Chapter 14
Born to Live: Challenging Killer Myths

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This chapter is an anomaly in this volume. It is about cooperation among primates, but cooperation for deadly violence against others of the same species. It is about warfare by chimpanzees and by humans. Whether chimpanzees make war depends on your definition. Mine has always been elementary: organized, potentially lethal violence against members of another group. Using this definition, there is no question that chimpanzees have the capability to make war and have done so on occasion. The patrols that often precede attacks, and the attacks themselves, display a high degree of intelligent cooperation. Male coalitional aggression is the label that has been aptly applied to chimpanzees and humans too.

What is very much in question is whether chimpanzees, and humans, are *predisposed* to war, whether our common evolutionary heritage has selected into our genes a tendency, a predilection, to attack and kill members of other groups. Just that claim has been made by many authors, most notably by Jane Goodall (1986), Richard Wrangham (Wrangham and Peterson, 1996), and Michael Ghiglieri (1999), in many forums both scientific and popular. I argue that it is wrong, fundamentally wrong. Chimpanzees—about which I am currently writing a book—have evolved a most flexible nature. With human beings, living in immensely complex social and symbolic worlds, that flexibility is squared. This is not to claim that we are born noble and peaceful. We are not species-ifically inclined against war either. Our orientation toward war, for it or against it, and our practice, depends on situations, inclusively defined as running from basic environmental circumstances, through social structures, to values and beliefs.

Challenging the myth of innate depravity, as Ashley Montagu (1968a) once called it, is the academic equivalent of whack-a-mole. Slap one hypothesis down, another pops up. True believers see confirmation of our evolved violent nature everywhere they turn, and they have forcefully presented this bleak view to the public and policy makers. Again and again, in this way and in that way, they claim

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that humans may be able to learn peace, but they are naturally inclined toward war. Furthermore, they warn, if we do not accept this unpleasant truth, we doom ourselves to unending violence.

I have been researching war for over three decades, and I think that anthropology has some important things to say about mass violence in our world today (see Ferguson, 1999, 2003, 2006a, 2009). But those points are covered over by the smoke and mirrors of evolved predispositions to kill. This chapter takes a very broad approach to assertions of an evolved war-proneness, touring through a number of overlapping sorts of arguments. The basic point is that although psychological Darwinist claims are extensive, they are not supported by evidence. What *is* supported is the basic premise of this volume, that it is human beings' evolved nature to keep themselves alive and well, by means of cooperation and altruism. Sometimes, that means going to war.

Chimpanzees as Natural Born Killers

A demonic perspective on chimpanzees provides foundation for the current psychological Darwinist perspective on war. As Wrangham (1999a:6) puts it, "selection has favored, in chimpanzees and humans, a brain that in appropriate circumstances, seeks out opportunities to impose violence on neighbors. In this sense, the hypothesis is that we have evolved a violent brain." "Chimpanzees and hunter gatherers... seek, or take advantage of, opportunities to use imbalances of power for males to kill members of neighboring groups" (Wilson and Wrangham, 2003:384). "a *necessary and sufficient* condition for intercommunity aggression is a perception that an opponent is sufficiently vulnerable to warrant the aggressor(s) attacking at low risk to themselves" (Wrangham, 1999b:15, my emphasis). And one more, to make clear I am not making up a straw-man:

> [S]election has favored a human tendency to identify enemies, draw moral divides, and exploit weaknesses pitilessly across boundaries. Among hunter-gatherer societies, inner-city gangs, and volunteer militias at the fringes of contested national territories, there are similar patterns of violence. The spontaneous aggressiveness of humans is a harsh product of natural selection, part of an evolutionary morality that revels in short-term victory for one's own community without regard for the greater good (Wrangham, 2005:19).

This scholarly version of this dramatic picture has been called the Rival Coalition Reduction Hypothesis (Wilson et al., 2004). Any opportunity to kill males of another group with impunity will be seized because loss of fighters reduces their ability to compete over the longer term. No immediate conflicts of interest are necessary. Against that, the RCRH, is the RCH or Resource Competition Hypothesis—where severe fighting across chimpanzee groups is a direct effort to protect food resources. I side with the latter to a point. My position is that heightened food competition, and other disturbances, all *linked to human impact*—or the Human Impact Hypothesis—are what lead to deadly conflicts between groups and other violence as well. This can be called RCH+HIH. (In this and later discussions of chimpanzees, summary statements are based on a book manuscript in progress, *Chimpanzees, Men and War*, and documentation will be provided there.)

Where did the idea of killer chimpanzees come from? It developed out of three field situations. At Gombe in Tanzania, there was the Four-Year War from 1974 to 1977. The story is almost as familiar as Cain and Able. Beginning in 1972, one intermingling community of chimpanzees split into northern and southern groups. In 1974, members of the northern Kasakela community began entering the rangelands of the southern Kahama group and brutally attacked individuals from it, especially males, whenever they caught one alone. By 1978, Kahama was entirely gone—presumed exterminated—and Kasakela began using their rangelands (Goodall, 1986:503–514).

The second situation also occurred at Gombe, right after the Four-Year War. From 1978 to 1982, the large Kalande community, formerly south of the now-gone Kahama, gradually began expanding their ranging northward, encroaching on Kasakela, which fearfully avoided the intruders. This "invasion from the south" is portrayed as a violent repeat of the Four-Year War (Goodall, 1986:514–517).

The third situation occurred 60 km south of Gombe, at Mahale. Adult male chimpanzees of K-group had disappeared over the years, one by one, starting in 1970. By 1982, all but one K-group male was gone. The larger, ever encroaching M-group assimilated K-group's range and a number of K-group females who remained in place. Little was made of the disappearances when they happened, but after the Four-Year War became known, Mahale researchers reinterpreted these disappearances as possible killings by M-group (Nishida et al., 1985). In many secondary sources, the killing off of K- by M-group is reported as a documented fact. The invasion from the South and the end of K-group were taken as confirmation of the dark vision that it is in their nature for chimpanzees groups to war on their neighbors.

Margaret Power (1991) is the main critic of this view. Her work has been largely discounted by chimpanzee researchers. I believe she was on the right track. Power stressed that both Gombe and Mahale were subject to major artificial provisioning, and that early observations there, and at Budongo and elsewhere, of non-provisioned chimpanzees, showed them to be less exclusive and hostile then the later Gombe portrait. But this difference between early and later observations has been blamed, by others, on fission–fusion confusion—researchers were misunderstanding normal separation and joining of individuals within one group, with two different groups coming together (Ghiglieri, 1984:8, 173–174). Yet the specificity of early observations goes against that interpretation, such as known Gombe males observed in the center of another group's rangeland (Goodall, 1968:214) or geographically distinct groups in the Ugandan Budongo Forest occasionally sharing a rich food source with each other and then going back their on separate ways (Sugiyama, 1968).

Power sees this difference in reports as a record of social change driven by artificial food provisioning. At Gombe, violence centered on banana distribution got so intense that it was cut back drastically via a series of experiments in controlled distribution (Wrangham, 1974). Power hypothesizes that this reduction, and the way the new banana systems operated, led to intense frustration. That generated aggressiveness among chimpanzees which were already socialized to violence, and they took it out on Kahama, the Four-Year War. Frustration led to aggression.

I follow all that, but go farther. My position is that the new ways of provisioning led to a serious food scarcity, evidenced by sharply declining body weights, and that a policy of banana-favoritism toward Kahama gave Kasakela a good reason to be extremely hostile toward them. While the local Kasakela chimpanzees had to wait in frustration for a bunch of bananas per week, the prodigal Kahama chimpanzees got bananas whenever they showed up (Goodall, 1986:503) Plus, there was a lot of sex and politics involved. (Now there is a good comparison to humans). Subsequently at Gombe, the "invaders from the south" appeared to be drawn to the feeding station (Goodall, 1986:516). Regarding Mahale, it seems that everyone agrees that what pulled M-group into K lands was the researchers' provisioning. I will return to this topic of human impact.

