeological chronologies discussed in this paper are based on tree-ring calibrated ^{14}C-ages measured on terrestrial samples. Numerical ages are given in the calendric time scale using [calBP] units with the year AD1950 = 0 calBP as reference. Conventional ^{14}C-ages (Tab. 1-3) are given on the ^{14}C-scale with units [^{14}C-BP]. All tree-ring calibrated ^{14}C-ages were obtained using CalPal software (www.calpal.de) based on methods described by Weninger (1997).

Lab-Nr.	14C-Age (BP)	Material	Culture	Site	Source
KN-4880	7726 ± 73	seeds	LPPNB	'Ain Ghazal	1
KN-4882	7809 ± 74	seeds	LPPNB	'Ain Ghazal	1
KN-4881	7880 ± 82	seeds	LPPNB	'Ain Ghazal	1
AA-25427	7910 ± 60	charcoal	LPPNB	'Ain Ghazal	1
AA-25428	7910 ± 60	charcoal	LPPNB	'Ain Ghazal	1
KN-4885	7939 ± 87	charcoal	LPPNB	'Ain Ghazal	1
KN-4879	7952 ± 77	charcoal	LPPNB	'Ain Ghazal	1
AA-25429	7980 ± 55	seeds	LPPNB	'Ain Ghazal	1
AA-5206	7990 ± 80	organic material	LPPNB	'Ain Ghazal	1
AA-25425	8080 ± 65	charcoal	LPPNB	'Ain Ghazal	1
KN-5056	8083 ± 47	charcoal	LPPNB	'Ain Ghazal	1
AA-5197	8090 ± 75	charcoal	LPPNB	'Ain Ghazal	1
KN-5055	8162 ± 62	charcoal	LPPNB	'Ain Ghazal	1
GrN-12972	8165 ± 50	charcoal	LPPNB	'Ain Ghazal	1
AA-25426	8205 ± 65	charcoal	LPPNB	'Ain Ghazal	1
KN-4877	8208 ± 77	charcoal	LPPNB	'Ain Ghazal	1
KN-4883	8230 ± 76	charcoal	LPPNB	'Ain Ghazal	1
KN-5054	8236 ± 81	charcoal	LPPNB	'Ain Ghazal	1
KN-4878	8253 ± 76	charcoal	LPPNB	'Ain Ghazal	1
AA-5199	8270 ± 75	charcoal	LPPNB	'Ain Ghazal	1
GrN-12971	8460 ± 90	charcoal	LPPNB	'Ain Ghazal	1
OxA-2412	8275 ± 80	charcoal	LPPNB	Azraq 31	1
Bln-5035	7887 ± 43	charcoal	LPPNB	Ba'ja	1
Bln-5036	7910 ± 44	charcoal	LPPNB	Ba'ja	1
Bln-5123	8100 ± 33	n.d.	LPPNB	Ba'ja	1
BM-2349	8190 ± 60	charcoal	LPPNB	Dhuweila	1
GrN-26146	8120 ± 60	charcoal	LPPNB	Khirbet Hammam	1
GrN-26147	8370 ± 40	charcoal	LPPNB	Khirbet Hammam	1

Table 1 List of ^{14}C ages from southern Levantine LPPNB sites (cf. fig. 2 and text). Sources: 1. Böhner and Schyle 2009; 2. Galili et al. 2002; 3. Haber and Dayan 2004; 4. Garfinkel et al. 2006; 5. Garfinkel and Miller 2002c; 6. Galili et al. 1997; 7. Burton and Levy 2001; 8. Kuijt and Chesson 2002.

Lab-Nr.	14C-Age (BP)	Material	Culture	Site	Source
GrN-17494	7825 ± 65	charcoal	PPNC	‘Ain Ghazal	1, 10
AA-5205	7895 ± 95	charcoal	PPNC	‘Ain Ghazal	10
GrN-17495	7915 ± 95	charcoal	PPNC	‘Ain Ghazal	1, 10
AA-5198	7960 ± 75	charcoal	PPNC	‘Ain Ghazal	1, 10
AA-5203	8200 ± 75	wood	PPNC	‘Ain Ghazal	1
AA-5201	8235 ± 70	wood	PPNC	‘Ain Ghazal	1
AA-5202	8310 ± 70	wood	PPNC	‘Ain Ghazal	1
OxA-7881	7630 ± 65	ash	PPNC	Ashkelon	1, 10
OxA-7916	7935 ± 50	ash	PPNC	Ashkelon	1, 10
OxA-7883	7990 ± 90	ash	PPNC	Ashkelon	1, 10
OxA-7915	7995 ± 50	ash	PPNC	Ashkelon	1, 10
RT-3043	7250 ± 45	waterlogged wood	PPNC	Atlit-Yam	2
RT-2479	7460 ± 55	waterlogged branch	PPNC	Atlit-Yam	2
RT-2475	7465 ± 50	waterlogged branch	PPNC	Atlit-Yam	2
PITT-0622	7550 ± 80	charred seed	PPNC	Atlit-Yam	2, 10
RT-2477, 2478	7605 ± 55	waterlogged branch	PPNC	Atlit-Yam	2
RT-944C	7610 ± 90	charred branches	PPNC	Atlit-Yam	2, 10
RT-944A	7670 ± 85	Hordeum	PPNC	Atlit-Yam	2, 10
RT-2493, 2495	7755 ± 55	waterlogged branch	PPNC	Atlit-Yam	2
RT-2489, 2492	7880 ± 55	waterlogged branch	PPNC	Atlit-Yam	2
RT-3038	8000 ± 45	human burial	PPNC	Atlit-Yam	2
Pta-3950	8000 ± 90	charred branch	PPNC	Atlit-Yam	2, 10
RT-2496, 2497	8170 ± 55	waterlogged plants	PPNC	Atlit-Yam	2
?	7562 ± 85	n.s.	PPNC	Hagoshrim	3
?	7735 ± 55	n.s.	PPNC	Hagoshrim	3
OxA-7921	7940 ± 50	charcoal	PPNC	Tel Ali	1, 10
OxA-7886	7975 ± 70	charcoal	PPNC	Tel Ali	1, 10
GrN-4823	7880 ± 55	charcoal	PPNC	Tel Ramad II	10
GrN-4822	7900 ± 50	charcoal	PPNC	Tel Ramad II	10
GrN-4427	7920 ± 50	charcoal	PPNC	Tel Ramad II	10
GrN-14539	7480 ± 90	charcoal	PN	‘Ain Rahub	1, 5, 10
GrN-1544	7360 ± 80	charcoal	PN	Byblos	5, 10
Ly-4927	7330 ± 70	charcoal	PN	Munhata	1, 5, 10
RT-1395	7400 ± 60	charcoal	PN	Nahal Betzet	1
RT-1544	7054 ± 78	charcoal	PN	Nahal Qanah Cave	1, 5, 10
OxA-13414	7135 ± 65	n.s.	PN	Sha‘ar Hagolan	4
OxA-7920	7245 ± 50	charcoal	PN	Sha‘ar Hagolan	1, 5, 10
OxA-7885	7270 ± 80	charcoal	PN	Sha‘ar Hagolan	1, 5, 10
OxA-9417	7285 ± 45	emmer seed	PN	Sha‘ar Hagolan	5
OxA-13275	7361 ± 35	n.s.	PN	Sha‘ar Hagolan	4
OxA-7917	7410 ± 50	charcoal	PN	Sha‘ar Hagolan	1, 5, 10
OxA-13293	7423 ± 38	n.s. (fill from well shaft)	PN	Sha‘ar Hagolan	4
OxA-7918	7465 ± 50	charcoal	PN	Sha‘ar Hagolan	1, 5, 10
OxA-13295	7479 ± 36	n.s.	PN	Sha‘ar Hagolan	4
OxA-13292	7488 ± 36	n.s. (well foundation pit)	PN	Sha‘ar Hagolan	4
OxA-7919	7495 ± 50	charcoal	PN	Sha‘ar Hagolan	1, 5, 10
OxA-13415	7510 ± 80	n.s.	PN	Sha‘ar Hagolan	4
OxA-13294	7726 ± 37	n.s.	PN	Sha‘ar Hagolan	4
OxA-13276	7815 ± 40	n.s.	PN	Sha‘ar Hagolan	4
OxA-13296	7896 ± 38	n.s.	PN	Sha‘ar Hagolan	4
OxA-13274	7900 ± 40	n.s.	PN	Sha‘ar Hagolan	4
HV-8509	6740 ± 90	bone	PN	Nizzanim	1

Table 2 List of ^{14}C ages from southern Levantine PPNC and PN sites (cf. fig. 2 and text). Sources: see Table 1.

Lab-Nr.	14C-Age (BP)	Material	Culture	Site	Source
RT-1853	5200 ± 70	n.s	Chalcolithic	Abu Halil	7
Ly-6258	5205 ± 95	n.s	Chalcolithic	Abu Hamid	7
GrN-17496	5651 ± 40	charcoal	Late Chalcolithic	Abu Hamid	7, 10
GrN-14623	5670 ± 40	charcoal	Late Chalcolithic	Abu Hamid	7, 10
GrN-16358	5745 ± 35	charcoal	Late Chalcolithic	Abu Hamid	7, 10
GrN-16357	6030 ± 60	charcoal	Late Chalcolithic	Abu Hamid	7, 10
Ly-6259	6135 ± 80	n.s	Chalcolithic	Abu Hamid	7, 8
Ly-6255	6160 ± 70	n.s	Chalcolithic	Abu Hamid	7, 8
Ly-6254	6190 ± 55	n.s	Chalcolithic	Abu Hamid	7, 8
Ly-6174	6200 ± 80	n.s	Chalcolithic	Abu Hamid	7, 8
RT-1610	5250 ± 55	charcoal	Late Chalcolithic	Abu Matar	7, 10
RT-1613	5275 ± 55	charcoal	Late Chalcolithic	Abu Matar	7, 10
AA-29771	6170 ± 55	n.s	LN / ECh	‚Ain Waida'	8
RT-2178	5125 ± 60	n.s	Chalcolithic	Cave of the Sandal	7
RT-1943	4700 ± 75	n.s	Chalcolithic	Cave of the Warrior	7
RT-1945	4910 ± 65	n.s	Chalcolithic	Cave of the Warrior	7
RT-1946	4925 ± 50	n.s	Chalcolithic	Cave of the Warrior	7
AA-13442	4995 ± 45	n.s	Chalcolithic	Cave of the Warrior	7
AA-22234	5120 ± 55	n.s	Chalcolithic	Cave of the Warrior	7
AA-22235	5140 ± 50	n.s	Chalcolithic	Cave of the Warrior	7
AA-22237	5420 ± 50	n.s	Chalcolithic	Cave of the Warrior	7
AA-22236	5600 ± 65	n.s	Chalcolithic	Cave of the Warrior	7
RT-1942	5640 ± 60	n.s	Chalcolithic	Cave of the Warrior	7
RT-2513	5660 ± 40	n.s	Chalcolithic	Cave of the Warrior	7
RT-1556	4658 ± 55	n.s	Chalcolithic	Dimona	7
RT-1210	5710 ± 75	n.s	Chalcolithic	Eilat IV	7
RT-926A	6340 ± 60	n.s	Chalcolithic	Eilat IV	7
RT-989	6470 ± 60	n.s	Chalcolithic	Eilat IV	7
RT-1213	5490 ± 60	n.s	Chalcolithic	Eilat V	7
RT-1211	5640 ± 60	n.s	Chalcolithic	Eilat V	7
RT-1212	5930 ± 80	n.s	Chalcolithic	Eilat V	7
RT-1216	6060 ± 65	n.s	Chalcolithic	Eilat V	7
RT-1851	5130 ± 55	n.s	Chalcolithic	Ein Um Ahmad	7
RT-1858	5190 ± 50	n.s	Chalcolithic	Ein Um Ahmad	7
RT-1852	5400 ± 70	n.s	Chalcolithic	Ein Um Ahmad	7
RT-1857	5575 ± 50	n.s	Chalcolithic	Ein Um Ahmad	7
RT-1859	5715 ± 70	n.s	Chalcolithic	Ein Um Ahmad	7
RT-1856	5815 ± 50	n.s	Chalcolithic	Ein Um Ahmad	7
RT-2058	4530 ± 85	n.s	Chalcolithic	Gilat	7
OxA-4011	5540 ± 70	n.s	Chalcolithic	Gilat	7
Beta-131729	5560 ± 50	n.s	Chalcolithic	Gilat	7
Beta-131730	5730 ± 40	n.s	Chalcolithic	Gilat	7
RT-1866	4810 ± 90	charcoal	Late Chalcolithic	Golan Site 12 (Rasm Harbush)	7, 10
RT-1862	4945 ± 65	charcoal	Late Chalcolithic	Golan Site 12 (Rasm Harbush)	7, 10
RT-1863	5130 ± 70	charcoal	Late Chalcolithic	Golan Site 12 (Rasm Harbush)	7, 10
RT-1864	5565 ± 60	charcoal	Late Chalcolithic	Golan Site 21	7, 10
Pta-4212a	5180 ± 70	charcoal	Late Chalcolithic	Horvat Beter	7, 10
RT-1750	6890 ± 50	wood	Chalcolithic	Kefar Galim	10
RT-1929A	5630 ± 55	olive stones	LN / ECh	Kfar Samir	6, 10
RT-1929	5630 ± 55	olive stones	LN / ECh	Kfar Samir	6
RT-1752	5750 ± 60	wood	LN / ECh	Kfar Samir	10
RT-1898	5790 ± 55	olive stones	LN / ECh	Kfar Samir	6, 10
RT-1930	5870 ± 70	olive stones	LN / ECh	Kfar Samir	6, 10
RT-1747	5890 ± 70	wood	LN / ECh	Kfar Samir	10
BETA-82845	6080 ± 70	olive stones	LN / ECh	Kfar Samir	6
BETA-82843	6100 ± 60	olive stones	LN / ECh	Kfar Samir	6

Lab-Nr.	14C-Age (BP)	Material	Culture	Site	Source
BETA-82847	6210 ± 80	olive stones	LN / ECh	Kfar Samir	6
BETA-82848	6230 ± 80	tree branch	LN / ECh	Kfar Samir	6
BETA-82844	6290 ± 60	olive stones	LN / ECh	Kfar Samir	6
BETA-82849	6350 ± 90	tree branch	LN / ECh	Kfar Samir	6
RT-1751	6495 ± 55	wood	LN / ECh	Kfar Samir	10
BETA-82715	6500 ± 70	olive pulp	LN / ECh	Kfar Samir	6
Pta-3820	6830 ± 80	tree branch	LN / ECh	Kfar Samir	6
Pta-3821	6830 ± 60	wood	LN / ECh	Kfar Samir	10
BETA-82850	6940 ± 60	tree branch	LN / ECh	Kfar Samir	6
RT-1360	7260 ± 80	wooden bowl	LN / ECh	Kfar Samir	6
RT-1947	6580 ± 70	n.s	Chalcolithic	Khashim et-Tarif	7
Pta-3374	5269 ± 60	n.s	Chalcolithic	Kvish Harif	7
OxA-1928	5310 ± 80	n.s	Chalcolithic	Lower Wadi Makukh	7
Pta-4339	6270 ± 70	bone	Chalcolithic	Megadim	7
Pta-3648	6310 ± 70	bone	Chalcolithic	Megadim	7, 10
Pta-3652	7060 ± 70	clay below site	Chalcolithic	Megadim	7, 10
RT-1948	5470 ± 70	n.s	Chalcolithic	Moon Valley	7
RT-1855	5355 ± 60	n.s	Chalcolithic	Moyat Dabaʿiya	7
RT-1965	5350 ± 60	n.s	Chalcolithic	N. Sinai Site A-173	7
RT-1962	5010 ± 55	n.s	Chalcolithic	N. Sinai Sites B50/51	7
RT-2129	5045 ± 55	n.s	Chalcolithic	N. Sinai Sites B50/51	7
RT-2132	4980 ± 45	n.s	Chalcolithic	N. Sinai Sites R45	7
RT-1518	4990 ± 50	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
RT-1513	5170 ± 55	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
RT-1630	5625 ± 70	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
RT-1506	5635 ± 70	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
RT-1608	5690 ± 55	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
RT-1663	5755 ± 85	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
Pta-3486	6130 ± 70	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
RT-1692	6350 ± 90	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
Pta-2999	6460 ± 80	n.s	Chalcolithic	Nahal Issaron (Uvda 14)	7
RT-1407	4990 ± 70	n.s	Chalcolithic	Nahal Mishmar Cave 1	7
RT-1409	5355 ± 55	n.s	Chalcolithic	Nahal Mishmar Cave 1	7
RT-1408	5575 ± 90	mat	Late Chalcolithic	Nahal Mishmar Cave 1	10
RT-1645	5535 ± 75	mat	Late Chalcolithic	Nahal Mishmar Cave 3	7, 10
RT-1543	5090 ± 75	charcoal	Late Chalcolithic	Nahal Qanah Cave	7, 10
RT-1545	5340 ± 57	charcoal	Late Chalcolithic	Nahal Qanah Cave	7, 10
RT-1723	6390 ± 70	seeds	Chalcolithic	Newe-Yam	7, 10
RT-1724	6565 ± 70	seeds	Chalcolithic	Newe-Yam	7, 10
RT-2387	5410 ± 50	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2377	5490 ± 55	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2376	5510 ± 45	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2378	5615 ± 45	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2374	5645 ± 60	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2388	5675 ± 60	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2386	5685 ± 80	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2379	5710 ± 45	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2383	5725 ± 50	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2373	5790 ± 45	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2391	5815 ± 90	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2382	5825 ± 50	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2381	5840 ± 50	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2394	5930 ± 50	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2384	5960 ± 85	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2396	6055 ± 85	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2395	6085 ± 50	n.s	Chalcolithic	Peqiʿin Cave	7
RT-2397	6100 ± 55	n.s	Chalcolithic	Peqiʿin Cave	7

Lab-Nr.	14C-Age (BP)	Material	Culture	Site	Source
RT-2385	6120 ± 55	n.s	Chalcolithic	Peqi'in Cave	7
RT-2392	6120 ± 55	n.s	Chalcolithic	Peqi'in Cave	7
RT-2380	6245 ± 55	n.s	Chalcolithic	Peqi'in Cave	7
RT-2393	6545 ± 50	n.s	Chalcolithic	Peqi'in Cave	7
OxA-3435	5270 ± 75	n.s	Chalcolithic	Sataf	7
RT-1809	5230 ± 55	n.s	Chalcolithic	Serabit el-Khadim	7
RT-1807	5250 ± 55	n.s	Chalcolithic	Serabit el-Khadim	7
RT-1811	5350 ± 55	n.s	Chalcolithic	Serabit el-Khadim	7
Hv-5296	4710 ± 50	n.s	Chalcolithic	Sheikh Muhsen	7
RT-1329	4260 ± 80	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1332	4700 ± 80	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1339	4940 ± 70	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1322	5190 ± 75	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1318	5240 ± 65	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1330	5300 ± 60	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1317	5330 ± 50	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1335	5370 ± 65	charcoal	Late C halcolithic	Shiqmim	7, 10
RT-1341	5370 ± 40	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1326	5420 ± 50	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1319	5450 ± 60	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1328	5520 ± 60	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1321	5570 ± 65	charcoal	Late Chalcolithic	Shiqmim	7, 10
RT-1334	5590 ± 60	charcoal	Late Chalcolithic	Shiqmim	7, 10
SMU-790	5523 ± 69	n.s	Chalcolithic	Site 332	7
SMU-809	5708 ± 81	n.s	Chalcolithic	Site 332	7
SMU-675	5789 ± 70	n.s	Chalcolithic	Site 332	7
SMU-649	5210 ± 51	n.s	Chalcolithic	Site 649 EX	7
SMU-743	4427 ± 68	n.s	Chalcolithic	Site 650	7
SMU-788	5523 ± 73	n.s	Chalcolithic	Site 713	7
SMU-742	5654 ± 57	n.s	Chalcolithic	Site 713	7
SMU-641	6403 ± 76	n.s	Chalcolithic	Site 713	7
TO-3408	6190 ± 70	wood	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
TO-3410	6350 ± 70	wood	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
TO-3412	6380 ± 70	charcoal	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
TO-4277	6490 ± 70	charcoal	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
TO-2114	6590 ± 70	wood	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
TO-2115	6630 ± 80	wood	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
TO-3411	6670 ± 60	wood	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
TO-3409	6900 ± 70	charcoal	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
TO-1407	7800 ± 70	bone	Middle Chalcolithic	Tabaqat al-Buma (WZ200)	1, 10
OxA-7805	5680 ± 45	charcoal	Late Chalcolithic	Tel Ali Ia	10
OxA-7802	5770 ± 45	charcoal	Middle Chalcolithic	Tel Ali Ib	10
OxA-7801	5815 ± 45	charcoal	Middle Chalcolithic	Tel Ali Ib	10
OxA-7804	5930 ± 45	charcoal	Middle Chalcolithic	Tel Ali Ib	10
OxA-7800	5950 ± 45	charcoal	Middle Chalcolithic	Tel Ali Ib	10
GrN-15196	5110 ± 90	dung	Late Chalcolithic	Teileilat Ghassul	7
GrN-15194	5330 ± 25	wood	Late Chalcolithic	Teileilat Ghassul	7
OZD034	5342 ± 71	n.s	Late Chalcolithic	Teileilat Ghassul	9
OZD033	5454 ± 58	n.s	Late Chalcolithic	Teileilat Ghassul	9
OZD029	5524 ± 88	n.s	Late Chalcolithic	Teileilat Ghassul	9
OZD032	5577 ± 71	n.s	Early Chalcolithic	Teileilat Ghassul	9
OZD028	5581 ± 67	n.s	Early Chalcolithic	Teileilat Ghassul	9
OZD031	5605 ± 80	n.s	Early Chalcolithic	Teileilat Ghassul	9
OZD024	5791 ± 86	n.s	Early Chalcolithic	Teileilat Ghassul	9
OZD025	5902 ± 71	n.s	Early Chalcolithic	Teileilat Ghassul	9
Pta-3460	6310 ± 70	charcoal	Chalcolithic	Tel Hreiz	7
RT-1749	5985 ± 55	wood	Chalcolithic	Tel Kones	10

Lab-Nr.	14C-Age (BP)	Material	Culture	Site	Source
RT-1748	5985 ± 70	wood	Chalcolithic	Tel Kones	10
Pta-2968	6040 ± 80	burnt bone	Middle Chalcolithic	Tel Qatif Y-3	1, 8, 10
HD-12336	5375 ± 30	charcoal	Late Chalcolithic	Tel Wadi Fidan 2	7, 10
HD-12337	5740 ± 35	charcoal	Late Chalcolithic	Tel Wadi Fidan 2	7, 10
HD-12338	6110 ± 75	charcoal	Middle Chalcolithic	Tel Wadi Fidan 3	8, 10
HD-12335	6360 ± 45	charcoal	Middle Chalcolithic	Tel Wadi Fidan 3	8, 10
RT-648B	5670 ± 85	n.s	Chalcolithic	Uvda 151	7
RT-640A	4800 ± 70	n.s	Chalcolithic	Uvda 16	7
RT-1739	6390 ± 70	n.s	Chalcolithic	Uvda 6	7
Pta-3621	6400 ± 60	n.s	Chalcolithic	Uvda 6	7
RT-628A	6560 ± 90	n.s	Chalcolithic	Uvda 6	7
Pta-3646	6969 ± 70	n.s	Chalcolithic	Uvda 9	7
RT-2186	6045 ± 65	n.s	Chalcolithic	Wadi Daba'iya	7
Beta-118580	6260 ± 40	n.s	Chalcolithic	Wadi Fidan 51	7
RT-1845	5240 ± 55	n.s	Chalcolithic	Wadi Watir VIII	7
RT-648A	5440 ± 80	n.s	Chalcolithic	Wadi Zalaka	7
Pta-3633	5590 ± 70	n.s	Chalcolithic	Wadi Zalaka	7
Pta-3655	5690 ± 50	n.s	Chalcolithic	Wadi Zalaka	7
RT-1546	4650 ± 75	n.s	Chalcolithic	Yotvata Hill	7
RT-1548	5465 ± 55	n.s	Chalcolithic	Yotvata Hill	7
RT-1547	5800 ± 45	n.s	Chalcolithic	Yotvata Hill	7

Table 3 List of ^{14}C ages from southern Levantine Late Neolithic and Chalcolithic sites (cf. fig. 2 and text). Sources: see Table 1.

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Conflict and Conflict Mitigation in Early Near Eastern Sedentism. Reflections

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In his keynote contribution Ofer Bar-Yosef makes a general statement relating to the combined demographic / environmental reasons for the occurrence of warfare in the Early Neolithic of the Levant. Although we must be grateful to Ofer Bar-Yosef for re-addressing this central issue of Levantine Neolithisation, and the new causes and roles of coalitional aggression under the conditions of settled life (and its environmental background) along with most of the author's ideas should be supported, there are still some important points that appear to be missing in his keynote which must be addressed. These points relate to the innovative social and economic mitigation mechanisms and structures that regulate conflict in sedentary environments, including the conflict to arise through the amalgamation sedentary land use and nature. In my view, there exists a special primacy of environmental factors influencing human conflict behaviour (and *vice versa*) under sedentary conditions, and these are embedded in the general ethos of human aggression under such conditions. Thus, I see it as imperative to discuss Neolithic warfare always in conjunction with early Neolithic conflict management and related social and commodification systems. Indeed, it is only through consideration of these factors, combined with insights from the spheres of human ethology and related fields, that we might better understand how and why aggression, violence and warfare emerged in the early Neolithic. Accordingly, the early Neolithic sedentary ethos – or the somehow provocative *Homo neolithicus* var. *orientalis* - perception in Gebel n.d. a, b – would be a substratum from which our topic could be approached in a more scientific way. Be this as it may, the hitherto essayistic nature by which the subject of warfare is treated is characteristic for our discipline, and the following reflections and comments are certainly no exception.

Neolithic Ethos and Warfare. On Understandings and Terminology

Aside from the general problem already addressed in the introduction to this Neo-Lithics issue (the limited consultation or non-involvement of disciplines specialized in human conflict in the archaeological conflict discussion) our discourse of the subject suffers from a misrepresentative terminology and implied personal perceptions of the scholars, including modern moral attitudes. Especially the latter require some degree of illumination if an author's particular and personal approach is to be understood successfully by his/her readers.

I fully share the understanding of Joachim Bauer (2008) that human aggression is rather a reactive programme than a human drive or need: Biologically anchored like fear, aggression developed during human evolution to help in situations of danger. Group-minded social behaviour and empathy dominate over aggressive behaviour; aggression, violence, warfare and the like represent rather the *ultima ratio* in the range of choices of human reactions. The complex relationships between kinds of conflict and kinds of violence, including their ritualised features, are determined by the life mode, and certainly sedentary life provided different frameworks than foraging ones. Aggression was certainly set free at different locations and situations in confined territories than was the case in open territories. Furthermore, aggression was related to community organisation, and must have been influenced by a complex system of risk weighting. Warfare, understood here as a coalitional and non-spontaneous (prepared and organized by a strategy) aggression of groups / communities against each other, aiming to reach a balance over a conflict/ subjectively disadvantageous matter, is just one form of violence and stress release. Environmental stress may have been countered by other sorts of violence, too, ranging from intra-community measures to spontaneous massacres against human and faunal competitors in the landscape.

There appears to exist a neurobiologically verifiable (J. Bauer, pers. comm.) need to punish unfair behaviour by others, aside from the general causes of human aggression: fear of physical and psychological pain, death; deprivation from / unbalanced distribution of resources or wealth; unbalanced social relations, social marginalisation, physical and cognitive confinement *etc*. While I see a basically shifted human ethos by Neolithisation (general territoriality becomes a confined territoriality; aggregation in social, economic and cognitive territories supported by a productive commodification, including ritual regimes/religions; general reciprocity becomes confined reciprocity; *cf.* Gebel n.d. a,b) which became the basis of our modern ethos, Joachim Bauer claims (pers. comm.) that the Neolithic ethos is neurobiologically rooted and has not shifted to any significant degree in the last 20.000 to 30.000 years. However, I wonder if the cultural manipulation and control of the human ethos has not reached a new dimension through the sedentary life modes which established in the course of five to six millennia during the Near Eastern Neolithic Evolution.

In this contribution I use the neutral term *conflict* in order to force definition for each concrete piece of evidence for Neolithic strife. The overall use of the

terms aggression, violence, warfare, raids, and the like is at least meaningless if not evaluated and described for the subsystems in which they occur and are relevant, *i.e.* local environment (biotic and abiotic resources); regional and long-distance biotic and abiotic resources; technological and innovation frameworks; social structure; economic system; as well as ideological and cognitive regimes. While the task of identifying and describing the nature of conflict should be subject of an interdisciplinary approach, a tool to provide an initial characterisation of the type of conflict could be simple if three different levels are involved: the ethological, the societal, and the political level. Accordingly, *aggression* remains a matter of ethos, *conflict* is firmly situated in societal contexts, and *warfare* receives its political dimension.

Significantly, most conflicts relate to disturbed and shifting integrities of tangible and intangible territories. Thus the territoriality approach (see below) is essential if we are to work on Neolithic conflict and conflict mitigation; at the same time, this is also an integrative tool for the various disciplines to be involved in research, *e.g*. behavioural ecology; psychologies of the environment, evolution and religion; cognitive neuroscience; neurobiology and social biology *etc.*).

A thorough analysis of disturbed territories is essential, as are studies which might inform us as to how imbalances in one territory might affect related territories (for an example, see below). Normally, a territory is considered optimal and flourishing if it provides stability through its size and balanced advantages to all, and if the costs of defending the territory are low in relation to the efforts involved in exploitation, acquisition, production, integration *etc*. Furnished with these tools, we might not only be able to identify Neolithic conflict levels and cases, but also reconstruct the organisational nature of an aggressive act. In this case, questions as to whether Neolithic warfare involved either fighting in organized formations or in the form of raids as known from modern primitive societies might even become obsolete.

