INTRODUCTION: HOW VIOLENT WAS THE MESOLITHIC, OR IS THERE A COMMON PATTERN OF VIOLENT INTERACTIONS SPECIFIC TO SEDENTARY HUNTER-GATHERERS?

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Why another book on violence in prehistory? Do we have enough evidence to draw meaningful conclusions on the importance and meaning of violent interactions among sedentary and semi-sedentary hunter-gatherers of Europe? What methodological and theoretical questions do we hope to answer with this volume? Many questions on the evidence and meaning of confirmed violent interactions remain unresolved even as more and more books appear on the topic. This volume was prompted by my own research in the Iron Gates Gorge and the differences in patterns observed between sites on the right bank of the Danube – Vlasac, Lepenski Vir, Hajducka Vodenica, Padina, Velesnica, and Ajmana – and Schela Cladovei, a coeval site on the left bank belonging to the same cultural sphere. So far, the evidence for violent interactions on the right bank is very limited and spread over time, while the left bank shows such an extreme incidence of violent deaths as to be very puzzling. The papers presented here reflect a similar puzzlement felt by each of the participants while examining the evidence of trauma and possible or probable interpersonal violence.

1. MESOLITHIC

Mesolithic times have been signaled 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, Vencl 1999), to the point that it is taken as a confirmed fact by non-specialists (De Pauw 1998). But 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?

This volume presents evidence of violent interactions in a group of societies conveniently defined as "Mesolithic:" three from different spots around Mediterranean: the *Iron Gates Gorge* (Serbia and Romania), the *Muge* and *Sado* valley shell middens (Portugal) and the recently excavated Iberomaurusien site of *d'Ifri n'Amar* (Morocco); earthen mounds from Eastern coast of Uruguay, the Epipaleolithic and Mesolithic sites in Ukraine, and a Neolithic site from China. Except for the latter, they have in common their "Mesolithic nature," defined here by a combination of economic practices (hunter-gatherers) and mobility patterns (semi-sedentary or sedentary), irrespective of the geographic area and temporal framework (Roksandic 2000: 4-6). For most of these populations, at some point in their history, contact with farming communities was possible (Lubell, Jackes, and Meiklejohn 1989, Radovanovic 1996a) even if it did not occur. Some of the groups participated in these communications through trade (evidenced by imports of non-local products) and possibly by other means. This period of latent and possible change had an important impact on the ideological integrity of these populations. In the case of the Iron Gates Gorge, it produced a stronger ideological integration of the community at a time when contact with farming societies became possible (Radovanovic 1996b, Radovanovic 1996c). It is often suggested that this kind of contact might have resulted in conflict through greater population pressure and territorial claims as well as other economic or ideological factors (as in Schela Cladovei). Our aim is to show whether or not we have evidence for that conflict in the archaeological and anthropological record of the sites presented.

2. INTERPRETATION OF ORGANIZED VIOLENCE

Another question of great importance to all of the participants in the volume is: How do we proceed from the evidence of an individual's violent interactions and death to interpretation of organized violence? And a step further in the same direction: Is all organized violence warfare? Indeed, how do we make this jump in interpretation based on skeletal data alone? The often cited massacre at Offnet (Frayer 1997) could have more than one explanation and could have involved more than one type of action. How do we interpret a cache

of bones: 1) a simultaneous burial of body parts rescued after a massacre; 2) a simultaneous burial of war trophies; 3) a diachronous burial of decapitated individuals sacrificed to a bloodthirsty god; 4) a diachronous burial of skulls reflecting the cult of ancestors? Any of these explanations, and a score of others, is possible. Only careful excavation and documentation can give us sufficiently fine-grained resolution to allow reliable identification of the synchronicity of the burials, a crucial argument in the interpretation of a collection as deriving from a massacre. Unfortunately, for Offnet, and many other sites excavated earlier in the twentieth century, this is not an option.

