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Pacifying Hunter-Gatherers

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Pacifying Hunter-Gatherers

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Abstract

There is a well-entrenched schism on the frequency (how often), intensity (deaths per 100,000/year), and evolutionary significance of warfare among hunter-gatherers compared with large-scale societies. To simplify, Rousseauians argue that warfare among prehistoric and contemporary hunter-gatherers was nearly absent and, if present, was a late cultural invention. In contrast, so-called Hobbesians argue that violence was relatively common but variable among hunter-gatherers. To defend their views, Rousseauians resort to a variety of tactics to diminish the apparent frequency and intensity of hunter-gatherer warfare. These tactics include redefining war, censoring ethnographic accounts of warfare in comparative analyses, misconstruing archaeological evidence, and claiming that outside contact inflates the intensity of warfare among hunter-gatherers. These tactics are subject to critical analysis and are mostly found to be wanting. Furthermore, Hobbesians with empirical data have already established that the frequency and intensity of hunter-gatherer warfare is greater compared with large-scale societies even though horticultural societies engage in warfare more intensively than hunter-gatherers. In the end I argue that although war is a primitive trait we may share with chimpanzees and/or our last common ancestor, the ability of hunter-gatherer bands to live peaceably with their neighbors, even though war may occur, is a derived trait that fundamentally distinguishes us socially and politically from chimpanzee societies. It is a point often lost in these debates.

Keywords: Hunter-gatherers, War, Chimpanzees, Peace, Comparative research

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In 1997 Lawrence Keeley coined the phrase “pacifying the past” as a critique of scholars who took what one might call a Rousseauian view of conflict in small-scale societies that predated the advent of the state. Along with such notions as living in harmony with the environment (Hames 2007) and primordial sexual equality and promiscuity (Ryan and Jetha 2012), Rousseauians believed that war and lethal violence were rare or absent in small-scale societies and did not become widespread and intense until settled agricultural life when war was “invented” and then elaborated and intensified with the development of the state. Keeley’s direct experience with this perspective stemmed from his research grants being rejected by funding agencies because he sought to investigate what he hypothesized to be defensive fortifications and other forms of archaeological evidence of warfare in non-state societies. Although his *War before Civilization* (Keeley 1997) made a significant impact, those who hold the Rousseauian position have responded, and a major target has revolved around the intensity and frequency of warfare and lethal violence among mobile hunter-gatherers. The terms “Hobbesians” and “Rousseauians” have gained some currency (Gat 2015), although alternative contrasts such as Hawks and Doves (Otterbein 2004) or warfare having a “long chronology” or “short chronology” (Allen and Jones 2014) make similar distinctions (see Allen 2014a for a review of these positions). However, I will use the Hobbesian/Rousseauian distinction following the penetrating analysis by Gat (2015) wherein he uses the Australian hunter-gatherer material to clearly expose conceptual and empirical problems for those who would pacify hunter-gatherers. Oddly, Douglas Fry, one of the Rousseauian leaders of this conservative counter, claims that the “pervasive intergroup hostility model” is somehow the main orientation held by many researchers (2006:10) who investigate hunter-gatherer warfare. Although this allegedly pervasive model includes such things as hunter-gatherer patrilocality and the closed nature of residential bands, the key claim he seeks to refute is that warfare has a deep history and was common among mobile hunter-gatherers. How one demarcates “common” from “rare” is not defined by Fry, even though comparative researchers have created measures of both warfare frequency, or how often it occurs (e.g., Ember and Ember 1992a), and warfare intensity, or the probability of an individual being killed by another human through war or homicide (Wrangham et al. 2006).

This Rousseauian perspective runs parallel to scholars who cast doubt on the causes, adaptive nature, and intensity of chimpanzee coalitionary violence. A number of researchers, most prominently Power (2005), claim chimpanzee violence is not “natural” or adaptive and is largely the consequence of outside factors such as research team presence and disruptions (e.g., feeding stations) or disturbances by farmers or the bush meat trade (Sussman and Marshack 2010). Such claims have been empirically discounted through a comparative analysis of 22 chimpanzee communities with and without significant contact (Wilson et al. 2014). Similarly, as applied to hunter-gatherers, tribal zone theory (Ferguson and Whitehead 1991) makes a parallel claim: tribal warfare was frequently initiated or intensified as a consequence of colonial invasions.

The goal of this paper is to assess and critique a variety of positions put forward by Rousseauians to diminish the frequency and intensity of warfare among hunter-gatherers. These tactics include the following: misrepresenting evolutionary theory, reclassifying hunter-gatherers; redefining warfare; censoring ethnographic accounts on hunter-gatherer violence; overemphasizing the role of colonial activities in increasing warfare; and questionable use of archaeological evidence and time lines. Finally, and most importantly, I will also argue that although coalitionary violence is a primitive feature of human life that is likely a continuation of the chimpanzee pattern of intergroup relations, the ability to have peaceful relations with neighboring bands is a unique derived trait that fundamentally distinguishes chimps from humans and may have been partially responsible for our rapid cultural evolution.

Misrepresenting Evolutionary Theory

Many Rousseauians consistently misrepresent evolutionary approaches to warfare and violence. This tactic is used because many of the Hobbesian theorists and researchers take an evolutionary perspective (Keeley is a notable exception). The argument made is that evolutionists claim humans are inherently or naturally violent because they have a gene or complex of genes that leads them to be aggressive. Consequently, they conclude that Hobbesians argue that war is

an ineradicable element of human life stemming from human phylogeny. Although ethological theorists such as Lorenz (1963) held such an incorrect view of gene-environment-culture interaction 55 years ago, this outdated form of biological determinism is fundamentally at odds with contemporary behavioral ecological or evolutionary psychological approaches to individual and collective forms of aggression often taken by Hobbesians. Just as troubling, many science reporters falsely associate evolutionary interpretations of violence as representing examples of “inherent violence” or warfare as “inevitable,” with screaming headlines such as “Is Violence Embedded in Our DNA?” (Gabbattiss 2017) or “10,000-Year-Old Massacre Does Not Bolster [The] Claim That War Is Innate” (Horgan 2016). The problem here is a crude and false black-or-white oversimplification of the contingent nature of warfare as proposed by evolutionary theorists.

