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THE CAUSES AND ORIGINS OF “PRIMITIVE WARFARE”

REPLY TO FERGUSON

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Ferguson’s comment on my article is generally fair. Precisely for this reason, it serves to highlight the endemic problems, indeed the deep sense of confusion, surrounding the anthropological study of the causes of “primitive war,” which I have tried to elucidate in my two-part article. In this restricted format the points I shall make must be telegraphic.

I begin with the one important question raised by Ferguson that was not explicitly addressed in my article. When did fighting start? Was it a new cultural invention, which began with agriculture and the state, as Rousseauites have believed; or was it as old as the species, and, indeed, the genus *Homo*, encompassing 99.5 percent of our past, when humans lived as hunter-gatherers?

Ferguson invokes the oft-mentioned point that there is little generally accepted evidence for fighting before the Mesolithic. But *can* there be, given the sort of evidence available to archaeology for earlier periods? There is a fundamental, built-in bias here, which is seldom addressed (Vencl 1984). Not only is the evidence from the Pleistocene extremely patchy; that which might indicate warfare can also be interpreted differently. Stone axes, spearheads, and arrowheads—all dual-purpose tools among historically known hunter-gatherers—can be claimed to have been used only for hunting. Wooden shields, leather body armour, and tusk helmets—again widely familiar from historical hunter-gatherers—are not preserved. Comprehensive examinations of large specimens of fossilised human bones have concluded that at least some of them were injured by human violence (including a Neanderthal man from some 50,000 years ago, found with a stabbing wound in the chest from a right-handed opponent) (Roper 1969; Eibl-Eibesfeldt 1979: 126-127; Trinkaus and Zimmerman 1982; Keeley 1996: 36-37). Still, hunting and daily life accidents are difficult to distinguish in fossilised injured bones from those caused by fighting. Under these circumstances, is there *any* evidence that can *possibly* persuade a systematic sceptic?

What is it then that suddenly makes the

archaeological signs of warfare from the Mesolithic, and even Upper Palaeolithic, less open to dispute? In one word—sedentism. It left evidence of fortifications, burnt settlements, and large-scale communal cemeteries—the sort of material evidence without which archaeology is in the dark but which is *necessarily* absent before sedentism. Seeing coins only where there is light from a lamppost in one of the most serious possible distortions. All the same, is that not precisely the question in dispute? Was it not in fact sedentism that inaugurated warfare, as Rousseauites have always claimed? What evidence and general perspectives are relevant for deciding the issue?

One such perspective is surely the violence patterns of other species in nature. Some history is necessary here—and it is truly remarkable. Rousseauism had its heyday in the cultural atmosphere of the 1960s and early 1970s. It also received a highly influential reinforcement from an unexpected quarter. One of Konrad Lorenz’s (1966) more resounding ideas was that intra-specific violence in nature was mainly “ritualised” and did not involve serious fighting and killing. Consequently, human violence suddenly appeared to be unique, enigmatic, and calling for some special explanation. It has been widely assumed that something must have gone wrong in our cultural evolution. However, since the 1970s extensive field studies of animal species in nature have completely refuted Lorenz’s assertion. Wide-scale intra-specific deadly violence has been found to be the norm in nature, including among our closest cousins, the chimpanzees (summaries in Carpenter 1974; Hausfater and Hrdy 1984; Huntingford and Turner 1987; Van Hooff 1990; Dennen and Falger 1990; also Tiger and Fox 1971: 209-210. Sources on chimpanzees are Bygott 1972; Teleki 1973; Goodall 1986; Itani 1982; de Waal 1996; Wrangham and Peterson 1997). The reason for this, as Darwin pointed out, is that conspecifics are the strongest competitors of one another, inhabiting, as they do, the same ecological niches and vying for the same resources and mates. Indeed, the killing rates among animal spe-

cies studied have been found to *exceed* those of modern human societies (Johnson 1972; Wilson 1978: 103-105; George Williams in Dennet 1995: 478), though, tellingly, they seem to fall in line with them when the comparison is made with hunter-gatherer and horticulturalist societies, whose very high killing rate statistics are cited in my article (extensively discussed in Gat 1999). Although the news has been slow in reaching some anthropologists, humans have lost their formerly supposed uniqueness in killing their kind. In this regard there is no longer anything particularly unusual to explain. On the contrary, it is the idea that humans were special in *not* killing their kind before their recent cultural takeoff that would appear to require some very serious explanation indeed.

From this general perspective let us proceed to the ethnographic record of historical hunter-gatherer societies. First we must dispose of an irrelevancy in the form of Ferguson's 'Tribal Zone' theory. I have no space here to deal adequately with Morton Fried's original gross overstatement of the issue (1968; 1975). The concept of the tribe—like any other broad but perfectly meaningful social concept—indeed covers a wide range of "segmentary" societies. All the same, the ethnographic record clearly shows that while states impacted heavily on tribes, largely by way of force, they were not the factor that brought them into being, as a so-called secondary phenomenon. *Inter-tribal* conflict predated the state and acted as one of the powerful formative forces on the tribe. More importantly, however, the Tribal Zone theory, as espoused by Ferguson and his associates, has very little relevance to the question at hand. For while rightly emphasising the havoc that state—above all Western—contact with tribal societies created, most of these anthropologists are well aware of the evidence for extensive and brutal warfare in the Tribal Zone before contact and take care to mention it, albeit very briefly (Ferguson 1992: 225; 1995: 14; Whitehead 1990: 160; Blick 1988 is the exception). They thus leave their readers with only the *impression* of a hyper-Rousseauite argument, because otherwise their point is very thin indeed.