If we accept that some of this evidence is strong enough to stand meticulous examination, is this indicative of organized violence? And further along the line of deduction, can it be interpreted as warfare? Ideally, only when we have answered all these questions in succession, and in the affirmative, can we presume to answer, by examining many of these individual societies, the question of whether the Mesolithic in general witnessed a higher incidence of warfare than previously. In that case, we can start building explanatory mechanisms for this elevated level of warfare. Unfortunately, we are still struggling to prove each case to be one of violence against a number of other possible explanations. Even where violence is proven beyond doubt, we have too little information to start delineating a picture of war and peace. Thus we are left with an examination of origins and causes of organized violence and a definition of warfare borrowed from the cultural anthropological literature. We propose that combining insights of cultural anthropology with skeletal evidence and contextual archaeological information will result in a more reliable picture of prehistoric warfare.

3. CULTURAL ANTHROPOLOGY ON ORIGINS OF WARFARE

After a period of relative neglect of warfare and violence in anthropology, there has been a revival of interest in theoretical questions regarding violent interactions in present-day small-scale societies and in archaeological populations. Recent editions – such as Reyna and Downs' series *War and Society* (in 5 volumes from 1992 to 1998), Haas' (1990) *Anthropology of War*, Ferguson's (1984) *Warfare, Culture and Environment*, to mention a few – confirm the growing interest in questions of war and warfare and the theoretical bases for understanding war and its impact on developing structures within societies. Walker (Walker 2001) laments the lack of significant contributions to the study of violence by anthropologists, as opposed to the importance it has for historians. Nevertheless, books like Kelly's (2000) *Warless Societies and the Origin of War*, Guilaine and Zammit's (2001) *Le sentier de la Guerre*, Carman and Harding's (1999) *Ancient Warfare*, and Keeley's (1996) *War before Civilization* – with their emphasis on understanding early prehistoric evidence – stem from the interest in discerning the predominance of violent interactions in the past that might help explain one of the most common but perhaps undesirable modes of human social behavior.

Warfare was always part of the explanatory mechanism for the archaeological record and has been invoked in interpreting a number of structures, arms, evidence of village burning, and evidence of multiple deaths. What is perhaps new in this recent attitude towards the study of past warfare is the quest for its origins, for an interpretation of its roots in human societies. The focus on non-state societies, whether contemporary or prehistoric, seems to represent a logical choice in such explanatory attempts. 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. This evidence must be paramount. Considering these societies as pristine is *passé* (to use Reyna's words: 1994: xiii), and only very few anthropologists would claim that they afford "an intimate glimpse beyond history" (Chagnon 1977:xii). Direct ethnographic analogy is often misleading as it takes evidence out of its historical context. 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 conjunctural events" (Simons 1999: 92). This local and historical character has to be kept in mind in all attempts to understand war and its background. Haas (1999) has stated that we can only begin to understand the origins of war by identifying the repeating patterns of warfare in pre-state societies. Keeley shows that warfare is present in the archaeological record of non-state societies and he demonstrates (1996:175) 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 warfare is universal and a given 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) which shows that all present-day small-scale societies have to be seen in the context of their interactions with the state societies and global economy. Similarly, an understanding of Mongol nomadic warfare is possible only when it is seen in the context of the socio-political milieu of the sedentary farming state of China (Barfield 1994).

There is no doubt that every human being is capable of violent behavior. Socialization and learning help direct and channel this type of behavior. 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. However, societies differ both in the amount and direction of violent behavior that is considered permissible or appropriate. Furthermore, war is "not related to violence as simply more of the same" (Kelly 2000): 21). This brings us to an important question in studying war: Can all violence be interpreted as warfare? When interpersonal violence in a studied group is rampant and involves more than one group

perceived as a, more or less, coherent unit, do we need to draw a line between warfare and feuding? And even more importantly for this book, how do we distinguish between them in the archaeological record? If we decide that warfare does not appear before a certain level of socio-political complexity (such as the state) within any society is reached (Reyna 1994b), then all of the violence experienced by the non-state organized groups remains in the domain of "resolution of individual personal grudges" (Reyna 1994a). If we conceptualize war as restricted to centralized polities (Reyna 1994a:xiv) the question of warfare in the Mesolithic does not even arise.