Richard Wrangham, a leading evolutionary researcher, is a frequent target of such critiques and clearly does not take this position when he says:

But the notion that behavioral evolution generates uniformity of behavior is wrong. In any animal species, tendencies for intergroup violence are strongly influenced by behavioral ecology (the costs and benefits of aggression); and in humans they are further influenced by social learning, including cultural norms. So variability of the practice of war is compatible with behavioral evolution (2013a:6).

Behavioral ecological research has repeatedly demonstrated that mammals are opportunistic and strategic, not blind automatons. Instead they behave as though they weigh costs and benefits before acting, in the context of their life-historical circumstances and culture. This elemental perspective is ignored by Rousseauians.

Minimizing Hunter-Gatherer Violence by Redefining Hunter-Gatherers

In attempting to understand the frequency and intensity of war among hunter-gatherers there is some debate on what constitutes a

prototypical hunting-and-gathering society representative of about 95% of our history as a species. According to scholars on both sides (Bowles 2009; Fry and Söderberg 2013) these societies are residentially mobile, small in size, and egalitarian. In the Standard Cross-Cultural Sample (SCSS), Murdock and White (1969) define hunter-gatherers as societies in which no more than 5% of food resources comes from domesticated resources. The 35 such societies out of 186 in the SCCS are frequently used to evaluate hypotheses about hunter-gatherers. In a reexamination of Robert Kelly's SCCS comparative work (2013) on warfare among hunter-gatherers as well as Ember's (1978) research, Fry (2006:103–5) argues that equestrian New World hunter-gatherers such as the Comanche and Chiricahua used by these researchers should be removed from consideration as prototypical hunter-gatherers because they have composite pastoral and foraging adaptations making them equestrian hunters. This is reasonable. Horses, after all, are a domesticated species and are a historically recent addition to a New World foraging lifestyle. Fry argues that socially stratified and semi-sedentary hunter-gatherers such as the Yurok and Klamath should also be removed. Such groups have lower levels of mobility, relatively permanent residences and substantial dwellings, and are more socially stratified than mobile hunter-gatherers. Prime examples are groups in the Northwest Coast of North America who rely on salmon and other anadromous fish they intercept in defended riverine spots as this crucial resource moves from ocean to rivers annually to spawn. However, removing them from comparative analysis may be questionable because we simply do not know enough about the prehistoric depth of foraging groups with these attributes. Hopefully, archaeological research will allow us to eventually date the advent of semi-sedentary hunter-gatherers. Be that as it may, these groups tend to have defendable or alienable resources (Manson and Wrangham 1991) and are predicted by behavioral ecological theory to be likely engage in warfare if only to defend their resources against others (Dyson-Hudson and Smith 1978). These two general restrictions leave Fry and Söderberg with 21 mobile hunting and gathering societies from the SCCS.

In developing a sample to test hypotheses about the kinds of hunter-gatherers who have characterized human existence for 95% of our time as a species, the degree and form of contact with

non-hunter-gatherers is a crucial issue. Wrangham and Glowacki (2012) note numerous problems with Fry's restricted sample (2007) of peaceful societies. Following the pattern set by Ferguson's tribal zone model (1990), they note that among the "21 nomadic hunter-gatherer societies listed by Fry (2007) as being peaceful, at least 13 (62%) interacted with pastoralist, farming, or state societies in ways suggesting that they were militarily and/or politically subordinate" (Wrangham and Glowacki 2012:21). That is, they were not hunter-gatherers interacting with other hunter-gatherers in a context resembling the EEA. In a response to Wrangham and Glowacki (2012), Fry and Söderberg (2014) ignore this critique.

I would single out the relatively peaceful Mbuti and Semang as prime examples of the problem of hunter-gatherer interaction with settled horticulturalists that casts doubts on their status as hunter-gatherers. In many studies of comparative violence (e.g., Keeley 1997; Knauft 1987) the Mbuti Pygmies and Semang are classified as hunter-gatherers. They were classified as hunter-gatherers by Murdock and White (1969) in the establishment of the SCSS, and researchers sometimes uncritically follow this authoritative classification. But are they truly hunter-gatherers or are they what Fox (1969) inelegantly calls "professional primitives"? That is, can and do they exist independently of horticulturalists? Nearly all Pygmy peoples (e.g., Aka, Mbuti, Efe) are associated with settled Bantu agricultural groups. They trade wild forest products for agricultural crops and metal goods, and Mbuti women may labor in agricultural fields owned by Bantu agriculturalists. Hart and Hart (1986) estimate that 60% of the Mbuti diet is from traded agricultural crops. Whether these relationships are based on political dominance of agriculturalists over Pygmies or a more egalitarian trade symbiosis seems to vary from place to place (Joiris 2003). This sort of economic exchange and agricultural dependence is also characteristic of the foraging Semang of the Malay Peninsula (Rambo 1988) as well. Rambo quotes Schebesta (1929:276) (a PAS, or principal authority source in Fry and Söderberg's 2013 sample discussed below) who states: "[the Semang] live in a kind of symbiosis with [agricultural Malay and Senoi] villages and settlements, for, in my opinion they are unable to subsist solely on the roots they find in the jungle." Underlying both the Mbuti and Semang cases is a debate about whether a pure hunting-and-gathering existence is possible in

African and Southeast Asian rainforests (Bailey et al. 1989), where both these groups live. This debate need not concern us here. What is relevant is whether groups such as the Mbuti and Semang were politically and economically independent hunter-gatherers at the time data on lethal violence was gathered. My read of the sources suggests they were not.

Definitions of War

Any estimation of the frequency and intensity of warfare requires a well-accepted definition of warfare or coalitionary violence, otherwise researchers will simply talk past one another and provide statistics that may be useless for evaluating descriptive generalizations or identifying the factors that account for variation in warfare. Some Rousseauians tend to favor definitions of war that typify patterns found in large-scale societies while classifying forms of coalitionary violence in small-scale societies as capital punishment or feud. The common definition of war embraced by most cross-cultural researchers (Ember and Ember 1992b:172) is as follows:

Warfare is defined as socially organized armed combat between members of different territorial units (communities or aggregates of communities). In the ethnographic record, such combat usually involves groups on both sides, but a warfare event could involve the ambush of a single person of an enemy or group. Thus, the phrase “socially organized” means that there is a group of combatants on at least one of the sides.

