

# THE CAUSES AND ORIGINS OF "PRIMITIVE WARFARE"

## ON EVOLVED MOTIVATIONS FOR WAR

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Azar Gat's argument represents a major advance toward realism in neo-Darwinian theory on war. Consistently reasonable, plausible, with substantial evidence (on some points), his basic argument is that a wide range of reasons for war are all part of an integrated motivational complex, evolved to deal with problems of survival and reproduction in our species past. Some, clashes of material (somatic) or reproductive interests, are "root causes" of conflict. Others are secondary or derived, "second floor" elaborations necessary for coping with more basic competition, including an impulse toward revenge, sensitivity to status, fear of sorcery, quest for power, even predilection toward sadism. His point is that cultural anthropologists have been mistaken in trying to identify one versus another as explaining war, because they all are involved, non-reductionistically, stamped into our species' mind by their complementary contributions to our evolutionary success.

In approaching war, he avoids the more dubious evolutionary constructs, such as "instincts to kill" (Ghiglieri 1999: 178), "Darwinian algorithms" for collective aggression (Tooby and Cosmides 1988), and unconscious tracking of reproductive advantages of violence (Chagnon 1979; 1987). I do not know how he categorizes his approach, but in emphasizing material self-interest and behavioral plasticity, along with directed efforts to maximize inclusive fitness, it appears to me as a development of evolutionary ecology. Evolutionary ecology has a great deal of overlap with ecological approaches that do not include reproductive interests. So Gat's view (Part II: 79) of reasons for war on the Pacific Northwest Coast is much like my (Ferguson 1984a) pre-contact model, though he brings in evidence of women capture which, as he notes (I: 28), I ignore. And there are major correspondences with the Divale and Harris (1976) model regarding female scarcity and fighting over women, although without the population-regulation element. The primary difference between evolutionary ecology and "regular" ecology is the former posits that human behavior is evolutionarily designed to maximize reproductive success along

with material well-being.

I have several major disagreements with Gat. First, throughout his arguments runs the assumption that humans practiced war throughout the hunter-gatherer past. I believe that assumption is unsustainable. The question of the antiquity of war has been raised but clouded by Keeley (1996), whose rhetoric exceeds his evidence in implying war is as old as humanity. The earliest accepted evidence for warfare, Site 117, near Jebel Sahaba, Nubia, is a cemetery dated to 12,000 to 10,000 B.C., in which 24 of 59 well-preserved skeletons are associated with stone artifacts interpreted as projectile microliths (Wendorf 1968: 90-93; Wendorf and Schild 1986: 818-824). Though late Paleolithic in standard periodization, these people had been experimenting with wild crop harvesting thousands of years before the development of agriculture elsewhere. This experiment was brought to a crashing halt by climatic change which would have put extreme pressure on all peoples throughout the region, especially those in favorable locales like Site 117 (Hoffman 1993: 86-90).

Northern Australia, a favorite illustration for Gat, is a unique area in terms of the depth and continuity of collective violence among mobile hunter-gatherers, with rock art images suggesting individual and small group combats from about 8,000 B.C., and larger group confrontations beginning about 4,000 B.C. (Tacon and Chippindale 1994). This was a time of massive ecological crisis, with rising sea levels drowning the rich plain that once connected Australia to New Guinea. Socially, we see signs of increasing complexity and cultural divisions (Jones and Bowler 1980:23; Schrire 1982: 7; Tacon and Chippindale 1994: 217, 224, 227). Why war became such an institutionalized pattern is suggested by historic observations: their reliance on water holes in dry seasons, sources that sometimes disappear in droughts, gave them an extremely concentrated and valuable resource to fight over (Meggitt 1962: 24, 42), as Gat notes (I: 23). This is the classic imagined scenario for group violence (as in the opening of *2001: A Space Odyssey*), but not expect-

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able in the vast majority of hunter-gatherer environments.