The definition of warfare Reyna offers is based on its proximate (stated and real) causes and he resorts to "grudge-accumulation process" as the explanatory mechanism for protracted tribal fighting in which members of villages became involved in raids and battles (Reyna 1994b: 42). But can we really emphasize causal factors as the determinant in our definition of warfare, and which cause do we consider: the immediate proximate cause, or the underlying one? It is more than obvious that proclaimed causes that prompted states to declare war have rarely been anything but a propaganda tool aimed at the state population itself, the people who had to accept and support the war. The motives of the society (or rather its elite) are usually well hidden behind ideological proclamations aimed not so much at the enemy or future historians but at one's own population whose dissent has to be prevented. Thus Keeley (1996: 114) asks: "Should any motive *declared* by anyone be considered? Should motives be inferred from the operation, results, and effects of specific wars or acts of war?" (Emphasis L. K.). Predominant motives for pre-state warfare (based on cross-cultural studies reported in Keeley (1996: 200) are revenge for homicides and various economic issues (p.115). Personal aggrandizement – prestige – as a motive is actually more commonly associated with higher levels of political centralization (chiefdoms and states). Apparently, subjugation and tribute are the only motives absent in non-centralized polities (p.116), the major reason being that a kin militia (typical of non-state societies) has no means of maintaining violence beyond a few weeks. Even when continuous raids result in a gain of territory (a common enough motive) through the driving out of the opponents, this is not a form of control, but a dispersion (Carneiro 1994, Reyna 1994a). Keeley argues, however, that repeated violence can result in "gift" giving (or tribute), a form of tax similar to "extortion rackets exercised by urban gangsters, rural brigands and pirates in civilized societies." Thus the motives of the centralized versus non-centralized societies cannot be easily distinguished – they do not offer a good set of variables for defining warfare.

A definition of warfare offered by Kelly seems to be applicable to all levels of political centralization and offers a good working definition for examining prehistoric warfare. Kelly (2000) considers war (including feuds) to be grounded "in application of the principle of *social substitutability*" (p. 21). And further, "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" (*idem*: 5). All of this would distinguish it from murder, duels and capital punishment, since these are directed against the perpetrator of a crime.

The cultural anthropological literature on warfare is mostly concerned with its evolutionary significance summed up as "when it started and how can we end it." Cultural anthropologists consider that biology plays a relatively unimportant part in the emergence of war (Carneiro 1994), although proponents of evolutionary ecology maintain that warfare is based in maximizing inclusive fitness (Gat 1999, Gat 2000a, Gat 2000b) and can not be regarded as characteristic of humans since it is based in the common heritage of social animals from chimpanzees to wolves (Wrangham 1999). Another commonly evoked source of warfare, population pressure – prominent since Thomas Malthus' famous *Essay on the Principles of Population* (1798) as major predictor of frequency of war – is not supported by cross-cultural studies (Keeley 1996:118). Kang (2000) demonstrates that under certain historical circumstances, warfare can result from underpopulation caused by environmental stress. However, Kelly (2000:chapter 3) suggests that population density does play an important role if we limit the analysis to either segmented or non-segmented societies. Keeley (1996: 119) recognizes that some relationship between population pressure and frequency of warfare exists, however, this relationship is either complex or very weak or both and he concludes that: "warring societies are equally common and peaceable ones equally uncommon at any level of population density" (120). Along the same lines, Walker states that "throughout the history of our species interpersonal violence, especially among man, has been prevalent. No form of social organization, mode of production, or environmental setting appears to have remained free of interpersonal violence for long." (Walker 2001: 573). 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), all societies will eventually indulge in war.