This definition is more or less identical to what Wrangham (1999:5–9) defines as between-group coalitionary violence and is in standard use by other comparative warfare researchers (e.g., Bowles 2009; Otterbein 2004). To make matters a bit more complex, if we turn to another well-known comparative scholar of warfare, Keith Otterbein (1968:281), we have something called “feud” that appears as a form of coalitionary violence within a community: “Warfare and feuding can be distinguished if feuding is defined as blood revenge occurring

within a political community and warfare as armed combat occurring between political communities.” The problem with this definition is that it combines motive (revenge) for lethal conflicts between coalitionary groups which are most often families or kinship groups. Nevertheless, feuding, regardless of the motive, is clearly a form of within-group coalitionary violence.

Fry (2006:88, 172–74) does not consider feuding between communities as warfare. Instead, Fry defines warfare, following Prosterman (1972:140), as:

A group activity, carried on by members of one community against members of another community, in which it is the primary purpose to inflict serious injury or death on multiple nonspecified members of that other community, or in which the primary purpose makes it highly likely that serious injury or death will be inflicted on multiple nonspecified members of that community in the accomplishment of that primary purpose.

This definition is very similar to Kelly’s (2002:5–6) concept of “social substitutability,” in which anyone in a neighboring community is a legitimate target of lethal violence even though the attacking party may hope to dispatch a specific malefactor. That is, if someone from one community kills or harms someone in another, members of the community who suffered the loss may target any individual in the perpetrator’s community. Consequently, co-residents are responsible for the deeds of their community members and are legitimate targets of reprisal should one of their own community wrong someone from another community. Clearly, this is a characteristic of the kind of warfare found in large-scale societies.

One must ask, what is the utility of distinguishing between various forms of coalitionary killing and declaring that one type is “true” war, as do Fry and Raymond Kelly (see Roscoe 2014 for an extended critique)? Whereas Fry provides no clear justification to distinguish feud from war, Kelly does so as to distinguish war from capital punishment. According to Kelly, capital punishment is a form of socially approved killing (therefore not murder), targeting a transgressor who has broken some sort of social norm regardless of whether the malefactor is

a member of one's community. Whether or not the killing is coalitionary is irrelevant. However, "true war" occurs when an entire community is found to be liable and therefore worthy of killing because one of their group has harmed a member of aggrieved group. It represents collective responsibility that focuses on the corporate nature of the contending groups. Using this definition, Kelly (2002:132) argues that war was rare or absent among a large number of what he calls non-segmentary hunter-gatherers even though coalitionary killings between groups occur. Non-segmentary hunter-gatherers are those who lack, among other things, descent groups and preferential marriage rules. Kelly (2002:123) without explanation, even goes so far as to deny that war occurs between chimpanzee bands, even though chimps target any individual in a neighboring band, when he states, "pongicide (apes killing one another) is an analogue of homicide and both are undoubtedly ancient. However, chimpanzees lack both capital punishment and war."

The problem with Kelly's and Fry's definition of war is that it conflates a cultural construct or rationale (socially approved killing in the case of capital punishment or group responsibility in the case of warfare) with a structural or behavioral definition of the action fundamental in any evolutionary analysis. We do not know much about chimpanzee culture or what is going on inside the mind of a chimpanzee or any other species that we might compare with humans. Although we can infer function or motivation for chimpanzee warfare (e.g., mates and/or food resources), warfare is defined behaviorally in terms of group residence and coalitionary lethal interactions between separate residential groups. If one examines the definition of war used by Ember and Ember and followed by most cross-cultural researchers, including archaeologists, motivation or social acceptability are not parts of the definition of war.

One way to view Kelly's and Fry's definitions of true war is simply that coalitionary killing has become more complex and organized through time. Humans and chimps engage in between-group coalitionary killing by targeting individuals who are members of other residential groups. As groups become more corporate, often through descent group organization, members cooperate and share rights and duties in the political arena (Fried 1957). They jointly defend one another and take lethal action on other groups. They become more strongly

united for certain purposes, including collective responsibilities for their members. Such groups also have the ability to stop war by using conflict resolution mechanisms that prevent their members from breaking a peace. The definition of “true war” employed by Fry and by Kelly allows them to claim war is relatively new and more common today than in the past, even though between-group coalitionary killings are an ancient human practice shared with chimpanzees, our last common ancestor, and hominins (Kissel and Kim 2019). What Kelly (2002:52, Table 4) has documented in his distinction between segmentary and nonsegmentary bands is that some hunter-gatherer bands are more corporately organized in their pursuit of war.

Censoring Ethnographic Reports and Lethal Events

As part of a special edition of journal *Science* entitled “Human Conflict: Winning the Peace,” Fry and Söderberg (2013) surveyed accounts of lethal aggression events among mobile foraging bands. The main results of their analysis are portrayed in their Fig. 1, a bar chart showing counts of individual and collective acts of lethal violence in a sample of 21 mobile foraging bands winnowed, as noted earlier, from the SCCS. Their Table 1 provides a breakdown of these events by “Reason.” They conclude that “Overall, the findings suggest that most incidents of lethal aggression among MFBS [mobile foraging band societies] may be classified as homicides, a few others as feuds, and a minority as war” (Fry and Söderberg 2013:270). To some extent, this is a useful approach because previous comparative data did not distinguish between homicides and warfare in causes of death (e.g., Keeley 1997; Knauff 1987). The goal of these earlier surveys was to measure the probability of being killed by another human whether through homicide or war to assess the demographic impact of lethal violence. Without precise differentiation they reasonably assumed death through war was more common than homicide. Comparative research on homicide and warfare shows a positive correlation between frequency of homicide and assault on one hand and the frequency of warfare in non-pacified societies (Ember and Ember 1997:10). But these measures were rank-ordered and referred to frequency (e.g., common versus uncommon) and not to intensity (e.g., deaths per 100,000/year).

This being said, the statistical and methodological problems with Fry and Söderberg's generation of the descriptive data sharply limit their generalizations and conclusions. I will deal with the methodological problems first and follow with the statistical problems.

