Chimpanzee Violence is a Serious Topic. A Response to Sussman and Marshack's Critique of Demonic Males: Apes and the Origins of Human Violence

Chimpanzee violence is a serious topic:

A response to Sussman and Marshack's critique of *Demonic Males: Apes and the Origins of Human Violence (Wrangham & Peterson 1996).*

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In *The Moral Equivalent of War,* published as tensions were mounting before the First World War, the American psychologist and philosopher William James (1910) brought together two superficially contradictory beliefs. On the one hand, he noted, "History is a bath of blood" (James 1910: 4). He wrote that among tribal societies "to hunt a neighboring tribe, kill the males, loot the village and possess the females, was the most profitable, as well as the most exciting, way of living" (James 1910: 3-4). He described Greek and Roman history as a panorama of war for war's sake, and he traced the same tendencies throughout history to the modern day. "Such was the gory nurse that trained soldiers to cohesiveness," James wrote. "Our ancestors have bred pugnacity into our bone and marrow, and thousands of years of peace won't breed it out of us" (James 1910: 6).

On the other hand, James was a passionate pacifist who regarded war as "a transitory phenomenon in social evolution" (James 1910: 8). "I devoutly believe in the reign of peace and in the gradual advent of some sort of socialistic equilibrium," he wrote (James 1910: 14). "The fatalistic view of the war function is to me nonsense …" (James

1910: 14). Squaring these sentiments with his belief that war is somehow part of our biology, he felt that the great challenge for the future was to find a moral equivalent of war, some activity that would substitute for its dangerous attractions. James' idea that young men need to be challenged with non-war activities has been credited with heralding such institutions as the Peace Corps.

I begin my comment on Sussman and Marshack's critique of *Demonic Males* with William James not because the details of James' ideas were necessarily right, but because I regard him as illustrating and validating the enterprise that Dale Peterson and I undertook. Being concerned that the unpleasantly martial facts of history might induce pessimism, James aimed for constructive thinking without being weakly utopian. So he developed a theory of violence and used it to foreshadow a more peaceful future. His theory was that young men enjoyed war because they liked the excitement of fighting: "The horror makes the thrill," he wrote. Accordingly, institutions were needed that would satisfy young men's need for such feelings without war. Thus he reconciled his understanding that violence comes from biological predispositions with a forward-looking and ultimately optimistic view of the human future. Tolstoy was likewise a pacifist who understood humans to be profoundly drawn to violence. But he had a different theory from James, which was that fear of the Lord was the only motivation for peace. *Demonic Males* has a different theory again. Among other things it proposes that men are inherently more dangerous than women, and that massive imbalances of power among hostile entities tend to induce violence. These various approaches have their differences but they are united in one vital way: they assume that an understanding of violence paves the way for reducing it. I am surprised and disappointed that Sussman &

Marshack take a different message from *Demonic Males*. Their notion appears to be that if a behavior is argued to have a biological basis, it must therefore be inevitable. I am surprised at their making this deterministic fallacy, especially since in Chapter 5 of *Demonic Males* Peterson and I explicitly rejected it. There were many who read *Demonic Males* without thinking that it implied any kind of biological determinism, but for those that did, I apologize for not making even clearer our repudiation of it.

My own reasons for interest in the evolution of aggression began with a personal exposure to inter-group hostility among chimpanzees in the early 1970s. Such interactions were then being detected for the first time. The revelation of intense violence and killing in a close relative raised obvious and important questions about the underlying biology for our research team, but it did not promote the sense of fatalism that Sussman and Marshack fear. Strikingly, the three senior scientists who were most closely involved with the discovery of chimpanzee lethal violence at Gombe in the 1970s have all subsequently campaigned particularly strongly against war. Jane Goodall (who was my field advisor for my PhD while I was studying chimpanzees in the early 1970s) was appalled by the discoveries of infanticide, rape and killing of adults among chimpanzees, and wrote eloquently and explicitly about them. Since 2002 Goodall has been a United Nations Messenger of Peace and a tireless advocate for non-violence and a sustainable world. Robert Hinde (my PhD advisor) has written extensively about the deep prospects for peace among nations, including a book on eliminating inter-state conflict (*War No More*, Hinde and Rotblat 2003), and continues to campaign at the highest levels for reductions in nuclear weaponry through his intense involvement with the Pugwash group. David Hamburg, who like Hinde visited Gombe in the 1970s during the time when

patterns of chimpanzee violence were coming into focus, was originally an academic psychiatrist. Hamburg was particularly strongly affected by the revelations. He produced an important analysis of chimpanzee violence with the same essential argument as in *Demonic Males* (Hamburg and Trudeau 1981), and worked for decades with various US administrations and at the UN to help reduce violence around the world. His very important book *Preventing Genocide* (Hamburg 2010) is rich in detailed practical solutions for international peace-making and war-preventing mechanisms, yet explicit about the lessons from evolution.