Much less often stressed is the fact that all these 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. As noted by Kelly (2000: 11), the importance of studying peaceful societies cannot be over-emphasized, yet the literature on it is much less abundant than on the warring societies (Sponsel 1994).

Any of the above-mentioned factors: biological, ecological and cultural will not necessarily result in warfare if the society is unsegmented. Unsegmented hunter-gatherers have a low frequency of warfare as they lack organizational features associated with social substitutability that are conductive to development of group concepts. Segmented foragers, on the other hand, show much greater frequency of warfare: 16 out of 17 examined (Kelly 2000: 51). Thus 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).

4. GROUPS ARCHAEOLOGISTS STUDY

That local history has to be a component in understanding warfare is no less true for the groups archaeologists study. The examples presented in this volume are societies of relatively long duration, and local history spanning anywhere between 1000 to over 1500 years. 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. Given concerns about preservation bias, inability to detect soft tissue wounds as causes of (violent) death (Jackes, this volume, Cunha, this volume), 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 era of the 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?

5. PAPERS IN THIS VOLUME

No lower or upper limit for the length of the articles was imposed, and the number of illustrations was left open to participants. This is reflected in individual papers. My editorial impact on the papers was minimal. Most of the comments on the first drafts of each paper were derived from the internal review process. I am most grateful to the participants in the volume who took time to review the articles and I hope that it created dialogue and helped exchange ideas. The internal review process was followed by an anonymous external review. The book presents an array of personal experiences, attitudes, interpretations and different positions proposed by the authors and while I do not necessarily endorse all of them, I appreciate the opportunity to offer them in one volume.

The first paper by Tracy Rogers examines our ability to determine warfare from the skeletal record from the perspective of a forensic anthropologist. Unfortunately, a growing number of forensic cases involving war dead from 20th century civil wars and mass burials, offers insights into the ways in which inter-personal violence is identified (cause), analyzed (mode), and interpreted (manner) in a modern context, and provides some recommendations for incorporating these analyses into the study of ancient aggression. Rogers concludes that, given the variety of reasons for engaging in warfare, it is unrealistic to expect warfare to exhibit a single diagnostic pattern in the bioarchaeological record. Modern warfare evidence exhibits greater variability in injury patterns and victim demography than homicide, which is a potentially useful indicator of warfare in past populations. The author stresses that our potential to recognize violence and infer warfare in the bioarchaeological record has to proceed through contextual analysis of: (1) the nature of conflict; (2) the type and seriousness of injuries sustained; (3) the demography of war dead; (4) the number of fatalities; and (5) the burial context. Since analysis of modern warfare and homicide demonstrates the potential scope of characteristics that may apply to ancient warfare, Rogers calls for a broader perspective to the study of conflict in past populations.

The further we go into the past, increasingly longer time sequences are collapsed, and the resolution we deal with becomes very coarse. We have to rely on any available evidence. What evidence can be accepted as sufficient for the definition of warfare and can absence of evidence be interpreted as the evidence of absence, these are the questions Mary Jackes attempts to answer through examination of different regions where she has first hand experience as both a cultural and biological anthropologist. Her understanding of trauma in the Portuguese Mesolithic series favours accidents over violence and warfare as explanatory mechanisms. A situation of undoubted extreme interpersonal violence in Kenya is shown to relate to within-population conflict caused by external pressures. Striking differences between known ethnohistoric data for North American groups of the Northeast Woodlands and the corresponding skeletal material bring forward in this chapter the full scale of ambiguity and difficulties in interpretation of prehistoric violence. That the problems become aggravated by excavation and curatorial practices is, alas too common knowledge for all of us, and we often have to "make do" with what little evidence we have. Osteological material from a Neolithic Chinese site which is the final focus of the paper, provides clear physical evidence of violence, despite the lack of archaeological recognition of conflict, thus pointing to the need to be wary of the interpretations in this sensitive area of anthropological study. Jackes concludes that we need to avoid simplistic explanations for evidence of violence and calls for the setting of that evidence within a broad context – one with chronological and social/geographical depth and breadth. Jackes concludes that, 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 when making broad statements regarding violence in a society – we must be sure that we are being strictly neutral.