Gat (I: 25) suggests that upper palaeolithic hunters in Europe from France to the Ukraine may have experienced territorial conflicts and warfare like that of historic North American buffalo hunters. New overviews of archaeological evidence show no indications of war whatsoever through the Upper Palaeolithic, in contrast to clear evidence in some Mesolithic sites (Chapman 1999:140; Dolikhov 1999: 77; Vencl 1999: 58). Summarizing information for early prehistoric North America, Haas (1999: 14) concludes:

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 that spread across the continent.

So it is around the world: the multiple archaeological indicators of war are absent until the development of a more sedentary existence and/or increasing sociopolitical complexity, usually in combination with some form of ecological crisis and/or steep ecological gradients. Then, signs of war become multiple and unambiguous (Carman and Harding 1999; Ferguson 1997, n.d.; Haas n.d.; Milner 1995; Roper 1975). This is not to suggest that war *never* happened in more ancient hunter-gatherer times, but the global pattern of actual evidence indicates that war as a regular pattern is a relatively recent development in human history, emerging as our ancestors left the simple, mobile hunter-gatherer phase.

War developed in more places and diffused outward as time went on, even to simple hunter-gatherers. But there is good reason to wonder if the high casualty rates reported, as for northern Australia in the decades before anthropological visitation and description, had not been impacted by "warrifying" tribal zone effects (Ferguson 1990a; Ferguson and Whitehead 2000). Certainly, the early contact experience of Northern Australia (Cole 1975; Meggitt 1962) is of the type that generated more war in other parts of the world. Such high levels of killing *might* be due to purely local causes, but that should not be *assumed* to be so without investigation of other possibilities. That is precisely the error that tribal zone theory is intended to address.

Having posited the widespread existence of war among prehistoric hunter-gatherers, Gat sets out to explain it. His ecological arguments also rest on questionable assumptions. Although Gat's (I: 22) ref-

erence to the rapid proliferation on human populations in uninhabited regions is a point well taken against assumptions of prehistoric population stability, the idea that paleolithic hunter-gatherers filled up all available niches is a "they must have" argument, which are always suspect. Although he notes that relative closure and defense of territory is a variable dependent on density of resources and other factors (I: 23, 25), he does not consider that according to optimal foraging theory—itsself closely associated with evolutionary ecology—open networks and sharing is often the most rational strategy, especially when resources are patchy and unpredictable. Which applied more, where, and when in our past, is anyone's guess. Even when people are spread all over, exit may remain an option to war. Among the Yanomami, who he grants hunter-gatherer-like status, even in the most densely settled and conflicted areas, people deal with violent conflict by moving away, or in with someone else (Ferguson 1995: 47), a pattern found throughout Amazonia (1989: 196).

Such simple ecological explanations of war were advanced in the 1960s, and generally abandoned with greater scrutiny. The volume *Warfare, Culture, and Environment* (Ferguson 1984b) was a recognition of this, and an effort to keep ecological considerations viable by recognizing the greater complexity of their role in war. Most ecologically minded analysts still are interested in possible relationship between growing populations and conflict, but the data just does not support a direct association of increasing density and increasing war (Keeley 1996: 118-120). The statistical research of Carol and Melvin Ember, which earlier dispelled the "myth of the peaceful hunter-gatherer," (C. Ember 1978) found that chronic, ordinary resource scarcity was not a significant predictor of war (Ember and Ember 1992). When you get down to cases, Yanomami (and other Amazonian) warfare cannot be explained as a result of conflicts over game (Ferguson 1989; 1995: 343-353).