Gombe and Mahale 1974–1982 were the basis of the idea that chimpanzees, and so humans, are inherently warlike. It took time for this to reach maximum public spread in major publications (especially in Goodall, 1986; Wrangham and Peterson, 1996; Ghiglieri, 1999). Paradoxically, during that time of writing and presentation, violence dropped off, with only one clear outside adult male killing from 1983 to 1998. Doubts about the normality of those type-case situations began to grow. But events since then, at several sites, especially at Gombe and Kibale in Uganda, seem to support the demonic view. It is common to read statements such as, "A growing body of evidence suggests that lethal intercommunity aggression is typical for chimpanzees across Africa" (Gros-Louis et al., 2003:341). However, if one sticks with the cases, (and if one leaves aside highly artificial captive-introduction experiments), there are only 13 instances where evidence indicates certain or very likely intergroup killings of adult males, in over 200 years of reported observations.

Nine of the thirteen killings come from three short periods, Gombe 1974–77 and 2000–2004 and Kibale 1999–2004. My count (to be documented in *Chimpanzees, Men, and War*) is as follows: at Gombe, 2 in 1974–1977 (Sniff and Charlie), 1 in 2002 (Rusambo); at Kibale, 5 from Ngogo in 2002–2004, at Kanyawara, 2, 1 in 1991–1992 (Ruwenzori) and 1 in 1999; plus 3 other singletons, 1 at Kalinzu in 2003, 1 at Tai in 2005, and 1 at Loango in 2005. Highly noteworthy, but typically unnoted, some of those situations are characterized by other forms of intense violence, not associated with the Gombe war vision: internal and external infanticides, internal killings of adult males, severe violence against outside or inside adult females, killing and eating of human infants, and markedly increased hunting. This broad spectrum of bloodletting suggests chimpanzee populations under stress–stress from humans.

Power's emphasis on the impact of banana provisioning was countered by evidence of territorial clashes and killings at unprovisioned sites, most notably Kibale. Proponents of evolved warlike tendencies routinely equate human impact with provisioning only. If no provisioning, then human impact is ruled out. There is much more to human impact than that. Habitat loss in unprotected areas and around or even within protected areas has eliminated chimpanzee rangeland. Snare poaching and retaliation for crop raiding has added to rangeland impaction, even within Parks. This has led, I argue, to intensifying territorial competition. Epidemics, some introduced through humans, caused major demographic disruption, and with social consequences we are only beginning to discern. Other huge unknowns are the effects of research and tourism, which are often extremely intrusive. We cannot specify their effects but are unwise to discount them.

The exclusion of human impact is part of a broader problem in field research, the nearly complete separation of writings on scientific research questions from discussions of human threats and conservation. Anthropology too was reluctant to acknowledge that their study populations were far from "pristine"—"they were hardly affected by the outside world when *I* got there." Primatology should avoid that mistake. The way to understand behavior is to examine responses to changing circumstances. This is very relevant to violence. Human impact on chimpanzee populations has increased greatly in recent years. Note that 10 of the 13 intergroup adult male killings occurred after 1998. As human impact intensifies in the future, I predict substantially more male/male intergroup attacks, and more of other sorts of violence, in sharp contrast to field observations from 1983 to 1998, just as colonial intrusions intensified indigenous warfare in tribal zones all over the world (Ferguson and Whitehead, 2000). As with human warfare, to be understood, chimpanzee violence must be seen in its historical context. If these acts of violence are seen as expressions of a dark chimpanzee nature, international support for their protection may decrease. If, on the other hand, they are seen as a consequence of human disturbance, support for protection may grow.

Unanswered Darwinian Questions

In an important sense, there is no necessary contradiction between my situational explanation of collective violence and views that posit evolved tendencies. Now, we are all nature–nurture interactionists. But in substance, the perspective that intense chimpanzee violence is associated with increased resource competition and other disruptions due to a human presence is very different from the idea that intense violence in the normal expression of evolved propensities. After all, if the point of the demonic and related arguments is not that chimpanzees and humans are *born* inclined toward war, that this inclination is coded in their genes, then what is the point? Yet, for all the emphasis on evolved tendencies, the evolutionary process leading to fixation of these tendencies remains surprisingly fuzzy, on several counts.

The ABC of Darwinism is variation and selective retention. Some individuals have a trait, some do not, and those that have it breed more. Add in consideration of inclusive fitness, and it is not just individuals that get selected but gene-sharing kin. Regarding chimpanzee wars, kin selection supposedly operates because males are philopatric. They (usually) do not leave their natal group, and so it is surmised that they share more genes with males of their own group than those of others, potential adversaries. This has not been demonstrated. Genetic comparisons showed no or only slightly higher relatedness of males within a group than among females, who typically migrate in from outside (Vigilant et al., 2001). No one has specified the demographic *model* that is supposed to select for demonic traits. It is by no means obvious how such selection could occur. If females regularly move to neighboring groups, generation after generation, then intergroup conflict means fighting with uncles, cousins, and nephews. In most theoretical applications of kin-selection, relatives that close would be working for *common* genetic interests.

Furthermore, the assumption that human hunter–gatherers are, like chimpanzees, patrilocal—and so the unspecified selection model works for us too (Wrangham and Peterson, 1996:65–66)—runs up against extensive evidence of residential variation and flexibility among foragers (Fry, 2006:167; Chapter 13, this volume). According to Wrangham and Peterson, one of the key parallels between chimpanzees and tribal peoples—specifically the Yanomami—is that females leave their own group to marry elsewhere. Unfortunately for that conclusion, the typical Yanomami marriage is village endogamous and both males and females stay where they are (Chagnon, 1968:69–73). So any selection model based on chimpanzee patterns would have only a variable *potential* application to simpler human societies.

Even if some statistical genetic benefit could be modeled for the very overlapping "us vs. them" of chimpanzees, the competitive advantage supposedly gained by eliminating individual males from neighboring groups could be swamped by the large fluctuations in group size. Killing off one enemy warrior would not make much difference in subsequent intergroup showdowns. The Gombe "invasion from the south" was supposedly halted by the maturation of a few Kasakela males.

The idea that this unspecified selection process fine-tuned a particular predisposition exemplifies an outmoded bean-bag image of genes, particular to particular traits. We now know it is hardly that simple. Genes are expressed in complex layers of interactions—systems of systems—all with external inputs. Their effects typically are not discrete. For instance, much attention has been given in humans to SLC6A4, the so-called anxiety gene. But this gene has also been associated (in the NCBI Entrez Gene database) with—alphabetically—aggressive behavior in children, alcoholism, anorexia nervosa, attention deficit hyperactivity disorder, autism, chronic fatigue syndrome, depression, heroin dependence, longevity, lymphoma, migraine, myocardial infarction, neuroticism, obsessive compulsive disorder, pulmonary veno-occlusive disease, schizophrenia, sleep apnea, sudden infant death syndrome, suicidal tendencies, and violent behavior. Select for one connection, select for all the others too. A "gene for" any aspect of violent intergroup competition would affect many other areas as well. Any inclusive fitness benefit of selection for intergroup violence would be weighed against countless other effects on lifetime reproductive success.

Moreover, in the demonic perspective, what is asserted to have evolved is not some single, simple tendency—such as a low-flash point for violence—but a complex suite of behaviors, including stealthy patrolling of borders, entering neighbors rangelands, careful monitoring of signs of adversaries, calculating numerical advantage, and collectively attacking. This would involve many, many genes.