Space Commodification and Properties. On Early Neolithic Territoriality

Territoriality in physical environments and intangible spheres develops when social units settle down in an area by claiming resources and establishing regimes through use, including the overworldly territories of belief systems, using ingredients of nature *etc.* The growth of groups and the availability of the resources in a region render territories subject to conflict when neighbouring claims start to overlap. At that moment territoriality becomes a matter of the exclusion of competitive beings and elements, and the formation of a stronger group identity among the beneficiaries (cohesive groups with coordinated activities). The main criteria of collective territorial behaviour are certainly the existence of stable social frameworks that enable claims and allow defence and territorial concessions. What differentiates the forager territoriality from sedentary territoriality lies in its productive milieu through which it operates and exists. "Political" territoriality however only develops when physical territories become important for the organisation of groups.

Three sorts of Neolithic territories might have existed (modified after Altman 1975 for the Near Eastern Early Neolithic):

1. Primary Physical Territories (intra-site and external): permanently, or nearly permanently, occupied; recognised by neighbours as a relatively permanent ownership; closely identified with the group through use of space; occupants in full control of use; intrusions by others understood as encroachments.

2. Corporate Physical Territories (intra-site and external): occupation repeated but not continuous; not subject to individual but to corporate ownership; use bound by certain conditions and functions; surveillance of use by representatives of social units.

3. Obtainable Physical Territories (intra-site and external): large number of individuals and groups interested in the use of the territory; rights to it disputed among these individual and groups, with a high potential for conflict; control of territory is subject to mutual agreement and corporate defence; uses of territory restricted or limited; its transfer into permanent ownership requires mutual acceptance or forced acquiescence.

A major cause of Neolithic territorial aggression was probably territorial crowding. Indeed, since the early Neolithic this factor must have been a major agent influencing all socio-economic and cognitive developments, including our post-Neolithic history: Increasing sedentism produced more confined territories in which aggregation, commodification, and innovation processes were the only factors capable of regulating pressures. When these processes failed to provide the necessary balance within the increasing number of confined territories, systems began to collapse. Such collapses could have been peaceful implosions (the vanishing of cultures, the adaptation of new life modes), but must have been – depending on the pressure system involved– also induced by accompanying aggressive acts. On the local and regional scale, raids and even organised warfare might have become one option of regulation. As mentioned previously, such options occurred only if mitigation initiatives through aggregation, commodification, and innovation measures became exhausted; this notion has to include the understanding that aggregation, commodification and innovation would have ultimately brought about growth themselves and thus triggered the very conditions which they primarily set out to avoid. Territorial aggression may have disappeared temporarily from larger regions, *e.g.* when the vast

alluvial lands and steppes of Mesopotamia or the semi-arid fringes of the Levant became subject to new subsistence modes (early hydraulic and pastoral socio-economies) in the later Neolithic. Unlike local territorial infringements, territorial crowding has the tendency for supra-communal, supra-local, and supra-regional overthrows. Territorial crowding includes such phenomena as over-populated villages, insufficient pasturelands for the increase of flocks, the disruption of social hierarchies through the inflation of prestige commodities, competition in social management solutions, and the like, and results in environmental, social, economic, and ideological stress and conflicts which increase with densities. Density in one sphere can easily provoke a hyperthrophic milieu. A good example of such a stress system is the recently-debated Mega-Site Phenomenon in the Jordanian mountain ranges (Gebel 2004). Here, the duration and intensity of combined aggregation, commodification and innovation seems to have damaged the social and economic behaviour and values of individuals and groups; it imploded most likely because social answers were not found rapidly enough ahead of prospering socio-economic developments. Consequently, levels of intra- and inter-group aggression must have increased.

The confined reciprocities in Neolithic times implied existential strategies for the joint survival of a sedentary community supported by concession orders and regulated by conflict regimes and – where we might agree with Ofer Bar-Yosef – warfare upon resident occupations. Neolithic human aggression was prompted by additional and different types of motivation (as compared with foraging structures), and conflicts must have reached much larger scales both in terms of quantity, *i.e.* the number of involved belligerents, and quality, *i.e.* weapons technology as well as offensive and defensive strategies. But the human ethos of aggression must not have increased *per se* through sedentarism: Sedentism developed a number of hitherto unknown or unneeded pacifying devices meant to cope with the enhanced conflict potentials created by the new aggregated tangible and intangible territorial densities.

Segregation Regimes and Aggregated Life Modes. On Mitigative Commodification

Our excavations do provide material evidence that reflects conflict mitigation aimed to support solidarity, integrative processes, interest balance *etc*. Conflict mitigation appears to be an ingredient of early Near Eastern Neolithic cultures: It is expressed by the new productive commodification regimes which supported newly emerging corporate structures via all sorts of segregation processes, such as labour division, site specialization, ancestral locations, possibly genderfication, supra-group feasts(?), new social hierarchies, boosting personal "prestige" good sectors, defensive structures(?), possibly even "commodity coupons" (Gebel n.d. b) *etc*. All this was supported by the establishment of sedentary moral and belief systems, now serving also as the cognitive agents of mitigation and survival of group integrity. In the economic sector, surplus production and storage appear to be the major agents of mitigation. Probably "markets" and "wealth" in the modern sense became regional elements of temporal mitigation and security before their tendency to become elements of conflict emerged.

In spite of the general problem of identifying aggression, conflict or warfare in the archaeological records, I would dare to state that we are generally able to identify more features of mitigative than aggressive behaviour. This of course has much to do with the lenses through which we behold our evidence, and the nature of such evidence. Mitigative behaviour is expressed rather in processes and by repetitious features inside settlements and cultures, whilst warfare is a restricted event that does not necessarily take place within settlements. I am however still far from the somewhat odd conception of a *peaceful* Neolithic society – homicide, skull traumata, sling balls, projectile points *etc*. do exist –, but it is (more) striking to see what has been subject to mitigative commodification in Levantine Early Neolithic societies in order to avoid conflicts. This ranges from the "dead in storage" under house floors to the creation of flexible groundplans (shifting floor levels and wall openings allowing new room associations) adapting to micro-changes in social relations; the diversification of goods and services or crafts; hierarchies in social and production spheres; and most likely also to ritual and symbolic regimes which connected communities beyond the regional level.

Initially, most productive commodification appears to have mitigative and regulating purposes, even if characterised by a segregative function. Conflicts appear when the (re)sources of commodification (*i.e*. productive value systems) become depleted and lose their basis or if competing commodification regimes become established. (Neolithic commodification is understood as the prolific milieus in which commodities – new technologies, objects, product standards and innovative substrata, services, exchange standards, ideas, belief systems *etc*. – were constantly created, altered and ex-commodified; commodities are more than goods, they are the social milieus of tangible and intangible things, *cf.* Gebel n.d. b.

Since mitigative conflict behaviour is reflected by commodification acts and processes, the study of commodification is an essential element of conflict study.

Large and Small Habitats. On Early Neolithic Levantine Warfare and Environments

Resident territoriality created philopatrial competition and mentalities that caused groups and group members not only to define and personalize territorial property but also to defend and control it. As already implied,

such territories are not necessarily physical, they can just as easily be ideological; in most cases conflicts over territories are concerned with physical and ideological territories, where one is used to support the (initial) claim of the other. Conflict potentials were likely multiplied by permanent residency, and principles of resident territoriality must have dominated all spheres of Neolithic life. Apart from the physical spaces (including natural resources such as springs, lakes, pathways, arable land, water/soil dams, minerals, hunting grounds *etc.*, as well as built spaces such as settlements, houses, rooms, graves, wells *etc*.) intangible territorities were domesticated (commodified), mostly to support the structures of physical territories. Indeed, it is highly likely that Neolithic populations distinguished physical and metaphysical space in quite different ways to how we moderns do.

The conflict/warfare discussion hardly distinguishes between conflict conditions in extensive and more restricted spheres or spaces. Translating this to environmental space and the Levant, one may say that our discussion should distinguish between the different conditions for territorial conflict in the more vast north and central Levantine habitats and the more sensitive and confined ones in the southern Levant. Even in the southern Levant and on a supra-regional level, one can distinguish between environmental conflict potentials within the Mediterranean zones and regions with access to the vast steppes with their migrating unglates in the semi-arid east.

It is one of my basic theses that the Mega-Site Phenomenon of the LPPNB Jordanian Highlands is a non-violent transgression of a socio-economic paradigm becoming successful while migrating from north to south and exploiting the rich animal protein resources and pastures to the east (Gebel 2004). The rapid establishment and decline of the mega-site culture appears accompanied rather by the emergence and implosion of commodification systems than by violence. But what about the situation prior to the LPPNB mega-sites in the more confined Mediterranean environments west of the Rift Valley? Here, we can expect territorial conflicts over habitats which reached the dimensions of organized warfare between neighbouring communities, and initiated what became later the mega-site socio-economy. I am not sure how "peaceful" the mega-site socio-economy was received by the MPPNB communities in the niches of the Jordanian Highlands; as of yet, it looks like an absorption of the indigeneous MPPNB by the more prolific LPPNB. Concerning the end of the LPNNB mega-site socio-economy we may assume restricted local conflicts over resources, but most likely these were minor through the rapid adaptation of a new life mode and its economy, the pastoralism which already developed during the mega-site times.

If we consider all of the Levant, I would agree with Ofer Bar-Yosef that areas with limited habitats are potential areas of territorial clashes and warfare originating in environmental causes. Such restricted habitats develop either by overexploitation as a consequence of demographic stress and/or cataclysmic land use, or even by minor climatic and other impacts (flash floods, droughts, earthquakes *etc*.) or a combination of the two. The southern Levant has many such regions in which territiorial clashes and warfare could emerge from such a background. When going further north, the Levantine habitats become larger and the network of geographical corridors is more extensive and complex. Here, for example in the alluvial plains and the steppes, territorial infringement and warfare as a consequence of limited habitats may not have played a major role, especially not in times of unfavourable climatic oscillations, and only the general sorts of territorial violation may have existed (vandalism, thefts, contamination *etc*.). Especially the vast grassland habitats of the northern Levant may not have witnessed warfare for environmental reasons until the emergence of the early city states.

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Prehistoric Warfare – Cause and Visibility

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The keynote by Bar-Yosef presents a hypothesis regarding "Warfare in Levantine Early Neolithic". The author is touching on one of the interesting issues of human behavior, a significant area of current study in archaeology (Guilaine and Zammit 2005, Maccall 2009). His goal was to consider whether Levantine Early Neolithic contexts present indications for acts of warfare emanating from increasing population densities and severe inter-group competition. As such, the keynote triggered off several thoughts on the causes of warfare and the archaeological visibility:

Causes for Warfare

The Neolithic time frame correlates to one of the important periods of change in human history: the turnover in subsistence strategy from hunting-gathering to food production. It was accompanied by major changes in human existence, probably having major effects on the relationship between communities. I agree that the concept of "war" precedes the appearance of "civilizations". Indeed, the change in human worldviews during the Neolithic and maybe even earlier, could have brought about a change in group and individual behavior patterns towards neighboring communities, and the implications of the terms 'us' vs. 'them' had to change. Thorpe (2003) loosely defined warfare as organized aggression between autonomous political units. We can suggest that already during the Natufian (which precedes the Neolithic cultures) the Levantine landscape was divided into autonomous units. Communities had unique markers to distinguish them from their neighbors (Goring-Morris and Belfer-Cohen 2010). Yet can we detect organized aggression or the underlying cause/s for it? Throughout the course of history until the present day, we can find variation in warfare dynamics within the context of the ideological, economic, environmental and demographic relationships of modern societies. Bar-Yosef highlights environmental causes, namely climatic dynamics as the main cause for the Neolithic aggression. Mostly, climatic conditions enabled Neolithic high population densities which were the immediate triggers of warfare. It seems that he adopts a materialistic perspective suggesting that warfare is apparent when there is food shortage (resulting from a climatic crisis) or no territorial living room (overpopulation in a given area). This is in accordance to the Malthusian approach assuming that war is one of the common consequences of overpopulation, along with disease and famine. This line of reasoning is also adopted in the "warfare theory" for state formation later on in human history (Carneiro 1986, Carneiro 19970, Johnson and Earle 2000). But can we explain warfare stemming from another, different mechanism acting at this time frame?

Indeed climatic events during this time frame had an effect on the density and crowding of populations, but to a limited extent. The climatic trend shows gradual shifts in climatic regimes. The regional climatic trend observed in the Central Southern Levant, preceding and during the Neolithic period, is different from the global climatic trend as recorded in the ice-cores, with regards the pace and volume of the changes. In addition, it seems that the influence of the "Younger Dryas" was less extreme in this particular region, especially in comparison with the effects of the "Old Dryas" on the local climatic trend in the Geometric Kebaran times (Grosman 2005).

The early Neolithic was the continuation of major social changes which started off in the Natufian, and had a pronounced effect on population dynamics. Previous studies (*e.g.* Rosenberg 1998, Gat 2000) demonstrated that warfare among hunters-and-gatherers correlates strongly with sedentism, suggesting that warfare could have been initiated before the Neolithic, during the Early Natufian where there is clear evidence for incipient sedentary lifestyle. Larger groups were settling in one place and individuals were communicating with non-kin members. The increasing social complexity (Kosse 1994) during the Natufian suggests the formulation of complex means to deal with growing group-sizes and scalar stress besides actual warfare, introducing an intermediate community larger that the simple hunter-gatherer group but smaller than those of agricultural societies. One can speculate that rather than being the cause of warfare, population density can be rather the result of warfare activities (Harrison 1993). Indeed, one of the possible cases of site abandonment presented by Bar-Yosef pre-dates the Neolithic, as he discusses the Late Natufian levels at Mureybet and Abu Hurreyra. Thus, theoretically there are grounds to search for warfare activities during the Natufian, the first sedentary societies living in small hamlets at the time of the "onset of 'history'" (Bar-Yosef).

The Archaeological Record

Whatever the cause for warfare maybe, it is very difficult to demonstrate through archaeological evidence its existence or absence. Primarily there are two direct types of archaeological evidence:

- skeletal pathologies resulting from violence;
- fortifications;

On the other hand, identifying an absence of warfare is

even more difficult, as every skeletal assemblage may produce signs of skeletal trauma derived from everyday activities. Fortifications can be interpreted also as portraying spiritual beliefs or are means of protection from the vagaries of natural elements.

Bar-Yosef points out various indications for warfare during the Early Neolithic, which lasts *ca.* 3,000 years: abandonment of villages; secluded and naturally well protected site locations; intensive building in confined spaces, fortifications, and relative high frequencies of arrow heads. All of these are rather indirect indications and as such are of a speculative nature. To date, the early Neolithic archaeological data provide rather poor or low visibility of warfare activities on skeletal remains, though one of the new and unique characteristics of the Natufian culture is the appearance of defined cemeteries. The sample sizes are even larger in the Neolithic. Indeed, with a population of *ca.* 450 individuals there are reports of violence in the Natufian (Bocquentin 2003, Bocquentin and Bar-Yosef 2004, Eshed *et al.* in press).

Final Point

We should bear in mind that whether or not population densities were the cause of warfare - warfare has a negative effect on population dynamics. Population decreases as a result of warfare activities, as these act to lower population sizes. This dynamic suggests that during the Natufian/early Neolithic the evidence of warfare will have 'silent' phases. We can assume that there were fluctuations between situations of warfare and its absence. Checking archaeological evidence is like thumbing through a photo album, observing finite differences between the pictures, without being able to observe the flow of changes that had occurred in the intervening time-span. So perhaps the abandonment of sites may suggest evidence of warfare yet the material accumulation on site averages both times of war and those of peace during the settlement existence. Although we need to apply new methods and "find evidence of victims of violence, burned houses …" (Bar-Yosef) it does not seem that violence in its most obvious form was part of the Natufian or even early Neolithic everyday life.

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Neolithic Warfare: Comments

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Warfare has been absent for too long from the theories on the « Neolithic Revolution ». It should be assumed, however, that the latter, though emerging during a long duration process, is thought to have generated envy of territories rich in water, arable land or pastures favourable for the herds. Forced displacement of populations, frictions between communities, thirst for individual or collective power, are some *casus belli*. The reincorporation of warfare into social relationships thus compensates for a scientific blindness maintained for a long time and based on the idea that this step towards agriculture occurred in a kind of general consensus within a pacified social context. The question was raised in Europe before the Near East, observing that the last hunter-gatherers of this continent may have known violent confrontations already, as it is attested by the skeletal remains of several Epipalaeolithic/ Mesolithic cemeteries (Voloshkoe, Vasilyevka I and III, Scheila Cladovei, etc. Vencl 1991; most recently Roksandic ed. 2004). Certain individuals seem to have been killed by blows to the neck as it is shown by the trophy skulls at Ofnet in Bavaria (Orschiedt 2005). More recently, the analysis of one of the most famous burials of the Téviec cemetery in Brittany, with above-ground structures made from antlers, has revealed that two individuals had been killed by blows to the head (unpublished, observations made by J. Braga).

These tensions were not fewer during the Early European Neolithic as it has been demonstrated by the « massacres » within the Linear Pottery Culture at Talheim (Germany) (likely involving the kidnapping of women, Bentley 2007) and of Asparn-Schletz (Austria) (Wahl and Koenig 1987). Hunter-gatherers and subsequent farmers thus were able to kill each other and various reasons can be evoked to explain these confrontations: "territoralization" following sedentarization, frontier conflicts, raids, without considering more "psychological" motifs such as breaking-off of alliances, insults, *etc*. O. Bar Yosef has the merit to raise the question of warfare during the incipient Neolithic in the Near East. While L. Keeley considered the Natufians to be a pacifistic population (Keeley 1996: 120), recent revision of the anthropological record of this culture stresses that traces of violence are not to be excluded (Bocquentin 2003). Nearly at the same time period, site 117 (Jebel Sahaba) in Sudan shows the existence of a population that was eliminated in the course of one (or two?) war(s) (Wendorf 1968).

Can we argue that an increase in the number of arrowheads implies insecurity or even warfare? It is indeed difficult to distinguish hunting, warfare or social parade exhibiting weapons. In Europe, the number of arrowheads considerably increases during the final Neolithic/Chalcolithic while archaeozoological determinations indicate a meat diet essentially based on husbandry (Guilaine and Zammit, 2001). The projectile points are likely to have been used in the social "sphere" rather than in the economic field. Is it like that everywhere? The case of Cyprus is interesting. Here, recent evidence from hunter-gatherer sites dated to about – 9000 BC (contemporary of the final stages of the PPNA) – Agia Varvara-Asprokremmos, Agios Tychonas-Klimonas – shows the abundance of projectile points in these contexts (McCartney *et al.* in press). At this time, neither agriculture is attested (it appears later, from 8500 BC on at Mylouthkia, well 116, and Shillourokambos (Willcox 2003) and neither is herding. Hunting of pigs then plays an important role and the arrowheads can be associated with this activity. But this does not mean that other motifs should be excluded. This period, between 9500 and 8500 BC represents the main stage of settlement on the island by mobile groups though living without doubt already exclusively on Cyprus (and not as temporary mainland « visitors »). This colonization may have been combined with a type of competition for the foundation of territories, various groups opposing each other in concurrence for the appropriation of certain space. Strangely, at sites of the following stage (early phase A of Shillourokambos: - 8500/- 8000), contemporary with the Early PPNB of the Levant, projectile points are less numerous (territories globally stabilized from now on?). Later, during the 8th and 7th millennium, however, the larger settlements are protected by walls: Tenta (Todd 1987), Khirokitia with two successive enclosures (Le Brun 1994).

In a larger sense, the territorial fixation of the first sedentary communities in the Near East could have exacerbated the veneration of « ancestors », considered to be the founders of each settlement, to be those who first appropriated a space, thus providing legitimacy to the subsequent generations as to their presence in these places. This approach which considers the ancestors as « beneficially » deceased (of which the memory is kept alive by rites and iconographic representations) seems more likely to me than the one calling for the notion of « divinity », a concept I believe to emerge later. Recently, A. Testart has proposed interpreting some of the removed and plastered skulls stemming from the PPNB or the Ceramic Neolithic in the Near East to be warfare trophies rather than representatives of an « ancestor cult » (Testart, in press).

Obviously, the reasons which caused the abandonment of certain sites cannot invariably be assigned to confrontations. During the 8200 BP event,

aridification processes drying up the water tables may have been responsible (Berger and Guilaine 2009). On Cyprus, the decline or even the disappearance of Khirokitian sites is perhaps related to this phenomenon, as destruction of certain settlements caused by warfare would in return have resulted in the foundation of new settlements. As a matter of fact, during the 6th millennium, there are hardly any sites known on the island: no substitution settlements but rather a kind of strong depopulation.

The last point is more hypothetical. If conflicts between the first farming communities have been frequent, what happened to the defeated? Were they killed? Were they integrated? Were they kept but with an inferior status? Concerning this last hypothesis, may we suppose a Neolithic origin for slavery?

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Early Neolithic Warfare in the Near East and its Broader Implications

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Evidence for the existence of, and the nature of, warfare in the prehistoric past has importance for our understanding of nature and reasons for warfare in general. Our best chance of eliminating warfare is to understand why it takes place. To limit our study to only a few recent centuries and a relatively small number of societies reduces the chances of finding deep understanding of the reasons. Thus, archaeology has something to contribute to furthering such an understanding. Highly relevant topics that archaeology can provide information on are the changes in warfare between types of social organization; and, changes in warfare as they relate to changes in carrying capacity. Such information is central to the questions of whether people fight over scarce resources and whether the nature of the social system is linked to why people fight, rather than just how they fight.

The Relevance of the Early Neolithic for Understanding Warfare

In the context of understanding warfare, we can usefully think of the Neolithic in terms of core areas where domestication originated and those areas that it spread into. Whether or not one agrees with what can be termed the Bellwood-Renfrew model of farmer spreads (Bellwood 2005), it is clear that domestication originated in only a few localities regardless of how it eventually spread. Whether or not the spread of agriculture was accompanied by warfare is one question, one which I will return to later. Another question, that of whether the adoption of agriculture in the core areas resulted in an increase or decrease in warfare is of theoretical interest, and there are only a handful of places to investigate this worldwide. For several core areas of domestication, either the data are so sparse, or the actual areas where domestication took place is so poorly worked out at this point, that little useful can be said. In fact, it is really only the Near East where there is enough information to even attempt at addressing the question of the intensity of warfare when domestication took place.

One can build two scenarios for core area warfare that are rather different from each other. One scenario is that warfare among non-stratified societies is very sensitive to carrying capacity stress (see LeBlanc 2003), and domestication increased the carrying capacity and we should expect warfare to decline. Conversely, it can be argued that while it is true that domestication would increase the carrying capacity, the process of domestication was so slow compared with potential population growth that, any gains in increased carrying capacity would have been quickly used up by population growth. Instead, competition for the best farm land would have increased the potential for warfare. This competition could have been between farming communities, and/or between farmers and foragers who would have existed in the general area of the farmers in territory that was not optimally suited to farming. This land would have been desirable once the farmer populations began to grow leading to conflict. Thus we might expect both farmer-farmer and farmer-forager conflict, even in those areas where the initial shift to farming took place. These two scenarios are quite different, and it would be good to know which is right. However, it is not clear there is presently enough information to determine this. Some of these issues are touched upon by Rosenberg (1998). He also nicely clarifies the relationship between population growth and population pressure, and notes that "population pressure is simply the persistent latent tendency toward recurring imbalance in population-resource ratios…." This idea is implicit in the following discussion.

One problem we face in the study of ancient warfare has been the pacification of the past. There has been a tendency to ignore or minimize the evidence for warfare in the prehistoric record. This is a worldwide problem, and in fact researchers in the Near East have been a bit more willing to acknowledge evidence for warfare than in other parts of the world. Marilyn Keyes Roper, who provided the first attempt at synthesizing early Near East warfare, and who said that most societies most of the time had warfare and who correctly understood the relevance of warfare in the past to understanding present warfare, nevertheless began her classic paper with

Recent reading of Jane van Lawick-Goodall's In the Shadow of Man and articles in the newly discovered peaceful Tasadays in the Philippines…. (Roper 1975)

Of course, we now know that Chimpanzees are very violent and have the equivalent of warfare, and the "peaceful" Tasadays were a fraud In fact, there are no known examples of foragers in a land of foragers living in peace. Roper was not alone in minimizing the basal level of warfare we should expect to find in the deep past, For example, Bar-Yosef (1986) suggested there was no meaningful social aggression in the Levant before the end of the 6th millennium, which is clearly wrong. That is, even those who were open to the existence of warfare in the past have had an unrealistic idea of just how much and for how long warfare has existed. It was not until the seminal work of Lawrence Keeley's War Before Civilization (1996) that archaeologists have

been more willing to see evidence for warfare for what it is and to begin to go beyond just noting the existence of warfare evidence. We see this both in books such as the Archaeology of Warfare (Arkush and Allen 2006), and non-archaeologists competently using archaeological data in their broader syntheses (Gat 2006). Of particular relevance is that people believed, and many still do, that foragers were peaceful. Thus, the presumption, although quite incorrect (Keeley 1996, LeBlanc 2003, Gat 2006), was that the deep human past was peaceful and at some point warfare started up. With that paradigm it is easy to see major fortifications and massive destruction levels as the initial evidence of a shift from peace to war. Yet, if foragers had as much warfare as anyone else, then we should expect evidence for warfare to be present but subtle long before large towns and great walls.

Turning to the Natufian and PPNA time periods, unfortunately, it is hard to predict what the signature of early sedentary population conflict would have looked like. We know from ethnographic evidence that foragers did not build fortifications, but did use the natural landscape for community defense. They would locate camps to be protected by vegetation, and would even plant vegetation to provide defenses. They would also locate settlements on landforms such as hilltops, or elevated landforms that can only be accessed over narrow constrictions, etc. Some, but not all, of these locations can be discerned today as defensive. Most foragers did not use cemeteries, unlike sedentary societies, so the skeletal record can be very sparse and skewed. Foragers did regularly use body armor and specialized weapons, but many of these were made from wood or other perishable materials, such as shields, clubs, and bark or fiber armor. And, as often noted, many other weapons could be dual purpose for either hunting or fighting, such as bows and arrows. It should be possible to determine whether such dual purpose weapons were used for warfare, in that arrow points were probably designed differently for hunting or fighting. For example, we would predict much larger quantities of these tools for warfare (stockpiling), etc. However, almost no research has been done on identifying these differences, so this is not possible at the present time. Thus, the sites of particular interest, those that are transitional between foragers and fully sedentary farmers are predictably some of the hardest to interpret in terms of warfare, and they are particularly hard to compare with later, larger farming villages.

Looking for Warfare in Prehistory

One of the problems we face in trying to study ancient warfare is simply being able to recognize it. We can produce lists of types of evidence, but we must realize that we would never expect to find them all in one site or even one region. Some lines of evidence require very good survey data. Some require being able to tell which sites are actually contemporaneous. Some requires large, well preserved skeletal series, which need to be studied by people who are trained to recognize evidence of warfare, and so on. As these conditions are not often met, the absence of evidence does not mean the absence of warfare. This can be seen in the recent evaluation of evidence for warfare in the Greek Neolithic (Runnels *et al.* 2009). Some lines of evidence had been considered before, but others had not. Some potential lines of evidence were almost non-existent, yet by looking at all the lines of evidence at once, a much clearer picture emerged.

A rather interesting example, not physically close to the Near East, but similar in other ways, is found in the American Southwest. Here, between AD1200-1400, the evidence for intense warfare is overwhelming, and almost all scholars accept its existence, although there is not consensus as to its causes (LeBlanc 1999). Yet, there is surprising little skeletal evidence for trauma, and very little evidence for specialized weapons. There is, conversely, ample evidence in the form of settlement patters, settlement layouts, and site burning. On the other hand, in nearby prehistoric California there is incredible skeletal evidence for traumatic deaths, but almost no settlement related evidence for warfare. I consider the lessons one can learn from the Southwest below.

In spite of this warning, one can try list the types of evidence that can be found. Settlement based data is particularly useful. Defensive walls and houses that form defensive barriers are good evidence of warfare or its threat. Gates designed to be defensive and bastions are particularly good evidence, but we know from ethnography that most egalitarian farming communities would not have had bastions nor gates that were built to be defended. (We would expect many gateways would have been barricaded by perishable materials when conflict threatened). Moreover, locating villages or towns on high ground may have sufficed for defense. In the American Southwest, some sites that are clearly defensive were on high ground but had no other defensive features. Moreover, we find sites that were on slightly high ground were occupied for only a generation and then were replaced with sites that had defensible walls. If one had found only the high ground sites one might offer other explanations for their locations, but once one sees the temporal sequence, it is clear that an initial attempt at having a defensive posture was quickly realized to be inadequate and a better solution was adopted. There are two lessons from this. First, people do not always get it right. Some defenses don't work and very poor defenses do not mean there was no warfare, but may simply mean defensive technology was in flux. Secondly, trends are important. Seeing a sequence of ever more defensive structures is significant, even if some of them do not appear very defensive. One problem in the Near East is the practice of building on the same location over time. This makes it hard to excavate large areas of the earliest occupations and so walls, gates and other such defensive features can be missed. Sometimes one must

rely on indirect evidence. When houses that appear to be contemporary are packed tightly together, this may signal the presence of a defensive wall that forces people to pack tightly inside it. I believe the tight packing of houses at Sitagroi (a Neolithic site in the Balkans) is an example of this, and the excavations simply did not extend far enough out to locate any potential walls (Renfrew *et al.* 1986). A similar argument can be made for Polyanista, in Bulgaria (Parkinson and Duffy 2007).

A second aspect to site settings is their distribution. We know that people often form defensive alliances and leave buffer zones between competing polities (LeBlanc 1999, 2006). They also locate villages or towns so that they can visually communicate with each other to obtain assistance from alliance members (*e.g.* Haas and Creamer 1993). Finding these patterns requires very good survey data. When some sites are destroyed or covered by later occupations, it can be hard to discern such patterns. However, even with more limited data, we should sometimes be able to spot evidence for alliances. There is great benefit to villages being spaced far enough apart so that they have a catchment that gives efficient access to farm land, firewood, wild plants, etc. The closer communities get to each other, the more restricted such access, the less efficiency, and the greater chance of competing claims to resources. In the American Southwest, for example, villages, even when they belonged to alliances, were apparently located at least 5 km apart whenever possible, and usually much further. We find site very close together only in times of maximal warfare, so such tight packing almost surely indicates evidence of propinquity for defense.