More problems are brought in with sociobiological concepts related to the pursuit of reproductive success. The endlessly repeated idea that it is in male's genetic interest to spread those genes around, whereas women seek to snag and hold one male provider—though asserted with relative caution by Gat (I: 27)—disregards one tremendously salient fact: to procreate, a man must eat. Unlike other animals, for humans that requires being an accepted member of a cooperative group of food producers. Any behavioral proclivity that interfered with that

would face severe selective pressure. To pursue reproductive success with multiple partners within one's immediate group could endanger acceptance within the group and the solidarity of the group itself. It would compete with other men who are close genetic relatives or potential "wife givers"—thus going against kin selection and reciprocal altruism. Plus, as Gat emphasizes (I: 27), one of the main reasons for violence and killing is sexual affairs, and there is nothing like being dead for cutting down on lifetime reproductive success. Thus it is by no means self-evident that the putative male reproductive strategy would, on average over thousands of generations, increase rather than curtail genetic success.

Gat (I: 27) follows the neo-Darwinian (see Buss 2000) line in explaining why women have affairs as an effort to get additional male support, or lay in "insurance" against future loss of their husbands. Why would women need insurance, if men are so eager to add another mate? Is there any evidence from tribal societies that widows go to men with whom they have had affairs? If males are innately jealous about paternity, and predisposed to violence and even killing at any suspicion of infidelity (as Buss especially emphasizes), extra support from a lover would have to be so limited as to be unnoticeable by a wary husband. Is it likely that such limited benefits outweigh the costs of being beaten or killed, regularly over thousands of generations?

Gat (I: 31) also invokes what Wilson and Daly (1985) call the "young male syndrome," the intuitively reasonable proposal that young unmarried men, peaking at twenty-five, are most prone to violence because in an evolutionary perspective such risk taking may be most crucial for their reproductive success. But Napoleon Chagnon's data contradict this idea, demonstrating that among Yanomami, a maximum of 5 out of 83 men under 25 years old (1988:989), and possibly none of them (1990: 50 n.1), have participated in a killing. Moreover, Chagnon (1968: 115, 129-130) reports that young men are among the *least* willing to engage in risky physical violence. Among Yanomami, killers are generally middle-aged married men with children. Logical and empirical problems such as these abound in the sexual selection models of neo-Darwinian thought.

Gat (I: 27-29) makes a good case that conflict over and capture of women plays an important role in many reported cases of warfare. Many have commented before on the prominence of fighting over women in simpler societies, including authors quite distant from sociobiology (for example, Collier and

Rosaldo 1981; Ferguson 1988; Harris 1984; Knauff 1991; Wolf 1987). An evolutionary perspective thus provides only an *ex post facto* explanation of a well-known phenomenon, explainable without reference to reproductive striving. But more importantly here, does conflict over women offer an explanation of war comparable to conflict over material issues? Gat says, "I think this question is in fact pointless. It artificially isolates one element from the wholeness of the human motivational complex that may lead to war" (I: 27). But the question is hardly pointless. Whether warfare among Yanomami, for instance, is over women, game, or Western manufactures leads to an entirely different set of expectations and proofs—and totally different understandings of the reality of their war. In my analysis of actual cases, conflict over women among Yanomami, though certainly prominent, is *not* an independent cause of war (Ferguson 1995: 355-358), but one manifestation of relationships that are severely strained by antagonistic interests regarding exogenous trade goods.

It is in this inclusive frame that Gat (II) discusses a range of secondary reasons for war, considerations of status, revenge, power, insecurity, the supernatural, cannibalism, play. True enough, such factors are clearly involved in processes leading to war in many situations. I too see many of them as part of an integrated motivational complex, but in a very different light than Gat. Rather than separate traits, each capable of leading to war, joined by common selective advantage in an imagined past, I see them as part of holistic relations between social groups in the present, with ideas of status, revenge, witchcraft, etc. brought into play in ways structured by underlying material interests (Ferguson 2000: 222-225). By claiming these all should be seen as equally valid explanations of war, because of their assumed evolutionary advantages, Gat side-steps the main issues in the anthropology of war, which is, what factors or conditions explain why people fight? His answer seems to be, "they all do." This renders his theory irrelevant to efforts to explain variations in war. If in an evolutionary perspective, the question of what factors best explain the occurrence of war is "pointless," does that not make an evolutionary perspective 'pointless' for those who think the question is important?

