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Archaeology, Cultural Anthropology, and the Origins and Intensifications of War

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We are now well into the second decade of archaeology's discovery of war. To judge from the recent acceleration of publications and research topics, it seems certain to become a major field of study (Bray 1986; Carman 1997; Carman and Harding 1999; Martin and Frayer 1997; Owsley and Jantz 1994; Rice and LeBlanc 2001; Tkaczuk and Vivian 1989). The question is no longer whether, but whither, the archaeology of war? How will archaeological theory and findings develop? How will they relate to established interests in cultural anthropology? What new issues will archaeology raise? The potential theoretical impact of archaeology is great. As we see in this volume, within and across regional sequences there is tremendous recoverable variability—in whether there was a lot of war or little to none, ranging from raids to imperial conquest, and in clearly distinctive phases of military practice. That military variability goes along with variation in material circumstances and social and political structures, providing abundant raw material for theory on the causes and consequences of war. While ethnography remains much richer in coverage and detail, especially in non-material realms, archaeology has the advantage of very long time spans, compared to the usual ethnographic blink of an eye.

Although a great many ideas are raised in this volume, the over-arching concern is the connection between war and political consolidation. I will come back to that in closing. But this chapter goes in a different direction. While all the cases in this volume focus on prehistoric situations where archaeology shows war as unmistakably present, this chapter is concerned with two transitions: from the absence to the presence of war, and from prehistory to history. I argue two positions: ethnographic reports over the past five centuries do not represent the intensity of war in humanity's far distant past, and war as a cultural practice did not always exist. This is no assertion of some utopian idyll, of primeval flower children. Clearly, evidence shows interpersonal violence in some very early human remains, and collective lethal violence against other groups—war—has always been a possibility. Perhaps mammoth hunters had problems with each other; maybe Neanderthals and Cro-Magnons really did not get along. We do

not know. But, I will argue, the preconditions that made war likely were lacking for most of humanity's really ancient history. If we stick with evidence rather than supposition, war was absent in many places and periods, it became much more common over time (although not in a straight line of increase), and in most very early archaeological traditions there are no signs of it at all. There was a time before war (also see Kelly 2000).

These claims are very controversial. In the past decade, the most prominent work by an archaeologist is Lawrence Keeley's *War Before Civilization: The Myth of the Peaceful Savage* (1996). This is a major book. It is the best, most comprehensive treatment ever of the *practice* of war by non-state peoples, a long overdue replacement for Turney-High's (1971) *Primitive War*. It is an effective antidote to the idea that such war was a harmless ritual, although whether this message will penetrate those non-academic circles where such misconceptions still run is another question. And it has been a great stimulus to a developing archaeological focus on war.

But there are problems with aspects of Keeley's book and other recent publications, most notably LeBlanc's, with Register (2003), *Constant Battles: The Myth of the Peaceful, Noble Savage*, Guilaine and Zammit's (2005) *The Origins of War: Violence in Prehistory*, and Otterbein's (2004) *How War Began*. Three important general issues are how the anthropology of war has been portrayed, the theoretical significance of Western contact in affecting indigenous warfare, and above all, the impression that has been given about archaeological evidence for war throughout the prehistoric record.

The first section of this chapter delineates the issues as framed by Keeley, LeBlanc, Guilaine and Zammit, Otterbein, and others. The second discusses the anthropology of war, and why the issue of Western contact has risen to prominence in that literature. The third, the substantive core of the chapter, is a preliminary overview of the archaeological record on the inception of war in many areas around the world. The fourth suggests reasons for the origin, spread, and intensification of war, including the impact of Western contact in Amazonia, and with special reference to the Yanomami.

The Issues

War Before Civilization is a general indictment of the anthropology of war. Keeley (1996; and see Keeley 2001:332, 342) claims that anthropologists have largely ignored what he calls "primitive warfare" (1996:4, 163), and have misrepresented it when they do discuss it, as "safe and ineffective" (1996:170), "undangerous, unserious, stylized, gamelike" (1996:41), and proposing "that nonstate societies were commonly pacifistic" (1996:25). He adds that a "handful of

social anthropologists have recently codified this vague prejudice into a theoretical stance that amounts to a Rousseauian declaration of universal prehistoric peace" (1996:20). Keeley identifies me as a prime example of these tendencies (1996:20–22, 163, 203).

More consequential than what he has to say about anthropologists is the image he creates about the ubiquity and intensity of warfare in the archaeological record. I will not dispute here his characterization of ethnographic reports for the past couple of centuries. There is no doubt that the vast majority of non-state societies practiced war, and war that produced high mortality over time. I do believe the *frequency* of war has been systematically inflated in standard data sets such as HRAF. Ember and Ember (1997:5) claim that 73 percent of non-pacified pre-state peoples make war constantly or every year—a remarkable proposition, but that is a subject for another investigation. More to the point for this volume, I *will* take issue with the projection of the ethnographic present throughout the archaeological past, the idea that recent measures of war by non-state peoples are normal for non-state peoples through prehistory.

Keeley's position on this point is clouded by ambiguous phrasing, as in this passage:

> [N]othing suggests . . . that prehistoric nonstate societies were significantly and universally more peaceful than those described ethnographically. The archaeological evidence indicates instead that homicide has been practiced since the appearance of modern humankind and warfare is documented in the archaeological record of the past 10,000 years in every well-studied region. (1996:39)

Significant *and* universal? *Anytime* within the past 10,000 years?

Keeley never categorically states that war goes back indefinitely in time. But readers *take this to mean* that war is the norm throughout our prehistoric past (Gourevitch 1996; Simons 1997). For example, readers of the *New York Times* learned:

> The wonder of Lawrence H. Keeley's "War Before Civilization" is not the eloquent case the author makes that war has been a terrible thing ever since people started killing one another shortly after they first began to walk the earth. The surprising thing is that he has to make such a case in the first place. (Lehmann-Haupt 1996)

That interpretation is supported by quotes from Keeley such as, "War is something like trade or exchange. It is something that all humans do" (Pringle 1998:2040). Keeley also dismisses the idea that Western contact played a crucial role in the warfare ethnography records, saying it "merely brought some new

weapons to fight with and new items to fight over," and claims that theorists who stress the importance of contact "deny the legitimacy of ethnography altogether" (1996:21).

Similar positions are advocated by others following Keeley's lead. LeBlanc and Rice (2001:5) speak of "a general avoidance of the topic" of war in anthropology. Walker (2001a:573) goes from noting the genuine lack of research on domestic and other "internal" violence among non-state peoples to imply that few anthropologists have studied war; and claims that "[t]he search for an earlier, less-violent way to organize our social affairs has been fruitless. All the evidence suggests that peaceful periods have always been punctuated by episodes of warfare and violence" (2001a:590). LeBlanc (1999:10–11) asserts that "the actual likelihood of there being a prehistoric interval of several hundred years' duration without any warfare seems small," and follows Keeley's dismissal of the impact of Western contact, stating "the colonial impact explanation seems close to being laid to rest." Several authors register varying degrees of skepticism about the significance of the Europeans' arrival for indigenous warfare (Bamforth 1994:95–97, 111; Lambert 2002:208; LeBlanc and Rice 2001:6; Walker 2001a:574).

Keeley's position on the ubiquity of prehistoric warfare has been affirmed and taken to a new level in LeBlanc with Register (2003). This book focuses more on archaeology, and is less critical of cultural anthropologists' research, though in passing it rejects an emphasis on European contact (2003:6). Its theoretical point is that the idea of non-state peoples being conservationists is a myth, arguing that population growth and resource depletion is what has made war so common—it is war for food (2003:9). That hypothesis is not relevant to this chapter. What is very relevant here is the assertion of the near universality of war throughout the archaeological record (see also LeBlanc, this volume).

> When there is a good archaeological picture of any society on Earth, there is almost always also evidence of warfare. . . . We need to recognize and accept the idea of a nonpeaceful past for the entire time of human existence. Though there were certainly times and places during which peace prevailed, overall, such interludes seem to have been short-lived and infrequent. . . . I realized that *everyone* had warfare in *all* time periods. . . . (2003:7–8, emphasis in original)

The editorial lead in a magazine exposition of his thesis reads: "Humans have been at each others' throats since the dawn of our species" (LeBlanc 2003:18).

Guilaine and Zammit (2005:ix–x, 20–22, 236–240 [French original 2001]) follow Keeley in emphasizing prehistoric violence, and portraying archaeology and anthropology as having artificially pacified the record. They do not assert that *all* prehistoric peoples had war, but argue that it was very common. They

focus on Europe, especially France, and provide an almost overwhelming compilation of detailed brutality. Although the great majority of their evidence is from the Neolithic and later, they extrapolate this violence into earlier times where such evidence is lacking, with reasoning such as this: "The theory that warfare occurred in the Upper Paleolithic societies of the West seems entirely plausible, in view of the constant levels of aggression displayed by present-day hunting populations such as the American Indians" (2005:21). This is precisely the type of extrapolation this chapter argues against.

Now this debate has been joined, and complicated, by Otterbein (2004:10–15, 41–43, 98, 177–180, 199), a founding figure in the anthropology of war, who argues that there were two sociological starting points of war. The following is a very simple version of a complex argument: Raiding and ambush were a natural outgrowth of male-centered, big-game hunters who were already organized for cooperative killing. Such war existed for millions of years and was especially common in the Paleolithic. In some areas, a later shift to broad spectrum foraging broke up that pattern, and war disappeared. The absence of war provided the necessary stability for plant domestication to occur—domestication could not happen with war. Settled farmers could later take up war, for instance to control trade routes, but where that war was practiced, evolution to a state would not occur. For a state to emerge, war must be absent prior to the consolidation of "maximal chiefdoms," which are "inchoate states," although more typical chiefs use violence in internal factional struggles, repressions, and feuds. When states emerge, they reinvent war, and war spreads through secondary formations exposed to them. Among still-warring game hunters, domesticates can spread from their original centers, and long-time farmers can learn war by interaction with hunters, thus explaining the warfare of many non-state agriculturalists. Underlying this theory is Otterbein's long-established position that social organization for conflict is more important than conflict over scarce resources in generating war.

Otterbein's position is very different from those of Keeley and LeBlanc, and overlaps with mine, in that it recognizes great areas were without war for very long periods. The big difference between us is that he sees war as practiced by big-game hunters, and ending with a shift to more sedentary foraging, while I will argue the reverse: that war seems absent in the Paleolithic, and emerges first with more settled foragers (although most of them are peaceable). Evidence regarding that distinction will be noted as it comes up.

In asserting the deep antiquity of war, both Otterbein and LeBlanc rely heavily on the work of Richard Wrangham and others (see Wilson and Wrangham 2003; Wrangham 1999) on intergroup violence among chimpanzees. LeBlanc and Register (2003:86) argue: "If chimpanzees have a form of warfare, then it

can be presumed that our forest-dwelling ancestors ('early hominids' or proto-humans) probably did too, because humans and chimps are so similar."

Intergroup violence among chimpanzees, and its implications for humanity, is a very large topic, which is evaluated in a book I am currently preparing. In that work I argue that the number of chimpanzee intergroup killings has been exaggerated, that where lethal encounters occur, they plausibly may be attributed to circumstances created by recent human activities, and that there is no basis for positing behavioral continuity of in-group/out-group hostility and killing in the transition from ape to human. Those issues cannot be discussed here, except to opine that chimpanzee behavior provides little support for the practice of war in humanity's distant past.