Interpreting skeletal evidence also presents some difficulties. One can find direct evidence of trauma, from blunt force trauma to the skull, to parry factures to the ulna, or projectile points imbedded in bone. However, skulls in poor condition might not preserve blunt force trauma. A projectile that was buried in the body cavity but did not get imbedded in bone might be perceived as a grave offering. A point that was removed before the individual was buried will be missed altogether. George Milner (2005) has shown that about 2/3s of all individuals with arrow wounds would have these wounds go undetected in the archaeological record.

More indirect evidence of warfare can be found in skewed sex differences in burial populations. The idea being that men may die in battle and be buried away from the settlement reducing the proportion of warrior aged men. Conversely, successful raiding may result in female capture, reducing the number of women in the burial population. Thus, although a useful idea, interpreting such data is difficult. More easily considered are mass burials. In low density societies like those we might expect to find in the early Neolithic, there would have been few, if any, epidemics that would have resulted in deaths so close in time that the individuals would have been buried together. Of course, accidents that killed multiple people could have occurred, but they would be expected to be rare, as would have some other social reasons for multiple burials. Individuals killed in a raid, etc. would then seem to be likely candidates for most of the multiple burials we find. Such burials are quite common in the archaeological record, but they are rarely considered or recognized as likely evidence for warfare. It is quite difficult to even find tabulations of their presence.

Prehistoric weapons present a different set of interpretive problems. A serious limitation is the perishable nature of many of these types of items. Shields, prior to metallurgy, would be very unlikely to ever preserve. Much ethnographically known body armor is made from fiber, bark or other perishable materials; and that made from bone (such as for Eskimos) may not be recognized as armor if found in fragmentary condition. What hinders some of these discussions is a bit of interpretive confusion. The unwillingness to accept sling missiles as weapons of war is such an example. In the Near East they have been found as stockpiles, and even stockpiled on the inside of defensive walls (Akkermans 1993), and in all known ethnographic examples where they are stored in quantity and/or carefully produced, they are for warfare. The idea that any but a small fraction of them would have been used for herding sheep or hunting is just silly. Also, sling missiles can be natural stones selected for shape and weight as Ghezzi (2006), has demonstrated for a defensive site in Peru. Similarly, mace heads or daggers that are considered ceremonial and not evidence for warfare is another misconception. Assuming they were just for ceremonial or display purposes, because utilitarian versions were not found, is like arguing that trophies with metal footballs were produced in areas where football was unknown. Daggers are weapons of war, not of the hunt, as are maces, and except in the rarest of circumstances so are clubs. It does not matter whether we find only ceremonial versions of these weapons or not, the existence of the form demonstrates that warfare was either present at the time of their production or not to distant in the past. (The English monarch still carries a ceremonial mace). Similarly, but slightly more equivocal are stockpiles of arrows and heavy spears. While both can be used for hunting, hunters would not have stockpiled arrows. Spears used for hunting and never used for warfare are ethnographically rare (harpoons are an exception, but they are distinctive). I have made no effort to exhaustive list the lines of evidence of warfare here, but have just tried to show how many lines of evidence there are and how many are often poorly understood.

Some Relevant Comparisons with the American Southwest

As has been touched upon above, the American Southwest has a wealth of relevant information that can help interpret and frame the information from the Near East. I briefly review some of this information to help provide a framework for interpreting evidence

for early farmer warfare. Besides being one of the better understood areas of the world, the Southwestern environment is similar enough to the Near East in that the same types of construction and evidence appears in both areas. Also, for the most part the social organization of the Southwest is similar to what we would expect to find during the early Neolithic.

Some of the earliest farming communities in the Southwest that were a result of farmer spreads have significant, but at the same time spotty, evidence for warfare. In the more southerly areas, defensive sites are about the only good evidence we have, but these are massively fortified hilltop sites. These fortifications sometimes took the form of building terraces while not having high defensive walls. Interestingly, over time, hilltops continue to be used, but they become less fortified, not more fortified (Hard and Roney 2004, Diehl and LeBlanc 2001). In the northern reaches, a few sites are located on defensive land forms, while many are cave sites. Cave sites are more defensible than many recognize (Haas and Creamer 1993). Also, the taking and curating of human trophy parts and even rock art provide levels of evidence for warfare not usually found or expected (Howard and Janetski 1992, Cole 1984). In addition, at the same time and the same general areas that we find defensive sites, there are also undefended sites. These sites may not be fully contemporary, or the undefended sites may have been located near refuge sites that were defensive. The lesson is, within the same overall area, rather different lines of evidence are found in different places and there was a mix of defensive and non-defensive sites. And, the defensive nature of sites was rather minor. Apparently, even when the threat of warfare was quite real, the effort put into defenses was rather minimal, and could vary considerably. We should think of early sedentary people's defenses in comparison to that of foragers, not in contrast to Bronze Age cities. Another interesting observation, as mentioned, is the paucity of skeletal evidence and specialized weapons that demark warfare in the Southwest. Even when warfare intensifies, such evidence is hard to find. That is, settlement layouts, and locations dramatically shift when warfare intensifies, but other lines of evidence show only slight observable changes. Again, the lesson is: One cannot rely on one line of evidence alone and comparisons between periods can be difficult if the types of evidence available also changes between periods.

A relevant story comes from a Hopi legend (Lomatuway'ma *et al.* 1993). A community, built much like Çatal Hüyük and some other Near East sites, was successfully attacked. The attackers brought flammables to burn the village, but once inside the town they began to loot and lost their cohesion. The defenders rallied and drove the attackers from the town and put out the fires. The result would have been only spotty evidence of fire and damage. Not all battles end in complete destruction.

Of particular interest is a sequence of events that took place over a century or so over much of the Southwest (LeBlanc 1999). There appears to be an interval during the Medieval Warm period where there was little warfare, probably due to the good climatic conditions. However, when the climate changed warfare began to increase. At first, people just lived a bit more nucleated. Houses were located to provide some barriers, but there were no gates or continuous circuit walls. Some communities did locate to high landforms, but often only a part of the community was in these spots. As warfare continued, sites were made more and more defensive, eventually resulting in sites with high, multistoried exterior walls formed from contiguous rooms, while others had free standing walls and towers, etc. Sites were no longer on high ground, but were near secure water supplies. It was then that the famous cliff dwellings were built, a still different form of defense. The lesson is that the response to warfare was gradual and hesitant. It appears that people did not want to expend the energy needed, or did not really believe the threat was real. It is quite likely that the earliest farmers in the Near East would have responded the same way. The first evidence for defensive features should be limited, inadequate, or poorly conceived, etc. However, even minor efforts at defense can be useful; for some enemies, even slight defensive features may be quite useful, and we should see such minor efforts for what they were – evidence for the threat of warfare.

Warfare Evidence in the Neolithic

My purpose here is to review some of the more commonly accepted types of evidence for warfare as well as some of the less often considered in the Near Eastern Neolithic. I stay within the Neolithic but do not care much about where in that interval. This is not to argue for warfare in any particular region or time interval, but instead to show how common such evidence is. Evidence of burning can be seen in such places as Level I at Mureybet which was destroyed by fire and Levels XVI and XVII had burned houses. Large sections of Çatal Hüyük were destroyed by fire (Mellaart 1967). The Hacilar IIa settlement was partially burnt, and was only partially rebuilt. Beidha had massive burning then a change in architectural form and layout, which is of special interest as Bar-Yosef (1986) argues, probably correctly, that the walls at Beidha were not defensive, but this ignores the other evidence for warfare at the site. What is of interest is that it is quite hard to burn stone or mud walled buildings even if they have wood and other flammables in their roofs. Accidental fires that spread beyond a single room are extremely unlikely (Icove *et al.* 2006). As noted above, such structures are so hard to burn that in the Southwest attackers would bring flammables to help get fires started, if they planned to burn towns they attacked. Trying to explain widespread burning as accidental is simply continuing to pacify the past.

Architectural evidence for defense includes Çatal Hüyük with the rooms walls forming a defensive barrier

combined with roof top entry. Roof entered rooms in massive room blocks are also known for Can Hasan III (French 1998) and other sites such as Umm Dabaghiyah Level II and III which had a defensive configuration (Kirkbride 1982), as well as rooms entered from roof, but it may be too late to be relevant. Similar, but with free standing walls is Hacilar IIa (Mellaart 1975) with a massive defensive wall with difficult of access entrance ways. Defensive walls at earlier stages at Hacilar are more equivocal. Maghzaliyah in Period 2 had massive defensive walls, and there was a new wall in Period 3 (Yoffee and Clark 1993). Possible defensive walls were found at Ras Shamra. Other sites with well constructed defensive walls include Aşikli (Esin and Harmankaya 1999), Musular (Ozbaşaran 1999), Kurucay, which also had bastions, but may be too late to be relevant (Duru 1999) to name some of the more obvious examples. Even low walls or terraces, and roof top entries can be useful defenses. There are a significant number of sites that have some evidence of walls that are not so obviously defensive. While a few walls may have other functions, in all likelihood, most were for defense.

In southwestern Anatolia, Lee Clare and colleagues undertook one of the few studies to look at settlement patterns for this time period. They find good evidence for site clusters and empty zones, which independently confirms other evidence for warfare for this region (Clare *et al.* 2008). Similarly, the site distribution map of so called mega-sites presented by Simmons (2007), suggests both site spacing and clustering that might represent buffer zones between these large sites and some close clustering of others for mutual defense. The size of these sites is also suspicious. Again based on analogy, community size is a good form of defense. One reason why sites would grow so large at this time could be for the defense they provided that would offset the high costs of living in them. Overall, it would appear that settlement pattern data has barely been searched for evidence of conflict.

At an extreme level, the evidence of cultural divisions within the fertile crescent and their development and persistence over time so painstakingly derived by Kozlowski and Aurenche 2005) provides a testable proposition. While warfare can exist within cultural regions, we would expect it to be more intense between such zones. So, one would expect there to be more evidence of warfare, especially site defenses, among sites on each side of such boundaries compared with those in the centers of each of these cultural zones. Again, the zones are most well delineated for later periods, but discovery of such a pattern would still be of interest.

Weapons of war are probably more common than recognized. James Mellaart (1975) noted that Mureybet had an obsidian dagger and Mace heads were found at Jarmo. They were also found at Hallan Cemi in a very early context (Rosenberg 1999) and Zawi Chemi Shanidar (Solecki 1981). At Çatal Hüyük, there was a "ceremonial" flint dagger and a cache of obsidian spear heads which are more likely un-hafted daggers and possible mace heads were also recovered. Tell Sotto had a possible mace head. Finds that may be too late in time to be relevant include mace heads (Yoffee and Clark 1993), and at least 1000 baked clay sling missiles in one room at Umm Dabaghiyah Level II and III. Yarim Tepe I had sling missiles. Even where we know warfare was intense, clubs, maces and daggers tend to be rare. The numbers of arrow points found at some of these sites are very large when compared with other parts of the world, and of special interest are large caches such as the 100+ arrow points at Beidha. The presence of significant numbers of arrow points has a good chance of being evidence for warfare. No known ethnographic societies had bows for hunting and not also for warfare, and many had bows only for warfare, but that is a rather weak line of reasoning. Instead, one can suggest that hunters do not need many arrows at one time, while warriors do. Moreover, the shift from micro blade points in the Natufian to larger points that required more effort and the changes in size (first getting larger, then smaller) over time, would seem to be responses to changes in the function of arrowheads or how arrows or bows were made and used. Yet there does not seem to be much change in the species hunted and there is a general decline in the amount of hunting, none of which would seem to invoke such changes. Conversely, changes in armor or shields, changes in bow design, or arrows (such as the use of foreshafts) would be likely reasons for the observed changes in the arrowheads. It appears that arrow points are common enough and change enough such that their role in warfare should be more seriously considered.

Skeletal remains are more problematic evidence, I believe in part due to the lack of focus on them. The Natufian skeletons are relevant. Fanny Bocquentin and Ofer Bar-Yosef (2004) found an embedded arrowhead in a skeleton that had been excavated many years ago and the point had never been noticed before. Similarly, Vered Eshed and colleagues (in press) found quite a high incidence of violent skull trauma in a large Natufian sample (although the study was not focused on warfare evidence and a full assessment of the potential evidence was not made, so a more meaningful interpretation cannot be derived from the study). And to the east the broadly contemporary Shanidar Cave skeletons also show significant evidence for violence (Agelarakis 1993). These findings do not fit with the earlier opinion of Belfer-Cohen (1995) that there is a paucity of such evidence from the Natufian. There seems to have been even less systematic search for evidence for warfare among Neolithic skeletal remains than among the earlier Natufian sample. Here again a comparison is useful. There was little discussion of evidence for warfare in the prehistoric skeletal remains from the California cultural area until Philip Walker and Patricia Lambert began a systematic search for evidence in Southern California (Walker 1989, Lambert 1997). Several additional studies from other parts of the state followed and it is now recognized that prehistoric California had one of the highest incidences of warfare

trauma known. One suspects this is more a consequent of careful study than cultural reality, and one suspects the low incidences in the Natufian and Neolithic of the Near East may also be a result of the lack of systematic research rather than cultural reality.

This does not imply there is no such evidence. From various sites throughout the relevant time period we find Tell Abu Hureyra had pits with bones and skulls, one with 16 bodies and only three skulls (Moore *et al.* 2000). Tell Sotto had a dismembered body and another one stuffed in a hole instead of a proper burial. Roper noted suspicious finds at Nahel Oren, Chukba, El-Wad, Erq el amar, and Eynan. Yarim Tepe I had at least two dismembered bodies (Yoffee and Clark 1993), and Hajji Firuz Tepe had an extremely high incidence of violent deaths (Voight 1983). There were a few traumatic injuries at Jericho and Basta had healed skull fractures, also a violent death at ‘Ain Ghazal and Ghwair I, but these examples tend to be evidence noted by the excavators, not the results of systematic study. Similarly, I can find no good studies of multiple burials. As noted above, multiple bodies buried at the same time should be extremely rare except as a result of warfare deaths. While other explanations are possible, warfare is a likely cause for at least some, if not most, of these finds, and they suggest that systematic work with Neolithic skeletons would find more examples and enable meaningful statistics to be generated that could be compared with other places and times.

Thus, virtually all types of evidence for which data has been collected show evidence of warfare. There is a considerable range of types of evidence in the above brief tabulation, none may be convincing alone, but in sum the evidence is substantial. Given the nature of the remains and the social structures involved, there is quite a bit of warfare evidence if one is willing to see it for what it is, and more focused studies such as that by Clare and colleagues are very likely to generate much more evidence.

Interpreting Evidence for Early Neolithic Warfare in the Core Area of Domestication

My purpose here is to consider how to use evidence of warfare to try and characterize what was taking place. We would expect the earliest villages that were presumably occupied all year by at least a meaningful part of their population to have little in the way of fortifications. Methods for constructing fortifications would have been in their infancy. Enemies would have existed in small groups. They would have used dawn raids as a preferred tactic, and large scale attacks would have been unlikely. We might expect warfare to be more like that we find for foragers than for long-settled farmers. Ethnographically, such foragers defend themselves by having watch dogs, being prepared to fight on a moment’s notice, and placing their houses so it was hard to sneak into the village and be able to get away safely once the alarm has been raised. Most warfare deaths would have happened away from communities. As time passed and settlement sizes and therefore the size of attack forces grew we should expect fortifications if warfare or its threat was common. However, we might expect such fortifications to be poorly designed and frequently modified until these techniques were invented and refined.

This is my problem with interpreting possible defensive features at Jericho (Bar-Yosef 1986). While not intending to argue for or against warfare at the site, we might usefully consider a framework for interpreting the features in question. Towers are rarely designed as fighting platforms unless they are actually bastions. An example from the American Southwest is illustrative. We know that warfare intensified during the AD1200s. We know that stone towers began to be constructed in large numbers at this time in the northern part of the area. Careful study shows that many were defensively designed and located. They took considerable effort to construct and isolated towers were sometimes located on isolated land forms enhancing their defensive potential. Yet, at the site of Sand Canyon Pueblo, where there is clear evidence for warfare, there is a defensive wall, and there are a number of towers (Lipe 1992). None of the towers is articulated with the wall; they are all inside, they are not bastions. Some towers are located to protect areas were a good wall could not have been built because of topography, but others are simply interspersed among the habitation rooms well inside the outer wall. We know from a number of sites that towers served as refuges, like the keep of a medieval castle. There were even secret tunnels leading from nearby rooms that led into the towers. It is easy to see how in case of an attack one could crawl from rooms into the tower to safety as the height of the tower made them essentially impregnable. As societies of this type do not have the logistical ability to lay siege, the towers would have worked just fine for the needed protection. Some few towers may also have served as observation and/or signaling towers to spot attackers or signal allies for help. That is, we have at Sand Canyon Pueblo clear evidence that towers were part of a defensive posture, but were not part of the wall defenses. The famous tower at Jericho is also apparently not part of the defensive wall system, but that does not mean it did not serve the defensive functions I have just described. Similarly, the wall at Jericho is hard to interpret for several reasons. Walls are sometimes built for more than one reason. Deflecting on rushing water and defense may both have been reasons for constructing one. Even walls that do not fully surround communities maybe defensive (the Jericho wall is equivocal in this regard), as sometimes some sectors are defended by vegetation, swampy areas, or the lack of cover for attack, etc. As Roscoe as wonderfully documented (Roscoe 2008), walls can be very useful not as platforms to fight on top of or behind, but serve to slow attackers down or make escape if they get inside settlements far too risky. What we can say is that people at peace rarely have perimeter walls and towers in their communities at the same time, and there

is nothing about the construction or placement of these features at Jericho that preclude these features from being defensive.

In the case of Jericho alternative explanations also have interpretive problems. The earliest wall did not have a ditch, and the later ditch was not well designed for defense. See Keeley *et al.* 2007 for the nature of defensive ditches. Why dig a ditch into hard ground with the intent of keeping silt from accumulating as has been proposed, when it would have been much easier to simply clear the silt from time to time? Why would anyone locate a community where it was so subject to flooding that it would take a wall 1.8 meters wide and 3.6 meters high to protect? It is far easier to argue that the defenses were not particularly well designed than to argue that the ditch was a rational response to silting or the location of the community was extremely badly chosen. The real point of this discussion is that evidence for warfare is both contextual and cumulative; there will always be alternative explanations which require just as rigorous evaluation as evidence for warfare.

Another consideration is the often voiced argument against features being for defense that there was no need for defenses because there was no one to fight with. This is a poor argument. Everyone always had neighbors. Even low density foragers could combine and have surprisingly large attack forces, and smaller settlements could do the same against larger ones. Farmers went over 100 km to attack other farmers in the American Southwest. The Andaman Islanders, very un-complex foragers, were able to muster almost 200 men to attack the initial British settlement. Given such possibilities and distances, any of the PPNA communities under consideration would have been vulnerable to attack by foragers or farmers.

Returning to the general topic of early Neolithic warfare, the question is what would you expect to find? As soon as there are substantial communities, evidence for warfare seems to be about as common as it is in other parts of the world where warfare is accepted as being present. It is really only the Natufian where evidence is not so obvious. But the Natufian sites are much more like forager sites than they are the later villages. Forager warfare is extremely hard to find archaeologically. We would expect any Natufian warfare to be somewhat easier to find than for foragers, but not much easier. Only very careful investigation and interpretation will reveal warfare evidence if it was present at this critical moment in time.

The Spread of Neolithic Warfare from the Near East

Examining warfare in the deep past is a relative question. We will never be able to show that there was absolutely no warfare even if there was none. Conversely, we would not expect there to be constant warfare in any given area at all sites. Thus, in spite of how we might formulate various scenarios, we are really discussing the relative amount of warfare in a particular region and time interval. One important question, as mentioned above, is whether the spread of farming was accompanied by warfare. But what we really mean is: What was the relative intensity of warfare associated with the spread of warfare in comparison with the prior state of warfare in the areas being expanded into? And, a second question: What was the relative intensity of warfare in comparison with the intensity of warfare in the zone where farming began? Thus, an understanding of warfare in core areas is relevant to the nature of warfare in the expansion zones. Unfortunately, it would appear we have much better data in for what took place in the expansion zones that we do for the core areas. This is not surprising as these episodes took place more recently in time, with larger populations, over greater areas, all of which we would expect to lead to more information.

If we take at look at just one path of farmer spread, that from the core Levant area, into Anatolia and thence to Greece and the Balkans and finally into northern Europe, we can get a sense of this difference in information. (Of course, there are many other interesting paths; this is just a path that is perhaps better understood than any other). Even a most casual perusal of the literature finds ample evidence of warfare in the expansion zone. My purpose here is not to try and answer the above questions, but to simply show the type of evidence and how much there is that can be brought to bear on them.

The evidence for warfare in western Anatolia is quite strong, and much of it has been mentioned above. Once one enters Europe evidence is equally substantial. The evidence for early farmers in Greece has been recently summarized and need not be repeated (Runnels *et al.* 2009), and Hoca Çeşme just barely in Europe had a substantive enclosure wall (Ozdogan 1999). There is ample evidence for warfare further into Europe (*e.g.* Keeley, and Cahen 1989, Burnez and Louboutin 2002, Chapman 1999, Christensen 2004, Dixon 1988, Kokkinidou and Nikolaidou 1999, Saville 2002) in spite of the protestations of some (*e.g.* Coudart 1991).

The conclusion one can reach is that the spread of farming along this path was accompanied by considerable warfare. Given the size of the sites, it is hard to see how the threat could have only been from foragers that were being displaced. (This appears to be the case in some instance, just not all of them). This is especially true for places like Greece that seem to have had a very low Mesolithic population. Thus, we must conclude that the spread of farming was accompanied by conflict between farming communities. Such warfare is certainly far more visible than any possible warfare among the non-farmers in the area of early farmers. It also appears to be more visible than evidence for warfare at the dawn of agriculture.

Conclusion

It would appear that the evidence for warfare is not particularly strong at the time of the earliest sedentism and domestication in the Near East. Evidence does exist, it is just not particularly strong. However, the evidence is very strong that warfare accompanied the expansion of farmers into the former zone of foragers. The most likely explanation for these observations is that warfare in the earliest periods was typical for that found among foragers, and it increased in intensity later in the Neolithic and the Neolithic expansion. Alternatively, warfare may have declined during the times of the earliest farmers and the relative paucity of evidence for warfare at this time is real. Unfortunately, I see no way of resolving this without a much more focused study of the question. This is too bad because it is this one slice of time and place that is so unique and the theoretical interest so relevant. What we must do for sure is not simply casually assume that the relative scarcity of evidence definitely means that the transition to farming was quite peaceful. We must recognize and regularly reiterate that it is at present an open question. It is important to know we don't know, rather than assume we do. The study of warfare is too important a topic to not try and get it right.

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Comment to Ofer Bar Yosef's Keynote: "Warfare in Levantine Early Neolithic. A Hypothesis to be Considered"

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In acknowledging the existence of violent conflicts in the Near Eastern Neolithic in his keynote paper, Bar-Yosef also notes the common categories of potential evidence for violence and warfare in the archaeological record. He correctly calls for renewed excavation strategies to better identify such features as defence walls around settlements, human remains bearing traces of violence (*e.g.* embedded arrowheads, lesions), and destruction levels with burned houses etc. Additionally, he discusses possible causes of violence and warfare in the Near Eastern Neolithic. Here, emphasis is placed upon overexploitation and / or episodes of climate change. These were factors, it is suggested, which caused disruptions to subsistence and led to the consequent abandonment of villages and population movements, culminating in conflicts over land and resources.

First of all I would like to stress that I highly welcome the interest in issues of warfare and violence by specialists for the Near Eastern Neolithic, particularly as this period was probably the most crucial in the cultural evolution of the Old World. In light of the research already undertaken on warfare and violence in prehistoric Europe and the Americas, where such studies have been on the agenda of archaeological research for in excess of 20 years, it is certainly high time that the geographical focus should shift to the Near Eastern Neolithic. Indeed, warfare in the Neolithic of the Near East has hitherto been practically ignored and only addressed quite generally by a very small number of authors (see Childe 1941; Roper 1975; Müller-Neuhof 2005[1]). In the following I would like to focus my comments on some particular points made by Bar-Yosef in his keynote paper. As such, I will deal with a) possible reasons for conflicts; b) the role played by conflicts in village abandonment, as well as c) with some methodological issues.

Bar-Yosef hypothesises that the agglomeration of groups in settlements, starting in the Early Natufian, resulted from the decision to live together for reasons of security. This is partly not convincing, primarily due to the fact that mobile populations command over a much higher number of conflict prevention and coping strategies than do sedentary populations. These strategies range from retraction to the formation of temporal alliances with other groups for active defence measures (assaults). Instead, it would appear that the Natufian, and even PPNA, agglomeration of groups in settlements was mainly linked to ecological conditions, which made it possible to reside within a specific territory for longer periods during the year, or in the case of the PPNA all year round. In addition to specific plant resources, advantageous ecological conditions resulting in increased sedentary occupation especially constituted proximity to abundantly available game. The existence of large numbers of bones of migrating mammals (gazelles) in the archaeological record of Natufian and also PPN(A) sites (*e.g.* Cope 1991; Tchernov 1991: 330, 1993: 14) shows that hunting these animals was one of the important tasks of subsistence acquisition. As these animals were not available all year round, such a task could only have been undertaken by groups of people.

Therefore, it is suggested that the individual location of settlements was dictated by the proximity to annual migration routes of animals on the one hand, and on the other by the existence of topographical features such as cliffs, fords, canyons, etc., which were used as traps when hunting quarry in large numbers in the short time window when they passed by. Such hunts, which would have been characterised by chasing the herds in the direction of these topographical features and by killing trapped and injured game in large numbers[2], were only realisable if a high number of individuals participated not only in the hunt itself but also in the processing and storing of animal products. This in turn required a sedentary life style, forcing groups to live together, more or less permanently, in larger settlements.

Hence, it was primarily due to economic factors that large groups of people agglomerated in settlements, especially in the PPNA, though conflict related issues would certainly have been of concern, too. For example, the stores that would have accumulated in these settlements would have been targets of raids, and the territories with their plant resources, lying as they did along animal migration routes and with their important topographical features, would needed to have been defended from rival groups, too. Thus, it is evident that larger groups of people living in such settlements facilitated a successful defence of both villages and their associated territories.

Next I would like to refer to Bar-Yosef's hypothesis regarding the "breaking up" of village communities, which in his opinion was caused by scalar stress related to overpopulation. Certainly, the existence of "scalar stress" in these communities can be assumed, but a wide range of internal conflict prevention strategies would also have been employed by these societies. These strategies served to minimize scalar stress and to reinforce communal solidarity. Indeed, the latter (communal solidarity) was of utmost importance for the survival of the community with respect to both the economy as well as regarding matters of defence (see above). Such strategies are visible in the

archaeological record, for example, in special buildings and installations which reflect communal (religious) thinking and acting.

A community split and the foundation of an "offshoot" community could have occurred due to very different reasons. In the (unlikely) case of "scalar stress" (internal conflicts) it can be assumed that such a split would probably not have transpired peacefully. The foundation of such an offshoot community in the range of the territory of the old one is therefore not very likely, as is suggested by Bar-Yosef in the case of the two sites Gilgal and Netiv Hagdud, which are located at a distance of only 1.5 km from each other. More conceivable is that the establishment of offshoot-communities occurred in other regions, located further away from the original settlement. On the other hand, it is likely that the main reason for the foundation of an offshoot community was a strategy aimed at obtaining better control, and simultaneously better means of defence, over an existing territory. Further, it might even have served the enlargement of existing territories with biotic and abiotic resources.

Finally, Bar-Yosef rightly requests intensified fieldwork to focus on the identification of evidence for violence and warfare. This is absolutely correct but should be further differentiated. For instance, it is not sufficient just to extend the areas of settlement excavations beyond the fringes of the sites in order to detect possible perimeter walls, serving as defence features, or to discover more burials in order to have more skeletal evidence for violence. In nearly all fieldwork projects to have been conducted at Neolithic sites in the Near East, there is already plenty of evidence for violence and warfare in the archaeological record, albeit that the detection of this evidence was not on the agenda of research strategies. The problem is that this evidence has rarely been acknowledged even by the excavators themselves. Due to the missing general focus on conflict, the direct indications for violence and warfare are not acknowledged, and indirect evidence, labelled by Sl. Vencl as "the archaeology of things unfound" (Vencl 1983: 122), has not at all been considered. Therefore, when starting with archaeological conflict research it is first of all necessary to acknowledge the crucial issue of conflict potential. This also implies that many conflicts never developed into violent interactions.

Thus, prior to looking for signs of violence and warfare, we should first consider conflict potential. Bar-Yosef is right to identify the relatively rapid increase in population density and resulting competition over land and other resources. Indeed, these resources would have been shortened by overexploitation, environmental destruction and climate change, all of which represent possible causes for conflict. However, on the other hand, these factors do not always lead to conflict. As a matter of fact there are a number of other potential causes for conflict which should be considered. As such, it is essential that the potential for conflict is identified for each region and also for each archaeological site in which research is carried out. Such analysis should comprise, to name just a few points, information relating to the environment, subsistence strategies in conjunction with the environment and topography, and the participation of the respective society in regional and interregional networks.

Referring to the probable causes for conflicts and the manners in which they were carried out, ethnographic and ethno-historical data of more or less comparable societies and subsistence economies should be consulted too. In this way, prevailing manners of subsistence strategy and the assumptive character of social stratification can also be addressed. Naturally, these examples can never be understood as one to one analogies, as Bar Yosef rightly argues, but as explanatory models and methods to limit the number of possible causes and character of conflicts. With such an analysis it is possible to characterise conflict potential and conflict mitigation strategies. In some regions, and even at individual sites, these strategies are expressed for instance in defensive measures.