In anchoring explanation in an unknowable past, the theory becomes unfalsifiable. If some frequently noted reason for war does not contribute directly to reproductive or somatic success, then it enhances social status, which itself contributes to such

success. If that cannot be posited, then it is a "maladaptive outgrowth and deviation from an evolution-shaped behavioral pattern" (II: 84). Is there any possible motivation that could not be explained in this way?

In discussing prisoner's dilemmas and related ideas, Gat (II: 78-79) lets game theory run away with him. Yes, such logic of insecurity is definitely a factor, but heavily tempered by other considerations. Take the Yanomami, about whom he quotes E.O. Wilson on their inability to stop fighting. But Yanomami, in practice, *do* end their wars. Active fighting between two groups rarely if ever lasts more than two years, lingering hostilities and suspicions notwithstanding (Ferguson 1995: 47-48). It is significant how light this extended discussion is on ethnographic citations, relying critically on two realist theorists writing on recent global international relations. One could take this as an effort to naturalize the realist paradigm, making arms races as natural as tree trunks (II: 79). Structural realism is heavily criticized in international relations theory today. Does it make sense to uncritically import it into anthropology?

To summarize, I have four main areas of disagreement with Gat. First, available evidence contradicts the assumption that warfare was a regular part of our ancestral environment of evolutionary adaptation. Second, simple population-to-resources ecological explanations of war such as Gat suggests have been investigated in ethnographically known societies, and generally disconfirmed (although more complicated and situationally limited ecological explanations are supported). Third, the common neo-Darwinian hypotheses on reproductively motivated behavior suffer from serious logical and empirical deficiencies. Fourth, I would put material self-interest as superordinate to any and all the other motives Gat discusses as a general, primary reason behind war. That last point requires some elaboration, which in the end may establish substantial common ground between Gat and myself.

Early in his argument Gat (I: 21) criticizes cultural materialists for taking the motive of material gain as simply a fact of life. Some history is needed here. Material goals in war were posited as part of a general shift from systemic to actor-based models, and against the dominant orientation that people will fight for whatever culture tells them to (Ferguson

1984c: 37-39; 1990b: 29-31). (In more sophisticated, often hermeneutic, theorizing, this remains a major approach in the anthropology of war [see Haas 1990], which is not addressed by Gat). Both Harris (1979: 62-63) and myself posited a limited number of evolved psychological propensities oriented toward maintaining material well-being. Combined with other elemental, uncontroversial dimensions of human nature—such as a sex drive, a desire to be esteemed by others, an ability to bond with young, behavioral flexibility, reliance on learning, and the capacity for cooperation—taking care of material well-being (rather than doing whatever culture tells you to do, or being directed by a multitude of Darwinian predispositions) can account for the spectacular evolutionary success of our species. But Gat is correct that we did not speculate on how these basic psychological orientations evolved. Speaking for myself, on the topic of war, that seemed far less important than formulating and testing hypothesis about the causes of war in the present.

For others, such as Gat, that is not enough. They seek an ultimate answer for war, one that foregrounds the fact that human beings *are* animals, that our species' behavioral capacities *did* come into being via natural selection. In Gat's work I see a possibility of developing common ground between the two interests. His theory does not *need* to posit war throughout our hunter-gatherer past. Most or all of the behavioral predispositions he proposes are much broader in significance than the restricted area of group conflict, and could evolve without it. We share a similar perspective on the importance of material goals. If he would put more emphasis on the proximate goal of sexuality without a reproductive calculus, on his point that "people widely desire love and sex for their own sake" (II: 75), we would be close to agreement there. I do accept that many of the "second floor" factors he discusses are reasons for war, which though structured by material concerns do have some autonomy and independent explanatory value. I see no reason Gat should object to the proposition that how explanatory any or all of them are for explaining war is a significant question for empirical investigation. How acceptable any of this will be to Gat I cannot predict. But regardless of his reaction, I believe this article has advanced the possibility of dialogue between materialist and evolutionary explanations of war.

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