The examples of James, Goodall, Hinde and Hamburg stand as challenges to the idea of Sussman and Marshack (S&M) that "if human problems such as warfare and killing are seen as universal, primordial, adaptive and natural, then … these views may become virtually immutable in the collective unconscious, diminishing our impetus for positive change." James, Goodall, Hinde and Hamburg show that exposure to chimpanzee violence, and in some cases an understanding that it was closely related to human violence, did not propel them into fatalism.

Similarly for my own part, I believe that studies such as *Demonic Males* can help promote the non-killing philosophy by grappling with such questions as why some species are more violent than others, and what the answers mean for our species' future. Of course whether or not that potential is realized depends on the validity of facts and theory, as S&M note. S&M argue that the evidence for chimpanzee violence in *Demonic Males* was inadequate. Certainly we knew less then than we do now. As it turns out, the evidence for chimpanzees killing each other has since mounted steadily since *Demonic Males* was published.

The frequency and distribution of lethal violence in chimpanzees.

The "real controversy", S&M suggest, lies in the question "just how common is coalitionary conspecific killing in chimpanzees?". S&M conclude that "in observations totaling over 215 years, there have been very few recorded instances of extreme violent behavior." Accordingly "the evidence and the interpretations are suspect and controversial." S&M imply that observations of coalitional killing by chimpanzees are so rare that they are unimportant and/or untrue.

Coalitionary killings among chimpanzees are certainly rare. Nevertheless current estimates suggest that they occur at a frequency not very different from war deaths among human pre-state societies (which themselves occur at a substantially higher rate than war deaths in twentieth-century industrial nations, Keeley 1996). In a survey of nine study communities in the five longest-studied populations of chimpanzees with more than one community, Wrangham et al (2006) reported that the median risk of violent death for chimpanzees from inter-community killing (69-287 per 100,000 per year) fell in the same order of magnitude as the median reported values for rates of death from warfare among subsistence-society hunters and farmers (164 and 595 per 100,000 per year, respectively). Wrangham et al's (2006) rates for humans came from the 32 subsistence societies for which data could most easily be found, from the famously pacific Semai to various war-like groups. Since any particular tabulation of frequency data would be altered by sampling different societies and different periods, the comparison does not justify strong conclusions about the relative rate of war deaths among farmers, hunters and

chimpanzees. Still, these estimates show that if violent death has been important among human populations, as few would deny, it is important for chimpanzees.

The five chimpanzee populations that provided data in Wrangham et al.'s (2006) study were Budongo and Kibale (Uganda), Gombe and Mahale (Tanzania) and Taï (Ivory Coast). Data came from up to 2004. At that time coalitionary killing had not been seen in inter-community contexts in Budongo or Taï. Inter-community interactions (and even the location of inter-community boundaries) in Budongo remain poorly understood but seven infant corpses have been found in contexts suggestive of intergroup killing (Reynolds 2005). Two cases of coalitionary killing have now been recorded in Taï (Boesch et al. 2008). Evidence of coalitionary killing has also come from a six-month study of unhabituated chimpanzees in Gabon (Boesch et al. 2007), from Kalinzu Forest in Uganda (Hashimoto and Furuichi 2005) and in the Republic of the Congo, where Goossens et al. (2005) described the results of 8 years of monitoring of 37 wild-born captive chimpanzees released into the Conkouati-Douli National Park. Goossens et al. (2005) reported that "encounters with wild chimpanzees were a major cause of mortality in released males, and 40–50% of released males would have died without veterinary intervention." They concluded that "males should not be released where wild chimpanzees occur, as they are likely to be attacked and killed." Boesch et al. (2007) drew two general conclusions: "1) wild chimpanzees may be very aggressive even in the absence of human observers, which can lead to conspecific killings, in contradiction to the suggestion of Clark (2002) and Power (1991); and 2) wild chimpanzees resort to intercommunity killing through most of their natural range, from groups in rather open habitat to ones in the dense forest, as well as groups that are artificially provisioned, ones

under regular human observation, and ones not habituated to human presence." Thus recent data has abundantly affirmed the conclusion drawn in *Demonic Males* that coalitionary killing is a characteristic behavior of chimpanzees.

S&M draw attention to three kinds of observation that appear to challenge that conclusion. First, they suggest that after Goodall began her studies in Gombe there were so many years without violence that non-violence should be regarded as the true pattern. But S&M are confused. They claim 24 years without violence, from 1950 to 1974. They should have said 11 years: Goodall's study began in 1960, and the first intergroup infanticide was seen in 1971. Even 11 years is too long a period to provide evidence about inter-community killing, because observers did not start following habituated chimpanzees throughout their territory until the late 1960s (about 1968). Up to that time, either the chimpanzees were poorly habituated to humans (from 1960 to about 1963), or the research team confined their observations to a small provisioning area visited by chimpanzees in search of bananas, close to the center of the study community's territory. After 1968 most observations outside the provisioning area were at first relatively brief (around a half day). In 1971 David Bygott became the first researcher to regularly follow chimpanzees to the borders of their range, and later that year he saw the first recorded inter-community infanticide (Bygott 1972). In 1972 I became the first observer to rely on all-day observations for my research, and like Bygott and subsequent researchers I regularly saw intensely hostile intergroup interactions. Given the overall rarity of inter-community violence, a latency of about three years from the start of regular observations in the natural environment to the first killing (1968 to 1971) is no surprise.