An identification of the potential for conflict makes the identification of evidence in the archaeological record easier, not least because we know what we are looking for. Nevertheless, we also have to take into account that some evidence may indicate a high potential for conflicts which never actually turned into violent acts but were just mirroring a defensive answer toward threat. On the other hand, other types of evidence, *e.g.* skeletal remains showing specific lesions caused by violent acts, can be interpreted as direct evidence for violence. The fact that lesions in skeletal material are very rare leads to an underestimation of the amount of violent acts visible in the burial context of archaeological sites. This is due to the fact that other types of evidence for violence in the burial context are mostly overlooked. I want to illustrate this problem with two examples from a long list of archaeological features we have to look for and to interpret correctly, but which do not directly document the existence of conflicts and violence in the archaeological record: 1. Evidence for soft tissue injuries especially in the abdominal region of a body caused by projectile points are frequently overlooked. Therefore it is important to know that ethnomedical observations in Papua New Guinea in the 1970s and 1980s have shown that just *c.* 10% of projectile point injuries are injuries causing bone lesions. Instead 90 % of projectile point injuries are soft tissue injuries, most of these in the abdominal region of the victim (Van Gurp; Hutchinson and Alto 1990) lacking any contact with bones. Such an observation is hardly acknowledged by archaeologists[3] who interpret the location of projectile points in the context of a burial. As long as the point is not embedded in a bone it is interpreted either as an offering or as intrusive. 2. Flakes or bladelets in burial contexts are hardly identified as possible projectile points. This is largely due to the fact that in the mind of many archaeologists they do not resemble morphologically "typical" projectile points. Although ethnographic

observations show that in combat bows and arrows and atlal spears were used, combatants were fighting at short to medial distances, mostly at no more than 50 metres apart (see for instance Gardener and Heider 1986: 140f; Meggitt 1977: 56). For such battles highly sophisticated aero dynamical arrowheads were not necessary, it was sufficient to use simple flakes and bladelets; items, having the advantage of being rapid and abundantly available. In addition these weapons, compared to an aero dynamical shaped point, cause more severe lesions[4] due to their large impact zone. It is highly questionable therefore to identify arrowheads in the archaeological record of Near Eastern Neolithic sites foremost as weapons of warfare[5]. A correct interpretation of archaeological features refers also to the interpretation of possible causes for conflicts and violence standing behind specific developments which can be observed in the archaeological record.

It is high time to discuss the conflict and warfare issue in the research of the Neolithic in the Near East. Hypotheses relating to the potential causes and consequences of conflict and warfare in these Neolithic societies are an important part of the discussion. But prior to commencing with further theories on the causes and consequences of violence and warfare, first it is more important to develop a mutual understanding of the lines of evidence which actually refer to their existence in the archaeological record of the Neolithic Near East. This would provide us with a much larger number of data and criteria which, on the one hand, would ease future fieldwork incorporating conflict issues in the research strategy and, on the other, yield a larger amount of data for further theory construction.

Notes

1 Ph.D. thesis (Freie Universität Berlin) is currently only published on microfiche. A short summary of its contents is published in Neo-Lithics 1/06.

2 The almost entire absence of evidence for embedded projectile points in animal bones discovered in Natufian and PPN sites might imply the application of this type of hunting strategy.

3 A rare exception are Anderson (1968) and Wendorf (1968).

4 This is also the case for transverse arrowheads.

5 But also the hunting function can be questioned due to the fact that we rarely have evidence for embedded arrowheads in animal bones. Only one example of an embedded projectile point is known to me from Late Neolithic Tell Sabi Abyad (Cavallo, Akkermanns and Koens 2000). The number of embedded bones in human skeletons of Near Eastern Neolithic sites is much larger (see Müller-Neuhof 2005: 131, Footnotes 243ff., 174: Footnote 367).

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The Neolithic Medium: Warfare Due to Social Stress or State of Security Through Social Welfare

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The emergence of a new way of life in the Near East based on the cultivation of certain cereals has always been considered a deviant happening in the multifarious history of civilization. It is for this reasons that, in looking at the Neolithic formation, the question "why" such a change took place has occasionally surpassed concerns in answering "how" and "when" it happened. In the quest to find a reasonable explanation to the question why, some sort of "stress" has always been considered as the main agent triggering changes that took place in the way of living. In this respect, since Pumpelly formulated the oasis hypothesis, deteriorating environmental conditions were taken as the source of the stress that led to the formation of the Neolithic way of life. Later, the impact of environmental stress was extended to rationalize the development and the expansion of Neolithic cultures. The keynote paper of Bar-Yosef presents a thorough conspectus of changing views on the emergence, development and dispersal of the Neolithic way of life, clearly revealing how some sort of "stress", either environmental, demographic or dietary, has always been attached to the term Neolithic. How these assumptions have been substantialized as more evidence became available in time – giving way to new postulations – has also been clearly presented in the introductory paper by Bar-Yosef.

During the last decade or so, among the agencies causing environmental stress, rapid increase in population has also been considered as complimentary to fluctuations in climatic conditions. It has been postulated that there was a demographic shift during the Neolithic period, Neolithic communities becoming crowded and consequently over-exploiting their habitat. Thus, almost all assumptions contemplating either the emergence or the development of Near Eastern Neolithic cultures has considered some sort of stress as the triggering agent. Once "stress" due to environmental restrictions is conceded as the main motive in defining evolutionary stages of the Neolithic culture, it thus seems evident to surmise that there will be greater stress on communities, in time leading to conflicts and eventually to warfare among groups sharing the same territory; the problem is, of course, how to verify such an event. Bar-Yosef has, very successfully displayed what to look for in archaeological record so as to see if there were any armed conflicts, listing issues such as abandonment of sites, defense systems, skeletal evidence, etc. Thus, we shall not go into any of these issues, but instead introduce a complementary criterion.

However, concerning over exploitation of the habitat, there is an issue that requires elaboration. In considering the environmental setting of Neolithic communities, it is almost customary to look to the Levant as a model where the topographical features are very particular and different from all other parts of the Near East. In the Levant, ecological niches are all aligned parallel to the Mediterranean coast as narrow bands, thus vulnerable, both the climatic changes or to human exploitation. However, further north in Anatolia, these narrow bands, not only spread out to cover large territories, but also change orientation. This has two implications; firstly, climatic conditions pertaining to the Levant do not necessarily apply either to Central Anatolian plateau or to Southeastern Turkey. Secondly, considering the wide extent of ecological zones in Southeastern Turkey, it is extremely difficult for any pre-industrial community to use up all resources to the level of exhaustion. Moreover, until the advanced stages of the Pottery Neolithic period, the density of sites does not allow us to consider demographic pressure in any part of Anatolia. Accordingly, unless there are extremely unfavourable conditions, stress due to environment should be considered with great caution for the Anatolian peninsula.

Generally speaking, the Neolithic communities of the Near East are so particular that nothing comparable can be found in the historic nor in the ethnographic record; this is particularly true for the social structure of early Neolithic communities. What is most striking is the wide spread dissemination of knowledge, technologies and commodities that lasted during the entire span of the Pre-Pottery Neolithic period. The Pre-Pottery Neolithic period is the time of innovative developments, from the simplest tools such as grinding slabs to complex technologies like burning of lime, from the structural designs of architecture to burial customs, from the procurement of cereals to the methods of food processing, everything is new; evidently, all have initiated in different areas, but then, rather rapidly, propagated to the entire extent of the Neolithic core area. It also seems evident that in the spread of new technologies or of the commodities, mobile/wandering craftsman played a significant role. With justification, "sharing of knowledge" can be considered as the esteemed value of the Pre-Pottery period; moreover, it should be considered that this endured in a considerably large area for several millennia with no apparent interruption. If there had been any stress or rivalry among various communities, as it is almost always the case in later communities, neither wandering craftsman nor free sharing of technologies and ideas could have taken place at such scale and over such a length of time.

Patterns of obsidian trade also support this view; since the incipient stages of the Neolithic period, from the 11th millennium onwards, obsidian from the Anatolian highlands was being passed on in vast amounts and distances. In the archaeological record so far, there is no indication of any interruption in this trade network up to the 7th millennium. In our knowledge, there is no other trade in historic periods that prevailed for such a long time. Moreover, none of the source areas seems to have taken any initiative to monopolize this trade; almost from every source, from those in the Caucasus, Bingöl, Van or from all of the sources in Central Anatolia, material, either as semi-finished products or as finished tools, were being circulated. Considering that obsidian is a valued commodity of that period, if there had have been a "profit-making" system, evidently there would have been disruptions to the system. Accordingly, in viewing the social structuring of the Pre-Pottery period, it is necessary to avoid constraints or biases that are applicable to later periods. Nevertheless, this does not imply that there was no violence during the Pre-Pottery period; it also seems evident that there are some skeletal material revealing cut-marks, fractures etc. It is also clear that humans sacrificed in ceremonies, although those sacrificed do not necessarily have to have been from alien groups; moreover, through the extensive presence of human skeletal material of the Pre-Pottery Neolithic, bones that reveal any sort of violence are extremely rare.

To conclude, the media of the Pre-Pottery period seems to be devoid of any stress; on the contrary it seems to be the time of exceptional security, at least in the areas north of the Levantine region. In such a dynamic period, when numerous innovative developments were taking place, this could only have materialized if social "values" were based on sharing and on the dissemination of knowledge, and this requires a social environment devoid of tension. It is also evident that this is a fragile system; once the concept of value is implanted to commodities themselves, it is no longer possible to sustain this system, conflicts and stress then take over. It seems evident that this cognitive changeover took place either by the final stages of the Pre-Pottery Neolithic or by the beginning of the Pottery Neolithic period. By the transitional stage from the Pre-Pottery to the Pottery Neolithic, indications of some sort of turmoil are apparent in most of the core areas of the Neolithic, seemingly except Central Anatolia. During this stage, whether called Final PPNB, PPNC or Transitional period, settlement sites have either been abandoned, or shrank in size, the orderly planned set-up of Pre-Pottery settlements are no longer upheld, special buildings or temples, sophisticated crafts, monumental statues all disappear. The distribution system of obsidian also changes notably during this era; for the first time there is evidence that certain trade routes have been dominated by some of source areas and the circulation is focused more on finished products then cores.

It is also of significance to note that by the later stages of this era, the number of settlements in Central Anatolia increases notably. A number of features such as plastered skulls, certain bone and stone tools that previously confined either to the Levantine or to Southeastern Turkey/Northern Syro-Mesopotamia began appearing in the Early Pottery assemblages of Central Anatolia, implying that there has been an endemic migration from the south, firstly into Central Anatolia, bringing in new elements and merging with the local cultures. Soon after, there is a more massive movement of groups towards the previously uninhabited parts of Anatolian plateau. Thus, it is possible to surmise that there was some sort of social turbulence, communities segregating and consequently migrating to other regions. What caused this turmoil is not clear, a number of different assumptions have been suggested for the collapse of the PPN culture, ranging from changes in the climatic conditions, to exploitation of certain regions or to social unrest triggered by the full-scale establishment of animal husbandry. It seems highly probable that different agencies played a role in different parts of the region. Nevertheless, it is evident that whatever occurred, it stimulated in the local communities a momentum migrate; to discern how peaceful this event was requires further data, and these should be tested with the parameters defined in Bar-Yosef's paper.

Early Warfare in the Near East

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Professor Bar-Yosef offers a pair of hypotheses to explain site abandonment in the Levantine Early Neolithic. One hypothesis suggests warfare as the cause, the other hypothesis, climate change and environmental degradation. However, Professor Bar-Yosef in his conclusion seems to favor a combination of both. Professor Bar-Yosef's Keynote offers a description of settlement activity in the Levant and beyond from the Mesolithic through the Natufian, PPNA, PPNB, and later. But the Keynote, while offering data on climate change, does not present a "history" of warfare for this region. I use the term "history" since Professor Bar-Yosef "refer[s] to the formation of Early Natufian, hamlets as the onset of '*history*'". Single quote marks in original. In my analysis I am going to cover a greater area, the entire Middle East, and a greater time depth. I will consider climate change, hunting and gathering practices, the domestication of plants, social and political development, and the locations where warfare occurred and did not occur.

The scientific method is usually multivariate, with more than one variable or condition used to explain or predict another variable or event. Professor Bar-Yosef's twin hypotheses name conditions that are present for explaining site abandonment. But the scientific method can also focus on the absence of conditions. Warfare is a common occurrence or condition, its absence is uncommon. Rather than treating warfare as a predictor condition, the absence of warfare can be the predictor. Professor Bar-Yosef's hypothesis that warfare causes site abandonment or destruction can be replaced with the hypothesis that if there is no war, the site will not be abandoned. I suppose it may be more correct to say that here is a hypothesis that has two sides or versions.

This treatment of the scientific method leads to two propositions or postulates of great relevance for the study of warfare in the Ancient Near East. *First, domestication of plants can occur only if warfare (raids, ambushes, and line battles) is absent from a region.* Absence of warfare joins other conditions that lead to the domestication of plants. Permanent long-term settlement leads to the domestication of plants. If the conditions are suitable, wild plants that can be domesticated, and climate, soil, and water permit plant growth (Otterbein 2010 [in press]). *Second, the origin of a primary or pristine state can occur only if warfare is absent from a region.* A pre-state society, such as a tribe or chiefdom, does not have a government that can conquer**,** incorporate, and control a neighboring settlement. A polity must, however small, be a state in order to be capable of carrying out conquests. The notion that states arise from chiefdoms through war does not make logical sense.

Two Paths to War

In an article on the origin of war (1997) I argued that warfare increased over time in the area we are considering. I shared the published article with Professor Bar-Yosef and he provided me with a detailed critique (October 26, 1998; Otterbein 2004:254). But I was not satisfied with my analysis, for I realized that when and where warfare occurred did not follow a uniform path. Rather, it occurred at different times and places. To explain the distribution I created a two-path approach to war, the first path being derived deductively and the second path inductively. The first path is a hypothesis, and the second path an explanation derived from data such as site information (Otterbein 2004:14; Otterbein 2006; Otterbein 2009: ii).

The Hunter/Gatherer Path to War

The first path begins in the Paleolithic when all Homo sapiens were hunter/gatherers, subsisting in most regions upon large game animals. This was true for both the Old World and the New World. The production of excellent hunting weapons, both the thrusting spear and the atlatl or dart thrown by a spear thrower, made such subsistence possible. Hunting weapons can kill not only animals, but also Homo sapiens. Encounters between hunter/gatherer bands or hunting parties can turn violent, projectiles can be fired at rival groups contesting hunting tracts or fallen animals. Projectile points in bone as well as some rock pictographs support this interpretation. Fighting between Homo sapiens presumably increased over time, until the supply of large game animals decreased. With the decrease, hunter/gatherer bands reduced their range and placed less emphasis upon producing excellent hunting weapons, two conditions that would lead to less inter-group fighting (Otterbein 2004:63-90).

It has become a well-known story, and I believe widely accepted theory, that climate change and overhunting by an increasing Homo sapiens population led to the demise and extinction of many large game animals. The new form of subsistence technology came to be known as the "broad spectrum revolution." The emphasis, by necessity, fell upon small game hunting and gathering. The nomadic or semi-nomadic hunter/gatherers became sedentary, hunted within a restricted area and built permanent circular huts and storage areas (*e.g.* Jericho and Abu Huyrea).

The sedentary hunter/gatherers were not warlike, and probably on only rare occasions engaged in warfare, usually for defense. This is true for those societies

described ethnographically in the past 200 years who are known as Foragers or Simple Hunter/Gatherers (Fry 2006:104). Other early types of hunter/gatherers who were not Foragers retained their bellicosity: Macrobands of Big Game Hunters; Australians; and Settled Fishermen or Complex Hunter/Gatherers. Macrobands were able to put into the field large parties of hunters who could become warriors if a provocation with another party arose. Australians for tens of thousands of years appear to have hunted kangaroos, and developed polygyny and virilocal residence. Big game hunting, virilocal residence, and polygyny form what I have called the "eternal triangle," a combination of traits that leads to raiding and warfare (Otterbein 2004:62). Settled Fishermen or Complex Hunter/Gatherers in recent centuries resided on the North West Coast of North America. They defended their settlements, which were often located near salmon rivers, and raided their neighbors. Their equivalent in the Upper Paleolithic would be large settlements along rivers or along lakes that utilized marine life. Jebel Sahaba near the Nile River engaged in warfare nearly 14,000 years ago (12,000 B.C.E.) as evidenced by a cemetery in which there is evidence that about 40% of the interred had been killed by stone-tipped weapons (Otterbein 2004:74). These were also locations worth defending. (These are the four types of hunter/gatherers which I have identified as existing in the Upper Paleolithic. For Recent Times I have identified four more types of hunter/gatherers [Otterbein 2009:68-74].)

The Primary State Path to War

The second path to war begins with the settled hunter/gatherers who did not engage in warfare. The absence of warfare sets the stage for the origins and development of agriculture, the first proposition described earlier. The settled hunter/gatherers lived along the Fertile Crescent occupying environmental zones referred to as the Hilly Flanks and the Piedmont Steppe. Wild plants, such as barley and wheat, growing on the Hilly Flanks were carried from this zone where Jermo was located to the Piedmont Steppe where Ali Kosh was located. Gatherers became agents of plant selection about 9,000 years ago (7,000 B.C.E.). These domesticated plants were carried further onto the Alluvial Dessert. In this zone far up the Euphrates River is Abu Hureyra, a settlement that was occupied by hunter/gatherers about 9,500 years ago. A thousand years later it was a large farming community. This settlement was occupied for more than 4,500 years (7,500 B.C.E. – 3,000 B.C.E.). What is so significant about this site is that there is no evidence for warfare. Likewise further to the west and south is Jericho, reputedly the oldest town in the world at nearly 11,000 years ago (8,500 B.C.E.). The first walls are regarded as flood protection by Professor Bar-Yosef. About 9,000 years ago (7,000 B.C.E.) the second wall was destroyed, providing evidence of warfare.

Mapping of sites where war occurred and did not occur reveals a geographic pattern. What has been referred to as Lower Mesopotamia (I include Abu Hureyra) contains sites where domestication took pace, agriculture flourished, and war was absent. From the Nile River north through the Levant and into Anatolia warfare occurred. With time domesticated seeds and livestock diffused—carried by individuals or spread from village to village—to regions west and north of the Fertile Crescent. Warring settlements, thus, came to be agricultural villages with the diffusion of domesticated plants and animals. Included among these settlements were Hacilar, Çatalhöyük, and Mersin. I believe that warfare was intensified by attempts to control trade routes from a volcanic area in east-central Anatolia where there were sources of obsidian used in the manufacture of weapons.

The second proposition described earlier argues that warfare not only has to be absent for domestication, but it has to remain absent while the socio-political order evolves. As settlements fission they become two-tier settlement hierarchies known as chiefdoms. If chiefdoms war they remain chiefdoms. They do not have the political organization that would allow conquest and incorporation into a three-tier settlement hierarchy. Internal developments may lead to the greater power of leaders. This usually occurs through internal conflict, whereby one leader kills his rivals or subordinates them to his will. The losers form a lower class within the polity. Three-tier polities arise, which war with each other. At this stage wars of conquest can occur.

For Lower Mesopotamia the site data show clearly that this occurs. Villages in the Eridu stage became minimal chiefdoms, then typical chiefdoms in the Early and Late Ubiad stages. There is no evidence of war and military organizations. At the beginning of the following Uruk stage, evidence of internal violence is found (maces and pictographs of subjugation) and three-tier polities arise, known as maximal chiefdoms or inchoate early states. War and conquest occur and four-tier polities emerge, known as typical early states (Otterbein 2004:142-158).

Archaeologists differ as to whether states are three-or four-tier. I subscribe to the former since I see the political organization of the three-tier polity as coercive. The population is controlled, conscription can occur, lower classes can be coerced into producing a surplus of agricultural produce and material goods, even standardized weapons for a conscript army. If an archaeologist subscribes to the notion that states are four-tier, he/she may conclude that three-tier chiefdoms may war and conquer each other, giving rise to the state. From my point of view there were many areas of the world where warring chiefdoms did not become states. The four-tier group argue for the Conquest Theory of the State. I do not. I subscribe to an Internal Conflict Theory (Otterbein 2004:96-110).

For many years the Conquest Theory (Herbert Spencer) was viewed as a dated 19th century theory. In

the 1960s, when I was a graduate student and young professor, a Confederacy Theory (Fred Gearing), a Consensus Theory (Elman Service), and an Internal Conflict Theory (Morton Fried) competed with each other as the explanation for the origin of the Primary State. By the middle of the 1970s the Conquest Theory had reemerged (Robert Carneiro). Some secondary states can be explained by conquest.

Conclusion

Professor Bar-Yosef focuses upon two hypotheses that could be considered rivals for explaining site abandonment: environmental factors and warfare. He also believes they can operate together. In my presentation I did not test the hypotheses, but rather tried to explain "what happened in history." I considered many factors, perhaps the most important being the destructive effects of warfare. I argue that warfare, if present, will prevent any preliminary steps toward plant domestication. Warfare destroys growing plants and stores of seeds, kills the gatherers, and forces site abandonment. Domestication of plants occurred on the Hilly Flanks and Piedmont Steppe of the Fertile Crescent where warfare was not occurring. In the Alluvial Desert south of the Fertile Crescent warfare was also not occurring. Here mature agricultural villages flourished. The farmers had a ready water supply from the Euphrates and Tigris Rivers. Through village fissioning chiefdoms arose. Still there was no warfare to halt the development of three-tier states. If chiefdoms had begun to destroy each other, the first pristine states in the world would not have developed. Other areas of the Old and New Worlds went through similar stages with warfare absent in north China, highland South America, and Central America. Primary states arose in those three regions. Warfare is not a causal factor in my interpretation of history. While it can be used to explain destruction and lack of development, its absence as part of a large scheme can explain both the origin of domestication and primary states. The absence of warfare may explain many other things.

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Commentary on "Warfare in Levantine Early Neolithic. A Hypothesis to be Considered"

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From Violence to Warfare

Warfare has always figured prominently as an explanatory model for certain aspects of the archaeological record, and has been invoked in the interpretations of structures, tools, evidence of village burning, and evidence of multiple deaths. Violence certainly represents one of the commonly encountered options in the repertoire of human behaviour, regardless of the type of subsistence or the level of social complexity of any individual society (Keeley 1996; Keeley 1997; Kelly 2000). But how do we proceed from the evidence of an individual's violent interactions and death, to the interpretation of organized violence, and, furthermore, from evidence of organized violence to warfare? Ofer Bar-Yosef's paper addresses some of these issues in the best and maybe the only possible way: by looking at the specific regional development and its ecological, historical and demographic concomitants, as revealed by the archaeological and bioarchaeological evidence. In addition to its unique geographic position, the wealth of research, excellent excavation techniques, environmental reconstructions and a number of sites with long cultural sequences certainly make the Levant an excellent place to attempt to understand the genesis and meaning of violence in prehistoric societies, and the potential causes of organized violence and warfare. What I would like to address in this commentary is related less to the critique of the evidence presented for the Near East, but acts more generally as a cautionary note against applying this well-rounded scenario proposed for the Levant to other parts of the world or to sedentary hunter-gatherers in general.

In many other areas of the world, even those with a long archaeological research tradition such as the Balkan Peninsula, we are still dealing with individual sites and cases where evidence is being interpreted as an example of violent interactions against other possible interpretations. In the Balkans, but also in many other areas of the world, understanding the role that sedentism played as a causative agent of warfare is impeded by incomplete evidence of the archaeological sequences predating agriculture, insufficient paleoenvironmental research, and a paucity of skeletal material. While sedentism and its associated higher population densities are generally considered as conducive to violent interactions among humans competing for limited resources, violence needs to be understood in its cultural and historical context, which is not always available to regional archaeologies. While population pressure – prominent since Thomas Malthus' famous Essay on the Principles of Population (1798) - is often evoked as a major predictor of the frequency of war, this is not supported by cross-cultural studies (Keeley 1996:118). Furthermore, Kang (2000) demonstrated that under certain historical circumstances, warfare can result from underpopulation caused by environmental stress rather than overpopulation.

Endemic Violence in the Mesolithic or Preservation Bias?

I find it unfortunate that the Mesolithic has been singled out as a period when the evidence for violence becomes far more common than in the earlier periods of human history (Frayer 1997; Thorpe 2000; Torres-Rouff and Costa Junqueira 2006; Vencl 1999). Is it really so? What unequivocal evidence do we have to claim that the Mesolithic was more violent than previous periods? And if that indeed was true, what explanations can be offered? Is the violence related to sedentism, accumulation, prestige, or other elements of the societal structure (Pospisil 1994); or might it not be a sampling error stemming from the fact that we have far more skeletal remains from the Mesolithic than from the earlier periods? If indeed we can demonstrate higher levels of conflict in the Mesolithic than in previous periods, what happens later: more conflict, less conflict? Does violence – and more specifically, organized violence – play an evolutionary role in creating large-scale aggregations with a centralized power structure (Carneiro 1994), is it the by-product of the centralization of power (Kang 2000) or should war and society be regarded as co-evolving, as Kelly (2000) proposes?

The Necessity of an Historical Context for the Interpretation of Warfare

The evidence gathered from present-day indigenous people practicing traditional ways of life, as well as historic accounts of these people, still provides the most immediate insight into the diversity of human responses. While direct ethnographic analogy is often misleading as it takes evidence out of its historical context, insights provided by these groups must be paramount. The recognition that these groups have their own history has to be the basic premise of all theory-building and explanatory attempts (Ferguson 1992; Marshall Thomas 1994) "Wars are often fought locally, even World Wars: they are conjectural events"

(Simons 1999: 92). This local and historical character has to be kept in mind in all attempts to understand war and its background. Keeley (1996) showed that warfare is present in the archaeological record of non-state societies and demonstrated that pre-state society warfare cannot be regarded as different in extent and lethality from wars between states. Nevertheless, Haas questions Keeley's contention that, as a given, warfare is universal and notes that Keeley "forces us to examine the critical question of why warfare appears and disappears at different times and places" (Haas 1999: 13). Whether analyzing causes of war in human society in general, or searching for similar patterns and causes on a regional level, it is crucial to take an historical approach to warfare from its emergence to its resolution. That an historical approach is crucial is also stressed by ethnographic research (Ember and Ember 1997; Ferguson 1992).

Furthermore, war is "not related to violence as simply more of the same" (Kelly 2000: 21). If we decide that violence does, but warfare does not appear before a certain level of socio-political complexity, such as the state, is reached (Reyna 1994), and conceptualize war as restricted to centralized polities (Reyna 1994: xiv), the question of warfare in the Mesolithic and Neolithic does not even arise. However, since no form of social organization or mode of production can be causally linked with war or peace (Ember and Ember 1997; Otterbein 1997; Otterbein 2000; Walker 2001), all societies will eventually indulge in war. Therefore, I favour the definition of warfare offered by Kelly, applicable to all levels of political centralization, which offers a good working definition for examining prehistoric warfare. Kelly (2000: 21) considers war (including feuds) to be grounded "in application of the principle of social substitutability": "the principle that one group member is substitutable for another in these contexts underwrites the interrelated concepts of injury to the group, group responsibility for the infliction of injury and group liability with respect to retribution" (Kelly 2000: 5). Unsegmented hunter-gatherers have a low frequency of warfare as they lack organizational features associated with social substitutability that are conductive to the development of group concepts. On the other hand, segmented foragers show a much greater frequency of warfare: 16 out of 17 examined by Kelly (2000). We could claim that recognition of group identity provides the best explanatory mechanism for the emergence of warfare. It is important to stress, however, that social structure in itself does not result in feuding or war. Certain external conditions will need to be imposed in order to generate warfare. Accordingly, Kelly states that "warfare is not an endemic condition of human existence but an episodic feature of human history (and prehistory) observed at certain times and places but not others" (2000:75). All societies will know periods of peace and stability, and I would not necessarily agree that peaceable societies are as uncommon as they seem to be: the lack of diversity in responses offered by modern societies to stressors resulting in warfare could be obscuring a number of possible responses in the past.

Understanding Archaeological Evidence

That local history has to be a component in understanding warfare is no less true for the Mesolithic and Neolithic groups that archaeologists study. Illustrative of the quality of evidence we are dealing with is the fact that we consider the sample size of 100 individuals from a single site of this period as substantial, and often make inferences based on less than 20 individuals. That the problems become aggravated by excavation and curatorial practices is, alas common knowledge for all of us, and we often have to "make do" with what little evidence we have (Roksandic 2004; Roksandic *et al.* 2006). Given concerns about preservation bias, inability to detect soft tissue wounds as causes of (violent) death, and the near impossibility of distinguishing between violence and accident, we are left with an even more difficult question. If we can indeed recognize the evidence for violence, how can we interpret it: are we dealing with short episodes of unresolved conflict with high mortality rates, or a constant but low rate of "endemic" warfare? And furthermore, if we can ascertain a case of intertribal warfare, can we consider the group (or as is currently done for the whole Mesolithic) as warlike? Could not the sporadic episodes of – even organized – violence, be just what they seem to be: episodes of stress resolved through conflict without further impact on the society and its long-term history? As Jackes (2004) points out, since there are inevitable political and judgmental overtones additional to osteological interpretations in the examination of violence in any given society, we must be extremely careful and strictly neutral when making broad statements regarding violence in any society.

The historicity of warfare requires the understanding and interpretation of organized violence through a culture-specific lens. While there is no doubt that every human being is capable of violent behaviors, socialization and learning help direct and channel this type of behavior as certain instances will be praised, others shunned in any given group. Every individual in a given group has to find the modality that will fulfill both individual needs and social expectations in a particular situation, including violence, and accordingly, societies differ both in the amount and direction of violent behavior that is considered permissible or appropriate. The value associated with violence, properly channeled within a cultural system, often finds some reflection in the symbolic behaviour of the group. The cultural specifics of a group – at any given time within its history – need to be examined against the backdrop of the available environmental, demographic and symbolic information to allow for building a strong and well constructed framework for understanding warfare in regional (pre)histories.

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Violence in Eden: Comments on Bar-Yosef's Neolithic Warfare Hypothesis

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When I first read Bar-Yosef's proposal on warfare in the Levant I must admit I was skeptical. We recovered a considerable number of PPN burials from 'Ain Ghazal (n=121) from the MPPNB, LPPNB and PPNC layers), and only one of these displayed any indications of having come to a violent end (see below). In the back of my mind, perhaps, was the feeling that the expansion of farming populations was an essentially peaceful process with little to upset the idyllic transformation of the Levant beyond the occasional interpersonal strike of remorseful anger that unfortunately had led to the death of a fellow resident of an agricultural settlement. But now that I have had some time to look through some of the background material from several of the excavation reports and other sources of information, with some incubation of the implications of the field data, I have come to confront my doubts of a scenario where bloodshed may have been a more common occurrence. Nevertheless, I think there is still some room for debate on what the nature of "warfare" in the Early Neolithic entailed.