Methodologically, the data sources used in the survey are censored through the use of Principal Authority Sources (PAS) in their sample of 21 hunting and gathering societies from the SCCS. These sources, as determined by Douglas White (1989), who created the standard, are argued to be the earliest reliable ethnographic sources for societies in the SCSS. Researchers use the SCCS as a sampling frame because it contains a common set of preindustrial societies scattered about the globe in an attempt to come close to an independent sample. This sampling frame has been the source of hundreds of comparative studies. Nevertheless, one is not required to use PAS sources when doing comparative research. Fry and Söderberg did so:

To circumvent sampling bias, rather than self-selecting cases, we derived the sample of MFBS based on the published rating criteria of other researchers. During data collection, we used only the principal authority sources (PAS) as the earliest, high-quality ethnographic descriptions available (Fry and Söderberg 2013:279).

This selection criterion poses a problem that leads to their results being an artifact of this restrictive set of sources by censoring potentially reliable non-PAS sources in their sample. The problem with some early, high-quality PAS sources is that some may contain little detail on violence and conflict resolution even though they may have rich and extensive information on standard ethnological topics, such as family, marriage, and religion. Another problem with their strict reliance on PAS is that some of their sources describe lethal events that are clearly a post-contact phenomena. Their PAS sources include cases of "execution of outsiders (such as missionaries, explorers, castaways, and colonists)" (in their online Table S3:23). If their goal is to evaluate claims that that "chronic raiding and feuding characterize life in a state of nature," or that hunter-gatherer "war deaths are substantial in recent millennia and in the Pleistocene" (Fry and Söderberg 2013:272), then documentation of lethal events of or by modern

outsiders is problematic. Such inclusions may inflate the number of deaths. Ironically, Fry (2013:13) strongly criticizes Bowles's (2009) use of Hiwi and Ache deaths from warfare because nearly all those deaths were at the hands of modern ranchers and other colonists.

The case study approach used by Fry and Söderberg may also hide the overall assessment of the frequency of violence. If a PAS ethnographer offers no or few case studies of violence and simply says that warfare is frequent, exacts a heavy toll on human life, and talks about displacement with only a few case studies, then a society can be shown to have few lethal events. Of course, the strength of the case study approach of Fry and Söderberg is that one can gain rich details on the causes and consequences of lethal violence. In the end, it is up to a researcher to select those sources she or he believes to be reliable and accurate, and by citing those sources in requisite detail to allow others to evaluate the quality of those sources. I will use examples of the Andaman Islanders and the !Kung in Fry and Söderberg's sample to illustrate the problem of censoring.

In "Warless Societies and the Origins of War" (2002), ethnologist Raymond Kelly details lethal violence among hunter-gatherers in the Andaman Islands. Therein he notes 13 lethal raids between Bea and Jarawa (Kelly 2002:100), with the Jarawa geographically displacing the Bea through warfare. In doing so, he relies on what he believes to be reliable historical accounts of British colonial authorities, such as Portman (1899). The two instances of lethal events for the Andaman Islanders in Fry and Söderberg's Fig. 1 (2013) is clearly discrepant.

A further example of the problematic use of PAS sources is seen in Richard B. Lee's masterful work on the !Kung (1979) wherein he describes 22 case studies of homicide, yet Fry and Söderberg's data (2013:271, displayed in their Fig. 1) show but four cases because Lee's work was not in the PAS. However, D. White in his PAS article classifies Lee's research as "Other dependable primary sources" (White 1989:23). Note that after Lee's publication, the figure for the !Kung was increased to 84 (Wiessner 2016), although many of the newly recorded deaths are clearly the consequence of alcoholism and other colonial influences. Although Fry and Söderberg discuss Lee's data to better characterize the nature and causes of !Kung interpersonal violence, that information is not represented in their Fig. 1 from which their major conclusions on mobile foragers' lethal events are drawn.

The two statistical problems involve demographically uncontextualized events and using “lethal events” as a measure without counting the number of deaths in each lethal event. The data shown in their Fig. 1 and Table 1 (2013:272) are raw counts of lethal events without controls for population size or time frame, making the results difficult to interpret within their sample or cross-culturally. That is, the counts have no denominator to control for time frame or population. In comparing rates of violence the standard is to provide deaths per 100,000 per year or proportion of causes of deaths as a consequence of violence (e.g., Bowles 2009), allowing a population-based comparative perspective. The earliest tally of deaths through violence from Knauft (1987) through Keeley (1997) to more recent tallies combining a variety of sources (Roser 2018) use these contextualized measures.

The second statistical problem involves counting lethal events without counting the number who died in those events. Fry and Söderberg’s measure of violence as a “lethal aggression event” is useful for gaining a sense of how common various types of lethal events occur, such as “intergroup events” or “interpersonal events,” and represents a noteworthy contribution to the literature. In their Table 1 (2013:272), 33.8% of these lethal events are classified as “intergroup events,” which is in accord with the standard definitions of warfare used by comparative researchers. However, the number of events does not fully measure the impact of violence. Evolutionary theorists argue (e.g., Bowles 2009) that the demographic impact of war measured as the proportion of violent deaths is the critical measure of the importance of war from an evolutionary perspective. Even those who do not take an evolutionary perspective, such as Keeley (1997:88), regard this measure as fundamental for understanding the demographic importance of warfare. Using lethal events would be like a biogeographer using number of rain events to predict biomass patterns across landscapes instead of centimeters of rain per year.

Fortunately, Fry and Söderberg (2013) provide data on number of deaths for each “lethal aggression event.” I reanalyzed the data provided in their online supplementary data (Table S4), where “number killed” was associated with “relationship” between “Killer(s)” and “Victim(s).” Unfortunately, in the “relationship” column for 18 of the cases, no relationship was specified, and many other entries in that column, such as “different clans,” did not permit a sure assignment

of within- or between-group killings. I then grouped number killed, as best I could into within-group and between-group deaths: within-group deaths mean that the killing took place within the same residential group and between-group deaths were killings that occurred between residential groups. Of the useable 116 cases of lethal events, 59 deaths (or 50.8%) were between-group deaths and 57 (or 49.1%) were within-group deaths. Note that a massacre among the Vedda accounted for 25 of these killings (Fry and Söderberg estimate between 20 and 30 deaths, so I took the middle value of 25 in my reanalysis). Although Fry and Söderberg show that within-group lethal events are more common than between-group lethal events (66% vs. 33%, respectively), death counts from warfare and within-group homicides were nearly equal in terms of total killed.