Positing an inborn predisposition to this complex set of social actions stands quite apart from most understandings of chimpanzee behavior, which for decades has looked to flexible ecological adaptation rather than inherited tendencies. While most social behaviors display ranges of variation, this war suite is said to be fixed. As Wrangham puts it: “Does this mean chimpanzees are naturally violent? . . .Alas, the evidence is mounting and it all points the same way. . . In this cultural species, it may turn out that one of the least variable of all chimpanzee behaviors is the intense competition between males, the violent aggression they use against strangers, and their willingness to maim and kill those that frustrate their goals.” (Wrangham, 1995:7).

But why fix *this* set of behaviors, when evolution left the rest flexible in responding to circumstances? What is the reproductive advantage of having the temperamental dial set to attack, rather than in neutral? How does that expectably lead to more genes in future generations than an open, unbiased disposition, to go with whatever works best, be it violence, avoidance, or tolerance? The demonic view holds that even when there may be advantages to getting along, chimps and humans will opt for violence, start a war. What is the reproductive advantage of an orientation that leads to sub-optimal actions? How does that enhance fitness, individual or inclusive?

The alternative for chimpanzees is that a violent disposition to others is acquired. We are all aware of chimpanzees’ prodigious ability to learn. Different groups have different learned traditions. (Some would say cultures but I would not). Some of these traditions seem related to environmental conditions but many do not. Still, that catalog of learned behaviors remains mostly limited to techno-environmental interactions, much like the trait lists of anthropology a century ago. It is more than possible that complex, patterned social behaviors can be learned and passed along, for example, the differing degrees of bisexual bonding comparing Tai, Gombe, and elsewhere, or even many of differences between chimpanzees and bonobos. What would happen if a bonobo were raised among chimpanzees or vice versa? I expect their behaviors would reflect the local custom.

Evolution of Violent Humans

Let us say for the sake of argument that chimpanzees are genetically predisposed to war. What does that mean about humans? The basic idea of the chimpanzee/human war analogy is that we share this violent predisposition—albeit much more elaborated among humans—because we inherited it from our last common ancestor. That ancestor was said to be pretty much a chimpanzee. As Wrangham and Peterson (1996:63) put it, “modern chimpanzees are not merely fellow time-travelers and evolutionary relatives, but surprisingly excellent models of our direct ancestors. . . [C]himpanzee-like violence preceded and paved the way for human war, making modern humans the dazed survivors of a continuous, 5-million-year habit of lethal aggression.”

Others see our apical ancestors quite differently. A behavioral synthesis in a 2008 issue of the *Journal of Anatomy*, explicitly focused on the last common ancestor, hypothesized "that the LCA displayed regional variation in certain behavioral traditions, 'self-awareness', and an enhanced ability to follow the gaze of other social agents. . . these behavioral characteristics are related to increased capacity of executive control to inhibit conventional responses in favor of social tolerance and seeking novel and flexible solutions to problems." (Sherwood et al., 2008:431). The chimpanzee model has been further undermined by recently released findings on 4.4-million-year-old fossil *Ardipithecus ramidus*, which showed less sexual dimorphism and smaller canine teeth than anticipated. As Owen Lovejoy (2009:74) puts it: "Comparisons of the *Ar. ramidus* dentition with those of all other higher primates indicate that the species retained virtually no anatomical correlates of male-to-male conflict. Consistent with a diminished role of such agonism the body size of *Ar. ramidus* was only slightly larger than that of females."

This is not the first time that living primates have been imagined as our ultimate progenitors. Baboon models were in vogue for some time (Jolly, 1970). A spirited case was made for the more peaceable, sexy, and female-bonded bonobo as the human template (Zihlman et al., 1978). The obvious point to be made is that no species living today represents our common ancestor 5–6 million years ago. But for argument, let us assume that our extremely great-grandpa did have an inborn predisposition to attack and kill his neighbors. Would modern men have gotten it from him, passed along over millions of bloody years?

If one considers all about those 5–6 million years, the huge unknowns that alone should be enough to dismiss any assertion of continuity in specific behavioral patterns. Wrangham and Pilbeam acknowledge this problem. Referring to human/chimpanzee parallels in lethal raiding, Wrangham and Pilbeam (2001:13) concluded whether this pattern of patrols and attacks was found in the LCA does not matter: "phylogentic continuity is impossible to confirm when it must traverse the great unknowns of 5 million years of hominid evolution. And more importantly, it has no explanatory value. The reasons why a behavior is shared must still be articulated for each species." There you have it from the author of *Demonic Males*—chimpanzee's collective violence provides no explanation for human collective violence, except, perhaps, by analogy.

Perhaps this proclivity was not passed down continuously from 5 million BP, they acknowledge. Without reference to the not-yet-described *Ardipithecus*, they note the reduction in both canine and body dimorphism in the later human line, a trend which usually is taken as an indicator of reduced male–male competition. Since later hominins thus appear to be comparatively nonviolent, they suggest that the bloody proclivities of the chimpanzee-like common ancestor were selected out, only to be selected back in at a more recent date. With bonobos, they were selected away, never to return.

If recent ancestors were inclined to war, then one would expect to find warfare present throughout the human archaeological record. That is what psychological

Darwinists routinely claim to be so, repeatedly citing two books (Keeley, 1996; LeBlanc with Register, 2003) which support that view. Those claims do not withstand scrutiny (Haas, 1999; Otterbein, 2004; Thorpe, 2003). They suffer from a compound misinterpretation: they note ancient cases where signs of war are present and extrapolate from them to the many more ancient cases where none are; they conflate later archaeological records with earlier records; and they assume that ethnographically recorded warfare of peoples in recent centuries is representatives of people millennia ago (Ferguson, 2006b; Chapter 13, this volume). That is assuming the ancient universality of war not documenting it.

War leaves archaeologically recoverable remains, in skeletal and settlement materials, and sometimes in tools and art. Globally, the pattern is that war signs are absent in the earlier archaeological records even where recovery of materials is sufficient to show war. After time—chronologies vary enormously in different regions—war signs unmistakably appear, and usually never go away. The appearance or intensification of war usually follows some combination of preconditions, including larger populations, greater sedentism (though not necessarily agriculture), increased trade, hierarchy, social bounding, and often, environmental reversals. The first established war findings date to around 10,000 years ago and gradually become more widespread and more intense around the world, ultimately leading to the frequently violent ethnographic universe recorded in recent centuries. The sum of early archaeological records from around the world contradicts the idea that recent, in evolutionary terms, human societies were characterized by violent competition and war (Ferguson, 2006b).

These are all scholarly objections. For the larger public, “chimps R us” carries the day. In the genes, they are 98% plus identical to humans. If we are so close in our DNA, how different could we be? This is a key icon of modern biomythology. As Marks (2003) details, and as post-genomic science continually updates, this figure is meaningless for the kind of behavioral questions we are discussing, especially as it seems gene regulation is the name of the game in our species’ differentiation.

In the churning sea of questions about human evolution, a few things are clear. What separates humans from chimpanzees includes a vastly expanded neocortex and cognitive abilities, and commensurate capacities for language and symbol. These watershed differences provided the basis for culture, which—emergent—actualized humanity’s “adaptive dimension” (Montagu, 1968b). The same human infant has the potential for being a pre-industrial hunter–gatherer or an astronaut, for being a genocidal slaughterer or a pacifist monk. That is pretty darn flexible, and humans can do it because culture *is* our nature. It is culture that made possible human beings’ spectacular reproductive success. It is culture that enables us to live cooperatively and interdependently, pooling our efforts to collectively cope with any environment on earth. It is culture that provides the means for our material and reproductive well-being. Yet in psychological Darwinism, even after this quantum leap in collective flexibility, we remain inherently violent because violence increases inclusive fitness.

Violence for the Genes

But claims for evolved tendencies for war are reinforced by assertions that both chimpanzees and humans actually *do* use deadly violence in ways that maximize their inclusive fitness. That is the ultimate commonality claimed to span the two species. The claim rests on little evidence.