There are several points in the Neolithic prehistory of the Levant when conditions may have been more conducive to violent conflict than others. One of these would have come during the later part of the PPNA, and the clusters of burials from Jericho, for example, might reflect increased stresses near the end of the 10th millennium BC. Another trigger may have been set at the end of the MPPNB, when considerable upheaval took place in the southern Levant (*e.g.*, Rollefson 2004; Gebel 2004), and when settlements grew to unprecedented sizes and demands on local resources were unmatched compared to earlier times. Then once again, after six or seven centuries, another tumultuous shift in settlement patterns took place at the end of the 8th millennium BC, when mega-sites along the highland spine of Jordan were severely depopulated or were abandoned altogether. In all these cases, it is very likely that "title" was claimed to resources, especially arable farmland, water, and other abiotic materials (Gebel n.d.), and that trespassing on such holdings could easily result in major intergroup violence. But overall, the expression of such periodic potentials of intergroup assaults has not appeared with any impact in the archaeological record.

The fertile mortuary data from Jericho, where nearly 500 bodies were recovered from prehistoric contexts (Kurth and Röhrer-Ertl 1980: 32), are ambiguous when it comes to evaluating the impact of violence on the local population. There are some suspicious clusters of burials that indicate that large numbers of people died simultaneously, which certainly would have been extraordinary circumstances of death. One of the more noteworthy cases involves the 12 skeletons inserted through a wall of the PPNA tower (Kuijt 1996: 324), which was possibly related "to some disaster, in which buildings were destroyed ... [and perhaps] associated with the great fire in the area to the south" (Kenyon 1981: 33).

In another case, Kenyon mentions the cluster of more than 30 individuals ending up in a jumbled cluster (Kenyon 1981: 78), possibly due to an earthquake during the PPNB Stage XVIII in Squares D1 and F1 (1981: 12). Cornwall described this mass burial as representing deaths possibly due to a plague, "for not one of the bones bore signs of violence such as might have been expected as the result of a massacre" (Cornwall 1981: 401). Bar-Yosef mentioned the likelihood that the PPNA wall and tower complex at Jericho was not related to defense against enemies but as a means to protect the community from flash floods, and this certainly has received a chorus of agreement. After all, if the PPNA population at Jericho was around 500 people, say, almost half of them were probably hunters and adept at the use of bow-and-arrow as well as spears and possibly propelled darts. What kind of community could raise enough people to assault such a concentration of archers well-versed in accuracy? Furthermore, where are they coming from in numbers sufficiently large to threaten the lives of the Jericho residents? But one aspect of the architecture of Jericho has been overlooked, perhaps. In Sq. M1 Kenyon recorded the construction of a PPNB Stage XII wall "incorporating massive orthostat slabs" (1981: 221) that appear to be sturdier than one would expect for flash flood protection (for which, it appears, there is also no evidence in the PPNA ditch). In the succeeding Stage XIII Kenyon described a possible gateway to a town wall in the same area (1981: 222), and "this new wall is a defensive wall as well as being a terrace wall, and this is the first town wall of the Pre-Pottery Neolithic period" (Kenyon 1981: 79).

As for indications of violent conflict, the cases are relatively skimpy in number. Müller-Neuhof identifies circumstantial evidence for conflict, including settlement location and structure, fortifications, a variety of burial information (including demographic statistics), weapons of close and distant combat, and iconography (Müller-Neuhof 2005: 425-430. Among the weaponry, he cites the widespread presence of projectile points and sling stones; both kinds of weapons could have been principally used for hunting, but there are some exceptional instances (see below). As for close combat, weapons such as maces, axes, and daggers are relatively widespread throughout the region during the PPN and PN periods (Müller-Neuhof 2005: 430).

Direct evidence of violent conflict is much rarer. Du-

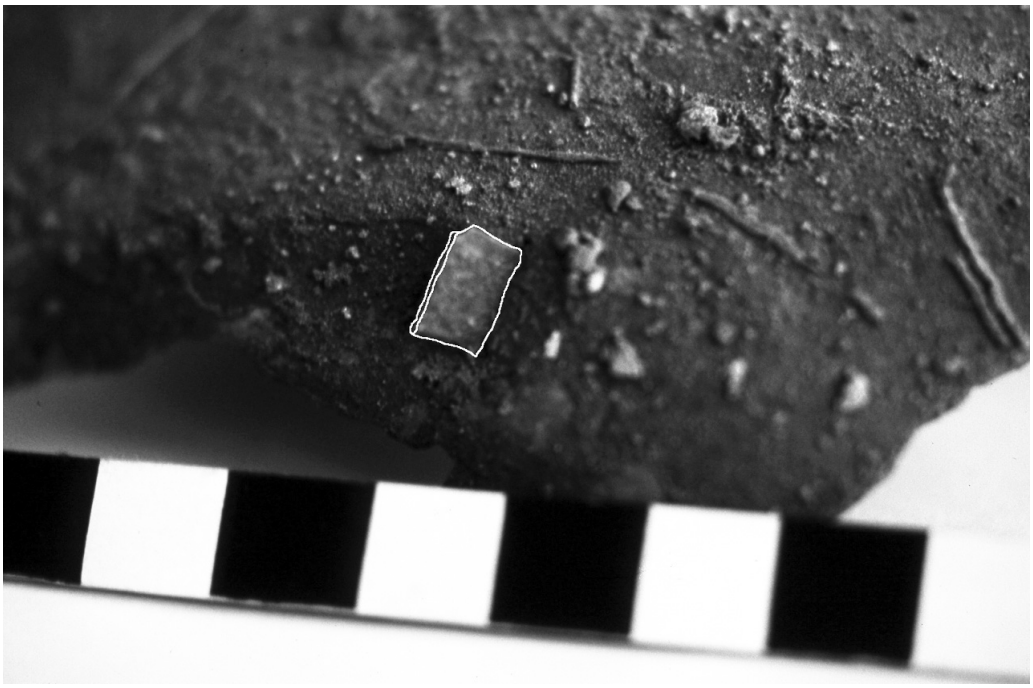

Fig. 1 Exterior of LPPNB male skull fragment with embedded flint blade, from 'Ain Ghazal. The blade segment is outlined in white. (Photo: H.-D. Bienert).

ring the LPPNB at 'Ain Ghazal, one "trash burial" (rare during this period) bore a clear indication of the cause of death. Many of bones of the lower part of the skeleton were missing, but some of the upper torso was in articulation, in a vertical position with the skull slipped down over the uppermost vertebrae. The most striking aspect was the presence of a thin flint blade snapped at both ends embedded in the left side of the skull, which had penetrated with sufficient force to drive a *c.* 3 cm diameter piece of the inner surface of the bone into the brain (Figs. 1 and 2) (Rollefson and Kafafi 1996: 22; *cf.* Grindell 1998: 377).

Fig. 2 Interior view of Fig. 1. The blown-away interior bone is indicated by a dotted line. (Photo: H.-D. Bienert).

Nemrik 9 provides a couple of cases of violent death. One burial was of a male whose skull contained two pedunculated projectile points, and another skeleton included a pedunculated point in the pelvic area; a broken el-Khiam point was found next to the broken arm of a third burial (Kozlowski 2002: 40). The rarity of these projectile point types in the Nemrik site has been taken as an indication that violence was not restricted to immediate neighbors, but to attacks by groups of people who had traveled great distances (Müller-Neuhof 2005: 260).

Other instances of violence have also come from southern Jordan. At Basta, the cranium of a boy 8-9 years of age revealed two severe blows: one was inflicted by a sharp-edged tool that penetrated the left frontal area, but this was not the cause of death. The second blow was fatal: it caused a web of fractures that reached from the occipital area to the right frontal bone (Röhrer-Ertl *et al.* 1988: 136). Other cranial injuries have been reported from Basta as well. Of a sample of 29 skulls, five exhibited "healed fractures of the skull vault" (Schultz *et al.* 2004: 260). Altogether, then, trauma affected one fifth of the examined crania from Basta, including the young boy described by Röhrer-Ertl *et al.*, and this statistic raises questions about the nature of the violence associated with the injuries. Müller-Neuhof also noted that the demographic data from Çatalhöyük indicated a disproportionately low representation of males in the mortuary population, especially in Layer VI compared to the earlier layers (2005: 428), reflecting perhaps the situation where the men had been killed elsewhere and buried. Mortuary statistics are often cited to reflect the conditions of the greater populations they were

once part of, but in the case of the southern Levant, this glimpse of pathologies must be regarded with extreme caution. Kurt and Röhrer-Ertl long ago warned that the mortuary data from PPNA+B Jericho was not representative and should not be regarded "even as pars pro toto (1981: 430; *cf.* 460). They concluded that "[i]n the Jericho PPNA+B only very few of the dead were placed inside the settlement under the contemporary surface" (1981: 432). This might partly be the reason that so little indications of violence are present in the large sample at Jericho.

This situation of under-representation of human skeletal remains was discussed in a recent article that asked "Where are the Dead?" (Bienert *et al.* 2004). It was suggested that perhaps 80% or more of the dead were buried off-site, and that certain criteria (such as primogeniture) were used to select family members for burial beneath house floors (Rollefson 2004: 170-171). As Bar-Yosef has remarked in this issue, violence was widespread if not particularly frequent. The cultural phenomenon surrounding the subfloor burial pattern so characteristic of the M+LPPNB in the southern Levant might be the reason that the "smoking gun" (such as the gruesome case for Talheim during the LBK Neolithic of Central Europe; Wahl and König 1987) for more extensive conflicts has not been found yet. The recent discovery of a cemetery near a Late Neolithic settlement on the Mediterranean coast (Galili *et al.* 2009) provides some promise that earlier PPN cemeteries might also be discovered to shed some light on the sociopolitical conditions of the Early Neolithic.

Bar-Yosef suggests that there was a kind of "weapons industry" that may have circulated projectile points by "mobile artisans" who presumably went from settlement to settlement to exchange their instruments of death as a way of earning a living. While this can't be refuted absolutely, the "arms trade" need not have been promulgated by roving flint knappers. Quintero's research has shown that while naviform blade production at 'Ain Ghazal was the work of specialists (Quintero 1998: 227-228), the loci of naviform production using local purple-pink flint indicates that these specialists were members of the local community.

Looking for correlations of collapsed buildings, burned buildings, and associated bodies that might reflect attacks on settlements from outside groups, as advocated by Bar-Yosef, might be a fruitless endeavor. First, I am unaware of any Iron Age style destruction layers in Early Neolithic sites. In our small sample of excavated houses from MPPNB 'Ain Ghazal, only two were found to have suffered significantly from fire damage (one utterly destroyed, one renovated), but the vast majority did not include any indications of intentional destruction; for the LPPNB, only one building burned, but again there was no direct tie with intentional conflagration. In these rare instances from 'Ain Ghazal, it is important to recall that there was a lot of exposed wood in the structures in most of the buildings (and at other sites as well), and accidental fires from sparks rising from the interior hearths to the dry rafters was probably an occasional calamity. Were bodies to be found in the interior of collapsed buildings, there must have been times when this seismically active area suffered its share of Neolithic destruction. This appears to be the case at LPPNB Ba'ja, for example (Gebel and Kinzel 2007: 32), and there are a few indications that 'Ain Ghazal also experienced an earth tremor during the LPPNB; seismic damage could very likely be accompanied by fires that started as a consequence.

While none of us would be surprised to find more indications of violent death in the Levant, it must be asked how much of the violence was interpersonal or intragroup rather than intergroup (the latter fitting more comfortably into my cognitive understanding of "warfare"). The few examples of reported violence do not necessarily imply anything beyond personal vendettas or perhaps some form of internal blood feud (although the situation at Nemrik 9 does seem to argue for "strangers in the land"). In any event, until we find a Talheim (Wahl and König 1987) example, we will not be able to conclude that the scale of conflict reached the level of intergroup raiding or warfare.

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War, Community, and Environment in the Levantine Neolithic

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In the keynote paper, Bar-Yosef proproses that widespread abandonment of Early Neolithic villages in the Levant was caused by warfare more than by environmental degradation or rapid climate change. As a specialist in New Guinea ethnography, I am in no position to judge this proposal on its archaeological merits. If analogy be allowed, however, the New Guinea evidence does support its prima facie plausibility. At contact, New Guinea was home to thousands of Neolithic communities ranging in density from 0.3 people/sq km to over 100/sq km and in size from about 20 to 2,000 or more people. As best can be judged, every one of these communities was at war with one or more of its neighbours on an episodic or permanent basis, and in several regions massacres and large-scale population displacement were common results.

Drawing on the New Guinea evidence, I want to make two, more specific points, one concerning evidence for warfare in the Levantine Neolithic, the other the specific conditions under which warfare results in settlement abandonment. In attempting to detect warfare in prehistoric contexts, little attention has been given to one of its more obvious signatures – community scale. Simply put, it is difficult to explain the formation and maintenance of large, nucleated communities except in terms of warfare. Biological and social reproduction requires a group no larger than the nuclear or extended family. Under Neolithic subsistence regimes, efficient procurement and stabilization of subsistence resources seldom requires the cooperation of more than about 25 people, 50 at the outside (Winterhalder 1986; Roscoe 2009:79). The Natufian hamlet settlement pattern of 30-50 (rarely 100) people (Bar-Yosef, keynote paper) might just about be explicable, therefore, in terms of subsistence optimization. But how are we to explain the early Neolithic villages of "250-400" people that replaced them within just a few centuries?

Anthropology has only recently begun to problematize this question of large-group cooperation (Boyd and Richerson 1988). Earlier, "group-living" was either taken as a primordial given, which is no explanation at all, or it was attributed to the localization of key resources – for example, rich patches of subsistence resources or areas particularly suited to defense or breeding (Alexander 1979:60; Smith 1981). The problem with this argument is that resource localization may account for physical aggregation, but it does not explain social organization – why clustered individuals should also form a social group. The tenants of an apartment block are physically aggregated but they are not thereby an organized social group. They might become one if, for example, their landlord proved delinquent in repairing facilities, but they do so in response to a challenge extraneous to the factors that produced their proximity. In the early Neolithic Levant, a resource localization argument might explain the emergence of settlements, but it is a sufficient explanation only if we assume that the residents did not constitute a social group – a highly unusual circumstance given what we know ethnographically about Neolithic society. (We might add that resource localization only produces nucleated settlements if a resource patch is distributed uniformly around a central point. If it deviates from this form – if, for example, important resources are linearly distributed along a watercourse, coast, cliff, or lake bank – then residences will be strung out, not nucleated.)

At present, the only plausible explanation we have for the formation and maintenance of nucleated Neolithic communities on a scale larger that required for reproduction or subsistence optimization is defense against attack (Alexander 1979:221-240; Roscoe 2009:80-85). By organizing collective action in mutual defense, such a community advances the common interest of its members in survival. It is large because defensive strength scales in direct proportion to numerical size. And it is nucleated because the efficiency with which members can rally to one another's aid in the event of an attack is thereby optimized (Roscoe 2009). The very fact that villages on the order of 250 to 400 people existed in the early Levantine Neolithic, in sum, is testimony to the presence of a significant threat of war.

Assuming, then, that warfare was present in the Levantine Neolithic, the further question is whether it can account for the widespread abandonment of villages between 11,700 and 8,200 BP. The answer is not as straightforward as it might seem. In New Guinea, it was a common occurrence in some areas, but a rare event in others. One of the key explanatory factors was the extent and density of landscape vegetation. In Neolithic warfare, extensive tracts of dense vegetation act as a kind of 'natural' defense against the offensive application of large-scale military force. In moving across such a landscape, warriors cannot advance on an organized front but must move in file along whatever paths traverse it. In consequence, their advance can be easily thwarted by an enemy force, even a vastly inferior one – the Horatio-at-the-bridge effect. Furthermore, they are vulnerable to counter-attack. If their intention is to launch a surprise attack and their target is forewarned – a common occurrence in regions of New Guinea where settlements are large and the potential for leaks therefore high – they are vulnerable to entrapment by an enemy waiting in cover along either side of their path. If they are attempting to chase down an enemy routed from a battlefield, they are similarly vulnerable if the enemy manages to rally his forces or if he has faked his retreat

– again a common occurrence in New Guinea. In dense cover, moreover, lines of retreat are also vulnerable. Thus, an attack can succeed but retreat prove catastrophic if an enemy that has greater familiarity with the terrain can dispatch a force to delay or halt the retreat (Roscoe 1996:653). Finally, the losses that can be inflicted in even the most successful attack are blunted because the surrounding vegetation provides a ready and effective refuge in which defenders can escape the predations of an attacker.

In New Guinea, the southern foothills of the East and West Sepik coastal mountains furnish an exemplar, a region of large villages set in dense primary or secondary rainforest. Here, the dominant mode of attack against a settlement was the small-scale hit-and-run ambush, an assault that might succeed in killing a few inhabitants and burning a house or two on the outskirts of a village but was incapable of dislodging or inflicting serious harm on a target that typically housed a far larger warrior force (Roscoe 1996:651-653). In this entire belt of settlement, some 300 kms long and home to many hundreds of villages, I know of only one 20th Century instance in which a whole village vanished as a result of military force: the destruction of the Urat village of Wundai by a coalition of seven neighbouring villages (Allen 1976:54).

Where large-scale massacre and displacement of villagers did take place in New Guinea was in broad, open grasslands such as those that characterized the valleys of the highlands, particularly the Eastern Highlands. These environments imposed few restraints on how a large warrior force chose to advance or retreat; there was little cover from which counter-attacks could be launched; and if a strike or battle was successful, there was limited refuge in which the vanquished could escape annihilation. Their main option for survival, in fact, was to flee to kin or allies in other districts. Under these conditions, surprise attacks commonly took the form of large-scale raids; open battles, when they occurred, were not infrequently decisive in their outcomes; and both commonly resulted in the massacre and flight of one side at the hands of the other. Among the Kamano of the Eastern Highlands, for instance, Fortune (cited in Mandeville 1979:112) discovered that every village except one in the vicinity of Raipinka had been routed "within living memory"; Mandeville (ibid.:112,122) estimated that Kamano villages were displaced from their lands in war every sixty years if not more frequently. Watson's data (1970:112) for the Tairora of the Eastern Highlands indicate that major displacements occurred every 25 to 50 years.

Reconstructions of paleo-vegetation in the Levant (Hillman 1996:164-165,190) show vast areas of steppe, desert-steppe, and woodland/forest-steppe during the period under consideration, precisely the kind of open environments that fostered massacre and flight in New Guinea. On this evidence, therefore, Bar-Yosef's hypothesis that warfare was responsible for the abandonment of many Levantine villages is plausible on its face. I shall leave to others a discussion of the archaeological signatures of war and displacement that we should expect to find in the Levantine record. I suggest, though, that closer attention is also warranted to how well patterns of village abandonment map onto the physiognomy of Levantine paleo-vegetation. In particular, if the New Guinea evidence is to be credited, it would be useful to compare the fates of villages located in steppe regions to those in woodland and forest areas such as those that blanketed regions of the Mediterranean coast.

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Warfare in the Neolithic? Methodological Considerations

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The documentation of the Middle East stretches continuously from the Lower Palaeolithic into modern times, meaning that the evidence is solid and abundant from the first region in the world where humanity crossed the threshold of history, as defined by being linked to written records. In this contribution, Bar-Yosef has made an admirable attempt to interpret the evidence of the Levantine Neolithic in a fashion compatible with the origins of warfare. The paper is particularly important because the Prehistoric evidence from the Near East is the most comprehensive, providing sequences, demographic and settlement patterns, tools, and burials – and the Neolithic must be a pivotal point in the history of warfare.

In general, however, because of the details, Neolithic warfare is a fraught theme. Firstly because discussions about violence and warfare in Antiquity and Prehistory have documented violence, but there is little agreement about warfare even for the Bronze Age, and the nature of Neolithic warfare remains obscure. Secondly because most definitions of warfare insist on a political role, and our failure to understand the politics of the Neolithic necessarily undermines our capacity to judge the nature of warfare. I contend that warfare is not mere intra-communal violence, but specifically includes the use of violent means by states to achieve the aim of subduing the will of the enemy and occupy territory; this is clearly visible from the Bronze Age onwards (Warburton 2001; 2006a). It could be reasonable to propose that whatever conflict took place between communities in the PPNA led to the phenomenon of something resembling warfare in the PPNB, and that this led to something more like warfare from the Uruk onwards. One might argue for conflict and territory in the Halaf and Ubaid – and it must have started somewhere. The issue is: when and where? Bar-Yosef has put this on the agenda. It is highly significant that Bar-Yosef links the threshold for human groups numbering 250 and more to the early Neolithic. Groups of this size simply cannot be documented earlier, and provide the demographic basis for sustained conflict. There can likewise be little doubt that the importance of arrow-heads in the PPNB should be related to violence resulting in fatalities. These two aspects alone suffice to satisfy the minimum definition of warfare (based on one proposed by the political scientist C. Cioffi-Revilla). The actual evidence thus allows us to reject hypothetical scenarios proposing that the origins of warfare lie, *e.g.*, in the European Palaeolithic, and to situate the origins of warfare as we understand it in the Levantine Neolithic (Warburton 2004/2008).

Thus, while fundamentally in agreement, as is customary I stress points of disagreement – in the hope that the discussion contributes to the development of a research agenda.

Development, Communities, Economics, Elites and War

In my view, Bar-Yosef has complicated the argument by suggesting that the Natufian represents the beginnings of 'history' and 'territorial ownership'. In principle, this should allow a scheme for following political developments interpreted in terms of warfare. Yet it is impossible to speak of 'history' as we have neither 'events' nor 'individuals' which can be understood as reflecting human purposes. In fact, we do not even have the real sequences; we only have hints at social phenomena. It is likewise impossible to understand what 'territorial ownership' might have meant for Natufian communities; even for the PPNB, discussions of elites and ownership have failed to demonstrate the existence of the phenomenon of private land ownership (as opposed to assuming it, and assuming that the evidence is compatible with that interpretation) – and certainly one cannot extrapolate 'territory' from the existing evidence.

More significantly, it is difficult to extrapolate from PPNA tool assemblages to cultural communities engaged in warfare: even those communities for which we really can argue social conflict (those associated with the PPNB mega-site phenomenon, based on secluded positions, etc.) frequently used the same tools. And it is almost impossible to demonstrate that the PPNA sites were actually occupied simultaneously (which would be the condition for conflict). It follows that (based on demographic units and evidence) there is no compelling basis to argue for Natufian or PPNA conflict on the same scale as that which could potentially be argued for the PPNB.

I would argue that these changes are necessarily linked to assumptions about economic growth, where archaeologists usually end up in circular logic with internal contradictions. One fundamental problem is that the models from the 1950s and 1960s proposed that warfare and state institutions emerged in a context where productive capacities and ownership led to the emergence of villages and elites in a commercial context. These models were then somehow mixed with Polanyi's scheme whereby markets were relegated to the background in the historical period beginning with the Bronze Age (*e.g.*, Renger in Leick 2007) – and the models have somehow survived the publication of evidence published since (*cf.* Yoffee 2005; Warburton 2006b, 2009).

However, the archaeological material clearly demonstrates that sites such as Göbekli emerged long before any change in productive capacities. Furthermore, the archaeological and philological material documenting exchange and markets reveals that these were far more important from the end of the third millennium onwards – and that the incipient origins lie in the late Neolithic at the earliest (when small quantities of lapis lazuli, gold and silver appear in the Levant). In the third millennium, large land owners and palaces are purchasing land; in the second millennium – when large quantities of lapis lazuli and silver are documented – land sales virtually disappear (*cf.* Godderris in Leick 2007). Thus, access to land only became an issue in the second millennium, after the emergence of commodity prices and markets. It is thus anachronistic to project a development whereby Bronze Age institutions eclipsed Neolithic markets and large land holdings.

Yet the Bronze Age elites did link ideology, commercial wealth, land-holdings and territorial gains to political and military activities. The challenge is linking the ultimate developments to the origins without being teleological. The emergence of something resembling warfare in the PPNB can hardly be disputed – but any explanation should recognize that the origins and the final forms need not be related.

Climate and War

In Bar-Yosef's contribution, the economic and social difficulties are compounded by the usual archaeological assumption that climatic change is the motor pushing demographic growth and thus political history. Bar-Yosef does allow that the climatic change may have triggered the demographic movements, and has thus slightly modified the format – but still assigns this a key role.

However, the one case where archaeological work in the historical period has attempted to demonstrate systemic collapse related to climatic change is that advocated by Harvey Weiss for the end of the third millennium. Yet aside from the fictive nature of the original 'Habur Hiatus', misleading chronological links provide the image of a collapse which is not real (Warburton 2007). Thus, archaeologists should actually withdraw from assigning too much significance to climatic change as such – let alone assume that demographic changes should be attributed to climatic causes.

There are two weaknesses in the case of Bar-Yosef's more nuanced argument. The first is necessarily that if the climatic change had actually merely served as a trigger to social conflict resembling warfare, then one would find some victors at the end of the PPNA and PPNB respectively. It is rather odd that all of the sites are abandoned in both cases. Obviously, something changed so fundamentally that the entire system was abandoned. If one were to associate this with warfare, it would imply a scheme resembling the destructions of the Peoples of the Sea at the end of the Bronze Age. The Egyptians may have survived and viewed their defensive measures as 'warfare', but from the standpoint of the Peoples of the Sea it can hardly be viewed as warfare in the traditional sense, as it lead nowhere. Thus, one has the impression of senseless social violence which did not lead to a new political configuration, but merely destroyed the preceding one.

In Bar-Yosef's argument, the second weakness is that he fails to take account of the realities of historical warfare. It is hardly original to suggest that the Arab-Israeli conflict which marked the second half of the twentieth century AD was not caused by either climatic change or demographic growth. It is true that the creation of the state of Israel as a political act opened the way to mass emigration and an unsustainable exploitation of the environment. Yet neither the Israelis nor the Palestinians seem to be deterred from claiming land threatened by a perpetually falling water table. Nor were the earlier conflicts between the British and the Germans in Palestine and North Africa caused by demographic or environmental change. Furthermore, no one would maintain that the Crusades, the Arab conquests, the Roman Empire, the ambition of Alexander, or the conquests of the Persians, Babylonians, Assyrians, and Egyptians were pushed by demographic or environmental problems.

In occasional historically documented incidents from ancient history, foreigners were deported from their homelands to work in the land which started the war (in the case of the Egyptians, Assyrians and Babylonians). Thus, this would have exacerbated demographic problems at home – had there been any. Otherwise, warfare was accompanied by the felling of economically important trees (as at Megiddo in Palestine where, for the siege, Thutmosis III made a palisade from orchards) and the destruction of harvests (as in the wars between the Hittites, Mitanni and the Assyrians in northern Syria). Thus in the Bronze and Iron Ages, warfare contributed to demographic problems and environmental chaos – but did not cause it. Foreign labour may have contributed to local unemployment in the victorious countries, but this is not discussed as being related to warfare. And seemingly, in the Iron Age Palestine was producing more olive oil than ever, so the results were not long lasting.

On the contrary, it is frequently argued that the Russian campaigns of both Hitler and Napoleon were defeated by the climate – but historians do not argue that they were caused by the climate. Thus in the historical period for which warfare is documented – and which provides the definitions of warfare –, demographic and environmental causes are irrelevant. Yet archaeologists are forever assuming that systemic environmental and demographic change accounts for warfare – whereas the very reverse is the case.

Summary

In fact the history of the Ancient Near East is a catalogue of wars of territorial conquest where the stronger tried to subdue the weaker – and a victor emerged until replaced by another. Warfare has political origins and purposes rather than social causes. Thus, in my view, Bar-Yosef has identified the time and place of the origins of warfare. His approach suffers from the usual failures of conventional archaeological thought, of (a) assuming that warfare has a "cause" rather than a "purpose", (b) that climatic and demographic explain economic and political change, and (c) that the details of the historical development of markets and states can be disregarded.

Models for the emergence and development of the Neolithic must be compatible with Göbekli where we have a massive community effort prior to a production or ownership economy. Göbekli would have been impossible without elite guidance, yet the economic markers are irrelevant. The models must be completely revised, starting with the knowns and eliminating the speculation. The approach of the post-processualists must be adapted to include ideology pushing social history from before the Neolithic; markets and ownership should only be allowed to push developments from the Bronze Age onwards. The emergence of states must be understood in political terms, and the role of states in pushing market forces must be placed in perspective. The role of the victors in terms of territorial expansion must overshadow concepts of ownership, violence and destruction when searching for warfare.

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Warfare in the Levant: Response Ofer Bar-Yosef

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I am indebted to all my colleagues who responded by submitting their thoughtful comments to my proposal to consider 'warfare' or 'inter-group violence' as an additional hypothesis to be considered (together with natural and social calamities such as successive droughts, diseases, and more) when we discuss the abandonment of Early Neolithic sites in the Levant. The commentaries are very rich in information, ideas, and interpretations, and thus there is no way that I can respond to or argue with every commentator. In addition, I refrained from fully referencing my current statements and thus avoiding the need to repeat the same references already cited. Only in a few cases do I make one or two references. I will therefore respond first by discussing issues of 'terminology', move on to the archaeological evidence and the limitation imposed on its interpretations, and will end with a few comments on general statements made by the reviewers.

Terminology is often a source for variable definitions. Let us take the term "feasting" that is used in the anthropological interpretation of particular faunal remains to record an event that bears the reversed sense to 'warfare', perhaps the means to avoid it or to reconcile in its aftermath. Feasting can take place within a large tribal annual meeting, for example, as known from yearly gatherings of Bedouin in Sinai next to the tombs of Sheikhs (e.g. Marx 1977). Feasting or potlatch among Northwest Coast Native Americans was conducted when a leader hosted guests in his family's house and held a feast for his guests. Socially the main purpose of potlatch was the re-distribution of wealth and as an act of reciprocity. However, we use the term 'feasting' when particular contexts are recognized archaeologically, even in the context of a small group (e.g. Munro and Grosman 2010; Goring-Morris and Horovitz-Kolska 2007; Twiss 2008).

In a similar approach M. Özdoğan in his commentary correctly refers to the obsidian exchange as a "profit making" system, and one may add that it is rare that reference is made to "merchants" or to how imported commodities were paid for. Nevertheless, even a rural network with a low level economic system can be described by using the same terminology used by economists. Once the self-supplying closed system of hunter-gatherers, enhanced by gift giving, gave way to the new Neolithic society, the increasing ratio between demand and supply within the Near Eastern interaction spheres gave rise to long distance connections beyond the boundaries of local tribes.