After discussing the chimpanzee material, LeBlanc and Register raise an idea with major significance. "If warfare has been part of the human condition for more than a million years (or six million years, depending on the start date), we just might be selected for behaviors that make us warlike" (2003:219–220). Although they go on to qualify the supposition, that idea succinctly identifies why the antiquity of war is such an important issue. Already, those who argue for a human biological propensity for war are citing Keeley in support (Fukuyama 1998:26; Gat 2000:165; Low 2000:213; Pinker 2002:56–57; Wilson 1998:341; Wrangham 1999:18)—despite the fact that Keeley (1996:157–159) himself disavows biology as "irrelevant." Now the circle is complete. While most archaeologists probably would agree with Underhill (this volume) that it is more productive to focus on the causes and consequences of war, rather "than endlessly debating whether inter-societal violence is an inherent part of human nature"—that debate is inescapable for archaeology. Like it or not, the archaeological record is central to this perennial question in Western culture.

The Anthropology of War and the Issue of Western Contact

Anthropological Visions

Whether archaeology, as a field, has willfully turned away from evidence of war, I leave to archaeologists to evaluate. In my reading, it seems variable. Clearly there was resistance to acknowledging war in the American Southwest (Solometo, this volume), but the issue of prehistoric war in the American Southeast has been actively discussed for many years (Dye, this volume). Without question, however, there is much more widespread interest in the topic today than even a decade ago. My first concern here is not with archaeology, but how Keeley characterizes the *cultural* anthropology of war of the past forty years.

To start, we can take up the claim that anthropology has ignored war. That was indeed true—forty years ago (Ferguson 1984a:6), not more recently. Compiling a bibliography of substantial anthropological discussions of war (including archaeology) in 1987, we quit around 1,500 citations, because there was no end in sight (Ferguson with Farragher 1988). The literature has grown by leaps and bounds since then. Even Otterbein's (1999) history of anthropological research on war was correctly criticized by Sponsel (2000) and Whitehead (2000) for ignoring entire areas of current research into war and other collective violence (and see Ferguson 2003). Tellingly, some international relations theorists are now looking for models of war in anthropology. Here is how one prominent political scientist characterizes anthropological research:

> For decades, anthropologists have been amassing a theoretically rich, empirically substantial, and methodologically self-aware body of statistical and case-study research on the relationship between war and culture in stateless societies and pre-industrial anarchic systems. (Snyder 2002:11)

Has anthropology portrayed war by non-state peoples as harmless, just a ritual or a game, with few casualties? Again, this once was true, long ago. Benedict (1959), Chapple and Coon (1942), Codere (1950), Leach (1965), Malinowski (1941), Naroll (1966), and Newcomb (1960), did say that primitive combat was largely a ritual without great consequence. Several other early writers, however, portrayed war as deadly serious struggle involving vital resources (Hunt 1940; Jablow 1950; Lewis 1942; Secoy 1953; Swadesh 1948). Otterbein (1999:794–799)—who critiques Keeley's history of the anthropology of war as inaccurate, and for having created his own "myth of the warlike savage" (1997)—does acknowledge a "myth of the peaceful savage," persisting as late as 1980. But besides those early citations I just listed, he provides no more recent cases of anthropologists promulgating that myth except to question the peaceable images about "Bushmen, Pygmies, and Semai" (1999:795–798). Each of those cases is a major debate in itself, and cannot be considered here.

In the present, two anthropological specialists on war and peace, Reyna (1994:55–56) and Sponsel (2000:837)—and only those two to my knowledge—argue that collective violence by comparatively egalitarian non-state peoples should be separated conceptually from the category of war, as defined by the practice of more centralized and hierarchical polities (though not necessarily states). Since the mid-1960s if not earlier, the vast majority of anthropological writers have agreed that among non-state peoples, war was very common and very consequential, both in casualties and in its impact on cultures. Few if any would fit the characterization on *War Before Civilization*'s dust jacket, that "for the last fifty years, most popular and scholarly works have agreed that prehistoric

warfare was rare, harmless, unimportant, and . . . a disease of civilized societies alone."

As for my own view on war (since this has been made an issue), my first published research (Ferguson 1984b:269), on Northwest Coast warfare, was explicitly in support of Swadesh's (1948) view of it as lethal struggle for material gain, against Codere's (1950) view that it was ceremonial with few casualties.

> Northwest Coast warfare was no game . . . war was deadly serious struggle. Sneak attacks, pitched battles, ambushes, prolonged attritional campaigns, treacherous massacres, sporadic raiding—these were facts of life from before contact to "pacification" in the 1860s. . . . Warfare was, in large part, a contest over control of valuable resources. . . . Wars fought solely to capture ceremonial titles or crests seem to have been rare, despite the prominence given to this motive in ethnographies. (Ferguson 1983:133–134)

That work also cites archaeological evidence to claim that a war complex went back to about 1000 B.C.—although now I would push that to 2200 B.C., at least (see below). In discussing the theoretical significance of Western contact (1990:238) regarding war by Amazonian peoples, I wrote: "*It is an indisputable fact that warfare existed in Amazonia before the arrival of Europeans*" (emphasis in the original); and for North and South America, "Even in the absence of any state, archaeology provides unmistakable evidence of war among sedentary village peoples, sometimes going back thousands of years" (1992:113). Finally, prior to the publication of *War Before Civilization*, I (1997) had completed an essay all about evidence and theory regarding war before states.

Contact

So, what is all this talk about the critical role of contact with states, especially Western states? I was a graduate student at Columbia University in the 1970s, which was then the hotbed of anthropological theorizing on war (see Ferguson 1984a). In endless discussions, it became increasingly apparent that existing explanations of war were inadequate. They were overly abstract, and detached from its real practice. Ecological hypotheses looked best from a distance, breaking down on close inspection of behavior (a point relevant to recent theorizing in archaeology, where something very similar to the old cultural ecology has reemerged; see LeBlanc, this volume). Social structural theories went around in circles—does war create social patterns such as patrilocality, or vice versa? In the absence of compelling theory, there was a resurgence of the tautology that ruled anthropological theory for decades—and is still quite current—"they fight because it is part of their culture." At the same time, there was growing recognition of the importance of a historical perspective in general, of the critical

importance of bringing in colonialism that had so often been ignored, and an awareness of the burgeoning ethnohistorical and regional literatures that matter-of-factly documented war as being highly responsive to changing contact circumstances. But history was paradigmatically excluded from anthropological theory on war. The goal of "Blood of the Leviathan" (Ferguson 1990) and *War in the Tribal Zone* (Ferguson and Whitehead 2000, orig. 1992) was to focus attention on historical connections, to begin theoretically mapping their dimensions and issues, and by doing so, create the foundation for a more realistic basis for explaining war.

Among anthropologists, there are significant differences in how the impact of contact has been conceptualized. Blick (1988)—who made the first broad statement on the issue—proposes a quantitative disjunction, between limited revenge fighting before contact and genocidal attacks after. I do not agree with either his general characterizations or the idea of a qualitative break. "Revenge" is more an idiom than a cause of fighting (Ferguson 1995a:354; cf. Ferguson 2005). Exterminative slaughter sometimes happens among non-state peoples, as in the northern Great Plains (Bamforth, this volume), and is remarkable even after contact, as in the rather extreme case of the Maori (Allen, this volume).

The position advocated by Neil Whitehead and myself is that Western contact generally transformed, frequently intensified, and sometimes generated war in extensive areas we call "tribal zones." For that reason, it is a mistake to uncritically project historically recorded war patterns into prehistory. Any effort to explain historic warfare should include historic conditions, although these by no means eliminate local factors engendering violence, or imply that there was no war before contact.

> For the anthropology of war, these findings suggest the need to reconsider current assumptions about the causes and practice of war in nonstate societies, which have been formed without reference to the contact-related variables identified here. What has been assumed to be "pristine" warfare now seems more likely to be a reflection of the European presence. This does not mean that nothing can be known about war outside the influence of Europe or other state systems. Archaeological data and judicious use of early reports from some situations can provide such information. The point, rather, is that we cannot discriminate precontact war patterns without a theoretically informed sensitivity to the influences of contact even in its earliest phases. (Ferguson and Whitehead 2000:27)

This is not much different from positions taken by some archaeologists dealing with this issue. Bamforth's (1994:112–113) thoughtful presentation of evidence of intense warfare on the prehistoric Great Plains concludes: "There is no doubt

that contact period processes had profoundly negative effects on indigenous peoples and that an examination of those effects is necessary in any attempt to use post-contact information to illuminate precontact ways of life." Lambert's (2002:208–209) overview of North American archaeological evidence for war stresses the value of archaeology because it "focuses on a time before Western European expansion, colonialism, and other processes that altered the character and trajectory of many indigenous American societies." Walker (2001b) has detailed the massive, early and continuing, disruption and destruction of Native American people and societies on the Spanish borderlands of Florida. And even with all the prehistoric warfare in North America, a recently described study by Walker (Lucentini 2002:A9) of over 4,500 skeletons:

> found that those from after Christopher Columbus landed in the New World showed a rate of traumatic injuries more than 50 percent higher than those from before the Europeans arrived. "Traumatic injuries do increase really significantly. . . ." These findings suggest "Native Americans were involved in more violence after the Europeans arrived than before. . . ." Walker said that although part of the increased injury rate doubtless stems from violence by whites themselves, it probably reflects mostly native-on-native violence.

All these comments relate to North America. The two chapters in this volume that focus directly on the impact of Western expansion make the point even more forcefully. The Maori were hit with the full range of contact effects: new plants, animals, economic opportunities, tools, and above all, guns. They underwent massive sociopolitical change and reworking of cultural themes, leading to an explosion of warfare that killed about one-third of their population in thirty years (Allen, this volume). Peoples of East Africa were supplied with guns for slaving and the ivory trade, leading to not only massive mortality in raids, but forced displacements and ecological destruction, along with famines and diseases made worse if not caused by these changes (Kusimba, this volume).

The point of tribal-zone theory is that *everywhere* in the world where colonialism is impinging, recorded warfare cannot be taken as representative of precontact violence until and unless historical factors possibly encouraging collective violence are investigated. That is a major difference between the approach advocated here and that of Keeley, LeBlanc, Guilaine and Zammit, Otterbein, and many others, who continue to describe ethnographically reported warfare as if it represents an earlier phase of societal evolution. It may be that war is not dramatically increased by contact—the opposite may occur—but a lack of impact cannot be assumed.

It is unfortunate that the question addressed in some recent archaeological

discussions is whether or not war existed before Western contact, or before states existed. To my knowledge, no one in contemporary anthropology suggests that it did not. Further, since I do not posit a qualitative break in the form of war before and after contact, I am not surprised to find prehistoric situations sometimes match or exceed the violence after contact. North American archaeology (and maybe beyond) seems inevitably headed for a conference titled something like "1250 A.D.," to comparatively examine the massive and widespread violence in the three centuries or so bracketing that date. But attention should also be given to the period from 1450 to 1550, or so, to ask if there was a lessening of actual violence in between this time and the first effects of Europeans. Only archaeology can reconstruct levels of violence from before and through contact, and could investigate—for instance—whether changing levels of violence accompany Western goods filtering in through trade networks, as MacDonald (1979) did for fort-building associated with protohistoric overland trade to the Pacific Northwest Coast.