For chimpanzees, if that were true, then certain kinds of killings should be expected according to the logic of inclusive fitness and certain kinds not. Expected would be killing not only of outside adult males but also of outside male infants. Not expected would be killing of adult or infant males within the group, which costs a present or future coalition member, or the killing of outside females of any age, who might immigrate and thus help propagate male genes. What is the record (in *Chimpanzees, Men, and War*)? Considering certain and very likely killings (and once again leaving out the captive introductions), there are 23 consistent with maximizing inclusive fitness and 25 that go against inclusive fitness—pretty much of a wash.

For humans, the violence-for-reproduction claim rests upon Chagnon's (1988) study of the Venezuelan Yanomami, which supposedly demonstrates that *unokai*—which Chagnon equates with men who have killed enemies—have more children than *non-unokai*. It would be hard to find a more thoroughly debunked claim in contemporary anthropology. Field ethnographers challenged Chagnon's data (Albert, 1989; Lizot, 1989). Fry (2006:184–199) and myself (1989; 2001:106–108) have taken on his statistical analysis. These rebuttals show that killers' alleged reproductive success is distorted by the following facts: (1) headmen, who have more wives, are all in the *unokai* category, thus raising *unokais'* apparent reproductive advantage, (2) the likelihood of becoming *unokai and* of having more children both increase with age, so age is a confounding variable that also inflates *unokais'* advantage, and (3) the sample is limited to living men. Known killers are often targeted for revenge killings, and dying younger obviously lowers lifetime reproductive success. If you only looked at the winners, then gambling would seem to be a good deal too.

Meanwhile, Moore's (1990) study of Cheyenne war and peace chiefs shows the latter had higher reproductive success. Dedicated warriors lived shorter lives with fewer children. If war-proneness were under genetic control, and conditions remained stable, the trait would be selected out quickly. Similarly, though on a group level, Younger's (2005) extensive computer modeling of social groups on islands shows that those led by more aggressive leaders tend rapidly toward extinction. The notion that being inclined to war leads to greater reproductive success is without empirical foundation.

Tooby and Cosmides (1988) posted an often-cited evolutionary psychological explanation of war on their website. They ask, since warriors often die, and being dead is bad for reproductive success, why would men risk combat? They posit three necessary conditions that would make our supposedly modular mind compute war as worthwhile in reproductive terms: "cheaters or non-participants must be identified and excluded (or punished) . . . the participants are rewarded or punished in proportion to the risks they have run, and in proportion to how important their contribution was to success." They do not provide a single example where these three conditions apply, so I (2001:110) considered them in relation to Chagnon's descriptions of Yanomami. None of the conditions hold true.

Mesquida and Wiener (1999) adopt Daly and Wilson's (1988:168–171) concept of a young male syndrome and apply it to war. They claim that wars are launched by young men because it suits their reproductive interests, to obtain mates directly or the resources needed for the attraction and retention of mates. For evidence, they cite national statistics which associate larger numbers of young men with more frequent or intense warfare. This association is real, as for example in Rwanda, and a good explanation already exists: those bulges of rootless young men are easily and cheaply recruited in political fights launched and controlled by older political leaders (Collier and Hoeffler, 2004).

As for tribal societies—such as the Yanomami (Ferguson, 1995), the Meru (Fadiman, 1982), the Cheyenne (Hoebel, 1978), or the Enga (Meggitt, 1977)—military decisions are typically made by middle-aged or older men, not young hotheads dreaming of glory and mates. In tribal combat, younger men generally are supervised and protected by their elders until they mature in the ways of war. Mesquida and Wiener's claimed behavioral generalization crashes against ethnography. In fact, one source they cite to show an association of intense warfare with the presence of more young men is the article previously mentioned (Moore, 1990), showing Cheyenne war leaders had lower reproductive success.

The biggest argument for war as reproductive contest is also the simplest: winners often prosper at the expense of losers. True, large population benefits and losses can accompany war. Evaluating that point requires consideration of a theoretical point of crucial significance. My longstanding materialist position is that wars occur when those who make decisions for war believe it is in their practical self-interest to do so. Practical self-interest means protecting or enhancing all the resources at one's disposal, the costs of obtaining them, physical safety, and—where such exists—political power (Ferguson, 1990). This is an all-important difference from psychological Darwinism, which holds that in addition to material well-being, humans also compete *directly* over reproductive success (Chagnon, 1990).

While that may seem to be a theoretical fine point, it leads to critically different expectations and understandings. It means that even if there is no competition over material resources, reproductive interests will still pit men against other men. In a broad sense, this goes directly against the key point of this volume: that cooperation is the more common, "natural" tendency in human evolution and behavior. More specifically, it means there is *always* a reason to make war. This is a testable theoretical difference: are conflicts over practical material issues more predictive of actual war than "reproductive" conflicts, even broadly defined. The basic goal of my book *Yanomami Warfare* (1995) was to evaluate these contrasting hypotheses against every reported case of warfare. Conflicts over access to the introduced necessities of steel tools and other Western manufactures are predictive. Disputes over women, in any form, are not predictive.

Monumental Myths

All of the particular theories previously discussed carry weight in popular discussions because they rest comfortably within larger cultural assumptions that humans are, by evolution, by nature, born to kill others. This part considers several of these larger, mythic frameworks. It could start with "Man the Hunter," but in this volume that is better left to others. Instead, it begins with the idea that men are "naturally aggressive," an opinion I frequently get from my students.

Research and speculation on the biology of aggression deal with individuals. War is a property of social groups, with dynamics that can only be understood at the group level. The disjuncture between the two is well-illustrated by Konner's (2006) state of the art review of the biology of aggression and war. He summarizes the neurology of aggression, effects of brain trauma, heritability studies, etc. All of that is dropped when he moves on to explaining war, where he invokes pseudospeciation (below) and long-standing psychological research on mass psychology and authoritarianism, which "does not contradict current views in evolutionary psychology" (p. 23). Then psychology is somehow put in harness with Malthus, who, Konner claims, explains the colonization of the Americas, both World Wars, creation of the USSR, Vietnam, etc. Konner opens this article with the standard biological pitch: "there is in human nature a natural tendency to violence and, additionally, to war, and... the failure to fully recognize this tendency – a common failure in academic circles – increases the risk" (p. 1). So it is instructive to read in closing, just how this bio-realization is important: only if we concede that war is natural, like disease, will the international community be sufficiently motivated to do something to prevent it (28). How does that follow?

Konner and many others use biology to explain why war is a male activity. This too is said to apply to both chimpanzees and humans. Wrangham titled his book *Demonic Males* because female great apes were, in comparison, nonviolent. This clear distinction has eroded because of accumulating evidence of severe attacks by females. Still, it remains true that males do more of it.

For humans, collective violence usually is a male thing. Regarding whether or what role biology plays in this, I am agnostic. Measures of male and female aggression vary. Domestic violence, in the current English-speaking world, is slightly more likely to be initiated by females, though males are much more likely to do serious harm (Archer, 2000). Adult males have far more testosterone than females, but testosterone levels rise and fall with social events and may be as much an effect as a cause of aggression (Sapolsky, 1997). Young boys routinely test out as more aggressive than girls, but this is already after significant gender socialization (Condry and Condry, 1976; Sidorowicz and Lunney, 1980). We know, from many, many cases, that women can both order and fight wars (Davis-Kimball, 2002; Edgerton, 2000; Jones, 2005), so this cannot be a simple question of biological capabilities.

A solid, empirically grounded biosocial theory (Eagly and Wood, 2003) can explain gender segregation in war, without invoking inborn predispositions. Two prominent cases of warrior women lend it support. The famous women warriors of early 19th century Dahomey had to bow to social mores, as they said they had become men (Edgerton, 2000). Contemporary women soldiers of Eritrea fought with distinction and ruthlessness alongside men, enabled and encouraged by that country's revolutionary ideology. When the war ended and they went home, they found great difficulty reentering the traditional female role (Bernal, 2001). In both cases, women excelled as warriors, but they could not be warriors and culturally defined "women" at the same time.