Correspondingly the term 'warfare' in my short paper was intentionally employed but did not carry the meaning of modern national warfare, or even war among city-states, as assumed by R. Bernbeck (this issue). I used this term intentionally in my short essay in order to initiate among us, scholars of the Early Neolithic period in the Near East, a discussion concerning inter-group violence during the Early Neolithic. This I did because I was under the impression that we still adopted the view, perhaps unconsciously, that Neolithic past societies were peopled by 'noble savages'. The "pacification of the past" (LeBlanc, this issue) had to be tackled, a view also shared by Müller-Neuhof (this issue). For this reason, I felt that a short note in "Neo-Lithics' would be the appropriate forum. The decision of the editors of 'Neo-Lithics' to open up this topic to wider discussion brought, not surprisingly, a large number of commentaries, and I thank all the contributors from whom I learned so much more about this subject.

One of the repeated issues in archaeology is the question which we ask ourselves over and over when we give our interpretations of the archaeological remains uncovered in the field and/or analyzed in various laboratories: How do we know what we know?

Several statements that we make (including my own) are not easily demonstrable in archaeology. It may suffice to mention the example of sedentism, which probably played an important role in the evolution of Levantine Neolithic societies. It is not just on the basis of excavated houses and storage facilities that we can claim most or all-year round sedentism, but we also need to provide biological and botanical indicators, such as the presence of commensals, plants collected through many months, as well as the aforementioned archaeological markers (houses, storage facilities, etc.), and still doubt can sometimes remain. Therefore, the proposal to see villages temporarily (seasonally) abandoned by most inhabitants is a valid model. A recent ethnographic example would be the stone built Arab villages located along the edges of the hills bordering the southern Levantine coastal plain that during the 19th century, and probably before, were mostly deserted during the summer season in favor of mid-coastal plain agricultural activities and a series of sheds or adobe buildings.

Sedentism, is a cyclical phenomenon, and is related to a set of factors such as subsistence strategy, areas of exploitable resources, need for group security, relative increase in population, control over territory, and presence of neighbors (Roksandic, Gebel, this issue). The emergence of more or less fully sedentary communities in the Near East occurred at a time when cultivation as the basic subsistence strategy was implemented. Hence, while we see the early expressions of sedentism among Early Natufian communities, a major site such as Eynan ('Ain Mallaha) documents

several long term abandonments that caused the natural fill of the houses by runoff moving alluvium and colluvium (unless they were filled intentionally), and the necessity of digging again in order to construct new pit-houses. Thus, the Early Neolithic (PPNA) sites, even if they were occupied for no more than several centuries, are the first archaeological expression of full sedentism. In addition, these villages document an increase in energy expenditure in the course of building semi-pit houses with mud brick walls and roofs.

Indeed, as several of the comments indicate, recording the regional population history, which includes territorial expansion and retraction (Gebel, Roscoe, Müller-Neuhof, Roksandic, LeBlanc, this issue), is of crucial importance. The increase in population was biologically enhanced through a new diet based on cereals and the full sedentism of females. One might ask whether these larger communities were the first to have more frequent intra-group violence than the Natufian, as suggested by Rollefson on the basis of recorded cases from several sites. I say, yes probably. Again, the reason might have been the territorial "packing" or "crowding" as discussed in detail by Gebel (this issue). He is right to suggest that the economic development of "marginal areas" during the PPNB alleviated temporarily (in the historical sense) the need for raids. The Mesopotamian plains opened up a vast space for agricultural system based on irrigation, and the semi-arid fringes of the Levant accommodated the newly expanding pastoral societies (Gebel, this issue). In addition we should also take into account the development of similar socio-economic systems in the Zagros hilly flanks and the intermontane valleys. Nevertheless, inter-group violence could have co-occurred, albeit that the skeletal evidence is still lacking. If wars between two groups only took place in the open steppic landscape, as suggested by Roscoe (this issue), and the corpses were left in the field, no evidence will ever be found in excavations unless elite members were 'brought-in' for burial.

The archaeology of the Early Neolithic indicates that the abandonment of villages did occur, and the question we should ask in every case is 'why'? The New Guinean evidence, as presented briefly by Roscoe, tells us that due to warfare villages were rebuilt every few generations. To be clear on this point, I do not propose to adopt the New Guinean analogy, but rather that we should learn that 'warfare' (inter-group violence) is an optional explanation. It took place within the social realm, and one may suggest that it is not triggered by soil depletion or climatic calamity, but just due to rivalry over political control, that in my view of the world is triggered by economic factors.

Storage facilities in Neolithic villages could have been one of several reasons for raiding and looting (Müller-Neuhof, this issue). The lack of such installations or their paucity in Natufian sites eliminates one of the main reasons for raids, resulting in a peaceful world where only personal rivalries ended in violent death (Grosman, this issue; Bouquentin and Bar-Yosef 2004). Therefore, it was only when social structure became more complex than the Natufian level that changes in societal behavior are expected (Rollefson, this issue). Evidence for increasing complexity and the changing relationships between Neolithic tribes could have led to the presence of captives who became slaves of a richer, stronger community (Guilaine, this issue; Bar-Yosef, in press; Rollefson, personal communication).

There is no doubt, as indicated by several comments, that death could be caused by wounds to the soft tissue and thus not recognized in the study of skeletal remains (LeBlanc, Roksandic, this issue and references therein). This only means that we have to look for other signs of injuries, causes of death, and the like. What I consider a temporary lack of evidence is sometimes filled by new data that originates from a novel scientific endeavor.

The general comment of K. Otterbein (this issue and references therein), who is well known for his writing on this subject, provides a comprehensive overview on the subject. We are generally in agreement that warfare as a group activity commenced only after cultivation and eventual domestication of cereals. Successful harvests by one group, if not related to their neighbors, may trigger the poorer community to raid the food of others. Thus, we are also in agreement that the acts of warfare augmented with the emergence of state societies.

Finally, several comments related to other general issues than the practical aspects that I wished to stress. D. Warburton took position with respect to the impact of climate on human societies in the Levant by citing the supposed "Habur Hiatus". While this is not in my field of expertise, I do feel that the fact that the 8200 cal BP Cold Event is receiving increased attention by archaeologists (such as at the meeting at the University of Leiden, March 2010) shows that more scholars in our field and related ones are finding that archaeological evidence also records the impacts of this event (see also Weninger et al. 2009). I

In addition readers may enjoy the survey of the Neolithic millennia that followed the "8200 cal BP cold event" by L. Clare. In this well-informed overview both the end of the PPNC, the demise of the Trans-Jordanian "mega-sites" and the cultural changes during the ensuing millennia raise the question of possible links to the impacts of several intervals of environmental crisis (RCC). His survey confirms how significant the geographic distribution and frequencies of precipitation are for the southern Levant, this region being more prone to successive droughts than the northern Levant. Therefore, it may have been environmental change in the southern Levant that led to the collapse of the mega-sites (Gebel, this issue), a process which was possibly accompanied by intra-group violence. On the other hand, the northern Levant benefits from the advantages of the Euphrates, the Tigris and their tributaries. The different rivers provided safe sources of water for drinking and irrigation. It is for this reason that the cultural history of the northern Levant, and its larger

area, played such a major role in the cultural history of the entire Ancient Near East.

In a few additional comments, D. Warburton suggests that we as archaeologists assume that warfare has a 'cause' rather than a 'purpose'. I am afraid I fail to see the difference. If your own harvest were to fail, and someone in a neighboring village had a full granary, and these neighbors were not your relatives and refused to share their food, you might indeed have good 'cause' to pillage their stores, this then being the 'purpose' of violence. Next, in his view we are wrong to regard climate changes (e.g. a consecutive series of droughts) as causal to economic change, which itself may lead to political upheaval. I assume that a large number of case studies from more recent history could be cited to demonstrate that environmental changes can play a major role. However, I do agree with him that Göbekli Tepe would not have been possible without elite guidance, but there is no way of supporting an elite without a large population. That this change in the overall demography of the region is a plausible scenario can probably be demonstrated if further support is found to confirm the observation that early cultivation of cereals led to the creation of the required surplus as early as 12,000 calBP, as indicated by plant remains and dates from PPNA Tel Qaramel (Willcox et al. 2009).

In sum, my proposal was not to see violence or warfare as the sole cause for the abandonment of Early Neolithic villages. I view it as one cause among many. Intra-group violence and social disagreements among rival clans from the same sedentary village can be just as much causal as, for example, endemic disease etc. I simply keep asking the question 'how do we know that our interpretation is the right one?' Without discussing all the various potential explanations I would like to repeat the request that I have made on several occasions. Can we find in the archaeological excavated deposits of our sites the evidence for the impact of climatic changes that we know from other sources such as the speleothems? Indeed, the same can be asked with regard to other posited interpretations, such as the nature of the relationships between communities during the early Holocene or even earlier, prior to the appearance of writing systems.

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The Göbekli Tepe "Totem Pole". A First Discussion of an Autumn 2010 Discovery (PPN, Southeastern Turkey)

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During the 2009 excavation season, the head of an animal sculpture made of limestone was identified on the surface of the southern slope of the south-eastern hillock of Göbekli Tepe (Schmidt, forthcoming). All attempts made to remove this sculpture from the surface soil failed. Subsequently, a small sounding was excavated in order to remove the head and to document its context. This sounding revealed that, in actual fact, we were dealing with what appeared to be the head of a large sculpture that was set in a stone wall. In 2010 a larger part of the same area (L9-46), measuring 5.00 x 6.00 metres, was excavated in order to better understand and to document the architectural context of this find. Only now have we realized that this relatively large sculpture is reminiscent of the "totem poles" known from the northwest coast of North America. It had been set in the north-eastern wall of a rectangular room and was not visible originally due to the wall completely covering the pole.

Following the documentation of the position and the context of the find, which belongs to Layer II (EPPNB), it was removed from the wall in accordance with the rules of the General Directorate of Antiquities of Turkey. It has the remarkable length of 1.92 metres (Fig. 1a-c) with an average diameter of 30 cm. Its weight, which as yet could be not determined exactly, must lie in excess of 500 kilograms, as even 10 workmen had serious problems lifting and carrying it.

The pole features three main motives, one above another. The uppermost motive depicts a predator, probably a bear or a large felid – a lion or a leopard – due to two preserved features of the head: the ears and the eyes. The frontal part of the head had been obliterated in antiquity; the surface of the break is covered with a thin limestone coating. Below the head, a short neck, arms and hands are visible. Their human like shape is remarkable. Although we might postulate that this depicts a "Mischwesen", such as the "Löwenmensch" from the Aurignacian site of Hohlestein Stadel in Southwest Germany, we still cannot eliminate the possibility that these features were intended to depict animal arms and legs and not human limbs.

The arms (or legs) are holding another head, which again lost its face in antiquity. Significantly, the motive of a wild beast holding a human head is well known from several sculptures from Nevalı Çori and Göbekli Tepe (Schmidt in press). For this reason it is very probable that the lost face of the head being held by the "Löwenmensch" (or bear/ lion/ leopard) was that of a human. This suggestion is further strengthened by the fact that human arms are depicted below the head. The hands are placed opposite one another and on the stomach of the individual. This is a manner which is clearly reminiscent of the T-shaped pillars.

Below the arms and hands a second person is visible. Fortunately, the face of this individual is completely preserved. In comparison to the first human, the head of the second person is relatively small. Also depicted is the upper part of the body, including the arms and hands. Below the hands there is an unidentified object. It seems likely that the person is depicted giving birth, albeit that a very different explanation is also conceivable, *e.g.* the person could be presenting his phallus.

Below the arms of the predator (or "Löwenmensch") at both sides of the pole, large snakes are visible. Their large heads (one is partly damaged) are situated just above the head of the small individual. Below the heads of the snakes, structures are visible which might be interpreted as the legs of the uppermost human.

It seems obvious that such a piece made of stone must also have had parallels in wood which have failed to survive the millennia. However, it should be noted that fragments of a quite similar totem pole-like object made of limestone were already discovered some 20 years ago in Nevalı Çori (Fig. 2; comp. Hauptmann 1991/1992, 1993; Hauptmann and Schmidt 2007 Kat. Nr. 101; Schmidt, in press, Fig. 16 and 17). This object was found in the Terrazzo Building in an EPPNB context; the pole was broken in several pieces and buried in the north-eastern bench of the building. Consequently, the pole itself could be of much older date, in fact it could even date to the PPNA period. The same may be true for the recent find from Göbekli Tepe, which had been invisible behind a wall. A detailed study of these remarkable objects and their contexts will be published elsewhere.

Acknowledgements: We would like to express our gratidute to the Ministry of Culture and Tourism of the Republic of Turkey for the kind permission to excavate the important site of Göbekli Tepe. We are very grateful to the representatives of the general directorate of Antiquities in 2010, Gülsüm Yaprak, Nedim Dervişoğlu and Aynur Talaakar, for their support of the project, and to Dr. Çihat Kürkçüoğlu for the close contact with to the general directorate beside his scientific work. We are also indebted to all other team members and the workmen of the season for their efforts. The project - directed by the German Archaeological Institute - is funded by the German Research Foundation (DFG) and supported by ArchaeNova e.V.

Fig. 1 Sculptured stone pole from Göbekli Tepe Area L9-46. ***a*** front part <GT10-_9532>, ***b*** left side <GT10-_9577>, ***c*** right side <GT10-_9650>. (photos: N. Becker, DAI)

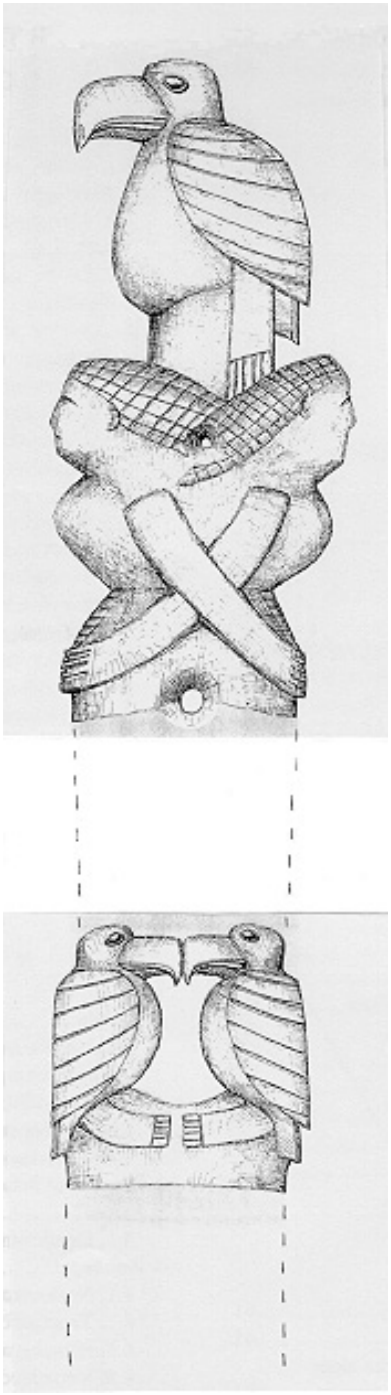

Fig. 2 The “totem pole” from Nevalı Çori (reconstruction and drawing: K. Schmidt)

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Current Neolithic Research in Armenia

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If the cultures that developed in the centre of the Southern Caucasus, of which Armenia is part, are compared to those of the northern Near East or the neighbouring regions bordering the Black Sea and the Caspian Sea, it is clear that there is a large gap in our knowledge of the beginnings of Neolithisation. Indeed, in the basin of the Kura, in Georgia and Azerbaijan, it is only at the beginning of the 6th millennium calBC that a culture appeared (the Shulaveri-Shomutepe culture) that possessed an advanced mastery of the domestication of plants and animals (Kushnareva 1997; Kiguradze and Menadbe 2004), whereas in the basin of the Arax the culture of Kültepe of Nakhichevan developed from the 2nd half of the 6th millennium cal. BC (Munchaev 1982; Narimanov 1987) (Fig. 1).

In Armenia, where ten years ago the Neolithic period remained very poorly known, the collaboration between the Institute of Archaeology of Yerevan and the French "Caucasus" mission enabled the discovery of two different cultures: a Mesolithic/ Early Neolithic culture on the eastern flank of the Aragats mountains (Kmlo-2 rock shelter) and a local variant of the Shulaveri-Shomutepe culture in the Ararat plain (Aratashen and Aknashen-Khatunarkh)[1].

Fig. 1 Main Neolitic sites mentioned in the text.

The Mesolithic / Early Neolithic of Kmlo-2

The Kmlo-2 rock shelter (Arimura *et al.* 2010), cut into the basaltic flows of the Aragats mountain carved by the Kasakh River (Fig. 2), was occupied during the prehistoric period by small human groups that hunted ibex, mouflons and deer. Remains of Caprinae have been found in the upper horizons of the prehistoric layer, but the wild or domestic status of the highly fragmented bones is difficult to determine. Only wild plant remains were found in this layer. The dating of Kmlo-2 is a difficult issue (Arimura *et al.* 2010), but excavations in 2009 and additional ^{14}C dating indicate that the site was occupied in three different phases, 11th-10th millennia, 9th-8th millennia and 6th-5th millennia calBC.

The inhabitants of Kmlo-2 produced their tools from obsidian pebbles washed down by the Kasakh River from outcrops situated near its source (Tsaghkunyats range), as well as from larger blocks which they brought from deposits that were one to three days distant by foot (Gutansar, Hatis, Arteni, Geghasar) (Fig. 3). The numerous debitage products, which represent 90% of the lithics, provide evidence for making tools on the spot. There is a large number of microliths (30%), including geometric pieces such as lunates and

trapeze-rectangles that probably served as barbs for arrows.

The most interesting objects for the study of relations with the neighbouring regions are obsidian tools with continuous and parallel retouch on one or both lateral edges, clearly executed by pressure flaking technique. These artefacts, original for Armenia and called "Kmlo tools", are similar to obsidian tools found on sites of the 8th-7th millennia calBC in southeastern Anatolia and northern Mesopotamia (Çayönü, Cafer Höyük, Shimshara, etc) and called "Çayönü tools" or "Çayönü rods" or "Beaked blades" (Redman 1982; Fuji 1988; Caneva *et al.* 1994; Mortensen 1970) (Fig. 4).

A use-wear analysis, carried out by L. Astruc (Arimura *et al.* 2006) on "Çayönü tools" and "Kmlo tools", shows some differences between the two groups of artefacts. Although the retouch seems to be similar, the blanks on which they are made, the retouching technique, the wear traces, and the methods of rejuvenation are different. According to the use-wear analysis, no direct relationship can be established between "Kmlo tools" and "Çayönü tools". Moreover, the geochemical analysis of 20 "Kmlo tools" has confirmed that all were made locally on obsidian from Armenian deposits (Tsaghkunyats, Arteni, Gutansar, Hatis, Geghasar) and that there was no import of artefacts or raw material from the northern Near East.

In Georgia, similar tools, called "hooked tools", characterise a culture attributed to the early Neolithic, the Paluri-Nagutnyj culture, that developed on the southwestern slopes of the Greater Caucasus (Grigolija 1977). Similar tools are also found on the high plateaus of southern Georgia ("Paravani group"), where the large obsidian deposit of Chikiani was exploited (Kiguradze and Menadbe 2004: 353-357). Most of these Georgian Early Neolithic sites are found at altitude, several are rock shelters, and all have produced only one level of occupation; unfortunately, none has yet been dated by 14C.

The chronological attribution of the "Kmlo culture", characterized by the presence of "Kmlo tools", has been recently clarified by 14C dating. The horizon in which the "Kmlo tools" appear has been dated to the first half of the 9th millennium calBC; these artefacts are numerous in the overlying horizons dated to the end of the 9th and to the 8th millennium calBC. They seem to have continued in the upper strata of the 6th-5th millennia calBC. This late date for the use of "Kmlo tools" is confirmed by the discovery of similar artefacts on other sites of the region, including the hunter's camp at

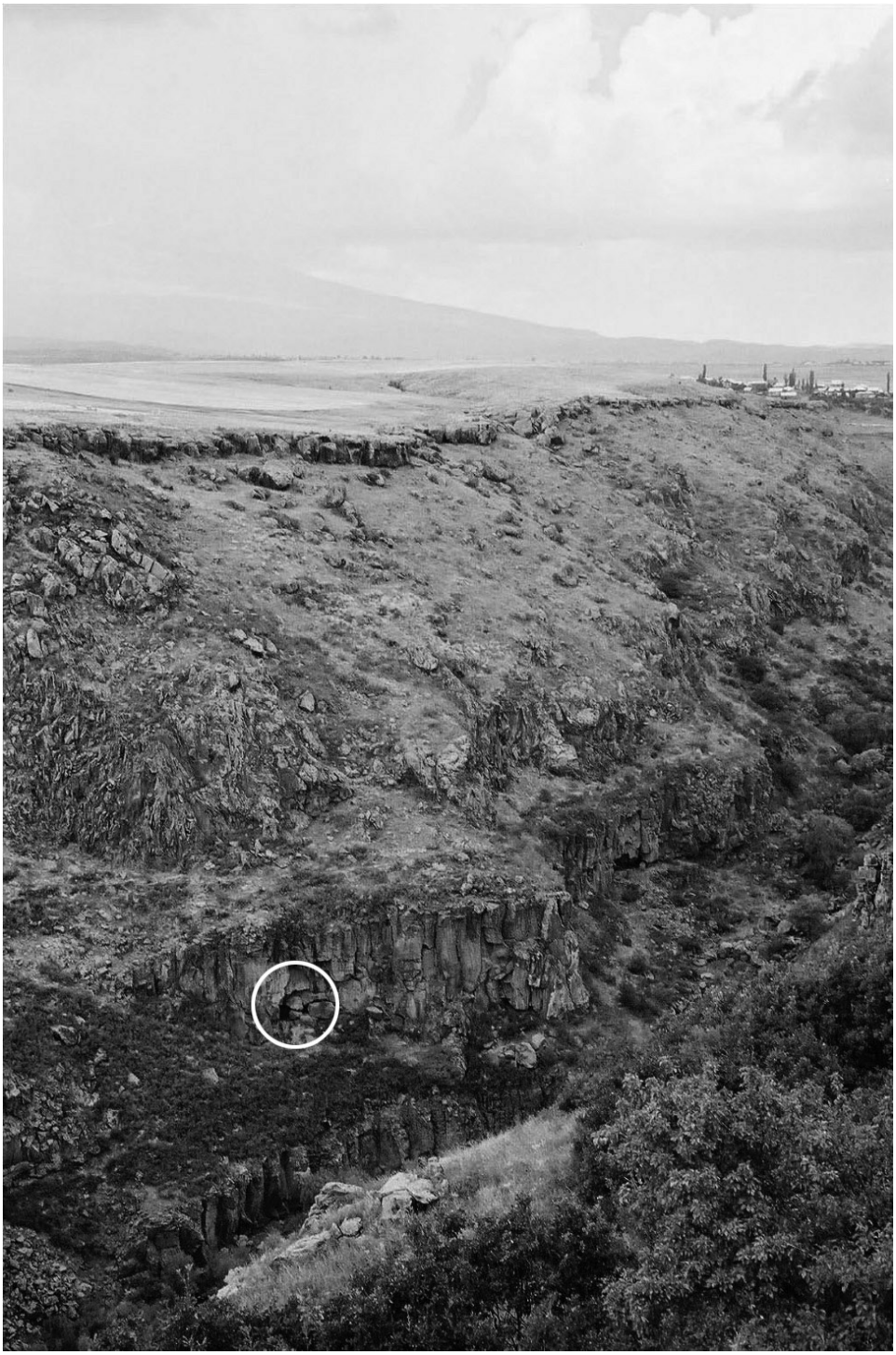

Fig. 2 Kmlo-2 rock shelter in the canyon of the Kasakh river.

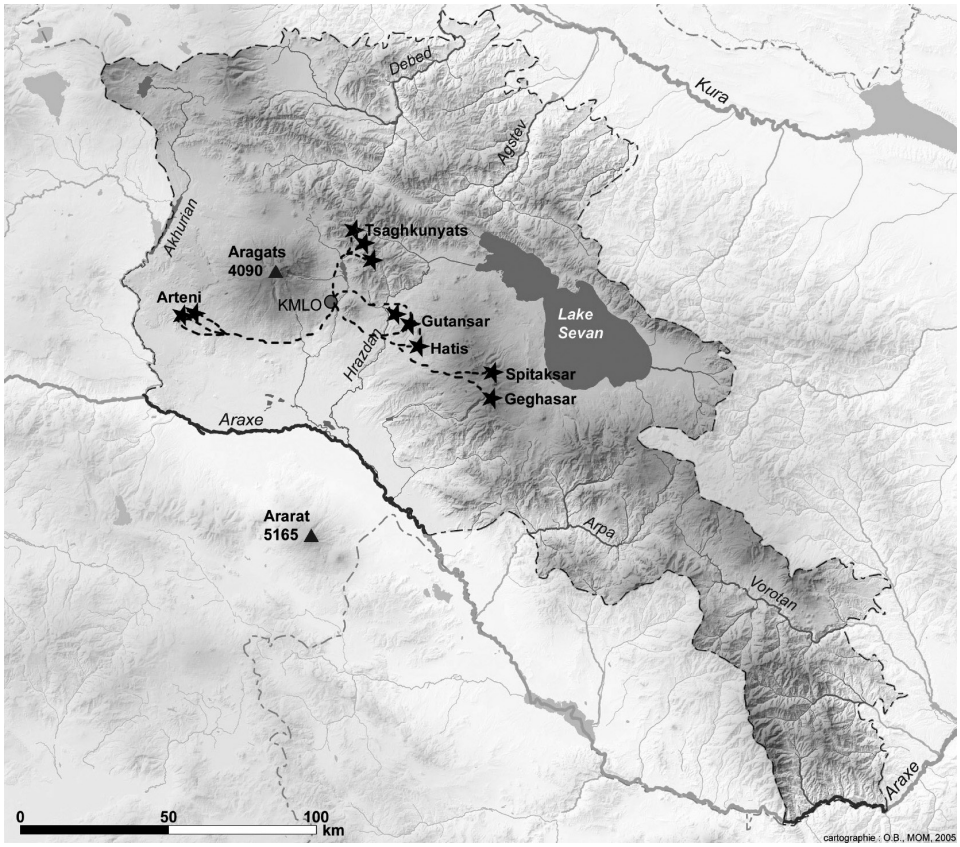

Fig. 3 Obsidian procurement of the Kmlo-2 inhabitants

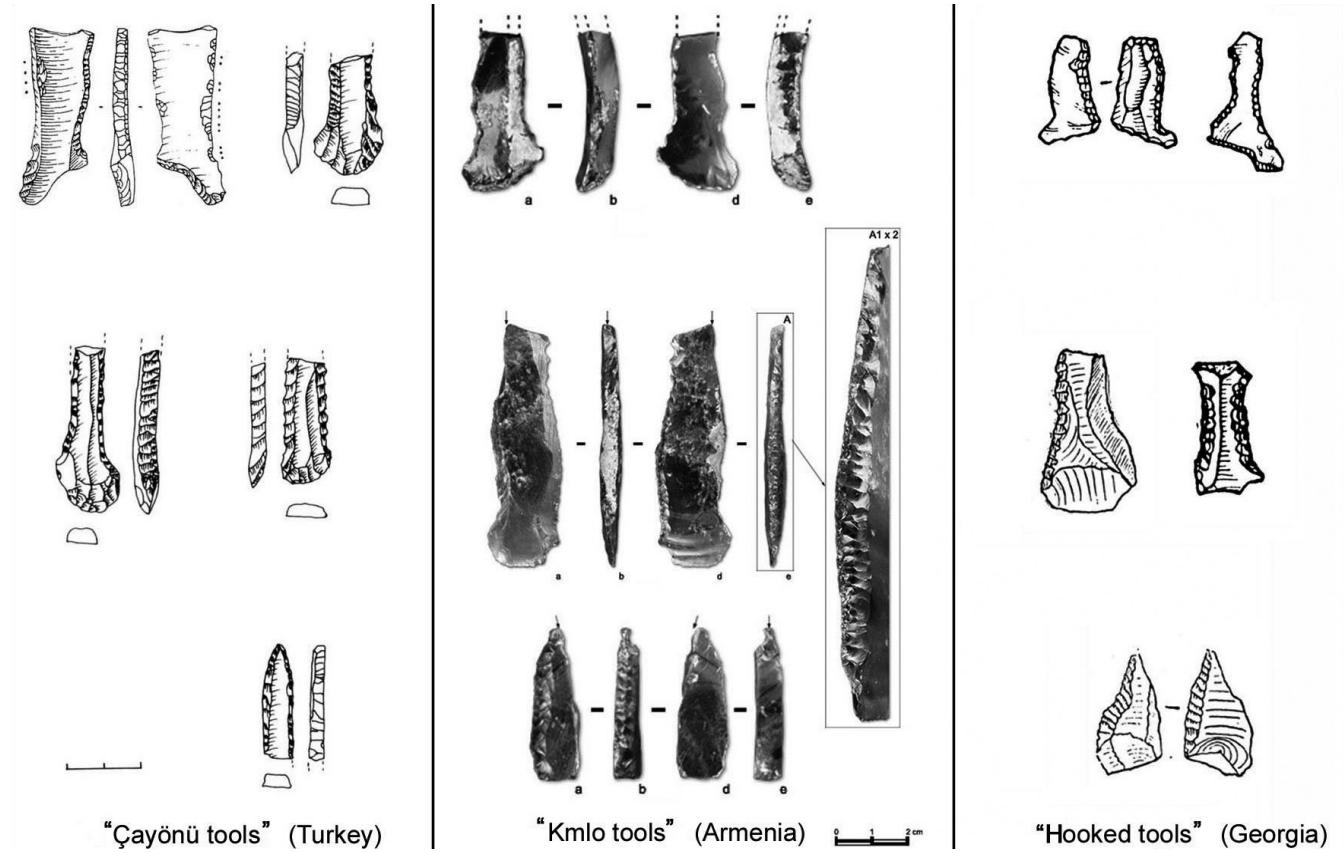

Fig. 4 Tools with an abrupt, regular, sub-parallel retouch.

Tsaghkahovit established on the northern flank of the Aragats in the 2nd half of the 5th millennium calBC (Arimura *et al.* n.d.).