Origins

But the archaeology of war must be very careful not to conflate *late* prehistory in North America or anywhere else with *all* prehistory. What sometimes seems lost in recent assertions that prehistoric warfare could be pretty terrible is the most significant question of all: can we identify an origin of war, or has it always been with us? Many have concluded that war is a relatively late human invention (Childe 1942; Ferrill 1985; O'Connell 1995; Roper 1969, 1975; Van der Dennen 1995:180–214; Vencl 1984:120–121). I am currently following in their footsteps, working on a global survey of archaeological evidence for war, from the earliest indicators up to the advent of historical records.

So what if no signs of war are found in early material? It is often said that "absence of evidence is not evidence of absence." Yet war leaves recoverable traces. Indicators of violence, or collective violence, in settlement and skeletal remains, weapons, and art have been reviewed elsewhere (Ferguson 1997:322–326; Lambert 2002:209–211; LeBlanc with Register 2003:58–64). By now they should be very apparent to readers of this volume. Yes, the chapters do raise cautions about evidence. Generally, the cultural presence of war is more visible than remains of actual violence (Arkush, this volume). Fortifications, nucleation, and movement to inaccessible locations all entail costs, and may be forsaken when war exists but only at low levels (Solometo, Allen, this volume). Even where war is intensive, central areas of cohesive war-making social groups may lack obvious signs of war, meaning researchers should look around the edges (Bamforth, Arkush, Connell and Silverstein, this volume). Skeletal material may be abundant, but unexamined for trauma, as in early China (Underhill, this volume). Yet when skeletons

are examined, war signs show up, as in the North American Southeast (Dye, this volume), and in New Zealand, where violence is very clear in a total sample of fewer than a hundred (Allen, this volume).

These and other concerns understandably make many archaeologists reluctant to conclude that there was *no* war when they find no *indications* of war. Caution is always merited for any given case. What I argue in this chapter is that the early absence of evidence is not rare. It is a global pattern, and as such, gains probative weight. What is equally telling, is that around the world signs of war eventually do appear, clearly, and then continue through time, even when there is no corresponding improvement in the recovery of all physical remains. The complete absence of war indicators is followed by their clear presence. Looked at from these perspectives, I conclude, this absence of evidence should be seen as *negative* evidence. The simplest explanation is that war develops out of a warless background.

The next section is a summary of this work, up to its current point. Three caveats are in order. First, the research broke off before completing the west coast of North America, and several major world areas remain to be done. Second, these are preliminary findings; more research and reconsideration is anticipated for all regions. Third, this summary is very stripped down, leaving out details, dating issues, and most discussion of environmental, demographic, social, trade, and political conditions. All that will come (I hope) in a longer work. This summary focuses exclusively on evidence regarding the inception and early development of violence.

A Survey of Evidence for the Earliest Warfare

The Earliest Signs of Violence

Popular notions of the antiquity of war still seem to reflect Ardrey's (1961; 1966) dramatic portrayals, based on Dart's (1957:207) "blood-bespattered, slaughter-gutted" view of our past. Roper's pioneering survey of Pleistocene and Upper Paleolithic evidence contradicted that view, although she concluded "sporadic intraspecific killing probably took place" in the latter period (1969:448). Since then, additional, previously accepted cases of violence have been reconsidered and rejected (Binford and Ho 1985; Boaz and Ciochron 2001; Brain 1981; Ury 1999:34; White and Toth 1989, 1991). Not challenged (to my knowledge) is the Paleolithic individual Skhul IX circa 36,000 B.P.—once but no longer thought to be Neanderthal (Arensberg and Belfer-Cohen 1998:312)—from Mount Carmel, with what appears to be a spear thrust through its leg and hip (McCown and Keith 1939:74–75). Beyond Roper, at least nine sites from Europe to In-

dia, from 34,000 B.C. to 8,000 B.C. include signs of violence (Bachechi et al. 1997:137; Guilaine and Zammit 2005:50; Keeley 1996:37; Wendorf and Schild 1986:62, 74) (not including Jebel Sahaba or North America, below). At least one of those is Mesolithic (Sharma 1963). These finds in the growing corpus of skeletal remains essentially support Roper's earlier view—scattered interpersonal violence, but in circumstances that could include accidents, non-lethal intragroup conflicts, individual homicides, or executions. Neanderthal remains complicate the situation. There is much skeletal trauma, but usually not like that produced by combat (Berger and Trinkaus 1995), with two exceptions: the partially healed cut mark on the rib of Shanidar III (50,000 B.P.+), which Trinkaus and Zimmerman (1982:62, 72) call "the oldest case of human interpersonal violence and the *only* possible one among the Neandertals"; and the recently identified healed fracture from a blade-shaped object on the top of the skull of St. Cesaire 1, circa 36,000 B.P. (Zollikofer et al. 2002). There seems to be at least one clear case of cannibalism from 100,000 B.P. to 120,000 B.P. (De Fleur 1999:19), and even the earlier *Homo antecessor* circa 780,000 B.P. appears to be cannibalistic Fernandez-Jalvo et al 1999). The famous Krapina remains were judged *not* indicative of cannibalism by Trinkaus (1985) and Russel (1987a, 1987b), but White (2001) argues they are.

Yet cannibalism does not necessarily mean intergroup violence. The clear-cut case of Anasazi cannibalism (below) is *not*—all agree—indicative of war. And since the position of Neanderthals as human ancestors seems increasingly doubtful (and thus too *their* "antecessors" [White 2000:499]), the relevance of Neanderthal behaviors for *Homo sapiens* is questionable. Regarding the replacement of Neanderthals by *Homo sapiens*, there is no physical evidence to support the often suggested scenario that it took place through violence, rather than through some other form of competition.

Given the limited number and completeness of early human or hominid skeletal remains, and the amount of trauma reported—some apparently human-induced, most not, some maybe—patterns of Paleolithic violence remain enigmatic. The volume by Martin and Frayer (1997; and see Walker 2001a; Zollikofer et al. 2002) makes a compelling case for the need to consider the currently neglected issue of intragroup violence, such as club fights or domestic abuse, as a source of skeletal trauma. Defleshing prior to burial or reburial also leaves peri-mortem nicks and cuts that have nothing to do with violence. Is it possible that some of the reported trauma came from intergroup violence? Certainly. It is equally possible that none of them did.

Other claimed evidence for Paleolithic homicide or war is European cave art. Three representations at Pech Mere and Cougnac have lines which appear to go up to or through human-like forms (Leroi-Gourhan 1968:325, 1982:50). To Ba-

chechi, Fabbri, and Mallegni (1997:136), who reproduce drawings of these figures, the lines suggest arrow shafts. To Kelly (2000:152–153), two of the images are "a portrayal of spontaneous conflict over resources," but by his definition, not war. For LeBlanc with Register (2003:5), they are "evidence of warfare." Guilaine and Zammit (2005:52–56) add other representations and see evidence of killings. For Otterbein (2004:71–73), they are proof of killings, and "the killings come at the peak of the hunting/warfare curve," although he then suggests they may represent an execution rather than battle.

Obviously, cave art is wide open to interpretation. Leroi-Gourhan (1968:323–325) sees them all as men "run through with spears." Giedion (1962:463–464) sees two of them as women, and one of unspecified sex, and concludes: "These figures depict no fight of man against man or of man against earthly foe. The 'arrows' that transfix the bodies of the masked or headless figures are magic projectiles." Either opinion is, of course, conjectural. For two reasons, however, I believe there is ample reason to question the consensus that these lines represent any sort of projectile at all. First, close examination of the representations (for good photographs and drawings, see Giedion 1962:462–467) shows that some lines are straight-ish, but others are decidedly curved or wavy. Compare this to the straight lines with exaggerated V-tips hitting large game, such as at Niaux (Giedion 1962:401–402). Second, in other cave art, from La Roche, Les Combarelles, and Abri Murat, similar lines go over, through, and around anthropomorphic figures, in ways that give no suggestion of projectiles (Giedion 1962:458, 459, 497). Perhaps the three representations are of a physical shooting, perhaps a shamanic one, or perhaps they are of something we cannot imagine. They do not prove the existence of human killings, and certainly not of war.

For evidence of collective violence, or war, the earliest accepted case remains the Nile Site 117, near Jebel Sahaba, very roughly estimated at about 12,000 B.P., where 24 of 59 well-preserved skeletons were associated with stone artifacts interpreted as parts of projectiles (Wendorf 1968:90–93; Wendorf and Schild 1986:818–824). This is a true outlier, without continuation, as that part of the Nile appears to have been abandoned soon after (Close 1996:47–50; Midant-Reyes 1992:63–64). The other exceptionally early evidence comes from northern Australia, where rock art suggests interpersonal violence between individuals or a few people from perhaps as early as 10,000 B.P., and group clashes from about 6,000 B.P. Here violence seems to continue thereafter (Tacon and Chippindale 1994).

Otterbein (2004:73–74) cites both cases in support of his theory that big-game hunters were warriors who gave up war as they moved into broad-spectrum foraging. The Australian case would support his association with big-game hunters, except that the art shows a progression from single fights to larger engage-

ments. Site 117 is a different story. These were semi-sedentary people, alternately labeled Epipaleolithic or Mesolithic. For several thousand years, people of the Qadan culture had relied on catfish, water fowl, and wild grasses on the Nile's broad flood plain. These were broad-spectrum foragers, not big-game hunters, and their turn to war seems to have occurred as the Nile cut a new gorge and eliminated the flood plain (Close 1996:47–50; Midant-Reyes 1992:63–64).

The Middle East and Asia

The Middle East

This part of the world is very well known through archaeological work. In the Mesolithic, the Natufian people, semi-sedentary hunter-gatherers from 10,800 B.C. to 8,500 B.C., left extensive remains, including 370 carefully analyzed skeletons. Only two indicate any sort of trauma, and neither those nor anything else suggests military actions (Belfer-Cohen et al. 1991:412, 420–441; Henry 1985:376). Roper followed her article on the Pleistocene and Upper Paleolithic with a survey of evidence in the Middle East (1975). She accepted the prevailing view that the first clear evidence of war is the initial wall of Jericho, circa 7500 B.C., although she notes that this is the sole evidence in the Levant region until the sixth millennium (1975:304–310). Since then, however, Bar Yosef (1986) has argued persuasively that this first wall seems intended for flood control, rather than defense. Even Keeley (1996:38) acknowledges the general absence of war evidence in the Early Neolithic, although he does not consider the significance of that fact, instead treating it as a curious oddity.

Roper (1975:310–312) surveyed 18 sites from the seventh millennium, and the existing claims for war in five of them. She concludes none show "conclusive evidence" for war. I agree, except for Catal Huyuk, in Turkish Anatolia (6,250–5,400 B.C.), where the sum of evidence—particularly burials with daggers and maces (which Roper does not mention)—does support its presence (Mellaart 1967:68–69, 207, 209). Three older Neolithic excavations from northern Iraq, however, post-dating Roper's survey, clearly indicate war. The earliest is Qermez Dere (8250–7700 B.C.), with maces and enlarged projectile points (Watkins 1992:65, 68–69); the others, from roughly a thousand years later, have, in one, a major defensive wall, and in the other, maces and skeletons in association with arrowheads (Kozlowski 1989:27; Lloyd 1984:33). I take this time and place as the origin of a pattern of regular warfare that has continued down to the present day.