Perhaps men are more aggressive than women. What does that tell us about war? Maleness is one part of biology. Biology is one part of aggression. Aggression is one part of combat. Combat is one part of war. An explanation of a part of a part of a part of a part of a social pattern says very little. It can be argued with at least as much support, that it is militarism in society that conditions male proclivities for violence (Goldstein, 2001). Could there be socialization for gender roles among chimpanzees and bonobos?

If innate male aggression were an important cause of war, we might expect men to relish the chance to kill enemies, to seek it out, as chimpanzees are alleged to. The record of the US military is totally the opposite. Men seek to avoid killing enemies and are traumatized when they do. A great deal of training is needed to make even them shoot (Grossman, 1996). An article by an Army major (Pierson, 1999) in *Military Review* advised commanders to identify the less than 4% of troops who are psycho or sociopathic because they are the ones who can be counted on to willingly kill. ("[A] controlled psychopath is an asset on the killing fields" [p. 61]) A recent evolutionary psychology book (Smith, 2007), premised on the evolutionary benefit of deadly violence, tries to deal with this conundrum by compounding inborn mental modules. A people-making module makes us unwilling to kill, but that can be overridden by other evolved modules compelling us to kill prey, predators, or sources of infection. If so, why then is there so much psychological stress, what turns the different modules on and off, and what is the "I" that puts all these modules together?

It is a short step to the next biological shibboleth, "pseudospeciation," the idea that humans have an inborn tendency to categorize enemies as less than human, and so to find it easier to kill (Eibl-Eibesfeldt, 1979:109–111). Dehumanization of enemies frequently occurs in war, and Goodall even applied the concept at Gombe, where Kasakela was said to "de-chimpize" Kahama (Goodall, 1986:532). But if this were an inborn human propensity, then there would be no inhibitions against killing. Obviously, in any war, a line must be drawn, which categorizes those on the other side—often people one knows very well—as meriting death. That can happen in many different ways and does not necessitate seeing enemies as less than human. Idioms of witchcraft and revenge often provide both moral and emotional reason to kill, but the concepts apply very clearly to human beings and only to humans. Pseudospeciation, like so much biological reductionism, slaps a label on the more complicated, and more interesting, practice of morally categorizing enemies.

Dehumanization takes us to the next mythic area, a big one—ethnocentrism. This tendency seems firmly grounded in chimpanzee behavior. Adult male chimpanzees routinely make agonistic displays at signs of outsiders—although there are also instances of tolerance. Goodall (1986:531–532) posits "an inherent fear of, or aversion to strangers, expressed by aggressive attack.” There is more here than mere xenophobia, she emphasizes. Gombe chimpanzees drew a line cutting off *known* individuals, former friends, and “de-chimpized” them to make them suitable to kill. But this is actually a theoretical challenge—not support—for the ethnocentrism explanation of war. The mother of all chimpanzee conflicts developed *within* one group, which only became separate as the conflict intensified.

The idea of in-group amity and out-group enmity comes from Spencer, and the term ethnocentrism from Sumner, both lead thinkers of Social Darwinism (Van der Dennen, 1995:448–452). Their ideas were consistent with the Hobbesian images of savages held by their contemporaries Tylor (1888:221) and Boas (1912). Skipping ahead over decades, ethnocentrism was given a sociobiological stamp by Shaw and Wong (1989) (also see Reynolds et al.,1987), who portray it as an expression of genetic competition—“our kind” share more of “our genes” —and take it up through ethnic and national conflicts. Ghiglieri makes the point with characteristic bluntness:

> Unfortunately, every race, ethnic group, and tribe has its prejudices. Nearly all have led to atrocities, many lethal, often including full-scale war. The message here is that the human psyche has been equipped by kin selection to urge men to eliminate genetic competitors. . . War itself, declared or otherwise, is often motivated by these instinctive genocidal goals. I believe this happens because men are born ethnocentric and xenophobic by nature (Ghiglieri, 1999:215).

This is not a fringe position. It is a cornerstone, for instance, of Niall Ferguson’s (2006:xliv–xlv) recent book explaining “twentieth-century conflict and the descent of the west.”

It is an erroneous position. Contra the social Darwinist imaginings, most tribal war is not between culturally distinctive groups, but similar ones, as illustrated by the segmentary forms of opposition so common around the world (Otterbein, 1973; Sahlins, 1961). The theoretical value of kin selection drops to practically nil a few steps away from ego (Chapais, 2001). Self-sacrifice for “our kind” does not make genetic sense in those terms, though it can be argued to do so in terms of mutualism, cooperation, and altruism—principal themes of this volume. Cultural assertions of common ancestry at the tribal level may be complete fictions, as one recent genetic study of Central Asian tribes has demonstrated—they are no more related within the tribe than the mean kinship of the regional population as a whole (Chaix et al., 2004). The notion that recent “ethnic” or “sectarian” conflicts involve longstanding cultural groups struggling against ancient rivals, has been debunked thoroughly and repeatedly. I call these “identerest conflicts,” emphasizing their highly variable fusions of identities and interests. Identerest groups are constructed in conflict, and they are constructed opportunistically, using multiple criteria, by political entrepreneurs seeking to forge a following (Ferguson, 2003).

Ethnocentrism, to some degree, exists in the very nature of culture. Humans learn that the way their group does things is the way things should be done. “Our ways” get the highest evaluations. But an empirical test of ethnocentrism in East Africa demonstrates that in-group/out-group contrasts beyond that baseline are extremely variable, concluding that Sumner’s image of natural and stark oppositions—the image adopted today by psychological Darwinism—represents the negative pole in variable patterns of intergroup relations (Brewer and Campbell, 1976:144). "Social identity theory does find that [in our competitive society at least], intergroup categorization in itself regularly produces favoritism toward the in-group and discrimination against the out-group." However, regarding actual intergroup conflicts, this "subjective" tendency is seen as playing a secondary role to the "objective" issues of history, society, economics and politics (Tafjel and Turner, 1986:14, 23). Intense ethnocentrism does not explain war, it accompanies war.

Which brings this tour of biological war myths to its last stop, territoriality. The concept itself developed in the study of birds and fish and then spread to other animals. As it spread, it became fuzzier. Does territoriality require active defense, or just regular usage? Is there a line between defense and advertisement (Van der Dennen, 1995:286–289)? Tinbergen (1968) projected the territoriality he saw in herring gulls on to human beings. Ardrey (1966) spread the idea as in *The Territorial Imperative*. It was a hit, a meme if you will, entering every day understandings and language. One reason for its popularity is that—loosely defined—it seems to apply to so many situations, as in Van den Berghe's (1974) sweeping effort to "bring beasts back in" to the study of human conflict.

Territoriality is a major interest of primatology. For chimpanzees, some question if the concept applies since chimpanzee groups commonly share extensive overlapping ranges (Mitani and Rodman, 1979). But agonistic displays in the presence of others, and the specific behaviors at Gombe, convinced Goodall that the label fit. She thinks that for chimpanzees, what truly departs from standard conceptions of territoriality is the deadly violence involved. That, of course, is the link to humans (1986:525–528). As put by Ghiglieri (1988:259), "Primitive hunting and gathering societies the world over exhibit. . . territorial defense and warfare basically identical in form *and* function to that of chimpanzees."

With the expansion of field observations over the years, it is clear that different chimpanzee groups relate very differently to the space they occupy. Some patrol borders, others do not; some male ranges are much larger than females, others only a little, etc. It is not too much to say that each study area has its own particular patterns. In some cases, lethal violence has been observed, in other cases, not. If lethal violence is seen as one variable aspect of territoriality, and if territoriality itself is situationally variable, how can there possibly be an innate predisposition to collective intergroup attacks?