The "Kmlo tools" thus appear to be one of the indicators of a culture established in the 9th millennium calBC on the high plateaus of western Armenia. It is possible that this culture developed locally and continued at least until the 6th-5th millennia calBC. At this time, a quite different culture appeared in the Ararat plain.

The Late Neolithic of the Ararat Plain

The Late Neolithic sites of Aratashen and Aknashen-Khatunarkh are located in the lower valley of the Kasakh River, which meanders in the Ararat plain before flowing into the Arax River. Aratashen, which has been excavated from 1999 to 2004, is a small elliptical elevation of about 60 m in diameter consisting of two Neolithic levels lying on the sandy virgin soil. At the periphery of the elevation, unstratified material has been found; this material, which consists mainly of Chalcolithic pottery and obsidian artifacts, comes probably from the upper part of the mound, destroyed by erosion over millennia and by modern levelling works (Badalyan *et al.* 2004a; 2007). As the stratigraphy of Aratashen revealed a gap between the Neolithic and Chalcolithic levels, it was decided to excavate another site, in order to fill this gap.

Fig. 5 Architecture of the lowest levels of Aratashen.

The site of Aknashen-Khatunarkh, located 6 km southeast of Aratashen, was partly excavated by R. Torosyan in the 1970s and 1980s; but the results of his work, carried out in the west sector of the hill, were not published. The new excavations by the Armeno-French mission began in 2004 and are still in progress (Badalyan *et al.* n.d.). The site of Aknashen-Khatunarkh is a mound circular in plan (about 100 m in diameter), with a flat top rising 3.5 m above the surrounding plain. So far the most complete stratigraphic sequence has been found in trench A. There, the cultural layer is more than 4 m thick and continues farther down, but the high level of the water table did not permit further excavation. The preliminary typological analysis of the material, mainly pottery, has enabled attribution of the lower horizons (V-II) to the Late Neolithic and the upper horizon (I) to the Early Chalcolithic. It seems that at the present stage of investigations there is no significant hiatus in this stratigraphic sequence.

The corpus of 14C dates shows overall concordance between Aknashen-Khatunarkh and Aratashen: the earliest levels (lowest strata of horizon V at Aknashen-Khatunarkh and horizon IId at Aratashen) belong to the very beginning of the 6th millennium calBC. At Aknashen-Khatunarkh, the upper Neolithic level (horizon II) covers the last centuries of the 6th millennium calBC; therefore the Chalcolithic level (horizon I), disturbed by medieval and modern intrusions, would belong to the first half of the 5th millennium calBC.

The inhabitants of these settlements were farmers (naked wheat, emmer, six-row barley, and lentil) and herders (sheep, goats, cattle and rare pigs). Constructions, circular in plan with diameters from 3 to 5 m, were built in pisé or, more rarely, in mud bricks. There is a high concentration of small structures within or outside the constructions; they were generally used as silos (to stock grain or sometimes tools) or as ovens (Fig. 5).

The obsidian tools are quite different from those of Kmlo-2; they are mainly on blades, produced by indirect percussion or by pressure flaking technique with crutch as well as with levers (Chabot and Pelegrin n.d.), a technique that appeared in the northern Near East at about the end of the 8th millennium cal. BC (Çayönü, late Pre-Pottery Neolithic) (Altinbelek *et al.* n.d.).

The lower Neolithic levels at Aratashen and Aknashen-Khatunarkh have produced an abundance of objects made of bone, horn and deer antler. The main types consist of awls, spatulas, "hoes", arrowheads, spoons, wide palettes and tubular casings. In the upper levels, a sharp decline in the quantity and variety of the bone industry can be observed: more than 80% of the bone artifacts are awls.

Some bone arrowheads have been found close to stones which present on their rounded upper part 1 to 3 wide transverse grooves in a U-shape section. Grooved stones are known in the Near East from the 11th millennium calBC onward, and two regional variants can be distinguished: in the Levant and western Mesopotamia, the groove follows generally the longitudinal axis of the tool, whereas in northeastern Mesopotamia and the Zagros (Zawi Che-

mi, Karim Shahir, Jarmo, *etc.*), they follow more often the transverse axis (Solecki 1981; Howe 1983; Moholy-Nagy 1983). The grooved stones of Armenia could be compared to this latter variant (Fig. 6).

Pottery is totally missing from the lowest levels of both sites; at present it is clear that the earliest sedentary communities in the Ararat plain did not use pottery. Later, coarse wares with mineral or mixed temper appear; chaff-tempered ware develops then, but remains rare in the Neolithic horizons. These potteries show reddish-brown to gray-black color; in some cases, they are decorated with applied elements such as simple knobs. There are in addition some rare sherds of fine painted ware, probably imported from northern Mesopotamia. Sherds similar to Samarran or Early Halaf wares were found at Aknashen-Khatunarkh in horizon V (Badalyan *et al.* n.d.), others with motifs characteristic of Middle/Late Halaf pottery were found at Aratashen in horizon IIb (Palumbi 2007).

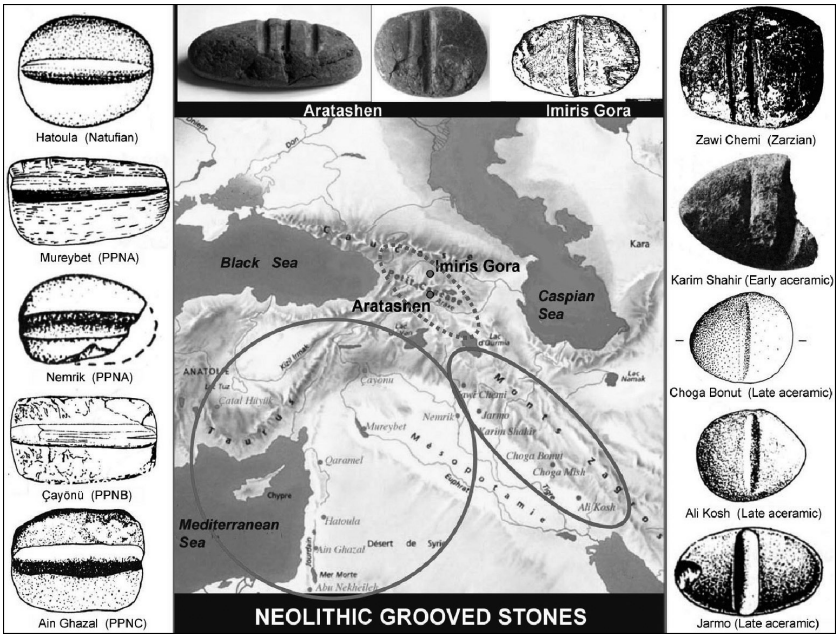

Fig. 6 Distribution of sites yielding grooved stones with longitudinal and transversal grooves.

At Aknashen-Khatunarkh, in the Chalcolithic horizon, chaff-tempered ware makes up the bulk of the pottery and is characterized by a combed treatment of the surface (a haphazardly executed series of incised lines over the body of the vessel) and by new decorations: a horizontal row of perforations below the rim, undulated rim, and notches on the rim. These features are characteristic of the pottery of the Early Sioni culture, which developed in the Kura Basin after the disappearance of the Shulaveri-Shomutepe culture (Kiguradze and Sagona 2003).

The Late Neolithic culture represented on these two sites in the plain of Ararat is closely related to the Shulaveri-Shomutepe culture that developed in the same period (6th millennium calBC) farther north in the Kura Basin. Both cultures have many points in common: in architecture, in lithic and bone industries, and in pottery.

At the site of Aknashen-Khatunarkh, which presents a stratigraphic sequence covering the phases of the Late Neolithic and the Early Chalcolithic, two factors stand out: a) change is completely progressive; b) there are important differences between the earliest and latest levels, indicating an evolution in the way of life. The first phase, with architecture in pisé and objects characteristic of the Shulaveri-Shomutepe culture, indicates a sedentary economy. The last phase is characterized by abandonment of constructed architecture, the rarity of groundstone tools, and the decline of bone and lithic industries. All these features, which are characteristic of the Sioni culture in Georgia, suggest a change in the economy towards more mobility.

Discussion

In order to better understand the Neolithisation process in Armenia, two topics are discussed here: a) the hypothesis that the search for obsidian, which is abundant in this country, led to the establishment of trade networks between this region and Mesopotamia; b) the role of the southern Caucasus in the emergence of hexaploid wheat culture in the Near East.

Obsidian Procurement

More than 20 sources of obsidian are scattered across the southern Caucasus, mainly in Armenia, but also in southern Georgia and southwestern Azerbaijan. The systematic characterization of the Caucasian sources was achieved through geochemical analyses and fission-track dating and this geological data served as a base for determining the origins of an important corpus of artefacts from sites dating to between the 6th to the 1st millennia calBC (Blackman *et al.* 1998; Badalyan *et al.* 2001, 2004b). These results were compared with the database for obsidian in the Near East.

These analyses have shown (Fig. 7) that the obsidian from the southern Caucasus was widely used in the basins of the Kura and the Arax Rivers, up to the shores of the Black Sea and the Caspian Sea. But it hardly circulated beyond the mountain ranges that border this region in the north (Greater Caucasus) and in the south (Anti-Taurus). Only a group of sources located in the upper basin of the Vorotan River (Satanakar, Sevkar, Bazenk) was exploited beginning in the 6th millennium calBC by populations settled in the basin of Lake Urmiah (northwestern Iran).

On the other hand, the Anti-Taurus possesses several deposits of obsidian that were largely exploited

during the Neolithic and Chalcolithic periods: a) the Bingöl and Nemrut Dag sources, which spread widely throughout the Fertile Crescent, but not to the north; b) the Meydan Dag deposit north of Lake Van, which had a broad diffusion in Northern Mesopotamia and is represented in the southern Caucasus only occasionally; c) the Erzurum region, whose populations exploited only the local obsidian. In fact, the obsidian sources located in the Lake Van and Erzurum regions represent less than 1% of the provenances of all the southern Caucasian archaeological samples analysed (Badalyan *et al.* 2004b). The near-absence of diffusion of obsidian from the northern Near East towards the southern Caucasus and from this region towards the south is noticeable and suggests that the obsidian exchange networks elaborated by the Mesopotamian populations did not play an important role in the process of Neolithisation of the southern Caucasus.

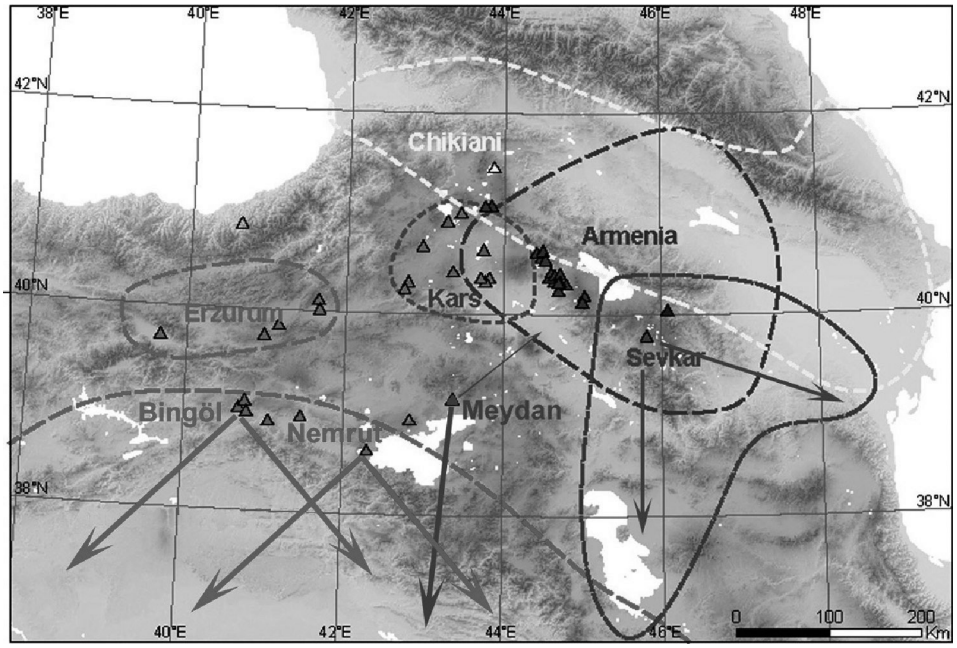

Fig. 7 Obsidian procurement in the northern Near East and the southern Caucasus.

Emergence of Exaploid Wheats

The assortment of cereals found on the Armenian sites of the 6th millennium calBC (Aratashen and Aknashen-Khatunarkh) is characterized by the abundance of naked wheat, whose species, Triticum turgidum (tetraploid) or Triticum aestivum (hexaploid), is difficult to determine (Badalyan *et al.* 2007; Hovsepyan and Willcox 2008; Badalyan *et al.* n.d.). Such a predominance of naked wheat is attested in the Kura basin in the Shulaveri-Shomutepe culture, where spelt wheat (Tr. spelta), a hulled hexaploid species, is also present (Lisitsyna and Priscepenko 1977; Janushevich 1984; Wasylikova *et al.* 1991; Zohary and Hopf 2004). The first hexaploid wheats were hulled products (Tr. spelta), but the naked derivatives (Tr. aestivum) could have appeared shortly after the formation of spelt, because the shift between hulled and naked hexaploid wheat was apparently produced by only two mutations (Zohary and Hopf 2004).

In the regions situated northwest of the Black Sea, in the Bug-Dniestr culture, the spread of spelt is dated to the end of the 7th and the beginning of the 6th millennium calBC (Janushevich 1984 ; Kotova 2009). However, genetic analyses show that the spelt wheat of Europe (Moldavia, northern Black Sea) and those of Asia (Caucasus, Iran, Afghanistan) do not have the same origin: European spelt wheat originated from hybridization between cultivated emmer (Tr. dicoccum) and club wheat (Tr. compactum), whereas Asian spelt wheat originated from hybridisation of tetraploid wheat (Tr. turgidum) with the diploid wild grass Aegilops tauschii (= squarrosa) (Dvorak *et al.* 1998; Yan *et al.* 2003; Dedkova *et al.* 2004).

In particular, molecular studies have revealed that populations of Aegilops tauschii native to Armenia and the southwestern part of the Caspian Sea belt are closest to genome D found in the hexaploid wheat (Dvorak *et al.* 1998). Thus, a hypothesis defined in the nineties (Nesbitt and Samuel 1996; Zohary and Hopf 2004) was largely confirmed by genetic studies (Lelley *et al.* 2000; Giles and Brown 2006; Kilian 2009): the most likely origin of the hexaploid bread wheat is the southwestern corner of the Caspian belt and the adjacent southern Caucasus. The hybridisation is generally considered to have taken place between 6000 and 5000 BC; however, as the recent excavations at Aknashen-Khatunarkh have shown that hexaploid naked wheat was already present as main cultivated crop at the very beginning of the 6th millennium calBC (Badalyan *et al.* n.d.), we must consider now that the hybridisation may have taken place earlier, in the 7th or even the 8th millennium calBC.

This domestication must be distinguished from the appearance of hexaploid naked wheat in the Middle PPNB (first half of the 8th millennium calBC) in southeastern Anatolia and northern Syria (Abu Hureyra 2B, Cafer Höyük, Halula, *etc.*) (Nesbitt 2002). A recent genetic analysis suggests that, in the Near East, there were at least two Aegilops tauschii sources that contributed germplasm to the D genome of Triticum aestivum (Giles *et al.* 2006), one giving rise to the lineage possessing the TAE1 allele and its derivatives, and the other giving rise to the lineage with TAE2 allele. The first hybridisation probably occurred at the beginning

of the 8th millennium calBC in southeastern Turkey and northern Syria, where local Aegilops tauschii has a high frequency in TAE2 allele; the second, more recent, hybridisation occurred in the southern Caucasus and in the southwest corner of the Caspian belt, where TAE1 is common (Giles *et al.* 2006).

This second domestication could have occurred among small population groups that came from the eastern Near East at a point in time when pottery was still unknown (until the beginning of the 7th millennium calBC), which would explain the absence of pottery in the earliest phase of the Shulaveri-Shomutepe culture. Then these groups could have evolved locally or become mixed with local populations. Such a "cultural diffusion model" would explain too the spread of agriculture in Europe during the Neolithic period (Morelli *et al.* 2010).

Conclusion

Current Neolithic research in Armenia has brought to light two different cultures: a) a Mesolithic/Early Neolithic culture with a microlithic industry (Kmlo-2 rock shelter) on the high plateaus of western Armenia; this culture evolved locally until the 5th millennium calBC (persistence of the "Kmlo tools" in this region); b) a Late Neolithic culture (Aratashen and Aknashen-Khatunarkh) in the Ararat plain, which constitutes a southern variant of the Shulaveri-Shomutepe culture, widespread in the Kura basin during the 6th millennium calBC.

From several cultural elements (farming, herding, debitage by pressure flaking with lever, imported Mesopotamian pottery, *etc.*), we can infer links between the Shulaveri-Shomutepe culture and the Near Eastern Neolithic cultures. However, other elements of the Shulaveri-Shomutepe culture (circular architecture, absence of pottery in the lowest levels, abundance of naked wheat, *etc.*) indicate its originality. Therefore, the origin of this culture could be due to contacts between Near Eastern farmers and local populations in the southwestern area of the Caspian Sea at the end of the 8th or beginning of the 7th millennia calBC.

Whatever the theory on the advent of agriculture in the southern Caucasus, the sites of this region where cereal crops such as spelt and bread wheat developed, remain to be discovered. Thus research must continue in order to discover sites prior to Aratashen and Aknashen-Khatunarkh and to better understand the populations of Armenia in the early Holocene.

Notes

1 The excavations at Kmlo (resp. M. Arimura) and at Aratashen and Aknashen-Khatunarkh (resp. R. Badalyan) were funded by the French Ministry of Foreign Affairs, the National Center for Scientific Research (C.N.R.S.) and the National Academy of Sciences of Armenia.

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TBAS 102: A Late Natufian Site in West-Central Jordan

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Introduction

Test excavations were carried in 2006 at TBAS 102, a Late Natufian site along the Wadi al-Qusayr in west-central Jordan. This fieldwork represents the initial investigation of this site and was carried out from May 21 to June 1. The excavation was part of a larger project designed to examine the transition from foraging to farming in the Wadi al-Hasa catchment system (Neeley and Peterson 2007). This included testing at both Natufian and Pre-Pottery Neolithic sites in the region (*e.g.*, Peterson 2007). The 2006 excavations as TBAS 102 yielded several interesting and significant findings. First, TBAS 102 represents the first radiometrically dated Late Natufian occupation in west-central Jordan. Second, the site is associated with marsh/wetland environments that have been key attractors for Late Pleistocene settlement elsewhere in west-central Jordan. And third, the presence of a Late Natufian occupation might facilitate a fuller understanding of the impact of the Younger Dryas in the steppic regions of west-central Jordan.

Site Location and Context

TBAS 102 is situated in the arid steppe/desert region of west-central Jordan, approximately 7 km south of the modern town of Jurf ed-Darawish (Fig. 1). The site is located along the south side of the Wadi al-Qusayr, a minor drainage just north of the Wadi al-Juhayra, which flows north to the Wadi al-Jurf. The last wadi continues northward before emptying into the eastern end of the Wadi al-Hasa, the largest east-west flowing drainage system in the region. The Wadi al-Qusayr is shallow in profile suggesting sporadic, low energy water flows. The north side of the Wadi al-Qusayr is bounded by the basalt flow originating from Tell Juhayra to the west.

TBAS 102 is one of ten Late Epipaleolithic/Natufian sites recorded along a short stretch of the Wadi al-Qusayr during a survey in 1999-2000 (MacDonald *et al.* 2004). The site area is small, measuring 15 x 20 m, and slopes gently from south to north. The northern boundary of the site is defined by a 17 m long irregular alignment of stones. The site was selected for excavation based on a high density of surface materials containing lunates and bladelet cores. In addition, potentially intrusive elements (*e.g.*, ceramics) were absent from the surface.

The cluster of sites in the Wadi al-Qusayr provides an interesting point of comparison with the much larger Wadi al-Hasa, where only three Natufian sites have been identified from the more than 1600 sites recorded there. The scarcity of Natufian sites in the Wadi al-Hasa might be due to geomorphic activities that limit site preservation or be a reflection of prehistoric behavioral patterns in which the Wadi al-Hasa was sparsely settled during this period. In contrast, the preservation of this cluster of Natufian remains in the Wadi al-Qusayr suggests repeated if not intensive occupation of this area at the end of the Pleistocene.

An increasingly key element in the pattern of settlement for west-central Jordan appears to be the presence of marl deposits. Marl deposits are found from the Wadi al-Qusayr north to Jurf ed-Darawish and in association with the marls are numerous Paleolithic sites (MacDonald *et al.* 2004; Moumani *et al.* 2003). These marl deposits were probably the result of shallow bodies of water more amenable to marsh or wetland environments rather than lakes. As such, they served as an attraction for humans and animals alike. A key ques-

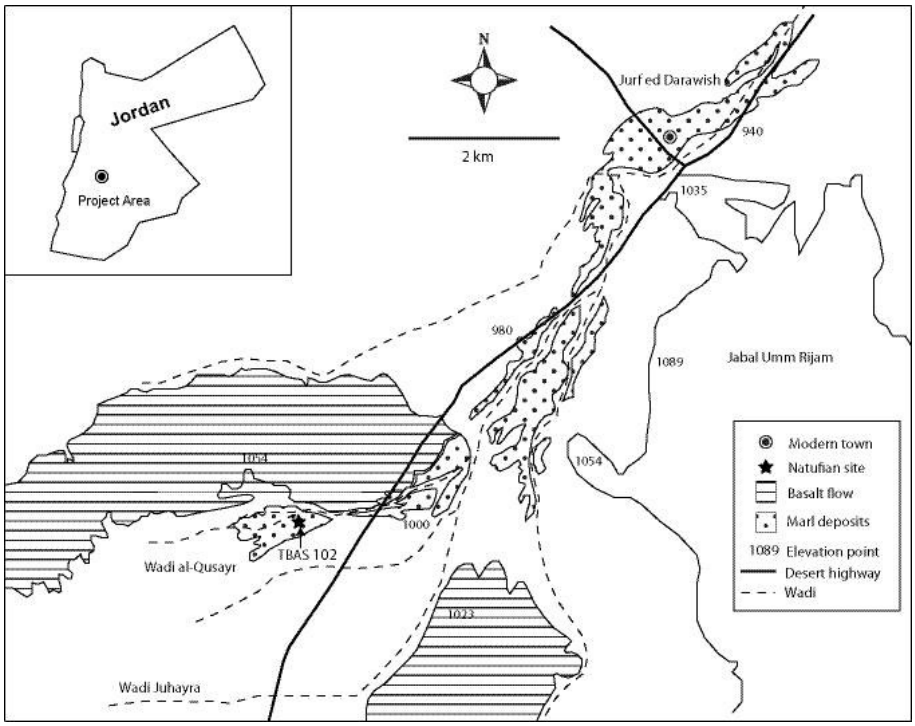

Fig. 1 Location of TBAS 102 in west-central Jordan

tion regarding these wetlands is when did they disappear? It is generally recognized that climatic changes associated with the Younger Dryas (12,900-11,600 cal BP) had a major impact on Natufian resource availability. Was this impact uniform over the southern Levant or might some areas have been only minimally affected by this change? The presence of a Late Natufian occupation in the Wadi al-Qusayr is intriguing as it might indicate that this marsh/wetland environment continued to be productive for Late Natufian populations in the region.

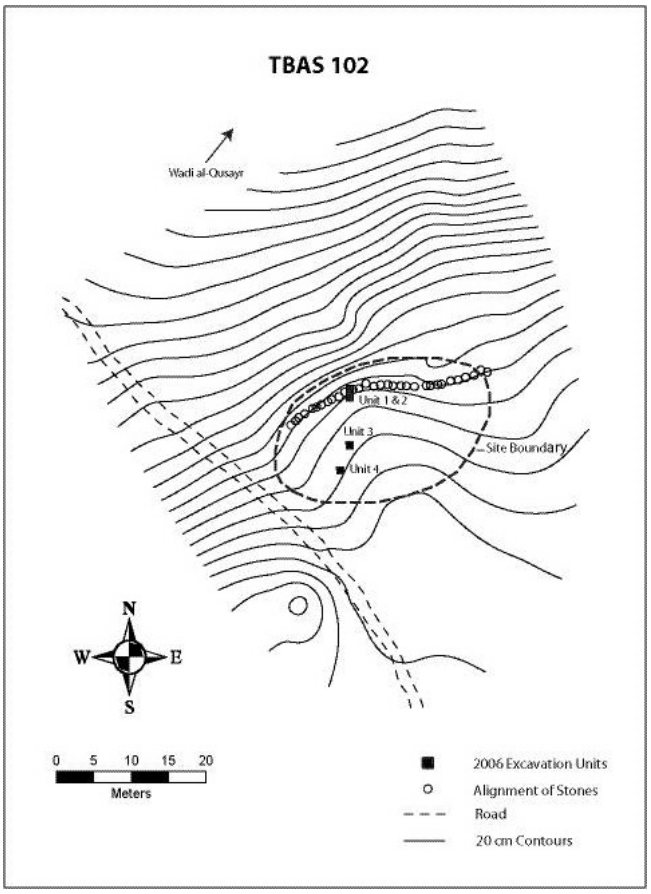

Fig. 2 Topographic map of TBAS 102 (prepared by Brett Hill)

Excavation

During the 2006 field season, 20 m^2 of the site were surface collected. The surface density of materials was used to select two of the four 1x1 m excavation units (Fig. 2). Units 1 and 2 bisected the irregular stone alignment on the north side of the site. These units were selected with the goal of defining and understanding the function of the alignment and its relationship to the Natufian occupation. Also, with the stone alignment running perpendicular to the slope, this raised the question of whether sediment deposition was significantly greater on the upslope side than the down slope side. The depth of archaeological materials and sediments in Units 1 and 2 was relatively shallow (25-30 cm) compared to the other areas of the site. Most of the cultural materials could be found in the upper level of each unit along with a high density of fist-sized cobbles. The stone alignment appears to consist of two courses of unevenly spaced stones with the larger stones on top nearer the surface (Fig. 3). There was no evidence of wall segments joining the surface alignment to form smaller divisions of space, nor did the alignment curve or come together as an enclosure. In terms of sediment deposition, there was little difference between the upslope and downslope sides of the alignment, suggesting surface erosion was minimal. The best guess as to its function is that of a windbreak, as some organic materials could have been placed between the stones, but even that is problematic since the alignment is down slope relative to the rest of the site and would provide limited protection from the wind. Alternatively, it is possible that the stone alignment post-dates the Natufian occupation. Although ceramics are absent, potential support for this interpretation comes from the differences in raw material use in Units 1 and 2 relative to the central area.

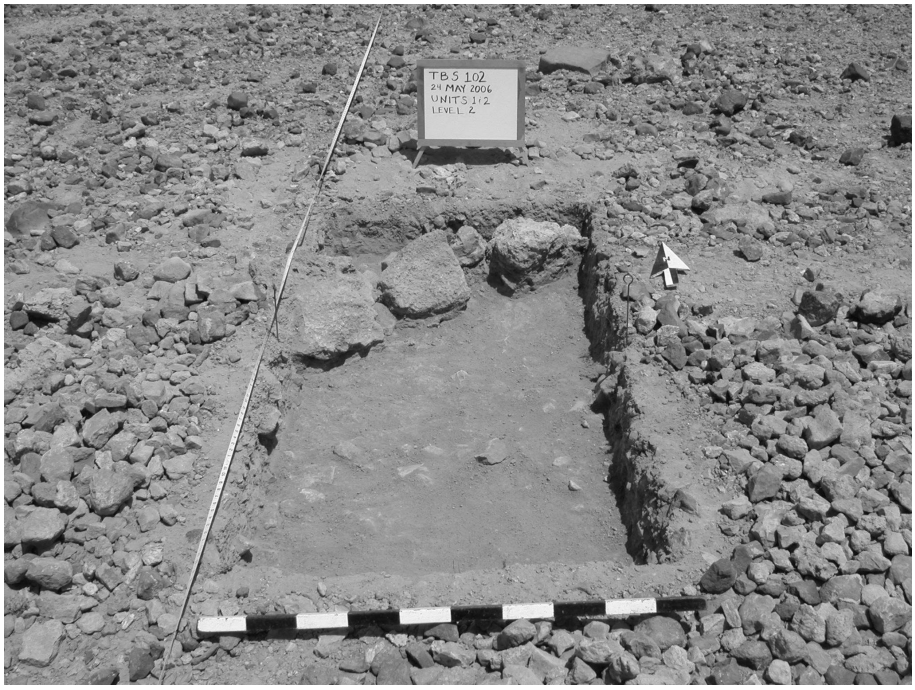

Fig. 3 Stone alignment feature in Units 1 and 2. View to the north

Units 3 and 4 were excavated in the center of the site where some of the highest density of surface remains occurred. In both of these units cultural materials were recovered to a depth of 35-40 cm below the ground surface. The upper levels tended to have the highest artifact densities along with greater frequencies of fist-

sized (and smaller) limestone rocks. The latter might represent the deflation of the ancient surface that has resulted in the stabilization of the current surface. The frequency of the cobbles decreased significantly in the lower levels of the excavation. Although formal features were absent from Units 3 and 4 and sediments tended to be very homogeneous (10YR 6/4 light yellowish brown), there was a small depression in the northwest corner of Unit 4 that contained less compacted materials along with ashy sediment associated with bone (burned and unburned) and charcoal inclusions (10YR 5/3 brown).

Chronology

The initial survey of the site indicated a Late Epipaleolithic component, but there was insufficient information from the surface remains to determine whether this represented an Early or Late Natufian component. The 2006 excavations indicate that TBAS 102 was a Late Natufian site based on two lines of evidence. First, two radiocarbon dates place the site in the early portion of the Late Natufian (11,500-10,800 BP uncalibrated; 13,500-12,700 cal BP) (Tab. 1). This is also close to the accepted time range for the onset of the Younger Dryas that resulted in cooler and dryer conditions. It is generally assumed that the Younger Dryas had a significant effect on Natufian settlement and subsistence strategies. The second line of evidence favoring a Late Natufian occupation at TBAS 102 comes from the stone artifacts. Lunates, the standard temporal marker of the Natufian, are all characterized by abrupt or steep retouch rather than the ridge-backed, bifacial Helwan retouch that is characteristic of the Early Natufian (Belfer-Cohen 1991).