Roper's (1975:317–324) survey indicates regional differences in the sixth millennium. Settlements around Israel and Jordan remain without signs of war, but southern Turkey and northern Iraq and Syria have several clearly fortified loca-

tions from 5900–5200 B.C., some of which, such as Hacilar II, were destroyed with other indications of attack (see Mellaart 1975:115–118). In the fifth millennium, unambiguous indications of war become common across and around Anatolia, by the mid-millennium forming a continuous strip from northern Iraq through southern Turkey. In contrast, the Ubaid people of the Mesopotamian plains did know war, as evidenced by maces, but remained without the fortifications, settlement destructions, and militaristic art later characteristic of that area (Roper 1975:323–328; Stein 1994:38–40). Around 4300 B.C., on the Anatolian coast at Mersin, there appears to be a true fort, rather than a walled village, which was destroyed after about a century and reoccupied by Ubaid people (see Mellaart 1975:126–129). From then on, "[f]ortifications became the rule rather than the exception" (Roper 1975:329). Signs of actual fighting remain rather limited in the (poorly excavated) transition to Uruk after 3800 B.C. (Wright 1986:335), but by the time of rival city-states a thousand years later, a variety of evidence indicates intense and frequent war (Jacobsen 1976:77–79, 224).

From the Nile to Harappa

By 4500 B.C. if not earlier, there were several interacting areas of urbanization and state formation reaching from Egypt to the Indus and into Central Asia (Lamberg-Karlovsky 1981; Rowlands et al. 1987; Tosi 1979). Along the Nile, leaving aside Jebel Sahaba, the initial development of war is not visible, given the absence of an early archaeological record due to erosions, and classical archaeologists' interests in later periods. Evidence begins around 4300 B.C., with settled farming villages. In one of the northern Nile traditions, Merimda peoples had pear-shaped, Mesopotamian-style maces. Far to the south, people of the Khartoum Mesolithic of the mid-late seventh millennium made stone disks which are similar to disk-shaped maces used in its later Neolithic (5600–4300 B.C.) That style mace was also found in a second agricultural (4000–3500 B.C.) tradition of the middle Nile, which was the center of Egypt's later unification. After 3500 B.C. the pear-shaped mace replaced the disk (Fage and Oliver 1975:499–510; Midant-Reynes 1992:92–94, 127–131, 193). How much actual fighting was going on is unknown, and remains highly controversial even for the later development of Egypt (Savage 1997). But clearly war was present, though whether it developed locally or via external influence, and when, cannot be ascertained.

East of the Caspian Sea was a variously named area I will refer to as Namazga, after its best archaeological sequence. Agriculture spread into this area sometime before 6000 B.C., and small, undefended farming communities grew up amidst much more numerous Mesolithic sites (Dolukhanov 1986a: 124, 128–129; Kohl 1981:ix). Indications of war—settlements with ditches and towers—appear only in the Chalcolithic (radiocarbon dates vary greatly), just before major

urban growth (Gupta 1979:56, 84–85). In the Bronze Age, from about 3000 B.C., large defensive walls become common, and by 2300 B.C. were spreading eastward along trade routes to Tajikistan and perhaps beyond (Kohl 1981:ix, xiv–xxii, xxix).

Further south, in the high country of Pakistan, early farmers were erecting village walls by 4000 B.C. (Miller 1985:39). To the east, down in the lowlands, at least some pre-Harappan settlements saw walls go up in their later phases, 3100–1900 B.C., and were subsequently occupied by people of the Harappan culture (Sankalia 1974:338, 342, 344, 357). For mature Harappa (2500–1800 B.C.), there is significant scholarly difference over the importance of war. Some see little, some a lot (for example, Jacobson 1986:160–162; Miller 1985:58). My reading of the evidence is that organized warfare was known, but less elaborated and practiced than in western or central Asia. In late Harappa, there are indications of intensifying warfare, although that evidence is accompanied by its own debates (Dyson 1982:421; Singh 1965:88–90, 121). But as Harappan civilization declined, evidence of widespread, very destructive wars becomes unmistakable, as narrated in the Rg Veda (Basham 1959:31–45; Singh 1965).

China and Its Environs

Like the Middle East, the Chinese archaeological record is massive, although, of course, mostly written in Chinese, and much of it has not been scrutinized for indicators of war (see Underhill, this volume). From the earliest Neolithic Peilikang phase, many villages and burials have been excavated, with no signs of violence. After 5000 B.C., distinctive regional Neolithics emerge. Among the Yangshao in the central Yellow River valley, many villages have surrounding ditches. Excavators interpret these as defensive, and some also have palisades. At least one late Yangshao village is surrounded by a rammed-earth wall (Chang 1986:87–90, 107–116; Needham and Yates 1994:241–242; Underhill 1989:229–230). One Yangshao skeleton has an embedded arrowhead, and another has been found from the Dawenkou Neolithic farther east, circa 5000 B.C. Underhill calls this "the only convincing skeletal evidence [for warfare] from the pre-Longshan period" (Underhill 1989:231), although again, few skeletons have been studied.

Regional Neolithics developed and expanded throughout the fourth millennium. By 3000 B.C., many distinctive features of Chinese civilization appeared in the extensive Longshan interaction sphere. In some regions of the later Longshan, there was war. Five locations after 2600 B.C. were surrounded by rammed-earth walls, with gates and guardhouses (Chang 1986:234–288; Liu 1996:264–272). Walls appear around settlements in Inner Mongolia, Yangzi, and Shandong regions around the same time, including a huge center in the latter, with major supplies of arrowheads (Chang 1986:248; Underhill 1994:202).

Even more unmistakable signs of war appear in Henan: skulls with scalp marks, bodies thrown down wells, new and deadlier weapons, etc. (Chang 1986:270–271; Liu 1996:264; Underhill 1989:231–235, this volume). Yet signs of war still show great variation, abundant in some areas, absent in others. Then comes the first known state, Erlitou (although Underhill, this volume, suggests earlier states may be found). As the Bronze Age proceeded to the Shang, war became a way of life (Yates 1999). Otterbein (2004:161–166) questions most of the evidence for war before Erlitou, seeing walls as efforts at flood control, and other indicators of violence as indicating internal political rivalries.

On the Korean peninsula, the early record is slim. I found no information bearing on war for the Chulman culture, Mesolithic people who became farmers. An agricultural Bronze Age people came in sometime between 1500 B.C. and 700 B.C., bringing Manchurian-style weapons (Barnes 1999:26, 160–161; Kim 1978). The Japanese record is better. Remains of sedentary complex hunter-gatherers, the Jomon, date from 11,000 B.C. onwards, in later times practicing some cultivation. Around 300 B.C., wet rice cultivators came to Kyushu from Korea, fusing with local cultures to become the Yayoi. From the beginning, the migrant villages showed numerous, clear, impressive defensive features and weapons of war. Of some 5,000 Jomon skeletons, ten show signs of violent death. Of about 1,000 Yayoi, there are more than 100 victims. Accounts from the contemporary Chinese court also document intensive war in Japan at the time of this transition (Barnes 1999:168–171, 218–220; Farris 1998:37–41, 1999:49–51; Imamura 1996:179–185).

Europe

Vencl, who more than anyone else has directed attention to archaeological signs of war in Europe (1984), concludes that there is no conclusive evidence of war up to and through Europe's Upper Paleolithic (1999:58), as does Dolukhanov (1999:77), despite the extensive archaeological record and the considerable social complexity of that period. Keeley (1996:37), LeBlanc with Register (2003:14), and others, however, do argue that there is persuasive evidence for war in the Upper Paleolithic in skeletal and other remains from Czechoslovakia. This is a critical point for their general arguments about the antiquity of war. Comparison of their claims with the empirical record, however, demonstrates that they are unsupported.[1]

As the herds of reindeer disappeared, a more settled forager's life developed through the ninth millennium. At a number of these Mesolithic locations, individual remains have been found with signs of violence, even killing. Constandse-Westerman and Newell (1982:75) surveyed Mesolithic skeletal material, and found of 59 mostly complete skeletons, five are associated with a "projectile,"

but conclude that other traumas are probably accidental. Three of the projectile instances are from around 4100 B.C., which is very late for Mesolithic, and well after the general shift to war discussed below. Guilaine and Zammit's (2005:75–77) survey shows eight Mesolithic sites in Europe, and four outside it (including Jebel Sahaba), but several of these have more than one burial with projectile injuries. One case worth special mention is the enigmatic "nests" of some 37 skulls, ochered and several with large depression fractures, from Ofnet in Bavaria circa 5500 B.C. (Frayer 1997), although Chapman (1999:105) concludes that these "indicate ancestral relations rather than a bloody incident." The separation and special interment of the skulls is not itself evidence of war, as such special treatment of one's own dead heads is common both archaeologically and ethnographically (Wright 1988). But the depression fracture seems difficult to explain as other than from violence. In sum, while it seems likely that some war was practiced in the European Mesolithic, many finds are ambiguous, and/or of single individuals, and still represent a small fraction of the osteological record (Chapman 1999:105–106). So while Vencl (1999:59) sees a broad, general shift toward war, Dolukhanov (1999:80) sees more limited and highly localized developments in response to particular conditions.

Northern Europe

After around 5500 B.C., agriculture spread across central Europe from the Black Sea to Holland. For an extensive time and area, most investigators see no indications of violent conflict, but rather exchange and fusion, among the scattered LBK farmers and Mesolithic people all around them (Barker 1985:139–147; Dennell 1985:135–136; Thomas 1999:150), although Keeley (1992; 1997) challenges that view. From around 4500 B.C., agriculture of the Tripolye culture spread, similarly without signs of violence with local Mesolithics, through eastern forests and steppes (Dolukhanov 1986b:117, 1999:81). But peace was not to last. By late LBK times, in its farthest penetration west (Belgium), palisades were put up around 4350 B.C. (Keeley 1997:312–314; Keeley and Cahen 1989). Enclosures of settlements became common across west and central Europe around the end of LBK, circa 4000 B.C. The purpose and meaning of enclosures is hotly debated, but a number incorporate clearly defensive features (Vencl 1999:68–69; Whittle 1985:85–86, 1988:1–6). Most significantly, at least four and possibly more later LBK sites, perhaps about 5000 B.C., include mass burials with unambiguous signs of violence—slaughters (Vencl 1999:60–64). To the east, Tripolye had fortifications, maces, and skeletons with trauma between 4400 B.C. and 3810 B.C. (Dolukhanov 1999:82).

Thus, across much of northern Europe, war shifted from a scattered and rare phenomenon to a common occurrence between 5000 B.C. and 4000 B.C. It

never ceased thereafter. New, distinctive regional cultures developed in many areas in the fourth millennium. These people began building hill-forts and burying their dead with battle-axes (Dolukhanov 1999:83; Schutz 1983:75–77; Vencl 1999:66–70). Similar developments lagged by centuries in the farther reaches of northern and western Europe. In Denmark, 22 skeletons from a Mesolithic cemetery from about 4100 B.C. include one with an apparently lethal arrow wound and two with healed fractures suggestive of serious violence. Two centuries later agriculture had replaced hunting and gathering. By 3500 B.C., they had all the defensive features and battle-axes of central Europe (Albrethsen and Petersen 1976:14, 20; Andersen 1993:100–103; Price 1985:351). In southern England, major reliance on agriculture was dominant by roughly 3500 B.C., and ramparted villages were common by 3000 B.C. Around 2600 B.C., the heavily palisaded hill-fort of Hambledon Hill was destroyed in what appears to be fierce combat (Bradley 1991:51; Mercer 1988:89, 104, 1989, 1999).