Variation in human beings' social orientations to space dwarfs that of chimpanzees. With people, the concept always involves the added dimension of symbolic construction (Ingold, 1987). In some cases, territorial defense seems a predictable response to concentrations of resources (Dyson-Hudson and Smith, 1978). In some cases it does not (Cashdan, 1983). Territorial identification is often more about social incorporation than perimeter defense (Kelly, 1995:185–189). Among Australian aborigines, foraging bands themselves are made up of members of multiple clans. Recognized clan titles to territory are not about restricting foraging by others—which is allowed—but about limiting access to sacred sites (Layton, 1986:22). As one ethnographer put it, "one could say that to own is to have the obligation to share" (Williams quoted in Ingold, 1987:134).

What about territoriality and war? For Ardrey, it was relevant for defense, to protect against intrusion, without which war would not occur. Tinbergen (1981), who was an inspiration for Goodall at Gombe, took it further to planned conquest and destruction of neighbors. The demonic perspective on apes and humans emphasizes territorial *acquisition.* Chimpanzees are said to go to war for "lebensraum," (Ghiglieri, 1999), to expand (Goodall, 1986:528; Wrangham, 1999b). In that they are said to be just like humans.

Obviously, many human groups do gain territory through war. Some of the earliest ecological models stressed this (Vayda, 1961). New Guinea warfare was at first proposed to be a "struggle for land" (Brookfield and Brown, 1963). More work, however, revealed that much New Guinea warfare lacked that dimension (Knauft, 1990:268–272). The case for Amazonian warfare as territorial expansion is even more tenuous (Ferguson, 1989b). In the ethnology of war, direct territorial acquisition—conquest—comes to the fore with developed social hierarchy—chiefdoms and above—where what is at stake is not the land itself, but the social wealth produced by subjugated peoples. In contemporary ethnonationalist struggles, land often becomes a potent symbol, worth killing for, not because there is too little of it to farm, but because land can be a potent symbol for self-aggrandizing ethnic entrepreneurs, a useful tool for mobilizing us against them. Territoriality is not a first principle that somehow explains human warfare. Rather it is a variable, and its expression and connection to intergroup violence is something that needs *to be* explained.

All these big, vague ideas—man the hunter, naturally aggressive males, pseudospeciation, ethnocentrism, and territoriality—are all too familiar aspects of our existence. We can see examples with our own eyes, at least through the media, and they repeatedly loom large in war. In that familiarity, they help create a public receptive to a simple explanation, that war is the product of a human nature evolved to struggle for reproductive superiority. These big notions provide a conceptual bridge from humans to chimpanzees that lend credence to all the smaller hypotheses discussed. But these half-formed concepts are just vague metaphors, with the patchiest empirical support. There is no evidence that they are orientations somehow encoded in our genes.

Proponents of biological explanations of war do not say it is some kind of fixed instinct. They always emphasize that our biological tendencies are mediated, channeled, and even redirected by culture. They do say humans have a decided tilt toward violence against outside groups, and that this leaning is a necessary factor for understanding war, from tribal peoples to world conflicts today. My position is that there *is no tilt*, no predisposition toward or against war.

Human beings—oriented to cooperation with others, living in symbolically constructed and learned social universes, and possessing language and the ability to communicate over time and space—are capable of almost anything. This, in my view, explains our unique evolutionary success. In this way, we *are born to live*, not to kill. But as Morton Fried (1973:355) once wrote on the idea of innate aggressiveness and war, "you can't kill a bad idea." He was probably right.

Still, anthropology does offer alternatives, very different answers to the question of "why war?" What follows is my alternative, developed over a few decades of studying war in many different contexts (see Ferguson, 1984, 1990, 1995, 2001, 2003, 2006a, 2009).

An Anthropological Alternative

With some oversimplification, my position holds that wars occur when those who make the decisions to start fighting believe that it is in their practical self interest to do so. Self interest is defined in terms of maintaining or enhancing: access to important resources, their costs, safety, and where applicable power. This is the position I have long argued against both biological reductionism, and those many anthropologists who claim that war is the acting out of a particular culture's symbols and scripts, and is *not* about practicalities.

However, the symbolic dimension is very real and important. Particular cultures have their own expectations about war. Purely cognitive factors affect who is considered friend or enemy, within more fundamental structuring forces. They establish what kind of actions are thought appropriate in war, and how war is emotionally experienced, understood, and remembered. The practical and the symbolic are integrated in war. For example, if one considers the warfare that follows Western intrusions into indigenous areas over the past 500 years, patterns are very comparable across world areas. Yet, any detailed investigation finds those war patterns are informed and acted out according to understandings, symbols, and values that are particular to one local culture. The general and practical—and the particular and symbolic—as incomparable as they are, are joined in actual practice. How can we understand this in theory?

My approach to that question goes under the label of *moral conversion*—practical interests are converted into moral claims to persuade others and to justify oneself. As a conflict situation builds, and different courses of actions are weighed, people who participate in the decisions will convert their own perceived self-interest into the highest applicable moral standards, whether that is preserving democracy or avenging witchcraft. No one would tell others, "risk your life because it is good for me." They say, "if you are a man, this is what you should do." This is manipulation, but not just manipulation. People try to minimize cognitive dissonance, and I think it is common, probably the norm, that those advocating wars that serve their interests, come to believe those wars are righteous. As I say, this *is* an oversimplification, but that is the gist of my approach.

To understand war, then, one must focus on the decision makers. That directs attention to the sociopolitical structure of a society. What different kinds of people, groups, and institutions contribute in what ways to decisions, and what are their interests in a given situation? Those interests are as much, sometimes *more*, about the decision makers' position than concerned with the relationships between the groups in conflict. How will one course of action or another, whether toward war or toward peace, affect leaders' standing among their own?

In relatively egalitarian societies, every man decides for himself on war. Yet, even there, there are often recognizable leaders, headmen, inconspicuous except in war, able to persuade and cajole but not able to give anyone orders. Even at that level, by virtue of their position, these elementary leaders have somewhat distinctive interests in any conflict situation and greater influence than others. One of the most common consequences of war is an intensification of control by those in leadership positions, that is, unless things go wrong and they end up with their head on a pike. War leaders' positions get elevated in wartime. Often, leaders favor war, because war favors leaders.

Among the relatively egalitarian Yanomami, fine-grained study reveals their Machiavellian maneuvering (Ferguson, 1995). Among the more conspicuous Big Men of New Guinea that maneuvering is easily seen (Sillitoe, 1978). With chiefdoms around the world, probably the most common explanation for their incessant warfare is "chiefly ambitions." With kings, it is almost too obvious to mention. In the archaeological record, one of the preconditions contributing to war is the development of hierarchy. The self-interest of leaders in contemporary world conflicts is plainly evident for anyone who cares to look.

This is hardly a new idea. What rarely is recognized, however obvious, is that this may be the central explanatory principle of war. Ask people why we have wars, and many will reply, just like that, that it is in human nature. Very few will say that it is because of the self-interest of leaders, although they will say "of course" if asked about that directly. When reporters contact me, they want to hear about human nature, not the machinations of decisions makers. But that is where we should be looking. That is where we should direct the public's attention. For me, this is the biggest problem with biological "explanations" of war. They lay down a smoke screen, closing out an alternative explanation which is much better grounded in theory and evidence, that encourages citizens to foreground the question they really need to ask. When leaders call for war, what is in it for them?

References

Albert, B. (1989). Yanomami "violence": Inclusive fitness or ethnographer's representation? *Current Anthropology, 31*, 637–640.

Archer, J. (2000). Sex differences in aggression between heterosexual partners: A meta-analytic review. *Psychological Bulletin, 126*, 651–680.