It is perhaps more important to point out that Fry and Söderberg's finding of "not much warfare" imprecisely replicates previous research. Ember and Ember (1997:7, Fig. 1.1), using the SCCS, show that on average, hunter-gatherers have the lowest frequency of war ("war occurs once every two years or so") compared with horticulturalists, intensive agriculturalists, and pastoralists, among whom war, on average, occurs "every year in a season." This result generally coincides with the finding of Wrangham et al. (2006:20, Fig. 2) showing the intensity of violent death is lower among hunter-gatherers (at 200 per 100,000/year) than "farmers" (at 600 per 100,000/year). It is possible that the hunter-gatherer estimates may be higher prior to contact because many of these groups were "pacified" (Michael Wilson, personal communication 2018). What these figures mean in more existential terms can be calculated in terms of the proportion of all deaths as a consequence of war. For hunter-gatherers, Bowles (2009), using a sample of 8 ethnographically described hunter-gatherers and 15 from the archaeological record, found that the fractions of adult mortality resulting from warfare were approximately equal at 14%. Max Roser (2018) has assembled a very comprehensive online lethal violence data base of anthropological and archaeological studies using published sources that began to accumulate after Knauff (1987) made his path-breaking start. I downloaded his "Share of violent deaths for non-state societies" CSV file, which lists percent of adult deaths due to warfare and other forms of lethal violence for an ethnographic sample that I divided into 7 hunter-gatherers and 18 horticulturalists. The

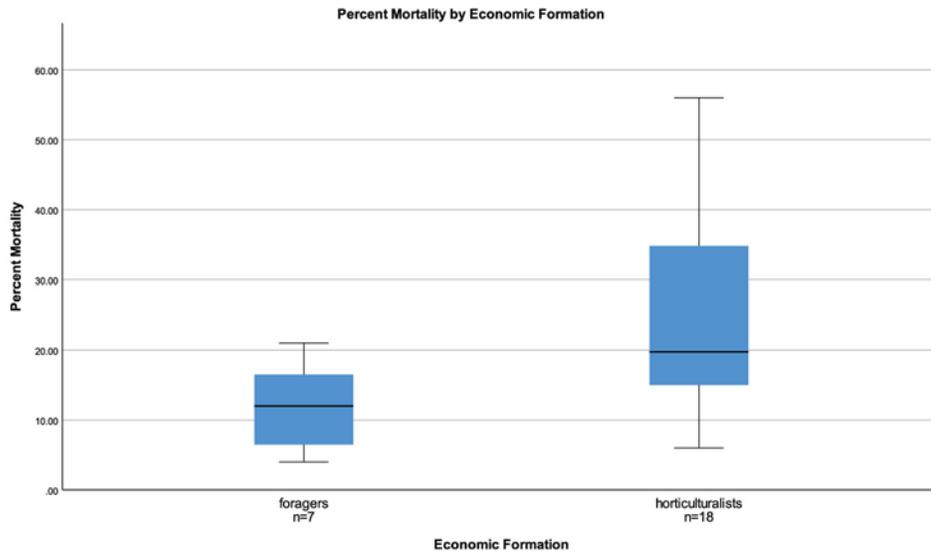


Fig. 1. Percent mortality from lethal violence by subsistence type

adult violence mortality percentages were 12% for hunter-gatherers and 25% for horticulturalists, and the difference is statistically significant ($t = -2.693$, $p = 0.016$; see Fig. 1).

Trends in Lethal Violence

The general trend documented by Keeley is that the demographic impact of war declined with the evolution of the state. Percent of population killed per annum is much lower (5 to 40 times lower) in state societies (1995:89, Fig. 6.1) than in non-state societies. The same pattern emerges in warfare deaths as a percentage of all causes of death (1995:90, Fig. 6.2). Within that range of social and economic complexity the pattern first shown by Wrangham et al. (2006: Table 6) and here (Fig. 1) indicates a doubling or tripling of warfare intensity among “farmers” relative to hunter-gatherers even though these rates sharply decline in state-level societies (Keeley 1995). Closer inspection of the societies in the Wrangham et al. data set and here indicates two things. First, the so-called farmers in Fig. 1 range from simple slash-and-burn horticulturalists to rainfall agriculturalists, all of whom lack draft animals, the plow, and other means of technological

intensification standard in state-level societies. In addition, many of these societies, especially those in Amazonia, depend heavily on foraged resources (Hames 1989). Second, these “farmers” are associated with headman (or big-man) political organization and not chiefdom-level societies governed by hereditary chiefs. At this point we do not understand the causes of this now well-documented uptick in war in subsistence regimes moving from hunter-gatherers to simple horticulturalists and/or how war intensity may have changed from egalitarian societies to more hierarchically organized big man societies to centralized chiefdom and early state-level societies.

Tribal Zone Theory

Tribal zone theory is a historical approach that focuses on how the predatory actions of colonial powers may have affected the intensity, frequency, and nature of warfare in small-scale societies. As colonial forces moved into indigenous tribal areas they engaged in territorial appropriation, trade, warfare, slaving, resource extraction, and introduced devastating diseases. In the process they also displaced groups, oftentimes forcing them into areas occupied by other tribal peoples and leading to an initiation or increase in intertribal warfare (Ferguson and Whitehead 1991). A classic example of this approach is found Secoy’s (1953) account of historical warfare on the Great Plains in North America. Access to horses and guns through the fur trade along with colonial displacement led to a dramatic increase in warfare in the North American Plains as well as in the Southwest. Colonists also sometimes employed indigenous peoples as mercenaries against other indigenous peoples. Tribal zone theorists do not claim that warfare and other forms of violence did not exist prior to colonization (Ferguson and Whitehead 1991). Ferguson emphasizes that accounts of tribal warfare by explorers, colonial administrators, early ethnographers, and traders have been used by ethnologists in comparative studies of the intensity and frequency of warfare without placing these accounts in their proper historical perspective.