Southern Europe

Closer to the Mediterranean, transitions to agriculture and violent conflict seem to be more variable, and sometimes obscure. Parts of Greece had a full Neolithic package by 6000 B.C. In Macedonia, early small-farming communities were undefended and near to water in low lands. By the final Neolithic about 4500 B.C. and 4000 B.C., settlements with formidable enclosures began to appear in elevated, defensible locations (Barker 1985:71–72; Kokkinidou and Nikolaidou 1999:92–96). In southern Italy, hundreds of Neolithic settlements on the Tavoliere plain and elsewhere were ringed by very substantial ditches from before 5000 B.C. to 3000 B.C. (Whitehouse 1987:358–359), representing perhaps the first pattern of regular warfare in Europe. However, further north in Italy over this time, settlements of farmers intermixed with hunter-gatherers, without such ditches (Barker 1985:65–67).

Iberia has dramatic evidence, but a complex pattern. The final phase of a long transition to a Neolithic life occurred by the third millennium. One cave at Alava, Spain contains remains of about 300 individuals, apparently deposited over time between 3800 B.C. and 2800 B.C. Nine have embedded arrow points, and many loose points are in the fill (Guilaine and Zammit 2005:152–154). A massive fortified site existed at Los Millares from 2500 B.C. to 1800 B.C.—but what enemy required such formidable defenses? Later settlements were smaller, on naturally defendable sites (Fernandez Castro 1995:17–23, 73 ff.; Monks 1997:13–17). Subsequent Neolithic and Chalcolithic remains display a variety of violent trauma, and there are extensive wall paintings that, in contrast to Paleolithic art, unambiguously depict armed-group clashes (Armendariz et al.

1994:215; Botella Lopez et al. 1995:70; Etxeberria et al. 1995:141–143; Guilaine and Zammit 2005:103–121, 156; Monks 1997:23–24).

France provides perhaps the best evidence for a late onset of war, somewhat ironically since this is the key area for Guilaine and Zammit. Brennan (1991) made a systematic direct examination of all available Middle (Neanderthal) and Upper Paleolithic remains from southwestern France (100,000–10,000 B.P.). These fragments represent 209 individuals, including a few widely known as supposed exemplars of violence. (Three other possible examples of violence were not available for examination). Of the total, she found a total of *five* fractures of any sort. Two Upper Paleolithic specimens had healed depression fractures on the skulls, but in a form consistent with an accident. Brennan (1991:206) concludes:

> There is little evidence in my data that traumatic injuries in these samples of Middle and Upper Paleolithic skeletons were common. The few traumas that are apparent can be as easily explained by accidental injury as by interpersonal violence. In fact the absence of a single parry fracture or wound to the left side of the head in my sample seems to belie some of the previously held notions in the literature of bestial behavior and violence for this time period.

Guilaine and Zammit (2005:49–50) acknowledge this study, and comment that if one were to go from the "evidence available," one would "conclude that aggressiveness was uncommon during this period." However, they choose another interpretation. Noting the few traumatized Cro-Magnon's found elsewhere in Europe (reported above under "The Earliest Signs of Violence"), they conclude that it was unlikely that they were peaceful.

Southern France shifts to cereal cultivation in the fourth millennium, and at the start of this transition, three sites show persuasive signs of cannibalism (Villa 1992:99; Villa et al. 1986). Skeletal trauma from projectiles becomes fairly common in the Neolithic (Cordier 1990). Guilaine and Zammit had two researchers compile an exhaustive list of all Neolithic arrow-inflicted wounds in France (2005:xii, 133, 241–251). The total number found from 5500 B.C. (late Mesolithic) to 3700 B.C. is just three individuals at three sites. But from 3600 B.C. to 2200 B.C., there are 41 sites, some with multiple victims. Although they minimize the significance of this dramatic research finding by observing that remains become more abundant as time goes on, the much wider occurrence of violence seems difficult to deny. Nevertheless, the percentages of victims remains quite small compared to other burial populations noted in this review. In tombs from 48 sites, comprising between 2,000 and 3,000 individuals, roughly 75, or under 4

percent have arrow wounds, including healed ones. Guilaine and Zammit argue that these may underrepresent actual violence. Certainly they might, but that makes three instances in which these authors argue *against* the evidence to speculate on the presence or intensity of war.

Bronze Age Europe

Dates for the Chalcolithic and Bronze Ages differ across Europe, but mostly fall into the range of 2300–700 B.C., with the Aegean area starting several centuries earlier (see Sherratt 1994). Much of the metal used in this period went into weapons, some for elite display rather than combat, some "ritually sacrificed" by being deposited in water. Weapons circulated over wide swaths of Europe and went through a succession of forms (Harding 1999; Kristiansen 1987; Osgood 1998; Randsborg 1992). It is not clear that this evident militarism is associated with an increase in actual fighting (Bridgford 1997:113–114; Robb 1997:136). Europe's first states, in Crete and Mycenaea, developed at the end of the Bronze Age, about 2000 B.C. and 1700 B.C. respectively, with much more emphasis on war in Mycenaea (Nikolaidou and Kokkinidou 1997; Wardle 1994). Both collapsed in the wave of widespread violence that swept from Egypt to middle Europe, around 1200 B.C., as the Iron Age, apparently, brought new and more powerful forms of making war (Drews 1993; Osgood 1998:77–83; Popham 1994; Randsborg 1992:199–201, 1999:191).

North America

A few preliminary comments must precede discussion of North America. First, my research ceased as I was working on the Pacific coast. That very extensive stretch will not be considered here, nor will the Great Basin, Arctic, and sub-Arctic. Second, as I was revising this chapter I learned of Lambert's (2002) excellent overview of North American materials. I find nothing to dispute in her assessments, but we do differ in that my overview is explicitly intended to highlight the increase in signs of war over time. Third, the North American material is more complicated than that of other areas already discussed, with very distinctive regional variants, and in some areas, more signs of collective violence at relatively early dates. Fourth, much of the following has been covered in greater detail by chapters in this volume. This survey looks for generalizations comparable to findings from other world areas.

About the earliest human inhabitants of the continent, we have been given two very different assessments of presence and prevalence of violence.

> The archaeological record gives no evidence of territorial behavior on the part of any of these first hunters and gatherers. Rather, they seem to have developed a very open network of communication and interaction across

> the continent.... [W]e find no sign anywhere in the archaeological record of even a *hint* of conflict or warfare. (Haas 1999:14)

> Proponents of simplistic materialist/ecological models that reduce warfare to competition over land and food will find little comfort in the evidence for frequent violent conflicts among earliest immigrants to the New World. These people lived at low densities and had ample opportunity to avoid violence by moving away from it but apparently were unable to do so. (Walker 2001a:591)

Two very early indications of violence are associated with the famous Kennewick Man, from 7000 B.C. to 5500 B.C., with a healed-over point (McManamon 1999), and his less famous approximate contemporary from Grimes Burial Shelter in Nevada, with obsidian embedded in his rib (Owsley and Jantz 2000). Besides Kennewick Man, Walker (2001a:588) provides reference to only one published work suggesting group violence, from Archaic Florida (Dickel et al. 1988)—a very significant case, but a late and singular basis for his generalization. Still, given the relative scarcity of skeletal remains this old, the number that have signs of violence must be taken as significant, and a mark against the perspective I argue here. The Eastern Archaic Woodlands is our next stop.

The East

Milner (1995, 1999) has been surveying the vast site literature for this region (and Dye, this volume). He (1999:120) notes some indications of violence for the Early Archaic about 8500–6000 B.C., but these "do not occur with any regularity" until the Middle and Late Archaic (6000–4000 B.C. and 4000–1000 B.C. [Fagan 1995:348]). An important early case is the one just mentioned (Dickel et al. 1988) from southern Florida, which is rather late at about 5400 B.C. to be classified as Early Archaic. A total of 168 individuals are represented in this very unusually well-preserved burial ground, and signs clearly suggestive of interpersonal violence occur in nine of them, including parry fractures, cranial fractures, and one embedded point. Another case is Mulberry Creek in Tennessee, 4000–3500 B.C. (Dye, this volume). But others see increasing cases of multiple traumatized individuals only in the Late Archaic, after 2500 B.C. (Gramly 1988:86; Munson 1988:12). Many cases of trauma are only single individuals, and so may not indicate war (Wilkinson and Van Wagenen 1993:198).

At least three Late Archaic areas do show clear evidence of war. Indian Knoll in Kentucky (4100–2500 B.C.) has 48 of 880 burials with embedded points, mutilations, or multiple burials (Webb 1974:147–155, 173–205). A few sites in central New York from about 2500 B.C. include skeletons with points and missing body parts (Ritchie 1980:77, 120). The 439 individuals from Tennessee

(2500–1000/500 B.C.) include 10 males with similar signs of violence, mostly from one location (Ostendorf Smith 1997; and see Dye, this volume). Yet Milner (1995:236) concludes that even later Archaic violence is limited compared to subsequent periods.

The Woodland Period after 1000 B.C. registers a clear decline in signs of violence in the great number of skeletal remains unearthed (Lovejoy and Heiple 1981:539; Milner 1999:122). But war did not disappear altogether, and Dye (this volume) still sees warfare as "widespread and endemic." Even among Hopewell people, usually characterized as peaceful traders, what appear to be trophy skulls are found (Owsley and Berryman 1975:50; Seeman 1988). In the middle Ohio Valley about A.D. 500, there is a shift from dispersed to nucleated and defendable sites (Dancey 1992). The arrival of the bow and arrow seems associated with more fighting in the seventh century (Nassaney and Pyle 1999), and sites in west-central Illinois show many signs of violence from the Late Woodland time on (Milner 1995:229). This long sequence creates problems for Otterbein's scenario of peace being structurally connected to broad-spectrum foraging and plant domestication. The Late Archaic was a time of increasing and more settled reliance on aquatic resources (Milner 1999:21), and the subsequent Woodland Period saw the domestication of sumpweed and sunflowers (Munson 1988:12–13; Watson 1988:40–43).

Yet the emergence of the Mississippian tradition by A.D. 800/900 still shows only limited preparations for violent conflict, although some settlement defenses appear as early as the eighth century (Morse and Morse 1983:237; Smith 1990). In the Southeast, at least, by A.D. 900–1050 there are indications of endemic warfare (Knight and Steponaitis 1999:10). A major increase in nucleation, palisading with sophisticated defensive features, and vast empty areas between centers begins after A.D. 1050 (Gramly 1988:91–93; Milner 1999:123–124), with the chiefly polities discussed by Dye (this volume). Around A.D. 1200, war clubs come to dominate over the bow and arrow, at least in combat around major centers (Dye 2002:128). In the twelfth century, similar defensive concentrations are seen in New York (Chapdeline 1993:197–201; Tuck 1978:326) and around that time, in Virginia (Roundtree and Turner 1998:279).

Late Prehistoric burials yield many individuals with embedded points and other trauma (Gramly 1988:87; Ritchie 1980:294). Around A.D. 1300, burials from Illinois of 264 fairly complete skeletons include 43 with such indications (Milner et al. 1991:583, 594). In a Michigan cemetery, nine percent of individuals have non-lethal depression fractures, mostly in females who may have been captives, and younger men are notably underrepresented (Wilkinson 1997:28, 35–38; Wilkinson and Van Wagenen 1993:193). In the central Mississippi/Ohio area after A.D. 1350, from an already nucleated base, there was additional con-

centration of populations, and abandonment of other areas, creating a vast "empty quarter" (Cobb and Butler 2002). Parallel variants are seen throughout the Southeast, giving rise to the sophisticated chiefly warfare recorded by the Spanish, although by then it existed in attenuated form (Anderson 1994:139–155; Dye 2002; Morse and Morse 1983:271–283).