Ardrey, R. (1966). *The territorial imperative*. New York: Atheneum.

Bernal, V. (2001). From warriors to wives: Contradictions of liberation and development in Eritrea. *Northeast African Studies, 8*, 129–154.

Boas, F. (1912). An anthropologist's view of war. Carnegie Endowment for International Peace, International Conciliation Pamphlet no. 52.

Brewer, M.B., & Campbell, D.T. (1976). *Ethnocentrism and intergroup attitudes*: *The east African evidence*. New York: Wiley.

Brookfield, H.C., & Brown, P. (1963). *Struggle for land: Agriculture and group territories among the Chimbu of the New Guinea Highlands*. New York: Oxford University Press.

Cashdan, E. 1983. Territoriality among human foragers: Ecological models and an application to four bushman groups. *Current Anthropology, 21*(1), 47.

Chagnon, N. (1968). *Yanomamo: The fierce people.* New York: Holt, Rinehart Winston.

Chagnon, N. (1988). Life histories, blood revenge, and warfare in a tribal population. *Science, 239*, 985–992.

Chagnon, N. (1990). Reproductive and somatic conflicts of interest in the genesis of violence and warfare among tribesmen. In J. Haas (Ed.), *The anthropology of war* (pp. 77–104). Cambridge: Cambridge University Press.

Chaix, R., Austerlitz, F., Khegay, T., Jacquesson, S., Hammer, M.F., Heyer, E., & Quintana-Murci, L. (2004). The genetic or mythical ancestry of descent groups: Lessons from the Y chromosome. *American Journal of Human Genetics, 75*, 113–116.

Chapais, B. (2001). Primate nepotism: What is the explanatory value of kin selection? *International Journal of Primatology, 22*, 203–229.

Collier, P., & Hoeffler, A., (2004). Greed and grievance in civil war. *Oxford Economic Papers.*

Condry, J., & Condry, S., (1976). Sex differences: A study in the eye of the beholder. *Child Development, 47*, 812–819.

Daly, M., & Wilson, M. (1988). *Homicide*. New York: Aldine de Gruyter.

Davis-Kimball, J. (2002). *Warrior women: An archaeologist's search for history's hidden heroines.* New York: Warner Books.

Dyson-Hudson, R., & Smith, E. (1978). Human territoriality: An ecological reassessment. *American Anthropologist, 80*, 21–41.

Eagly, A.H., & Wood, W. (2003). The origins of sex differences in human behavior: Evolved dispositions versus social roles. In C.B. Tavris (Ed.), *Evolution, gender, and rape* (pp. 265–304). Cambridge: MIT Press.

Edgerton, R.B. (2000). *Warrior women: The amazons of Dahomey and the nature of war*. Boulder, CO: Westview Press.

Eibl-Eibesfeldt, I. (1979). *The biology of peace and war: Men, animals, and aggression*. New York: Viking.

Fadiman, J. (1982). *An oral history of tribal warfare; The Meru of Mt. Kenya*. Athens: Ohio University.

Ferguson, N. (2006). *The war of the world: Twentieth-century conflict and the descent of the west*. New York: The Penguin Press.

Ferguson, R.B. (1984). Introduction: Studying war. In R.B. Ferguson (Ed.), *Warfare, culture, and environment* (pp. 1–81). Orlando, FL: Academic.

Ferguson, R.B. (1989a). Do Yanomamo killers have more kids? *American Ethnologist, 16*, 564–565.

Ferguson, R.B. (1989b). Ecological consequences of Amazonian warfare. *Ethnology, 28*, 249–264.

Ferguson, R.B. (1990). Explaining war. In J. Haas (Ed.), *The anthropology of war* (pp. 26–55). Cambridge: Cambridge University Press.

Ferguson, R.B. (1995). *Yanomami warfare: A political history*. Santa Fe: School of American Research Press.

Ferguson, R.B. (1999). A paradigm for the study of war and society. In K. Raaflaub & N. Rosenstein (Eds.), *War and society in the ancient and medieval worlds: Asia, the Mediterranean, Europe, and Meso-america* (pp. 409–458). Cambridge, MA: Center for Hellenic Studies and Harvard University Press.

Ferguson, R.B. (2001). Materialist, cultural and biological theories on why Yanomami make war. *Anthropological Theory, 1*, 99–116.

Ferguson, R.B. (2003). Violent conflict and the control of the state. In R.B. Ferguson (Ed.), *The state, identity, and violence: Political disintegration in the post-Cold War era* (pp. 1–58). New York: Routledge.

Ferguson, R.B. (2006a). Tribal, "ethnic," and global Wars. In M. Fitzduff & C. Stout (Eds.), *The psychology of global conflicts: From war to peace, Vol. 1: Nature vs. nurture* (pp. 41–69). Westport, CT: Praeger Security International.

Ferguson, R.B. (2006b). Archaeology, cultural anthropology, and origins and intensifications of war. In E. Arkush & M. Allen (Eds.), *The archaeology of warfare: Prehistories of raiding and conquest* (pp. 469–523). Gainesville, FL: University of Florida Press.

Ferguson, R.B. (2009). Ten points on war. In A. Waterston (Ed.), *An anthropology of war: Views from the frontline* (pp. 32–49). New York: Beghahn Books.

Ferguson, R.B., & Whitehead, N.L. (2000). *War in the tribal zone: Expanding states and indigenous warfare.* Santa Fe: School of American Research Press.

Fried, M. (1973). On human aggression. In C. Otten (Ed.), *Aggression and evolution* (pp. 355–362). Lexington, MA: Xerox College Publishing.

Fry, D.P. (2006). *The human potential for peace.* New York: Oxford University Press.

Ghiglieri, M.P. (1984). *The Chimpanzees of the Kibale Forest: A field study of ecology and social structure.* New York: Columbia University Press.

Ghiglieri, M.P. (1988). *East of the Mountains of the Moon: Chimpanzee society in the African rain forest.* New York: The Free Press.

Ghiglieri, M.P. (1999). *The dark side of man: Tracing the origins of male violence*. Reading, MA: Perseus Books.

Goldstein, J.S. (2001). *War and gender: How gender shapes the war system and vice versa.* New York: Cambridge University Press.

Goodall, J. (1968). The behaviour of free-living chimpanzees in the Gombe Stream Reserve. *Animal Behaviour Monographs, 1*, 165–311.

Goodall, J. (1986). *The chimpanzees of Gombe: Patterns of behavior*. Cambridge, MA: Belknap Press of Harvard University Press.

Gros-Louis, J., Perry, S., & Manson, J.H. (2003). Violent coalitionary attacks and intraspecific killing in wild white-faced capuchin monkeys (*Cebus capucinus*). *Primates, 44*, 341–346.

Grossman, D. (1996). *On killing: The psychological cost of learning to kill in war and society.* New York: Little, Brown and Company.

Haas, J. (1999). The origins of war and ethnic violence. In J. Carman & A. Harding (Eds.), *Ancient warfare: Archaeological Perspectives* (pp. 11–24). Phoenix Mill: Sutton Publishing Ltd.

Hoebel, E.A. (1978). *The Cheyennes: Indians of the great plains*, 2nd ed. New York: Holt, Rinehart, and Winston.

Ingold, T. (1987). *The appropriation of nature: Essays on human ecology*. Iowa City, IA: University of Iowa Press.

Jolly, C.J. (1970). The seed eaters: A new model of Hominid differentiation based on a baboon analogy. *Man, 5*, 5–26.

Jones, D.E. (2005). *Women warriors: A history.* Washington, DC: Brassey's.

Keeley, L.H. (1996). *War before civilization: The myth of the peaceful savage*. New York: Oxford University Press.

Kelly, R.B. (1995). *The foraging spectrum: Diversity in hunter-gatherer life ways.* Washington, DC: Smithsonian Institution Press.

