

GENOCIDE

Edited by
A. Dirk Moses

CRITICAL CONCEPTS IN
HISTORICAL STUDIES



GENOCIDE

Critical Concepts in Historical Studies

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A. Dirk Moses

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CONTENTS

VOLUME II GENOCIDE BEFORE MODERNITY

Acknowledgements vii

PART 5

Pre-history, biblical and classical antiquity 1

18 Archaeology, cultural anthropology, and the origins and intensifications of war 3
R. BRIAN FERGUSON

19 Ofnet: evidence for a Mesolithic massacre 57
DAVID W. FRAYER

20 Parallels with a divine command to eliminate a group of people 87
LOUIS H. FELDMAN

21 Mass murder, bloodshed, and clemency 141
BENJAMIN H. ISAAC

22 Anger, hatred and genocide in Ancient Greece 151
DAVID KONSTAN

PART 6

Medieval and Early Modern Periods 171

23 Bread, cheese and genocide: imagining the destruction of peoples in Medieval Western Europe 173
LEN SCALES

CONTENTS

| | |
|--|------------|
| 24 Chingis Khan | 193 |
| JOHN JOSEPH SAUNDERS | |
| 25 The final blows, 1734–1771 | 221 |
| PETER C. PERDUE | |
| 26 Genocidal massacres in early modern Southeast Asia | 267 |
| BEN KIERNAN | |
| PART 7 | |
| ‘Tribal’ Societies | 297 |
| 27 The Iroquois practice of genocidal warfare (1534–1787) | 299 |
| JEFFREY P. BLICK | |
| 28 The violent edge of empire | 328 |
| R. BRIAN FERGUSON AND NEIL L. WHITEHEAD | |

Part 5

PRE-HISTORY, BIBLICAL AND CLASSICAL ANTIQUITY

ARCHAEOLOGY, CULTURAL ANTHROPOLOGY, AND THE ORIGINS AND INTENSIFICATIONS OF WAR

R. Brian Ferguson

Source: Elizabeth N. Arkush and Mark W. Allen (eds), *The Archaeology of Warfare: Prehistories of Raiding and Conquest*, Gainesville: University of Florida Press, 2006, pp. 469–523.

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 non-state 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 human-kind 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 politics (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