The third paper, by Eugenia Cunha, Claudia Umbelino and Francesca Cardoso brings more data on the Portuguese Mesolithic sample: the material from Muge housed in the

Porto Museum and the less known material from the shell middens of the Sado valley. It complements the information on Portugal given in the previous paper and concludes, along the same lines, that we do not have any reasonably strong indication that violence amounted to warfare at the sites of either the Muge or the Sado valley in the Mesolithic. Even though some trauma is present and some of it can be interpreted as violent trauma there is nothing that would suggest elevated levels of interpersonal or intertribal violence or warfare.

In the fourth paper we stay in the same general area and move south from the Iberian Peninsula to Morocco. Ben-Ncer brings us an interesting case of decapitation and dismemberment of a small child within the context of Iberomaurisien with some additional evidence of non-lethal conflict from the well-known site of Tafarault. The author examines two alternative explanations: that the child was killed by decapitation, or decapitated and dismembered after death. While I would not necessarily regard this decapitation in the same light as the author, since burial rituals are varied and not always conforming to present-day ideas of what constitutes a 'proper' burial, the case is interesting and confronts us with a number of questions: What was the purpose of this act? Burial ritual? Exorcism? Sacrifice? Given the most orderly burial of other young children, why was this child singled out? Are we dealing here with the phenomenon of substitutability indicating warfare by Kelly's standards discussed above, or could this be explained as evidence of for example witchcraft, among a number of other possible options. Given the sporadic evidence of non-lethal violence from Tafarault, warfare is an unlikely interpretation, however, as Mary Jackes has shown in this volume, since absence of evidence can not be regarded as evidence of absence, we can only hope for more material to come out of the new excavations currently underway in Morocco.

With the fifth paper we move to the Iron Gates Gorge Mesolithic material of Lepenski Vir type. Traumatic lesions caused by violence are presented in detail with all additional demographic and archaeological data from the burial context. They are contrasted with the evidence from Schela Cladovei, a coeval site on the left bank of the Danube. The likely cases of violence on the right bank of the Danube are few and randomly distributed throughout the three periods examined, while violence is more rampant at Schela Cladovei and tightly clustered in terms of absolute chronology. The dates for the Schela sample are on the borderline between the Pre-contact Mesolithic and the times when the contact with Neolithic farmers becomes possible, while most of the violent episodes on the right bank of the Danube fall into pre-contact times. Their random distribution and lack of special burial treatment indicates that the inter-personal violence does not amount to feuding or warfare (as discussed above) while the left bank data indicate an episode of conflict restricted in time. There is no indication that warfare was endemic in the Iron Gates Gorge Mesolithic. Furthermore, it cannot be associated with the stress caused by advancing farmers, as most of it happened when the contact was non-existent or at best sporadic and did not result from any major movement of populations.

Choyke and Bartosiewicz's article on osseous projectiles brings another aspect of inquiry that has not been performed on any of the Mesolithic series previously discussed. Both stone and bone projectiles are found embedded in human bone at Mesolithic sites. Stone projectiles predominate because of either their greater functional value or different taphonomic properties. In the case of the Swiss Late Neolithic site the authors discuss taphonomy, typology and function of bone projectiles and examine their potential role in hunting *versus* warfare. While the authors conclude that a distinction between hunting implements and arms can not be made on the basis of typology, and while taphonomy plays an important role in the frequency of bone projectiles at different sites, especially excavation and collection strategies, these objects should be routinely examined in the context of warfare. The expediency of the bone artifact production argues for their remaining in situ in case of conflict where they could indicate an attack from the outside group. The authors compare frequencies of bone projectiles in Lepenski Vir sites and their Swiss Neolithic site. While a greater percentage of osseous projectile points within the bone artifact assemblage from Lepenski Vir and other Iron Gates Gorge Mesolithic sites could be interpreted as showing evidence of possible warfare, the authors caution that different collection practices (namely large number of small bone artifacts collected through sieving in the Swiss Neolithic) could have produced this unbalanced picture. More research in this domain, from and around the sites where defense structures confirm the existence of conflict is needed to shed more light on these numbers.