In a series of publications, Ember and Ember (1992a, 1997, 2001) have put the tribal zone hypothesis to test by examining warfare frequency before and after “pacification.” By pacification they mean

the time at which external powers imposed administrative control on the people they invaded (Ember and Ember 1992b:169–71). They then compared the frequency of warfare combining all sources (before and after pacification) with accounts of warfare prior to pacification. They were able to show that the frequency of warfare was greater prior to pacification than after. At the low end of the scale, warfare coded as “absent or rare” went from 27.6% in the overall sample down to 8.98% in the unpacified sample, and at the high end, warfare coded as “constant or any time” went from 38% in the overall sample to 56% in the unpacified sample (Ember and Ember 1997:5). One of the reasons they used this division was to “distinguish the truly more peaceful societies from the societies that had had peace imposed upon them” (Ember and Ember 1992b:169). We can draw two conclusions about outside contact. The first, which aligns with Ferguson’s emphasis on historical context, is that history and nature of contact with outside forces in some cases do affect tribal war by increasing its frequency and intensity. Second, colonial contact much more generally decreases the frequency of warfare, which is the opposite of Ferguson’s historical perspective that contact tends to increase the frequency of warfare.

As noted above, the perspective in tribal zone theory is similar to Power’s (2005) claim that chimpanzee violence is a consequence of unnatural, outside influences such as feeding stations, the bush meat trade, and agricultural intrusions and is not adaptive. Comparative data from 22 chimpanzee study sites by 30 researchers (Wilson et al. 2014), however, provides clear evidence that chimpanzee violence is not caused by human contact or other unnatural conditions. As might be expected, Ferguson, the leading proponent of tribal zone theory, is developing a critique of the comparative research on chimpanzee warfare in all its historical complexity currently entitled “Chimpanzees, War, and History: Are Men Born to Kill?” (https://www.researchgate.net/profile/R_Brian_Ferguson ; also see Ferguson 2014). Note the subtitle suggests that he may claim that Hobbesians believe human killing is coded in genes and is inevitable. (For an examination of Ferguson’s shifting position see Gat 2015:113, and see Wilson 2014 for a point-by-point rebuttal of Ferguson’s critique.)

Archaeology, Agriculture, and Warfare

Based on archaeological evidence, Haas (2001; Haas and Piscitelli 2013) makes two arguments about the antiquity of war: (1) prior to the invention of agriculture 10,000 years ago there is scant evidence of warfare in the archaeological record and (2) contemporary or ethnographic accounts of warfare among hunter-gatherers cannot be used to inform the past history of violence because expansion by farmers and later colonial empires disrupted hunter-gatherer society. It is quite true that there is little evidence of warfare prior to the 10,000-year date of the transition to agriculture (but see Wendorf 1968 and Mirazón Lahr et al. 2016 on archaeological evidence of hunter-gatherer war prior to this date). To a great extent this lack of archaeological visibility is a direct consequence of differences in the archaeology of hunter-gatherers and farmers (Wilson 2013). As detailed by Bamforth (2018) and many others, compared with hunter-gatherers, agriculturalists are more sedentary; exist at higher densities; have more durable dwellings and, on occasion, fortifications; create larger middens; and oftentimes have cemeteries near their settlements. Finally, the older the site, the more likely that taphonomic processes (e.g., erosion) will destroy, obscure, or dramatically alter a site. Consequently, more recent archaeological remains of sedentary people are much more likely to be preserved and encountered by archaeologists than those of earlier hunter-gatherers who leave a light imprint on the landscape.

More to the point, the use of the “prior to 10,000 year mark” by Haas and Piscitelli (2013) is deceptive for a number of reasons. It is true that agriculture originated in the Middle East about 10,000 years ago (Bar-Yosef 2017), but it developed much later in other parts of the world, and never developed in other places, such as Australia, prior to recent European colonization. At the base of their argument is the general claim that ethnographic and historical accounts of hunter-gatherers are tainted by contact with agriculturalists. This contact led to an increase in warfare as a consequence of agricultural expansion into hunter-gatherer lands and is consonant with the tribal zone perspective of Ferguson (1990). Although agriculturalists can dominate the political and social lives of hunter-gatherers through trade and intermarriage (e.g., Headland and Bailey

1991), this position lacks archaeological and historic credibility outside of Europe and the Middle East (but see Gat 2015 on Old World warfare). More to the point, it is a red herring. There is considerable archaeological evidence of high levels of lethal violence and warfare among hunter-gatherers in Native North America (Schwitalla et al. 2014) without contact with agriculturalists. In Australia (Pardoe 2014), agriculture was completely absent until 1788 when the British first colonized the continent. The important edited volume by Allen and Jones (2014) provides archaeological surveys and case studies of intense warfare among North American and Australian hunter-gatherers in the absence of contact with agriculturalists (see also Lambert 2002 for a classic survey of North America). Research on Californian hunter-gatherers is of particular significance because of the tremendous amount of skeletal evidence of lethal trauma in areas far from agricultural centers (Allen et al. 2016). And in Australia, archaeological evidence as well as historical and ethnohistoric evidence document frequent warfare from ecologically rich drainages such as the Murray River as well as resource-poor desert areas as well (Allen 2014b). Although Haas's 10,000-year mark may be relevant in certain Old World contexts, it is completely irrelevant for other parts of the world.

War Is a Chimpanzee Invention and Peace Is a Human Invention

A key element in the distinctive evolution of human society is the development of peaceful relations between residential groups (Rodseth and Wrangham 2004; Rodseth et al. 1991), or what Chapais (2010:41–43) calls “male pacification” and “between-group pacification.” In 1940, Margaret Mead claimed that “Warfare Is Only an Invention—Not a Biological Necessity” (cited in Gat 2015:123). Whether or not war is a cultural invention boils down to unproductive semantics. The subtext of Mead's statement is that war is simply an arbitrary cultural invention and not a part of human nature. Consequently, there is hope for an end to war. From an evolutionary biological perspective, war and peace are simply human capacities that are elicited under specific conditions that we would like to understand more fully to promote peace.