Context	Material	Lab No.	Conventional Date BP	2 Sigma Calibrated Date BP
Unit 4 Level 2	organic	Beta-229411	11 040 ± 60 BP	13100-12860 Cal BP
Unit 3 Level 3	charcoal	Beta-221179	11 170 ± 70 BP	13410-12980 Cal BP and 12940-12910 Cal BP

Table 1 AMS Radiocarbon dates from TBAS 102.

The presence of a dated, Late Natufian occupation in west-central Jordan is important as the previously recorded or excavated Natufian sites from the Wadi al-Hasa have been assigned to the Early Natufian. This has potential ramifications for our understanding of the long-term viability of paleoenvironments in west-central Jordan and the patterns of mobility and settlement at the beginning of the Younger Dryas.

Lithics

Excavations at TBAS 102 yielded 9,870 pieces of chipped stone (Tab. 2). All aspects of the reduction sequence appear to occur on site based on the range of products recovered. This suggests that raw materials were acquired locally, given the large number of cores and the quantity of material generated from reduction activities. Although the source has not been identified, one possible location of lithic raw materials could be the chert formations found on the Jabal Umm Rijam to the east. Interestingly, the raw materials used at TBAS 102 appear to differ in size and texture from raw materials used at earlier Paleolithic sites along the Wadi al-Qusayr. This indicates the preferential selection of this raw material over raw materials that presumably were widely available in the past.

Reduction activities resulted in the greater production of flakes (58% of the debitage) over blades/bladelets (only 27% of the debitage). This emphasis on flake production is also apparent among the discarded cores where 53% of the complete cores are flake types. However, if core types are sorted by raw material (classified into fine and coarse-grained categories), the flake cores only constitute 39% of the fine-grained core types versus 82% of the coarse-grained materials. This indicates that the type of raw material has an effect on the type of core reduction activity (flake or blade/bladelet based).

Debitage type	Unit 1 &2	%	Unit 3	%	Unit 4	%	All	%[1]	%[2]
Cores	35	1.7	24	0.7	53	1.3	112	1.1	1.8
C.T.E.	16	0.8	25	0.7	25	0.6	66	0.7	1.0
Blades (complete)	37	1.8	109	3.0	178	4.2	324	3.3	5.1
Blade fragments	187	9.3	609	16.7	625	14.8	1421	14.4	22.6
Flakes (complete)	189	9.4	322	8.8	490	11.6	1001	10.1	15.9
Flake fragments	534	26.6	985	27.1	1187	28.1	2706	27.4	42.9
Tools	97	4.8	122	3.4	230	5.4	449	4.5	7.1
Microburins	27	1.3	104	2.9	93	2.2	224	2.3	3.5
Spalls	0	0.0	0	0.0	9	0.2	9	0.1	0.1
Chips (< 10 mm)	515	25.7	720	19.8	619	14.6	1854	18.8	-
Chunks	371	18.5	614	16.9	719	17.0	1704	17.3	-
Total	2008		3634		4228		9870		6312
[1] percentages for all debitage categories, [2] percentages excluding chips and chunks, C.T.E. = "core trimming elements"									

Table 2 Debitage Counts and Percentages from TBAS 102.

Debitage type	Units 1 & 2				Units 3 & 4	
	Fine		Coarse		Fine	
	n	%	n	%	n	%
Complete flakes & blades	108	52.4	98	47.3	577	86.6
Cores	23	65.7	12	34.3	62	80.5

Table 3 Debitage and Core Frequencies by Raw Material Type.

Retouched artifact	U 1&2	%	U 3	%	U 4	%	All	%
Burins	0	0.0	3	2.5	2	0.9	5	1.1
Notches	9	9.3	10	8.2	22	9.6	41	9.1
Scrapers	1	1.0	7	5.7	4	1.7	12	2.7
Endscrapers	7	7.2	3	2.5	12	5.2	22	4.9
Retouched Flakes	37	38.1	18	14.8	55	23.9	110	24.5
Retouched Blades	14	14.4	16	13.1	53	23.0	83	18.5
Perforators	0	0.0	0	0.0	1	0.4	1	0.2
Backed Blades	0	0.0	0	0.0	3	1.3	3	0.7
Backed Bladelets	15	15.5	19	15.6	31	13.5	65	14.5
Truncations	4	4.1	6	4.9	14	6.1	24	5.3
Backed and Truncated	1	1.0	1	0.8	0	0.0	2	0.4
Lunates	9	9.3	39	32.0	33	14.3	81	18.0
Total	97		122		230		449	

Table 4 Retouched artifact frequencies from TBAS 102

These differences in raw material also appear to have a spatial element at TBAS 102. A comparison of the cores and debitage from Units 1 and 2 (bisecting the irregular stone alignment) and Units 3 and 4 (in the center of the site) bear out these differences. In the latter two units, fine-grained raw materials are numerically dominant over the coarse-grained raw materials for both the cores and the debitage with a minimal ratio of 4:1 (Tab. 3). In contrast, the fine-grained *vs*. coarse-grained ratios in Units 1 and 2 range from 2:1 among the cores to almost 1:1 for the debitage. This difference in raw material use might be attributed to the spatial

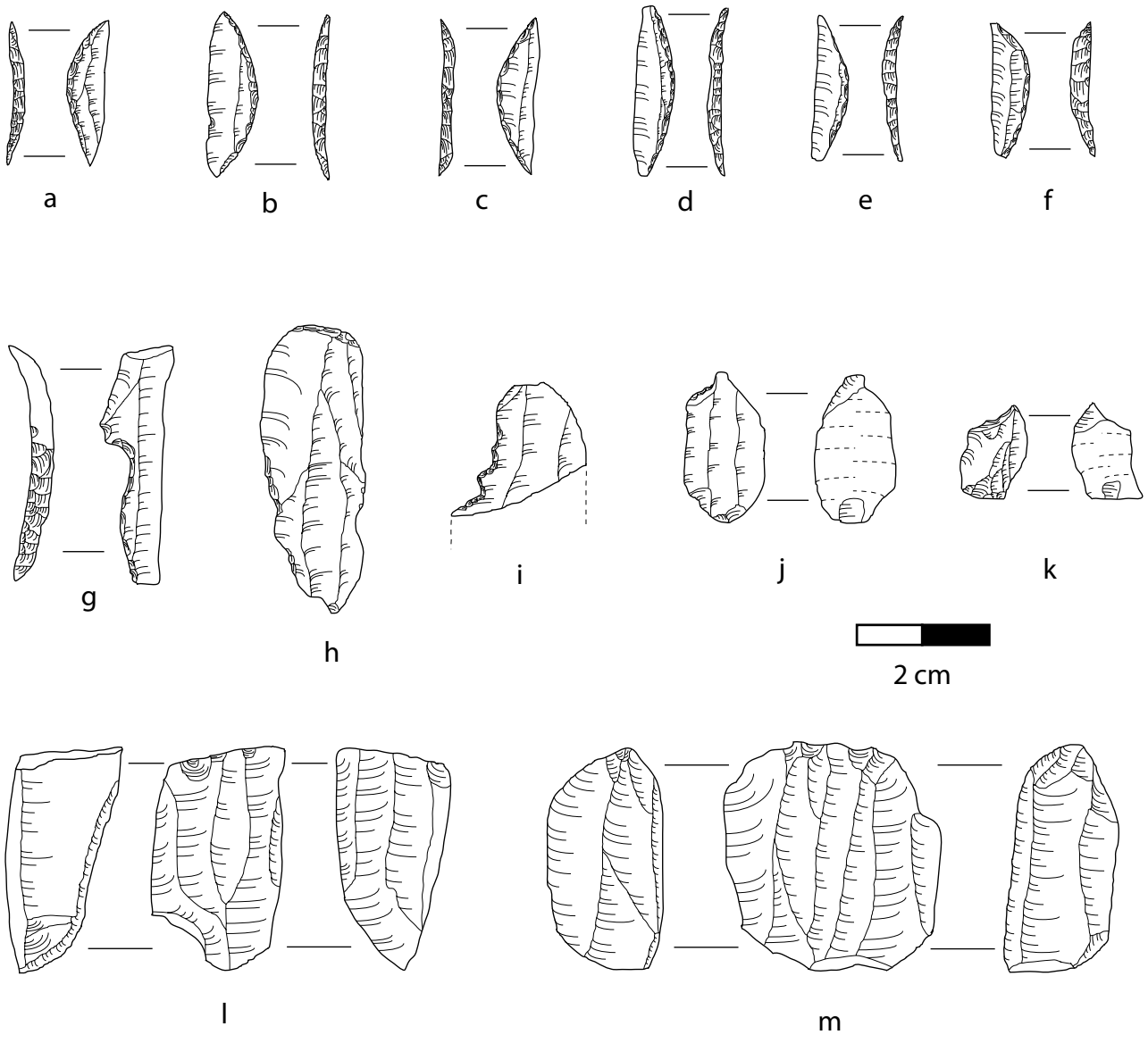

Fig. 4 Lithic artifacts from TBAS 102. a-f: lunates; g: partially complete lunate; h: endscraper; i: denticulate; j-k: microburins; l-m: bladelet cores.

organization of activities across the site or the presence of a non-Natufian component at the site, possibly associated with the stone alignment on the northern edge of the site.

A total of 449 retouched tools were recovered from TBAS 102 (Tab. 4). The most numerous of the tool types were the retouched flakes and blades constituting 43% of the tools. Among the larger tools, notches (9%) and endscrapers (5%) were well-represented. The remaining large tool classes were generally poorly represented with less than 2% of the tool assemblage. Within the microlithic tools, lunates and lunate fragments were best represented (18%) (Fig. 4). Backed bladelets are also well-represented (14%) in the assemblage. Finally, there are a few truncations (5%), but the combination of backed and truncated pieces is very rare.

Shell/Ornaments

A total of 451 pieces of shell were recovered from TBAS 102. The marine shell, comprised of *Dentalium and Nassarius gibbosulus*, represents 2.4% of the sample (Tab. 5). These are indicative of a Mediterranean and possible Red Sea origin, suggesting acquisition through trade from sources to the west and possibly the south. All of these marine shells have been modified through perforation or shaping and likely functioned as personal adornments. The bulk of the shell material (97.5%) is of freshwater origin (primarily *Melanoides tuberculata* and *Melanopsis buccinoidea*). These are not culturally modified like the marine specimens, but are important indicators of past environments as they live in freshwater contexts (*e.g.*, springs and streams). Their presence at TBAS 102 supports our reconstruction of the local paleoenvironment characterized by wetlands potentially fed by spring deposits.

Species	Origin	TBAS 102	%
Melanoides tuberculata	Freshwater	424	94.0
Melanopsis buccinoidea	Freshwater	13	2.9
Bulinus truncatus	Freshwater	1	0.2
Xerocrassa sp.	Land	2	0.4
Nassarius gibbosulus	Mediterranean Sea	2	0.4
Dentalium shells	Mediterranean and Red Sea	9	2.0
Total		451	99.9

Table 5 Shell species and frequencies from TBAS 102.

Faunal Remains

The faunal assemblage from TBAS 102 was very fragmented and difficult to assign to taxon, thus limiting the interpretive value of this data. Evidence of cultural activity was identified in the form of burning and cutmarks on some of the remains. Only 23 of the 867 faunal fragments at TBAS 102 could be identified by taxon, representing 14.2% of the bone weight. For those identifiable fragments, the majority (69.6%) were attributed to gazelle with minor representations of cattle, equid, and caprine (Tab. 6). The greatest numbers of identifiable remains (82%) were recovered in Unit 4. The remaining identifiable elements came from Unit 3 while Units 1 and 2 did not yield any fauna assignable to taxon. Interestingly, six identifiable fragments were recovered from the potential feature (pit-like depression) in Unit 4 (levels 3 and 4). These included three gazelle specimens, two equid, and one caprine. Other material from the feature included unidentifiable scapula fragments that were equid-sized and probably part of the identified equid remains in the feature. The proximity of these skeletal parts to one another suggests that the material is more or less *in situ* and not the result of post-depositional activities.

Taxon	Unit 3	Unit 4	N	%
Gazelle	4	12	16	69.6
Cattle	0	3	3	13.0
Equus	0	2	2	8.7
Caprine	0	2	2	8.7
Total	4	19	23	100.0

Table 6 TBAS 102: Number of identified animal bone fragments by excavation unit.

Overall, the faunal remains from TBAS 102 are consistent with an open, steppic environment, although the presence of cattle/auroch would require regular access to water resources. This again suggests the presence of localized springs or marshes.

Conclusions

Given the small size of the site, TBAS 102 probably represents a short-term, seasonal camp along the Wadi al-Qusayr. A long-term encampment might exhibit a greater spread of materials than found here. However, the density of materials (9000+ artifacts from 4m^2) suggests occupation intensity beyond that of a specialized task site (*e.g.*, the Natufian occupation at Yutil al-Hasa [Olszewski *et al.* 1994]). It is also possibly that the site was reoccupied periodically, especially if the stone alignment were part of the Natufian occupation. The labor investment in this sort of feature might serve as a landmark for subsequent occupation.

The excavations at TBAS 102 represent the first dated Late Natufian site in west-central Jordan and the addition of sites from this time frame enhances our understanding of prehistoric land-use practices. The location of the site in a wetland setting indicates that the resource base of these regions was both diverse and stable enough to support hunter-gatherer use during the late Pleistocene. Furthermore, TBAS 102 (and possibly other Natufian sites in the Wadi al-Qusayr) provides

an opportunity to examine the effect of the Younger Dryas on land-use practices. In particular, do these environments persist in the face of region-wide declining conditions or are these small wetlands in west-central Jordan stable enough to enable Natufian populations to succeed in the marginal zones of the Levant?

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Shir, West Syria

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The site of Shir in West Syria was founded around 7000 BC and inhabited throughout the 7th millennium. The final stage of occupation can probably be dated to between 6300 and 6100 BC - however, these dates still require confirmation from ^{14}C dating. Thus, it seems that the site was abandoned towards the end of the 7th millennium BC; at least no traces of younger occupation were found. It is still unclear whether there is a direct causal connection between the abandonment of the site and a period of Rapid Climate Change (RCC) (8.6 - 8.0 ka calBP). Indications of so-called rubble slides were not found at the site of Shir (cf. NeoLithics 1/09).

The Late Neolithic site of Shir is located near the provincial capital Hama (Fig. 1) and was discovered in 2005 during a regional survey. Since 2006 it has been the subject of a joint project by the German Archaeological Institute at Damascus and the Syrian Antiquities Department.

The settlement is situated atop a limestone plateau, about 30 m above the flood plain of the Nahr Sarut, a tributary of the Orontes which is today retained in a small reservoir during the spring (Fig. 2). Climate and vegetation in the region are still markedly Mediterranean: the annual precipitation averages approximately 400-500 mm, permitting for rain-fed agriculture. The present day landscape is characterized by intense crop cultivation with no remaining original vegetation. However, palaeobotanical investigations have shown that an open oak forest existed in the vicinity. In addition, a dense floodplain forest with the corresponding wildlife can be assumed in the Sarut river valley.

With an area of 4 hectares, the site of Shir in one of the medium-sized Neolithic settlements. Geophysical investigations have shown that the site was built in a semicircle arrangement around an open space in the east. According to previous studies the site was occupied solely during the 7th millennium BC and probably abandoned by the end of the 7th millennium BC. All levels of the site contain pottery; evidence of a post-Neolithic settlement was not found. In the 3rd millennium BC a new settlement was established in the vicinity of the Neolithic site - today called Tall ash-Shir.

Until now excavations in Shir have focused on three areas of which two are of specific interest. The western part of area K-M 7-8 was excavated down to bedrock in a small sounding. Here the entire sequence comprises 6 m of deposits with six building levels, most of which contain rectangular buildings with thick lime plaster floors. One calibrated ^{14}C-date from the lowest level points to and age of between 7080 and 7030 BC. The uppermost Levels IV to VI can be dated to the period around 6650 to 6450 calBC.

The second excavation area L-O 20-21 is located in the northeastern part of the settlement. Here a building complex of *ca.* 28 m in length and 5 to 6 m wide was uncovered; it appears mainly to have fulfilled a storage function. According to the pottery typology the complex can be dated to about 6300-6100 BC. It probably represents the last phase of the settlement before it was abandoned. However, ^{14}C dates are still required to substantiate this age.

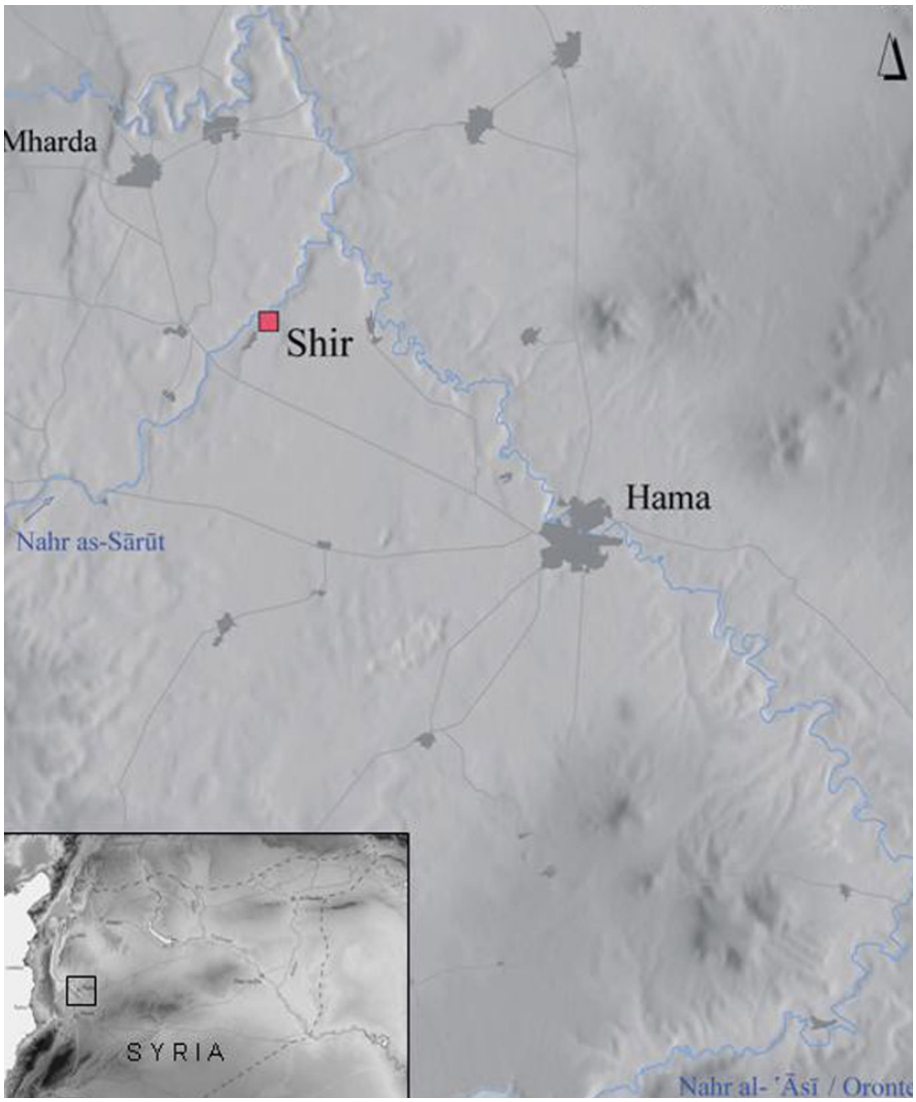

Fig. 1 Location of Shir

Fig. 2 View of of Shir located on the limestone plateau near the Nahr Sarut

Shirai, Noriyuki

2010 *The Archaeology of the First Farmer-Herders in Egypt: New Insights into the Fayum Epipalaeolithic and Neolithic*
PhD Thesis, Leiden University
Leiden University Press
(ISBN 9789087280796)

Abstract

This thesis explores how and why crop farming and animal herding started in a particular time period in a particular region of Egypt. The earliest Neolithic farming in combination with herding in Egypt is known in the Fayum, which is a large oasis with a permanent lake in the Egyptian Western Desert. This is owing to the arrival of Levantine domesticates in the 6th millennium cal.BC. Neolithic farmer-herders in the Fayum relied heavily on hunting and fishing, which had been the major subsistence activities since the preceding Epipalaeolithic period. There are no remains of substantial dwellings to indicate that these farmer-herders lived a sedentary way of life. Previous researchers have thus asserted that the Fayum people were nomadic and moved seasonally. A major research question is whether such an assertion is really supported by other archaeological data. Considering the harsh desert environment, it seems unrealistic that all the people moved far away from drinking water and rich wild food resources at a permanent water source, even temporarily.

Research on lithic artefacts used by the Epipalaeolithic hunter-fishers and Neolithic farmer-herders in the Fayum reveals where lithic raw material was exploited and where and how tools were made. This gives a clue as to the mobility and residential strategy of the Fayum people. Fayum Neolithic farmer-herders preferentially procured larger lithic raw material from more distant sources than Epipalaeolithic hunter-fishers did. In addition, the Neolithic people invented much larger and more elaborate hunting weapons than their Epipalaeolithic predecessors by using large raw material. Questions are why Neolithic people took such longer distance trips, and why they invested more time and labour in making such weapons despite the arrival of domesticated animals. Furthermore, although the data are scarce, the number of hippopotamus and crocodile seem to have increased in the Neolithic faunal assemblage compared with the Epipalaeolithic one. A question is why such an increase occurred in the Neolithic.

These changes in the Neolithic indicate people's adaptation to new subsistence activities. It is plausible that the Neolithic people had to take the herd of domesticated animals for grazing, particularly when crops were growing in farming plots which would have been located around lakeshores. Collecting lithic raw material would have been embedded in the pastoral grazing trips. The appearance of new hunting weapons and the increase in the number of hippopotamus and crocodile in the Neolithic would be due to a new predator-prey relationship in the Fayum ecological system caused by the arrival of Levantine domesticates. Farming and herding in the Fayum lakeshore environment would not have been possible without the protection of farming plots and herds from hippopotamus and crocodile by the people who inhabited lakeshores. On the other hand, increasing dependence on these new subsistence activities was not possible without a constant supply of larger raw material for toolmaking, which was probably enabled by an increase of logistical moves of individual members from a residential group.

Despite the lack of substantial dwellings, other circumstantial evidence suggests that the Fayum people were not nomadic but were tethered to lakeshores. The introduction of farming and herding would not have taken place in the Fayum without a lakeshore-tethered if not fully sedentary lifeway. However, the success of a farming-herding lifeway in the Fayum would not have been possible without the reorganisation of mobility, which led to decreased moves of residential bases and increased logistical moves of individuals. A simplistic dichotomy between either sedentary or nomadic does not precisely describe the situation of the Fayum Neolithic farmer-herders.

The last question is why Levantine domesticates were introduced in the Fayum, even though wild food resources seem to have been constantly available and more efficiently exploited than domesticates. If farming and herding had turned out to be unsuitable in the Fayum environment after an initial attempt, they would have dropped out of the Fayum subsistence. However, Fayum Neolithic people made unprecedented time and labour investments in lithic raw material procurement and toolmaking for new activities. It is obvious that the people kept making special efforts to maximise the yield of farming and herding. It is assumed that domesticates were added to the diet of Fayum Neolithic people when some essential wild food resources became temporarily or perpetually unavailable. This could have been caused by either unusual weather conditions and environmental disturbances, or the loss of access to the essential resources due to population increase and overcrowding in a circumscribed area like the Fayum. Therefore, it is important to consider the social context of the beginning of farming and herding in the Fayum in a wider geographical and chronological framework.

In the Fayum Neolithic, the number and density of sites are larger and higher than those in the Epipalaeolithic, and population increase in the Neolithic is evident. General population increase in the Egyptian Western Desert since the 8th millennium cal.BC is attested by the wide distribution of human occupation loci and the fast spread of similar material cultures. The recurrence of depopulation in arid regions and population aggregation in well-watered regions of the Western Desert is also well documented. It is likely

that such a demographic trend in the Western Desert affected the Fayum. The Fayum was rich in wild food resources, and the balance between human population size and available food resource amount would have been maintained well below the carrying capacity of the Fayum in a natural state. However, the influx of migrants from outside the Fayum must have sooner or later upset this balance, and Fayum people would have had to increase the carrying capacity of their habitat by means of food production. This would be the reason why the Fayum people did not give up farming and herding despite the supposed difficulties in taking care of domesticates in this specific environment.

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Gregg, Michael W.

2009 *Organic Residue Analysis and the Earliest Uses of Pottery in the Ancient Middle East*
Ph.D. thesis, Department of Anthropology, University of Toronto
Supervisor: Dr. Heather M.-L. Miller

Abstract

In this dissertation, I discuss the role of organic residue analysis in identifying economic activities and subsistence practices associated with the first uses of pottery in the Middle East, and present the results of my analyses of 280 potsherds recovered from 22 Neolithic and early Chalcolithic settlements dating between 7300 and 4300 cal BC. The adoption of pottery vessels in the early agricultural villages and pastoral encampments of the Middle East was not a uniform phenomenon, with this new technology not immediately of benefit, apparently, to all human groups.

Results of my analyses have demonstrated that 'conventional' solvent extraction and alkaline hydrolysis techniques have limited utility in the recovery of diagnostic organic compounds from pottery from early ceramic horizons in the Middle East (Gregg et al. 2007), and that increased yields can be achieved through the use of a microwave-assisted liquid chromatography protocol (Gregg *et al.* 2009; Gregg and Slater 2010). My research has also established that there is greater diversity in the fractionation of stable carbon isotopes associated with the synthesis of fatty acids in domesticated animals than has previously been reported. In many instances, the ranges of modern isotopic values that have been used to categorize animal fats in archaeological potsherds in northern Europe cannot distinguish between the $\delta^{13}C$ ratios of ancient dairy residues and carcass fats of ruminant and non-ruminant species in central Europe or the Middle East (Gregg *et al.* 2009; Gregg and Slater 2010).

In light of these results, I evaluate the diagnostic potential and limitations of different methodological approaches in the recovery and characterization of organic residues, and I propose a series of measures that will allow more confident categorization of the substances in early pottery vessels from the Middle East. I also make a number of recommendations for archaeologists considering the use of organic residue analysis, and suggest some practical ideas on how to develop the degree of confidence necessary to assess the methods used in acquisition of molecular and isotopic data, and ultimately, to evaluate the adequacy of the analytical criteria used to address specific archaeological research questions.

A PDF of my thesis is available through the University of Toronto Research Repository at: https://tspace.library.utoronto.ca/handle/1807/19039. A published volume (Gregg 2010a) is available from Oxbow Books.

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Rokitta-Krumnow, Dörte

2010 *Lithikfunde des 7. Jahrtausends v.Chr. in der nördlichen Levante. Die Entwicklung der Steingeräteindustrie der spätneolithischen Siedlung Shir/Syrien*
PhD Thesis, Free University Berlin
Supervisors: Prof. Dominik Bonatz, PD Dr. Karin Bartl

Abstract

The Late Neolithic site of Shir was discovered in 2005 during a survey of the Middle Orontes by the German Archaeological Institute (Oriental Department) directed by Karin Bartl (2003-2005). It is situated ca. 12 km northwest of the provincial capital of Hama on a plateau ca. 30 m above the valley of the Nahr as-Sarut, a tributary of the Orontes. The floodplain occupied by open forest, as well as the existence of permanent water supplies including rivers, wadis and karstic springs and its fertile soils and mild climate, make this region very suitable for agriculture. These factors enabled a long and continuous prehistoric occupation of the settlement covering the whole of the Pottery Neolithic (ca. 7.000 – 6.200/6.100 BC).

This study is mainly concerned with the lithic assemblages obtained through excavation in areas K7 and L7 from 2005 to 2007. More than 35,600 flint and obsidian artefacts were analysed. The analysis of primary and secondary production led to questions concerning the procurement of raw materials, raw material economy, the process of production (in particular, methods of core reduction), choice of blanks, tool kit composition, and generally the organisation of lithic production.

Shir was well supplied with raw material, as flint of high quality was available from directly beneath the settlement. Other types of high-quality flint were also available in close proximity, thus flint was the lithic raw material most commonly used. Besides flint, obsidian from the Cappadocian sources of Göllü Dağ and Nenezi Dağı was used in smaller amounts (<2%).

The kinds and number of cores and debitage from core preparation prove that flint was worked within the settlement. Waste from all stages of the sequence of lithic production was found at Shir. Flake production was predominant, but uni- and bi-directional blade production was practiced as well. It is not clear at this stage whether obsidian was also being processed at Shir. The absence of cores and debitage from core reduction from the record makes it more probable that blanks or tools were imported.

Tool production focused mainly on blades, especially sickles, burins, borers, projectiles and scrapers. Furthermore, retouched blades were more common than retouched flakes. This trend is discernible in all building levels: tools derived from blades were nearly twice as frequent as those derived from flakes.

Typologically, the lithic industry of Shir is similar to that of contemporary settlements in northwest Syria. The tool kit was mostly made up from non-formal tools that could have served multiple functions. Besides those, scrapers and sickles were common. Sickles decreased in frequency over time, which can be explained with a more effective organisation of work or a change in mounting techniques. Borers and burins were infrequent in all building levels, as were projectiles and daggers. Spear heads, projectiles, as well as sling stones and bolas, formed parts of a spectrum of weapons, providing evidence for different hunting techniques. During the period of settlement at Shir, standardised tools decreased in frequency. Noteworthy is the occurrence of long bi-directional blades and naviform cores in the younger building levels.

Production on the household level can be differentiated from specialised production by analysing the kind of core reduction (naviform core technology, flake technology, unidirectional blade technology, etc.). The existence of specialisation in lithic craftsmanship can be proven indirectly by the find of depots of long blades, the obsidian industry, and bead production. The deposition of long blades in particular link Shir to a PPNB tradition, as these were rather infrequent in PN settlements.

Supra-regional comparisons can demonstrate that the local source of flint of high quality led to a survival of PPNB artefacts until the Pottery Neolithic. PPNB technology retentions in Shir can probably be explained by this fact, although a preceramic phase of the settlement has not been found yet. Changes in the lithic industry of Shir were induced by tradition, functional aspects, organisation of craftsmanship (specialised vs. non-specialised) and changes in subsistence strategy (hunting, domestication *etc.*).

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