The Great Plains

I found less information about early times for this area, but one detailed survey of remains from Texas, Oklahoma, New Mexico, Kansas, and Colorado suggests a late onset of war (Owsley 1989:131–133; Owsley et al. 1989:116–119). By my count from data charts, there are 173 individuals from all periods up through Woodland, and 447 from the late prehistoric (after A.D. 500 [Fagan 1995:139]). Of the former, the only indication of violent death is one woman, with two blows to the head. Of the latter, if one includes individuals in multiple burials where some individuals have embedded points, there are 74 cases of probable violent death, in one site accompanied by deliberate village destruction. After A.D. 1000, along the Missouri and Mississippi, Mississippian peoples surrounded large villages with ditches and palisades, and buried at least one chief with an ornate mace. There was a further nucleation accompanied by abandonment after A.D. 1350 (O'Brien and Wood 1998:288–292, 318–320, 331–333, 344). Fortification and Mississippian connections reached into northeastern Iowa from A.D. 1100 to A.D. 1200 (Alex 2000:134, 155, 182), but further west in Kansas and Nebraska there are no indications of fortifications or defensive locations (Krause 1970:106, 111; Wedel 1986:100).

Oneota people expanded from southwestern Minnesota through Iowa and much of Missouri and Illinois and environs after A.D. 1250, and especially after A.D. 1350, accompanied by fortified sites, skeletal trauma, and thunderbird iconography which in historic times is associated with war chiefs (Alex 2000:182–188, 200, 207–209; Bamforth, this volume; O'Brien and Wood 1998:345–347, 357). While a few individual skeletons show signs of violence before A.D. 1250 (Hollimon and Owsley 1994:351; Olsen and Shipman 1994:384), worse times were coming.

In the generally accepted picture, immigrants from the Central Plains tradition moved northward, initially without fortifications or other indications of war. But in the century after A.D. 1250, they and the previously resident Initial Coalescent people engaged in major fort building, some destroyed while under construction. Many, many skeletons indicate violent death, preeminent being the Crow Creek site, conventionally dated at A.D. 1325, where a minimum of 486 individuals were slaughtered (Hollimon and Owsley 1994; Kay 1995; Pringle 1998:2039; Willey 1990; Zimmerman 1997). Bamforth's contribution

to this volume requires major revision of this view. War is detectable from A.D. 1000 on, associated with particular intervals of drought, not one long period, and Crow Creek's date cannot be specified more precisely than in the broad vicinity of A.D. 1400.

The Southwest

A great deal has been published on prehistoric warfare in the Southwest, especially over the past decade. LeBlanc's book (1999) is the most comprehensive, joined by a set of case studies (Rice and LeBlanc 2001). Despite an extensive earlier record, the first clear evidence of war I have seen dates from several centuries after the beginning of maize and squash agriculture, which occurred around 1500–1000 B.C. (Wills 1988:149). In southwestern Utah, within the Anasazi area, Weatherill's Cave 7 contains remains of some 90 individuals, most if not all of whom were slain, some mutilated. Artifacts place this within Basketmaker II, or between 500 B.C. and A.D. 500 (Hurst and Turner 1993:167, 170–171; although LeBlanc [1999:310] puts this at about A.D. 0, and Lambert [2002:220] puts it at "A.D. 400?"). Three other Basketmaker II sites may have been massacres, and there are numerous other signs of deadly violence (LeBlanc 1999:140–144). War signs decrease in Basketmaker III (A.D. 500–750), and come back in greater frequency in Pueblo I (A.D. 750–900). For Mogollon people, there are some defensible hilltop locations before A.D. 600 and more indications of war after A.D. 850; for Hohokam, there is nothing conclusive for war until the thirteenth century (LeBlanc 1999:129–149, 2000:94–95; Nelson 2000:326–327; Solometo this volume).

Violence among Anasazi from A.D. 900 to A.D. 1150/1250 is ambiguous and extremely controversial. Although one fringe location was fortified about A.D. 900 and abandoned after a massacre about A.D. 1000 (Eddy 1974:81), in the central areas of Chacoan Anasazi cultures there are no indications of war, suggesting the existence of a "Pax Chaco" (Lekson 2002:613–614). There are, however, non-lethal traumas, especially among women (Martin 1997) and signs of brutal killing interpreted alternatively as cannibalistic terror perpetrated by the political elite, or slaying and dismemberment of witches (Bullock 1998; Darling 1998; Kantner 1999; Turner and Turner 1999; Walker 1998). No one, however, seems to question that *some* cannibalism occurred, and that this was *not* in a context of war.

Signs of tensions—the abandonment of Chaco, opening spaces between groups around Kayenta—develop in the century after A.D. 1150, leading to tribal nucleation and movement into cliff dwellings around A.D. 1250. Abandonment of northern and some southern Anasazi areas and clustering of remaining settle-

ments occur after A.D. 1275 (Haas 1990; LeBlanc 1999:264–270, 2000:45–54; Upham and Reed 1989). Rice and LeBlanc's (2001) volume of case studies, with a concentration on Hohokam and Sinagua regions, all concern the "narrow time frame, from the A.D. 1200s to the early 1400s" (2001:2), when there is the most evidence of war throughout the Southwest, although preceded by a century of increasing tensions. Hohokam areas, too, were abandoned by the late fourteenth century (Fish and Fish 1989:119–121; Wilcox 1989:163). Solometo (this volume) describes similar intensification of conflict on the Mogollon rim, from limited indications of war after A.D. 850, to clear intensification in A.D. 1150–1250, followed later by abandonment. Further south, in northwestern Chihuahua, the large center Casas Grandes was burned, with crushed bodies and deliberate destructions, then abandoned in A.D. 1340 (Ravesloot and Spoerl 1989:131–134). By the early fifteenth century, what remained of the entirely rearranged population seemed to engage in less war, though war was still being practiced when Coronado arrived (Creamer and Haas 1998:55–57; Haas and Creamer 1997:241–243; LeBlanc 1999:264, 305).

Section Conclusion

There are numerous regions of the world where good archaeological data are available for centuries or even millennia before any suggestion of war appears. This is so for the Middle East, Central Asia, the Indus, China, and Japan. Later in each of these areas, archaeological evidence of war becomes clear and continuous. For Egypt, the critical developmental period is lacking. Europe may be more complicated, with scattered instances of (possibly) collective violence in the Mesolithic, an initial and apparently peaceful spread of Neolithic traditions (with one probable exception from Italy), giving way to a widespread pattern of warfare from the fifth millennium on.

North America is even harder to summarize, with different and sometimes conflicting trends in different areas. Although there are a few very early indications of violence, in most areas there are either no suggestions of war in the earliest material, *or* signs of violence which appear quite limited compared to later prehistoric times. Lambert's (2002) review of the North American literature indicates that this generalization applies to regions I have not yet fully researched. I would note in advance of more complete discussion that the northern Northwest Coast stands out as the earliest start of a war pattern in North America, about 2200 B.C., that then continued in practice down to historic times (Ames and Maschner 1999:209–210; Cybulski 1992:156–157, 1994:80–81; Moss and Erlandson 1992:81). Although I have yet to go through all the material for Mesoamerica, South America (touched on below), Africa, Melanesia, and the

Pacific, my preliminary readings suggest little to contradict this picture of a relatively late emergence of war. Discussions of Palau (Liston and Tuggle this volume) and New Zealand (Allen, this volume) are consistent with that.

Although episodes of war are possible any time in human prehistory, there is no convincing evidence of collective intergroup violence any time before 10,000 years ago (except Jebel Sahaba), and in many parts of the world much more recently than that. I realize that evidence can be read in different ways, and many will not be convinced that war was absent. But I think it is difficult to disagree with the assertion that the presence of war, and its intensity, is highly variable. To claim that "war is something . . . that all humans do," or "*everyone* had war in *all* time periods" is contradicted by the evidence.

Another point of general agreement should be that, even though times of more war were sometimes followed by times of less war, the overall, long-term trend was for more war over time. Besides the evidence I have presented, that simple fact is apparent in this volume. In Palau, war appears in the record around A.D. 600. The Maori brought war with them to New Zealand, but war intensified greatly around A.D. 1500. In the U.S. Southeast, there were several long steps in war development beginning by 3500 B.C., but the intensity of war surged in the 500–600 years before contact. Looking at ancient times, we see in China war became more common over the course of the Neolithic, and still more common in the Bronze Age. Oaxaca is a particularly important case. Although the chapter by Redmond and Spencer in this volume picks up around 700 B.C., when there were already warring chiefdoms, Flannery and Marcus (2003) begin their article further back in time: the time frame of 8000–2000 B.C. is characterized by "warless societies," and signs of war first appear around 1540 B.C. but are then absent from 1100 B.C. to 800 B.C.—although Otterbein (2004:123–126) questions that earlier evidence of war. Later prehistory indicates much more war than earlier prehistory.

This is a critical point. Those who suspect war in earlier times when evidence is lacking are relying on two overlapping bases. One is theory about why war occurs. Theories are legion, and according to many, some war is always expected. The other basis—upon which most theory is drawn—is ethnographic observations of war over the past five-hundred years. A major point of this chapter, of course, is that Western contact frequently led to intensified warfare among observed peoples. But what the archaeological record demonstrates is that prehistoric warfare got much worse in later prehistory, *before* any outside contact. Thus, war among ethnographically observed peoples is doubly inappropriate for forming opinions about humanity's distant past.

The next section presents my ideas—tentative empirical generalizations—about how war developed out of a warless background, and why it became more

common over the millennia and often intensified with Western contact. Then i ties all these issues together with a discussion of Venezuelan prehistory and the Yanomami.

The Spread of War up to the Ethnographic Present

Why War Became Common

In this condensed summary of early evidence of war, I have not discussed the material and social correlates of that development. I will not speculate on direct causes, but several general factors seem implicated as preconditions, which in variable combinations, make the origin and/or the intensification of war more likely. (Most of these are discussed in some detail in Ferguson 1997:334–337.) One is a shift to sedentary existence—though not necessarily agriculture—or at least to increased dependence on fixed sites. Another is generally increasing population within broad areas. Two others, although these seem less relevant in earlier North American sequences than in the Old World, are the development of social ranking and increasing trade, especially of status goods—although in North America the ambitions of chiefs are major factors in the later intensification of war. A fifth is the development of social institutions for bounding groups in conflict. Kelly (2000:44) and Boehm (1999:90–98) make a more specific claim, that it is the development of segmental kinship systems that enables war. Rice (2001) has found support for that idea in the American Southwest, and Flannery and Marcus (2003) in Oaxaca. Finally, a serious ecological reversal, involving climate change or anthropogenic resource degradation, is often implicated in the origin and/or intensification of war.

The general absence of these preconditions can explain why human populations did not develop cultural practices of war in earlier times. But over the millennia, these preconditions became more widespread, and war arose in more regions of the world, as described in the previous section. What happened after that, after war began in different areas but before there were any states around to influence things?