Lillie presents us with a critical overview of the published sources relating to three Epipalaeolithic cemeteries from the Dnieper Rapids region of Ukraine, and outlines the evidence for violence that has been recorded on the human skeletal remains from these sites. The skeletal evidence suggests that the increased use of the bow and arrow in hunting was accompanied by its increased use in inter-group violence. The osteological and lithic analyses carried out by Russian researchers on Epipaleolithic skeletal remains, highlight a prevalence of injury unattested to in the later Mesolithic and Neolithic cemeteries from this area, studied by Lillie. The restructuring of the fauna and flora in the steppe and forest-steppe zones of Europe at the transition to the Holocene period appears to have resulted in the potential for conflict over access to certain resources among the indigenous hunter-gatherer populations. In effect, the early absolute dates from Vasilyevka III, alongside the relative dates for Voloshkoe and Vasilyevka I, suggest that the inter-group violence potentially highlights the early stages of territoriality when regional groups would have competed for a preferred location, primarily for the exploitation of freshwater resources. Once these territorial rights were established, it appears on the current evidence, that the need for violent conflict to maintain them was unnecessary throughout the subsequent Mesolithic, Neolithic and Eneolithic/Copper Age periods in this region. While environmental changes can be regarded as the underlying cause, social restructuring seems to be the explanatory mechanism for the violence at the particular time.

In the last article Pintos Blanco discusses what he sees as evidence of interpersonal violence recovered from the burials of a group of complex semi-sedentary hunter-gatherers of Uruguay known in the literature as 'the culture of mound constructors' (*la cultura de los constructores de cerritos*). This culture of long duration represents increasing segmentation of the society and 'monumentalisation of death.' While cut marks, secondary burials and burnt bones brought forward by the author could be explained as easily within the context of burial ritual, the author suggests that they might be expressing situations of inter or intra community struggles. The evidence is interpreted in the context of both the underlying environmental and the superimposed cultural features: changes in diet and eventual adoption of food production, accompanying variations in lithic and ceramic technology, and increased monumentalization indicating increased territoriality and segmentation of the society. The author proposes a positive correlation between the process of space hierarchization through monument construction (earthen mounds), the monumentalization of death, the evidences of violent human interactions, and increase in production (processing cost, diversification of diet, domestication). The notion that segmentation of society plays a crucial role in interpersonal violence is furthered, and although the evidence of violent interactions itself is less than ideal, a strong case is built on the bases of the archaeological evidence.

CONCLUSION:

As a framework for this volume, I defined Mesolithic societies as sedentary or semi-sedentary prehistoric hunter-gatherers with no temporal or geographical limitations usually associated with this term, allowing for comparisons between temporally and geographically remote regional groups. While the number of societies presented could have been much larger, the articles in this volume present a number of different approaches, focuses and expertise. What seems to unite them is the call for minute examination of osteological evidence and broad understanding of contextual data.

Returning to the question we asked at the very title of this introduction: "is there a common pattern of violent interactions specific to sedentary hunter-gatherers?," we have to answer that in general Mesolithic societies differ little in the amount of violence from other small-scale societies. Where evidence of violence amounts to conflict between distinct groups (warfare), it is temporally restricted and dependent on local histories. In that respect it does not differ from any other period and any other form of subsistence pattern or social organization. While sedentism and territoriality could have played important roles in an increased incidence of warfare, the beginning of conflict between groups as defined by Kelly (2000:21), is grounded "in [the] application of the principle of *social substitutability,*" and cannot be placed in the Mesolithic. Furthermore there is no evidence that the Mesolithic was either more or less violent than other periods of human (pre)history.

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