Let us then turn to the ethnographic evidence of historical simple hunter-gatherers, who are the closest equivalent of presedentary (pre-Mesolithic) *Homo sapiens* (and earlier *Homo*). This is the Rousseauite true line of defence. As pointed out in my article, in this context Aboriginal Australia constitutes a unique, huge, continent-size, isolated laboratory, free

from contact with either farmers or states. The evidence regarding belligerency from all over Australia, including *all* ecological niches, is quite clear. Ferguson implies that my examples refer to the resource-rich northern territories, where dense and more sedentary hunter-gatherers lived. In fact, he is well aware that I cite equally the highly dispersed hunter-gatherer regional groups of the mid-Australian Desert, whose population density, as I mention, is as low as 1 person per 35 square miles. He claims disingenuously that there was something special about their violent struggle to control water holes, *one* of the causes of their warfare, indeed a basic somatic resource, which like food, particularly game, has nothing special about it.

If Australia is a unique continent-size, isolated laboratory, Tasmania is even closer to the ideal of isolation and backwardness, the backwater of backwater. There were an estimated 4,000 Tasmanians when the Europeans arrived, and their population density was among the lowest there is. Their island had been isolated from mainland Australia for more than 10,000 years, and their technology and social organisation were the most primitive ever recorded. They did not even have the boomerang. Still, lethal raiding and counter-raiding took place among their groups. Territorial boundaries were kept, and mutual apprehension was the rule. No Rousseauite "free rangers" were to be found there, but again "centrally based wanderers," confined within their ancestral home territories (Plomley 1966: 968-969; Roth 1899: 14-15, 82; Jones 1974: 328; Ryan 1981: 13-14). As I have shown in my article, all the available evidence indicates very high killing rates among all known simple hunter-gatherer societies, some of which were almost as isolated as the Australians and Tasmanians.

Thus, both the patterns of deadly violence in nature and the ethnographic record of simple hunter-gatherers clearly suggest that intra-specific human violence—and the threat of it—while obviously undergoing transformations and varying in form through human history (Gat 1999), are on the whole as old as humanity itself, indeed as old as nature. As I sought to show, intra-human violence in our "evolutionary state of nature" was also caused by much the same reasons that perpetrate violence in nature at large. They are somatic and reproductive competition, and behaviour patterns that emanated from this competition, such as the quest for status, insecurity, and so forth. Again, throughout nature these patterns have been shaped by natural selection. Ferguson,

like many other anthropologists, is confounded by this perspective, which goes against everything that anthropologists were trained to think throughout most of the twentieth century. His arguments are often perplexing and even border on the naive.

For example, Ferguson argues that co-operation and open social networks, as well as retreat, would be better options than conflict for pre-agricultural people. It does not seem to occur to him that the ethnographic record (as, indeed, historical human societies and those of other species) patently show that *all* these strategies—conflict, competition, co-operation, disengagement—are in fact variably used and intermixed, depending on the circumstances. A similar failure, despite Ferguson's professed holistic quest, is revealed in his discussion of the human motivational complex. During the last decade he himself has come to realize, and he writes in his comment, that both somatic *and* reproductive factors are the two complementary elements of human existence. Still, this conclusion finds no traces in his work. Competition over women, universally attested as a major, if not the major, cause of deadly violence in practically all ethnographic studies, is never even mentioned in his own very detailed case studies. Again, he apparently seems to believe that this tremendous selective force—reproduction conflict—cannot adaptively be the cause of violence in humans, disregarding the fact that it is patently so throughout nature (including, as he fails to recognize, among social animals). He suggests that competition over women would be harmful for social cohesion and co-operation in acquiring food and that it is mortally dangerous. He must be thinking of a perfect world in which inherent tensions and uneasy compromises between desired goods within human and social reality, and, again, in nature at large, have never been heard of. Speed versus strength, sociability versus individualism, are trivial examples of the endless design and behaviour compromises struck by organisms, “choosing” between various desirable properties for inclusive fitness along the course of their particular evolutionary path. Ferguson's failure

to grasp this fundamental interconnection is most strikingly manifest in his insistence on defining a *primary* motive within the human motivational complex, rather than recognize the evolutionary rationale that shapes and goes through it. It is as though he were asking what is really the thing people are after in going to the supermarket: bread, meat or cheese. In fact, it is only in *specific* cases that the question of the more prominent motive is meaningful. This, finally, leads to some broad theoretical issues. Ferguson raises the question of the Popperian possible falsification test for evolutionary theory. In this limited space I can only comment briefly. First, it must be mentioned that while this test is certainly not meaningless, Kuhn (1970) has since shown how more intricate is the process of falsification and verification in the heuristic relationship between theory and facts. Bearing this in mind, I note that evolutionary theory *has* constantly been subjected to specific empirical tests—including, increasingly, its parts that pertain to human behaviour—and many possible finds can in principle throw it into doubt. In any case, it is truly ironic that Ferguson should invoke the possible falsification test, for his materialist position is wholly immune to any evidence, as his intellectual acrobatics with respect to human sexuality demonstrate.

This is the crux of the matter. Ferguson's conciliatory stance is welcome. However, as I have written in my article, the synthesis offered is between the somatic and reproductive motives rather than between the evolutionist and materialist theories, for evolutionary theory already encompasses and explains both elements. It is not in a spirit of bad sport that I have to insist that materialism is not a theory in any meaningful scientific sense. It is a more or less adequate working assumption, a postulate, at worst a dogma, lacking a true explanatory rationale. Like many common sense approaches, it contains kernels of truth and valid insights, whose deeper logic, as I have argued, is explained by evolutionary theory.

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