War spread. Not automatically, or quickly, as is sometimes imagined in parables of anarchy (Schmookler 1984). But eventually, spread it did, through some combination of contact stimulus and converging conditions. This is seen in contexts already discussed. In China, signs of war became more common through the extensive Longshan interaction sphere and beyond (Chang 1986:270–271; Liu 1996:264; Underhill 1989:231–235, this volume). In Japan, war was brought by immigrants from Korea, but then spread through the islands (Barnes 1999:168–171; Farris 1998:37–41; Imamura 1996:131). In far western

and more northern Europe, the first cultivators replaced hunter-gatherers and made war without the peaceful centuries of earlier expanding farmers (Anderson 1993:102; Bradley 1991:50–52; Mercer 1988:89, 104, 1989, 1999). In the center of North America, the Mississippian period saw the rise of nucleated, fortified settlements after A.D. 1050, and over the next century or two this pattern spread outward in all directions (Chapdelaine 1993:200–201; Morse and Morse 1983:256, 263–266; Roundtree and Turner 1998:278–280). On the Pacific Northwest Coast, the war complex developed in the north gradually came to characterize more dispersed and less hierarchical southern groups (Ames and Maschner 1999:209–210; Coupland 1988:207–212). And as described by Allen (this volume), later Polynesian seafarers carried a warrior complex along with them to new islands. So even before states, war was becoming normal in ever broader areas of the tribal universe.

Then what happened once states appeared? In theory, successful states suppressed collective violence within their administration, although this varied in practice (Ferguson 1999:404–405). There are indications that ancient states fostered violence and war among non-state peoples around them. The rise and fall of states could create sweeping waves of war. The development of the Zulu state was followed by a spreading chain-reaction of terrible violence, known as the *mfecane*, although the growing European presence may have played a role in this horror (Cobbing 1988; Hamilton 1995). The collapse of Teotihuacan led to destabilization and war throughout northwest Mexico (Nelson 2000). In the Andean highlands, Arkush (this volume) describes "chain reactions" of war spreading out from political centers through peripheral areas.

Relatively stable, central states, the sources of our earliest histories, commonly saw themselves as surrounded by fierce "barbarians" (Ferguson 1999:418–420). This was not *merely* ideological projection. But the origins of such militarism are obscure. How much was an internal development? How much from state stimulus? Probably the best bet is that a pronounced military orientation developed as part of long-term interaction between emerging state centers and peoples of their peripheries. It is clear, however, that ancient states commonly fostered ethnogenesis and tribalization, and enlisted these "martial tribes" as "ethnic soldiers" to, among other things, project force farther into their hinterlands than encumbered state armies could manage economically (Goldberg and Findlow 1984). It also seems a safe bet that this, at least sometimes, spread intensive war far outwards.

Militaristic states, over time, replaced comparatively non-militaristic ones. In Peru, after 1000 B.C., several apparently theocratic states were conquered, incorporated and culturally transformed to fit the mold of expanding militarists (Pozorski 1987). After the decline of the relatively unmilitaristic Harappa,

the Indus region became part of the endless wars of the Rg Veda (Singh 1965; Srivastava 1984). Finally, ancient states fostered war even distant from their frontiers. From the first century A.D., Roman, Arab and other demands for slaves sent waves of violence spreading inland from the east coast of Africa (Edgerton 1972:161; Kusimba, this volume; and see Lovejoy 1983). Sri Lanka experienced repeated, intensive warfare related to control of the expanded Indian Ocean trade of the ninth and tenth centuries A.D. (Gunawardana 2000:78–79). So the existence of ancient states further contributed to the prevalence of war among non-state peoples.

Then came the European expansion. Over all, European expansionism was *more* disruptive and "warrifying" than that of ancient states (Ferguson 1993). Whereas ancient states would move into contiguous areas, subject to a long history of interaction, Europeans crossed enormous distances and oceans. Doing this, they introduced new diseases, plants, and animals that massively disrupted contacted groups. Europeans had trade goods that were in great demand, and, especially later in time, military and transportation technology and techniques that could revolutionize warfare. European expansionists were not unique in seeking captive labor, but the vast extent of their operations was. The same goes for the European quest for land cleared of previous inhabitants. All of these aspects of European expansionism sent out shock waves that went far beyond frontiers, often preceding any Western observer. This area of impact Neil Whitehead and I call a "tribal zone" (Ferguson and Whitehead 2000). While Western contact did not *always* lead to more frequent or destructive warfare—sometimes the opposite occurred—case studies collected in *War in the Tribal Zone*, and many others, leave little doubt that in many, many parts of the world, European expansion after A.D. 1500 led to more war among non-state peoples, before any pacifying effect set in.

Amazonia and the Yanomami

To make this discussion of archaeology, history, and the temporal increase in war more unified and concrete, let us consider war among Amazonian peoples, and particularly the Yanomami of the Upper Orinoco region—long considered a type case of pristine, primeval warfare. Initial historical reports from the coasts and major rivers of northwestern South America document large settlements, some organized as chiefdoms, usually readily able to demonstrate substantial military capabilities (DeBoer 1981, 1986; Medina 1934; Morey and Marwitt 1974; Myers 1988; Whitehead 1988). How long had this pattern existed? Excavations of major settlements along the lower Amazon and middle Orinoco, the latter beginning around 2100 B.C., are remarkably free of war signs (Roosevelt 1980). One good sequence comes from a tributary of the Rio Apure, which itself

flows into the Orinoco, about 500 km from ancestral Yanomami lands. Here chiefdoms and war appear together—after major population growth, along circumscribed agricultural land, in the contact zone between lowland and Andean peoples (where war was already well-established)—around A.D. 550. Chiefdoms did not appear on the middle Orinoco until A.D. 1100. By A.D. 1530 and the first historical accounts from the savanna near the Apure, powerful chiefs lived in large fortified villages, with extensive irrigation systems, and some were able to raise fighting forces reportedly exceeding ten thousand men (Roosevelt 1991; Spencer 1998:127–129; Spencer and Redmond 1992:135–137).

As researchers now mine long-neglected historical documents, especially for Venezuela and environs, we are getting a much clearer picture of extensive, interconnected political systems, with ties reaching west to the Andes, south to the Amazon, and north through the Caribbean (Arvelo-Jimenez and Biord Castillo 1994; Chernela 1993; Whitehead 1988, 1994). War was certainly an integral part of these systems, but how the war-making river chiefdoms related to more mobile peoples of the interior such as Yanomami—whether in symbiosis, conflict, or both—is at present a very open question. Within decades of first contact, these exposed peoples were involved in military resistance to the Europeans, and soon after that were ensnared in imperial rivalries and slave raiding (Whitehead 1988:71 ff., 1994, 2000). Even as early as the time of Hans Staden (1928), marooned in Brazil in 1550, war clearly reflected decades of major European influence (Ferguson 1990:241). These regional sociopolitical systems were destroyed and their peoples eliminated within the first century or so of contact (Hemming 1978:29–42; Whitehead 1994, 1999), long before any anthropologist was around to theorize their wars.

The ethnology of war has relied heavily on Amazonian cases, but as noted earlier, our theoretical paradigms ruled history out. When it is brought back in, *all* of the key cases upon which theory has been built have been obviously, dramatically impacted by the Western intrusion. Europeans directed native peoples to attack others who made trouble or who were allies of imperial rivals, or to produce purchasable captives, or just to divide and conquer. New diseases interpreted as witchcraft led to fighting, and Indians displaced from one region came into conflict with others. Western goods stimulated war as objects of plunder, and caused disputes over trade control (Ferguson 1990). Which brings us to the Yanomami specifically.

It is very common to hear it said that even in the mid-1960s, Yanomami lived unaffected by "civilization." This is why their warfare is so often claimed to represent the human condition as it existed in our evolutionary past. Both premise and conclusion are very wrong. Yanomami have been indirectly and directly af-

fected by the European expansion since they became targets of other natives seeking captives to trade to the Europeans in the late 1600s.

A Spanish observer in the 1740s reported that along the Upper Orinoco, not far from Yanomami lands, local peoples were "peaceful" except for the recently intrusive Guaipunaves—slave takers for the Portuguese—who decimated local populations (although the possibility of anti-Portuguese bias must be considered in this account) (Gilij 1965 I:55, II:57, 289). By this time, the highland home of the Yanomami was being targeted by slave raiders coming from every direction—and they were still being victimized by raiders on a smaller scale a century or more later (Ferguson 1995a:77–82, 181–186). The Spanish established their first outpost on the Upper Orinoco in 1756. Over the next few years, they witnessed the diminishing Guaipunave enter into new violent conflicts with remnants of once powerful peoples from the Negro, who were fleeing the Portuguese there. By 1761, all the survivors were escaping the Europeans by moving into highland forests, leaving the Orinoco and other rivers nearly abandoned (Ferguson 1995a:82–85).

All early reports of Yanomami making war come from periods of specific and intense disturbance, either as targets of slave raids, or in situations of marked disparities in recently introduced Western goods (Ferguson 2001). The bulk of *Yanomami Warfare* (Ferguson 1995a) is devoted to detailing evidence that Yanomami wars over the past century or so are results of tensions associated with unequal distributions of steel tools and other Western manufactures. Yanomami of the Orinoco-Mavaca area in the mid-1960s—the subject of Chagnon's (1968) *Yanomamo: The Fierce People*—were additionally suffering massive, broad-spectrum disruption of their lives related to the presence of missionaries and other outsiders, which directly encouraged their unusually high level of interpersonal violence (Ferguson 2000). This is quite different from LeBlanc's claim (this volume) that Yanomami had chronic warfare because they had reached their carrying capacity.

Based on the connection of all known fighting to external factors, and my own historical ecological reconstruction of their pre-steel subsistence as more mobile and lower density (Ferguson 1998), I offered the "hunch" that before Columbus, war *between Yanomami communities* was "limited or even nonexistent" (Ferguson 1995a:75). Because Yanomami built no major structures, used few stone implements, and consumed the bones of their dead, we will never know for sure. Archaeologically, they are nearly invisible. Sometimes I do wonder if that hunch goes too far. But raising that possibility is, I believe, a useful caution for those who would project ethnographic war patterns into distant prehistory.

Conclusion

The issue is not whether there was war before civilization. No serious scholar doubts that there was. The issue is how to explain war, both specific wars and war in general as a part of the human condition, and how ethnology and archaeology can join forces in this quest. For those indigenous peoples once distant physically, technologically, economically, and militarily from expansionist Europe, the need to figure Western contact into explanation of post-contact war seems elementary. Archaeology has the ability to investigate pre-contact times, and to witness the effects of contact from its earliest phases. But that is only the beginning. Archaeology can investigate the military effects of ancient states on peoples around them and along their trade routes. Archaeology can address the question of how and why war spread outwards from its original centers, how its practice and elaboration changed over time, and what factors contributed to its genesis in the first place.

Archaeology and ethnology should join, where appropriate, in theory. A robust theory of war should be capable of explaining the origins of war, its transformation in European tribal zones, and everything in between. Regarding Northwest Coast warfare, I developed (1984b) a pre-contact model that grounded war in geographic and temporal variations of critical subsistence resources. With post-contact depopulation, that model no longer applied. Yet war went on, variably, and in some cases quite intensively. Those variations in practice can be explained primarily as efforts to increase supplies or profit from trade in Western goods, or to feed the slave trade which existed at contact but got worse later. An explanation of Northwest Coast warfare must attend to both archaeology and history.

That study was an effort to create a *testable* theory of war causation, aimed at explaining which groups attack, which groups are attacked, periods of intense war, and periods of peace. A somewhat modified, greatly elaborated, and avowedly scientific version of that approach was developed for the Yanomami case (Ferguson 1995a:21–58; and see Jones 1998; Steel 1998). For the Northwest Coast, explanation of prehistoric warfare was much more broad-brush than that of historically observed events, in part because there was much less archaeology to go on in the early 1980s. But we can never expect the same detail of sequences through archaeology as through history, and that poses a problem for theoretical unification.