What is lost in this semantic debate is that from a phylogenetic perspective it is clear that peaceful relations between human residential groups are a derived feature that fundamentally differentiates humans from chimpanzees and perhaps our last common ancestor. Chapais (2009:232–33) in *Primeval Kinship* traces the evolution of phylogenetically derived human social characteristics such as relatively durable pair bonds in the context of multi-male mixed-sex groups. He recognizes that “peace prevailed at the intratribal level (between bands), but not at the intertribal level” and hence that “the tribe did not eliminate intergroup hostility but brought about a major change in the level of social structure at which hostility was taking place.” This fact is typically ignored in the discussion of the evolution of human warfare by both camps. Unlike chimps, hunter-gatherers can have peaceful relationships with neighboring bands that include prolonged visits by families, marriage, trade, and resource sharing in times of need. For the past century and in various ways this wholly distinctive human feature has been recognized by major social theorists such as Edward Tylor, Leslie White, and Claude Lévi-Strauss (Rodseth et al. 1991) in studies of inter-band marriages. Among chimpanzees, neighboring groups are territorial and between-group encounters range from avoidance with occasional skirmishes to predatory invasions when raiding parties seek to pick off lone individuals or small groups who are numerically outnumbered by raiders in the balance of power model (Wrangham 1999). As first documented by Goodall (1986), one group may exterminate another and take over its territory. However, among bonobos relations between neighboring groups are generally benign. Despite long-term observation at many sites, there is no sure evidence of intragroup or intergroup killings. When bonobo bands meet in border areas, interactions range from noisy agonistic displays to relatively tense but peaceful minglings, including co-nesting (Pisor and Surbeck 2017), and even food sharing along with a high degree of social tolerance between groups (Fruth and Hohmann 2018). However, nearly all these interactions last no longer than a day or so and groups part with no exchange of individuals. Upon sexual maturity, chimp and bonobo females, but not males, join neighboring groups; they lose contact with their natal band and they do not return. It is fair to say that bonobos have peaceful inter-band interactions but they lack systematic social intercourse and partial, relatively long-term fusions among neighboring bands or band members.

Chimp and bonobo separatism is in sharp contrast to humans. Human bands typically have peaceful relations with their neighbors even though war may occur. The hunter-gatherer literature abundantly describes intergroup visiting between members of distinct residential groups for various lengths of time and for different purposes, and in some instances individuals or families may take up semi-permanent to permanent residence in neighboring bands. These interactions encompass trade, visiting relatives, ceremonies, and aggregations around dense seasonal resources (Layton et al. 2012). For example, among the Gwi Bushmen “Not only do individual families visit neighboring residential bands but an entire band may move into the territory of another during times of plenty for festive purposes and may also temporarily join another when local resources are scarce so long as they have the permission of their neighbors” (Silberbauer 1972:296–97).

Both Layton et al. (2012) and Chapais (2010) characterize hunter-gatherer social organization as multilevel such that local residential bands are linked to other bands through a variety of ties, creating a regional band structure (see also Wilson and Glowacki 2017:486). This settlement pattern has been recognized using terms such as macro band, ethnolinguistic unit, and regional band to encompass entities within which marriage, trade, and so on, can peaceably occur (e.g., Damas 1969). Following Helm (1965), I use the term “band” or “local band” to characterize an entity that lives together most of the year even though some may have a fission-fusion structure whereby families or task groups separate during the year only to reaggregate on a regular basis. A regional band is composed of local bands who often interact peaceably and speak a common language. Layton et al. (2012:1217–19, Table 1) provide demographic characteristics for 26 hunter-gatherer societies that comprise local and regional bands (or what they call a “community”). They define a regional bands as an “aggregation of bands between which individuals can move with a minimum of formality, within which most marriages take place and that is frequently characterized by a distinct dialect or language (2012:1221). Regional bands on average are composed of fifteen to seventeen local bands, and total regional band size is about 250–500. Using a larger sample, Binford (2002) uses the term “ethnolinguistic unit” instead of regional band, and with a much larger data base he calculates a mean of 839 inhabitants for regional bands and a mean local band size of 54.

This is not to say that local bands within a regional band do not engage in internal warfare. They do, and how often this occurs is a matter for empirical research. It is also probable that relations between regional bands tend to be more hostile, and external war (war between different cultural or ethnolinguistic units) may be more deadly (e.g., Burch 1974, 2005). Comparative research by Walker and Bailey (2013) using a combination of hunter-gatherers and foraging horticulturalists from lowland South America shows that although internal war is more frequent than external war, external war is more deadly. One probable reason for this difference is that such groups represent different cultures and speak different languages or dialects, making not only conflict resolution difficult but also the establishment of prior friendly relations through various forms of peaceful social intercourse.

From a phylogenetic perspective, between-band coalitionary violence or warfare may be a primitive or ancestral trait in humans shared with chimpanzees and perhaps our last common ancestor, whereas variable peaceful relations between human bands in a region is a derived trait. Other primitive traits we share with chimps may include reconciliation, sympathy, consolation, kin altruism, and perhaps reciprocal altruism (de Waal 2010; Silk and Boyd 2010). In a number of publications, Boehm (e.g., 1993) sketches the evolution of primitive and derived traits in humans and chimps that relate to within-group violence and conflict resolution. Chimps and humans use coalitions to battle for dominance (de Waal 2010) and later reconcile to reduce the possibility of further conflict between individuals and coalitions. Boehm's (2012) so-called reverse dominance hierarchies arose in humans to check the dominance of alpha males and their allies leading to a lower reproductive skew. How this arose is unclear, but the development of effective weapons (equalizers) to diminish physical strength differences between males is a strong candidate (Gintis et al. 2015).

The ability of hunter-gatherer bands to peacefully coexist with some of their immediate neighbors is the single most significant social attribute that differentiates humans from chimpanzees and may well be an elaboration of bonobo intergroup relations. This is something that Rousseauians and Hobbesians ought to recognize in their debates about the nature of coalitionary violence. This derived trait may have been crucial for the spread and elaboration of culture (Hill et al. 2014).

For example, Layton et al. (2012) estimate that human communities (regional bands) are, on average, at least five times larger than chimpanzee bands. Peaceful and regular interaction between local bands permits the sharing of important discoveries vital to survival, such as food processing techniques, tool innovations, and traditional ecological knowledge (Henrich 2004). For example, primatologists have argued that the impoverished tool repertoire of orangutans relative to chimps (Fox et al. 2004) is a consequence of social group size that affects the rate of adaptive innovations.

Is War Adaptive?