Konner, M. (2006). Human nature, ethnic violence, and war. In M. Fitzduff & C. Stout (Eds.), *The psychology of global conflicts: From war to peace, Vol. 1: Nature vs. nurture* (pp. 1–40). Westport, CT: Praeger Security International.

Knauft, B. (1990). Melanesian warfare: A theoretical history. *Oceania, 60*, 250–311.

Layton, R. (1986). Political and territorial structures among hunter-gatherers. *Man, 21*, 18–33.

LeBlanc, S.A., with Register, K. (2003). *Constant battles: The myth of the peaceful, noble savage.* New York: St. Martin's Press.

Lizot, J. (1989). Sobre la guerra: Una respuesta a N.A. Chagnon (*Science* 1988). *La Iglesia en Amazonas, 44(April)*, 23–34.

Lovejoy, C.O. (2009). Reexamining human origins in light of *Ardipithecus ramidus*. *Science, 326,* 74, 74e1–74e8.

Marks, J. (2003). *What it means to be 98% chimpanzee.* Berkeley, CA: University of California Press.

Meggitt, M. (1977). *Blood is their argument: Warfare among Mae Enga tribesmen of the New Guinea Highlands*. Palo Alto, CA: Mayfield.

Mesquida, C.G., & Wiener, N.I. (1999). Male age composition and severity of conflicts. *Politics and Life sciences, 18*, 181–189.

Mitani, J.C., & Rodman, P.S. (1979). Territoriality: The relationship of ranging pattern and home range size to defendability, with an analysis of territoriality in primates. *Behavioral Ecology and Sociobiology, 5*, 242–251.

Montagu, A.M.F. (1968a). The new litany of "innate depravity," or original sin revisited. In A.M.F. Montagu (Ed.), *Man and aggression* (pp. 3–17). New York: Oxford University Press.

Montagu, A.M.F. (1968b). (Ed.) *Culture: Man's adaptive dimension*. New York: Oxford University Press.

Moore, J.H. (1990). The reproductive success of Cheyenne war chiefs: A contrary case to Chagnon's Yanomamo. *Current Anthropology, 31*, 322–330.

Nishida, T., Haraiwa-Hasegawa, M., & Takahata, Y. (1985). Group extinction and female transfer in wild chimpanzees in the Mahale National Park, Tanzania. *Zeitschrift fur Tierpsychologie, 67*, 284–301.

Otterbein, K. (1973). The anthropology of war. In J. Honigmann (Ed.) *Handbook of social and cultural anthropology* (pp. 923–958). Chicago: Rand McNally College Publishing.

Otterbein, K. (2004). *How war began*. College Station, TX: Texas A&M Press.

Pierson, D. (1999). Natural killers–turning the tide of battle. *Military Review (May–June)*, 60–65.

Power, M. (1991). *The egalitarians–human and chimpanzee: An anthropological view of social organization.* New York: Cambridge University Press.

Reynolds, V., Falger, V.E.E., & Vine, I. (Eds.). (1987). *The sociobiology of ethnocentrism: Evolutionary dimensions of xenophobia, discrimination, racism and nationalism*. London: Croom Helm.

Sahlins, M. (1961). The segmentary lineage: An organization of predatory expansion. *American Anthropologist, 63*, 322–345.

Sapolsky, R.M. (1997). *The trouble with testosterone*. New York: Scribner.

Shaw, R.P., & Wong, Y. (1989). *Genetic seeds of warfare: Evolution, nationalism, and patriotism.* Boston, MA: Unwin Hyman.

Sherwood, C.C., Subiaul, F., & Zawidzki, T.W. (2008). A natural history of the human mind: Tracing evolutionary changes in brain and cognition. *Journal of Anatomy, 212*, 426–454.

Sidorowicz, L.S., & Lunney, G.S. (1980). Baby X revisited. *Sex Roles, 6*, 67–73.

Sillitoe, P. (1978). Big men and war in New Guinea. *Ethnology, 16*, 252–271.

Smith, D.L. (2007). *The most dangerous animal: Human nature and the origins of war*. New York: St. Martin's Press.

Sugiyama, Y. (1968). Social organization of chimpanzees in the Budongo Forest. *Primates, 9*, 225–258.

Tafjel, H., & Turner, J.C. (1986). The social identity theory of intergroup behavior. In S. Worchel & W.G. Austin (Eds.), *Psychology of intergroup relations*, 2nd ed. (pp. 7–24). Chicago: Nelson-Hall.

Thorpe, I.J.N. (2003). Anthropology, archaeology, and the origin of warfare. *World Archaeology, 35*, 145–165.

Tinbergen, N. (1968). On war and peace in animals and man: An ethologist's approach to the biology of aggression. *Science, 160*, 1411–1418.

Tinbergen, N. (1981). On the history of war. In L. Valzelli & L. Morgese (Eds.), *Aggression and violence: A psychobiological and clinical approach* (pp. 31–38). Saint Vincent: Edizioni Centro Culturale.

Tooby, J., & Cosmides, L. (1988). The evolution of war and its cognitive foundations. *Institute for Evolutionary Studies Technical Report 88–1.* Accessed June 7, 2011. http://www.psych.ucsb.edu/research/cep/papers/Evolofwar.pdf

Tylor, E. (1888). *Anthropology: An introduction to the study of man and civilization*. New York: D. Appleton and Co.

Van den Berghe, P.L. (1974). Bringing beasts back in: Toward a biosocial theory of aggression. *American Sociological Review, 39*, 777–778.

Van der Dennen, J.M.G. (1995). *The origin of war: The evolution of male-coalitional reproductive strategy*. Groningen, Netherlands: Origin Press.

Vayda, A.P. (1961). Expansion and warfare among swidden agriculturalists. *American Anthropologist, 63*, 346–358.

Vigilant, L., Hofreiter, M., Siedel, H., & Boesch, C. (2001). Paternity and relatedness in wild chimpanzee communities. *Proceedings of the National Academy of Sciences, 98*, 12890–12895.

Wilson, M.L., Wallauer, W.R., & Pusey, A.E. (2004). New cases of intergroup violence among chimpanzees in Gombe National Park, Tanzania. *International Journal of Primatology, 25*, 523–549.

Wilson, M.L., & Wrangham, R.W. (2003). Intergroup relations in chimpanzees. *Annual Review of Anthropology, 32*, 363–392.

Wrangham, R.W. (1974). Artificial feeding of chimpanzees and baboons in their natural habitat. *Animal Behaviour, 22*, 83–93.

Wrangham, R.W. (1995). Ape cultures and missing links. *Symbols*, Spring, 2–9, 20.

Wrangham, R.W. (1999a). Is military incompetence adaptive? *Evolution and Human Behavior, 20*, 3–17.

Wrangham, R.W. (1999b). Evolution of coalitionary killing. *Yearbook of Physical Anthropology, 42*, 1–30.

Wrangham, R.W. (2005). Planet of the apes. *Harper's, 310 (January)*, 15–18.

Wrangham, R., & Peterson, D. (1996). *Demonic males: Apes and the origins of human violence*. New York: Houghton Mifflin Company.

Wrangham, R., & Pilbeam, D. (2001). African apes as time machines. In B. Galdikas, N.E. Briggs, L.K. Sheeran, G.L. Shapiro, & J. Goodall (Eds.), *All apes great and small: Vol. 1: African apes* (pp. 5–17). New York: Kluwer/Plenum Publishers.

Younger, S.M. (2005). *Leadership, violence, and warfare in small societies: A simulation approach*. Los Alamos: Los Alamos National Laboratory.

Zihlman, A.L, Cronin, J.E., Cramer, D.L., & Sarich, V.M. (1978). Pygmy chimpanzee as a possible prototype for the common ancestor of humans, chimpanzees, and gorillas.