One way to bring the long spans of prehistory together with history is through a programmatically modified version of cultural materialism (Ferguson 1995b:30–32). This modification preserves the principle of infrastructural determinism but in a non-reductionist way. Structure and superstructure are seen

as vast conjunctures of variables with substantial causal autonomy. It also enables a theoretically consistent integration of an enormous number of linkages between war and society. A recent article (Ferguson 1999) is a compilation of these linkages in a systematic comparison of war and society among non-state peoples vis-à-vis among ancient and medieval states. More systematic comparisons of archaeological data with ancient and medieval states could bring new perspectives to questions such as those asked in this volume.

For instance, one generalization from the cases here is that war does not always lead to political consolidation. It can enforce or lead to fragmentation as well. That is no surprise from the perspective of ancient states, where unification and break-up frequently alternate over centuries. LeBlanc (this volume) offers a new and interesting hypothesis that consolidation of two polities formerly separated by buffer zones can lead to a much larger and more potent polity than its former peers. Some cases in this volume, especially that of Monte Albán, seem to support this idea, though others, such as the upper Belize River valley and the frontier between Aztec and Tarascan empires, suggest less vacancy at peripheries. More generally, the comparison of tribal peoples and ancient states (Ferguson 1999) makes the point that "the space between" polities is anything but empty. It is highly and variably patterned at levels of infrastructure, structure, and superstructure.

Looking at ancient states quickly reveals basic variations in political autonomy and consolidation. The older distinction between territorial conquest and hegemonic domination is a necessary starting point, but not nearly sufficient for understanding militarily based integration. Relations between dominant and subordinate centers vary along scales that range from alliance, through domination, to incorporation; and from trade, through tribute, to taxation. Often transitions occur as a gradual, incremental process, rather than a sudden event. These shifts may be difficult to distinguish archaeologically, but the possibilities should be kept in mind while framing hypotheses. Further, not only do political centers grow, they also decline in strength, and one of the recurrent themes among ancient states is the gradually increasing autonomy of what were once tightly dominated sub-polities.

Cases in this volume show some variations of consolidation (although pigeonholing cases is often tenuous): from tribal in the narrow sense of political unification without center or pronounced hierarchy (such as the northern Plains and Southwest, sometimes), through an array of small and large chiefdoms (as in the Southeast, Palau, New Zealand, and eastern Africa), through equal/independent or ranked/consolidated stratified peer polities (as with Maya), to various forms of expansionist states (as in Mexico, the Andes, and China). This shows that consolidation must be seen as a three-dimensional process, with the axis

of power joining geographic layout. Again and again authors in this book have asserted that war was a result of chiefly (or higher) political ambitions. And it is most important to emphasize, that all those discussions of military success conferring prestige, and of the legitimating effects of militaristic iconography, involve hierarchical arrangements *within* the war-making polity. There is an internal/external dialectic in hierarchical politics.

It must also be emphasized that war is no sole, prime mover in political development. Many other cultural spheres are engaged. Because this volume is about societies that practiced war, it may give an unbalanced picture of war's importance in that process. Even in the cases here, there are variations in the significance of war, with Andean state expansionists appearing significantly less violent than Mesoamerican. Elsewhere there may be emergent ancient states with little if any warfare.

Previously I (1994:101–104) suggested that war and peace may have self-reinforcing tendencies which ramify throughout societies, so that there could be alternative militaristic and peaceable trajectories toward complexity. Consideration of ancient and medieval states (Ferguson 1999:400) added the idea that societies may differ in the extent to which the institutions of political, military, and religious leadership are separated, or unified. Taking all of this in suggests that there is no one, single path toward state formation. Our goal should be to develop a comparative political sociology of hierarchical intergroup relations. Those considering these issues might benefit from a text and area that is not often considered, Gottwald's (1979) massive, very anthropological study of Israel, 1250–1050 B.C.

Ancient and medieval states suggest other elements that could be a part of this political sociology (also see Andreski 1968; Otterbein 2004). How much is internal production rearranged to support armies, and/or as a consequence of military subjugation? To what degree are military organizations put to use in other kinds of labor? Does military administration spur development of a literate bureaucracy? Are there common and elite forces (or more tiers than that), and how does that articulate with domestic stratification—for example, are there military aristocracies, and is upward mobility into them possible? Do soldiers have to return home by season, or are they freestanding professionals? Are ethnic soldiers—units of culturally distinctive peoples—incorporated into large armies? Not all of these may be subject to archaeological investigation, but the questions must first be raised to find that out.

Disentangling this snarled thicket of causality is a labor for generations of scholars. One step we may plant firmly now is that war as a regular practice, war as a social institution, *had a beginning*. If it had a beginning, then war is not an inevitable expression of either human nature or the nature of societal existence.

To recognize an ancient beginning is to conceptualize the possibility of a future end to war. But we cannot deal with the origins of war if we continue to project observations from recently observed indigenous peoples backwards through time. The view espoused here is that the origin and early development of war should be approached in its own time, and its own terms—not blinkered by the ethnographic present.

Numerous times in this volume, authors have suggested that the wars they uncovered had three kinds of causes: struggle over important productive resources, the ambitions of political leaders, and local cultural beliefs which provide both justification and an impulse toward war. In closing I wish to highlight this consensus, which I believe would be seconded by a great many other archaeologists and cultural anthropologists.

War is a result of basic material concerns, filtered through an internal/external political system, pushed along by values that encourage militarism. In direct contrast to those biological explanations that are reinforced by a war-forever-backwards view, this simple, communicable conclusion has implications for how we understand war in the world today, from so-called "ethnic conflicts," to terrorism and the war in Iraq (Ferguson 2005). If we want to understand all this violence, we should begin by identifying who is calling the shots, what are their material and political interests, and how do they selectively employ cultural identities, symbols, and values in leading people into war. In my estimate, that—not "war is in our blood"—is the critical implication of the new archaeology of war.

Notes

1. LeBlanc with Register (2003:124) and Keeley (1996:37) rely on what Keeley calls "the celebrated Upper Paleolithic cemeteries of Czechoslovakia" to support the deep antiquity of war. Since their portrayals of the remains of these mammoth-hunters seems quite conclusive, it is important to consider those claims against current evidence and interpretations, for the findings from Predmosti, and the three clustered sites of Dolni Vestonice I, II, and Pavlov.

Keeley (1996:37) writes that these imply, "either by direct evidence of weapons traumas, especially cranial fractures on adult males, or by the improbability of alternative explanations for mass burials of men, women, and children—that violent conflicts and deaths were common." Hill and Wileman (2002:17) cite Keeley in referring to "the mass homicide in Czechoslovakia where groups of men, women and children—the males showing signs of cranial injuries—have been dated to between 34,000 and 24,000 years ago."

LeBlanc and Register (2003:124) provide the most extensive discussion of these findings, referring to Dolni Vestonice:

> The well-known "village" consisted of a very large structure obviously occupied by many families, similar to the Iroquois longhouses, surrounded by some smaller

> structures. The entire area was surrounded by a wall or fence of mammoth bones. Typically this sort of barrier is used ethnographically around the world for defense. A number of multiple burials—several people placed in the same grave at the same time—have been found at Dolni Vestonice, especially mass burials of fighting-age males, a number of whom also have wounds to the head. It is unlikely that several males in their prime would die from disease at the same time. They could have been killed in a failed mammoth hunt, but death from warfare is certainly more plausible. This "village" was located on a high point of land—hills provide a good deal of defense especially against spear-throwers, the best weapon of the times. Almost every line of evidence for warfare I would expect to find for this type of forager has been identified at Dolni Vestonice.

LeBlanc and Register (2003) do not cite any reference in support of these characterizations, but they seem to rely on Klima (1962), whose comments do support some of their claims. Additional research and analysis has occurred since then. What are more current understandings of these sites?

There is a mass burial at Predmosti, estimated to date from 27,000 B.P. to 25,000 B.P., as part of a very long sequence of occupations. It was excavated in 1894, using the crude methods of the time. The site was later destroyed by brick makers, and almost all the skeletal material was destroyed during World War II. What we know about Predmosti is from the report of the investigator. The mass grave is 4 m-by-2.5 m, including remains of 18–20 individuals, 12 of them children, all covered by rough limestone slabs (Allsworth-Jones 1986:152–153, unpaginated appendix entry 12; Svoboda et al. 1996:62, 141, 226). *There is no reason to assume that these people died or were buried at the same time.*

Jiri Svoboda believes that these represent "a pattern of gradual additions of bodies within a long-term burial area." He bases this conclusion on the fact that Predmosti has "disturbed and incomplete skeletons, with only portions of a few bodies in anatomic order. This is in strong contrast to the almost complete burials of Dolni Vestonice. . . . [implying] that earlier bodies were disturbed while adding new ones" (personal communication 2003). This can be seen by comparing the diagram of the Predmosti mass grave and photo of the DV triple burial in Svoboda et al. (1996:168–169). Mass burial, yes. Mass killing, highly unlikely.

There is no mass burial at Dolni Vestonice or Pavlov. There is one triple burial of two males and an individual of undetermined sex, but more likely female (DV 13, 14, 15, about 26,600 B.P., Klima 1987), three other individual skeletons, and numerous scattered remains (Sladek et al. 2000). Some skulls, such as that of DV 16 (Svoboda and Vlcek 1991:326) have small depressions, consistent with non-lethal fighting, (along with other post-depositional fractures) (Svoboda et al. 1996:147; Svoboda, personal communication 2003). Erik Trinkaus characterizes them as "pretty minor bumps on the head that. . . . [would not] have been noticed for more than a week or so. I do not find them very convincing of interpersonal violence—just general tough lives" (personal communication 2003). The exception is DV 12, with a *healed* 3 cm depression on its forehead (Trinkaus et al. 2000:1119). Whatever was going on, these are *not* people killed by blows to the head.

The strongest claim for violent death at Dolni Vestonice is not mentioned by Keeley or LeBlanc. Klima (1987:835), in his initial report on the triple burial, comments: "The remains of a thick pole, stuck deep into DV XIII's hip up to the coccyx support the contention of his forced death." Further analysis of the site, however, led to the conclusion that the pole was just a piece of a wood structure that was placed over the bodies and burned, then collapsed into the bodies (Svoboda et al. 1996:64, personal communication 2003).

As for the other lines of evidence cited by LeBlanc with Register (2003): there is no large structure, nothing like a longhouse at Dolni Vestonice, only small ones of about 4–5 m diameter, although at DV I, two of these seem connected. Except for those two, we do not know if these were contemporary or sequential occupations—the "village" is speculation. In this region, some such Upper Paleolithic structures are outlined (not walled) in stones or bones, such as one described at DV I, believed to have been occupied for about two years. However, those of DV II—the location of the triple burial—appear even more temporary; no outlines, only artifacts around a hearth. Although there are large deposits of mammoth bones at these and other Moravian mammoth-hunter sites, there is no surrounding wall of any sort at any of the locations. Although one part of Dolni Vestonice is on a projecting spur of land, its defensive value is questionable. The spur is on the lower slopes of a mountain, overlooking a river valley—like settlements in the region, probably to monitor migrating large game. If anyone wanted to win a spear-throwing contest with residents of Dolni Vestonice, all they would have had to do was walk a little farther uphill (Svoboda et al. 1996:146–147, 151–155, personal communication 2003).

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