Rousseauians argue that warfare is not evolutionarily adaptive for empirical and apparently political reasons. Denial of the antiquity and significance of war stems from a worry that if warfare is an adaptation it will either condemn us to perpetual violence or diminish our attempts to prevent war (Fry 2006:234–38; Sussman and Marshack 2010; see Wrangham 2013b for a response). This sort of political argument is irrelevant: the answer to the question of whether war is adaptive can only be answered with empirical research and not moral alarm about the consequences of such knowledge.

Fry and Söderberg (2013:272) state, “the evolutionary logic of fighting, and the observation that killing is an exceptional event in human societies leads to the counterhypothesis that lethal behavior has been strongly selected against, not favored, in comparison to more restrained conflict behavior.” From an evolutionary perspective, whether or not a certain kind of behavior is common or exceptional is much less important than the measurable fitness consequences of the behavior. Fig. 1 shows human violence is a major cause of death in small scale societies, especially for males even though it may be “an exceptional event.” At the individual level of selection, Chagnon (1988) demonstrated that Yanomamö warriors known as *unokais* (those who had killed enemies) had greater reproductive success than those who had not. Among Nyangatom herders on the Ethiopian-Sudan border, Glowacki and Wrangham (2015) provide evidence that elders who were prolific raiders had more wives and children than other elders, thus replicating Chagnon’s Yanomamö findings. In a sample of

eight Yanomamö villages, Chagnon reports that 11–17% of all married women were forcibly abducted from neighboring villages by militarily more powerful groups (Chagnon 1997:88, Table 2.1). In their survey of warfare in the 186-society SCCS sample, Ember and Ember (1992a) show that, in 73% of cases, victors sometimes drove vanquished from their lands, and in 90% of the cases, victors took transportable resources (e.g., domesticated animals and crop stores). In contrast, Beckerman and colleagues' (Beckerman et al. 2009) research on the Waorani, an Ecuadorian group much like the Yanomamö, found a negative relationship between zealous (those who frequently raided) and non-zealous warriors in terms of reproductive success. Importantly, Beckerman et al. argued that the Waorani were warring themselves into demographic decline because a large fraction of deaths were women. Clearly much more work is needed in this area that may benefit from a multilevel adaptationist approach (e.g., Bowles 2009).

A unique characteristic of human warfare meriting better understanding is revenge, a phenomenon noted by many (see Gat 2000 for an evolutionary analysis of revenge and Beckerman and Valentine 2008 for case studies in Amazonia). Walker and Bailey (2013) surveyed warfare and homicides in 11 pre-contact lowland South American societies totaling 1281 deaths. Revenge was the motivation in 70% of these killings. Furthermore, they concluded, "The average percent of violent deaths across all 11 studies is 30% indicating that violence was probably a potent selective pressure for many traditional societies" (2013:31). In standard evolutionary approaches to violence the focus is on reproductive advantage through acquisition of food-related resources or mates. How revenge may be explained with evolutionary logic is a major challenge. It is perhaps the case that a posture for revenge, for previous attacks or supernatural depredations (e.g., sorcery accusations), may lead to deterrence or motivate resource or mate acquisition.

Conclusion

I have argued that Rousseauians have used a variety of tactics to diminish the significance and the reality of war among hunter-gatherers. These tactics include redefining warfare, ignoring archaeological

evidence of war among hunter-gatherers, censoring sources, and focusing on the frequency of warfare while ignoring the demographic consequences of warfare. Researchers who take an evolutionary approach to human warfare generally take a Hobbesian approach by arguing that warfare has a deep human history and has played a significant role in human social evolution. Furthermore, they have empirically established that war is less frequent among hunter-gatherers than among more intensive economic formations (Ember and Ember 1997) and the mortality rate for violence among hunter-gatherers is lower relative to that of agriculturalists (Wrangham et al. 2006). In response to tribal zone theorists and others, Ember shows that even after she adjusts her sample to eliminate complex fishing hunter-gatherers and equestrian hunter-gatherers, warfare is rare or absent in only 12% of her sample of mobile hunter-gatherers (Ember 1978:443).

There are at least two outstanding problems in the evolution of warfare we ought to address. How do we account for the tremendous upsurge in war deaths, on a per capita basis as we move from egalitarian to semi-egalitarian horticultural groups, only to decrease sharply with the advent of the state? Another issue is how to make sense of revenge as a cause of war, which seems to be absent in chimpanzees but common in humans.

At this point, the positions taken by Wiessner and Robert Kelly are the ones we ought to follow. In an interview by Culotta (2013:224) accompanying the Fry and Söderberg (2013) piece in *Science*, Wiessner is paraphrased as follows: “But given that some foragers do fight fiercely, she hopes the battle lines among scientists will shift to asking what promotes and what inhibits warfare” and “We should be asking how coalitionary aggression, which does appear in our phylogeny, was harnessed among our successful ancestors.” A similar view is stated by Robert Kelly: “the better question is: when do foragers resort to war?” (2013:158).

We have made some progress in understanding war for both humans and *Pan*. There are three different theories on factors that lead bonobos to be pacific and chimps to be warlike (White et al. 2013). Likewise, to understand variation in warfare among hunter-gatherers we have cross-cultural statistical analyses that point to resource competition, such as Ember and Ember’s (1992b) resource unpredictability and Kelly (2013) on population density. Manson and Wrangham

(1991) point to the presence or absence of alienable resources as determining whether resources or mates will be targets in warfare. One of the problems faced by comparative researchers is an overreliance on the 35 hunter-gatherer societies in the SCCS or Fry and Söderberg's (2013) exclusions to reduce the sample to 21. Use of the SCCS as a sampling frame was designed to overcome what is known as Galton's problem of statistical independence. But it does not (Dow and Eff 2008). One way around this problem is to use phylogenetic comparative methods (e.g., Mace et al. 1994) to control for common ancestry in order to test for functional relationships on the causes of warfare. This method is gaining acceptance in cross-cultural research. Using this method would allow us to take advantage of a much larger data base, such as Binford's *Constructing Frames of Reference* (2002), which contains information on more than 300 hunter-gatherer societies. Finally, archaeological data on recent hunter-gatherers largely isolated from non-hunter-gatherer societies, such as the Central California Bioarchaeological Database (Pilloud et al. 2014), may lead to new insights. By following these avenues we would then be in a much better position to identify the factors that promote war and peace among hunter-gatherers.

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