Second, S&M suggest that some of the reported killings might have been deaths

from other causes. The quality of observation certainly varies. At one extreme, several killings in Kibale (Ngogo) have been filmed. At the other are cases of "suspected" killings (such as the death of Humphrey in Gombe, mentioned by S&M), in which a healthy adult male in the prime of life died without any known cause, except that circumstances suggest he had been a victim of an inter-community interaction. Wrangham et al. (2006) distinguished killings by whether they were "observed", "inferred", or "suspected", and gave frequency estimates both including and excluding "suspected" cases. Inclusion of "suspected" cases raised the estimated death rate from inter-community killing from 69 to 287/100,000/year, and from intra-community killings from 271 to 287/100,000/year. Whether or not "suspected" cases are included, the median death rates remain significant.

Third, S&M note that there are several other sites (they say "five") where chimpanzees have been studied without any records of lethal violence. Certainly there are various sites where scientists have studied chimpanzees without any record of coalitionary killing or other kinds of violence. In some cases there are obvious explanations. The chimpanzees may remain too poorly habituated to be observed closely throughout their territory (e.g. Lopé, Gabon; Mt Nimba, Guinea; Ugalla, Tanzania). The study community may be isolated from other chimpanzees (e.g. Bossou, Guinea; Kyambura, Uganda). Or the population density may be so low that inter-community interactions are very rare (a possible contributor to the lack of reports for Fongoli, Senegal). I agree with S&M that further data will be valuable. I look forward particularly to the discovery of peaceful association between members of neighboring communities, because such novel data would raise fascinating questions about behavioral variation and

its causes.

In sum the idea that the evidence for chimpanzee violence is "suspect and controversial" is not tenable. Killing by chimpanzees is rare but it has a wide geographical distribution and is recorded persistently, even though its occurrence is reduced by chimpanzees tending to avoid the borders with their neighbors (Wilson et al. 2007). Everyone involved with the care of chimpanzees in sanctuaries knows that adult males are so dangerous that they must be managed with great caution in order to avoid severe injury or death to conspecifics and even to humans. It is time to shift the conversation. Although coalitionary killing has been documented in some other primate species (Gros-Louis et al 2003, Campbell 2006, Valero et al 2006), chimpanzees have a predisposition for coalitionary killing far exceeding that in most primates. The question is why.

Explaining lethal violence in chimpanzees.

The only developed theory of the evolution of coalitionary killing in chimpanzees with which I am familiar is the imbalance-of-power hypothesis, foreshadowed by various writers (e.g. Goodall 1986, Manson and Wrangham 1991, van der Dennen 1995), described in *Demonic Males* and elaborated by Wrangham (1999). S&M summarize the theory and characterize it as having three main components, each of which has a "serious flaw" that they articulate. For the sake of brevity I restrict my responses to S&M's "serious flaws".

S&M's first objection concerns the concept of "coalitionary killing." S&M believe this term is illegitimate because in their view, the claim that humans, chimpanzees and

ants exhibit "coalitionary killing" is bound to mean that in each species this behavior has "the same biological bases and motivations and therefore … driven by the same underlying natural causes."

I am surprised by this objection, and regret that Peterson and I did not make our position clearer. Contrary to S&M's inference, I used "coalitionary killing" solely as a descriptive term to denote two or more individuals acting jointly to kill a victim. The use of this term does nothing to constrain our understanding of the underlying biology. Specifically the description of coalitionary killing as occurring in both chimpanzees and humans has no bearing on the motivations that can be ascribed in each case. In a similar way I might describe bat, birds and moths as animals that "fly", but by doing so I assume nothing about the aerodynamic principles followed by the different species.

Whether or not the biology underlying coalitionary killing is in fact the same in chimpanzees and humans is an open question, though of course I hypothesize that there are important continuities. Answers will depend partly on which aspects of biology we are interested in, including adaptive consequences, neural architecture, proximate stimuli, developmental precursors, etc. They will not depend on our system of ethological categorization of behavior.

S&M's second objection is that the imbalance-of-power hypothesis fails to explain why coalitionary killing is so rare. For example they ask: "With regard to the imbalance-of-power argument, are we to believe that whenever a group of chimpanzees or humans perceives weakness in another individual or group, that group will attack and kill?" (Given that the intended victim can sometimes escape, the question would be more appropriate if it ended "will attack and *try to* kill?") S&M's discussion of this question

reveals some misunderstanding of chimpanzee grouping dynamics and the imbalance-of-power hypothesis.

S&M imply that the imbalance-of-power hypothesis would predict that any meeting between a solitary male and a larger group should lead to an attempted kill. However, encounters between a solitary male and a larger group ordinarily occur within communities, i.e. between individuals who are part of the same social network. In these cases the imbalance-of-power hypothesis does not predict attempts to kill, since (according to the hypothesis) not only must there be a sufficient imbalance of power that the aggressors can attack with impunity, but there must also be a state of hostility between the two meeting groups. In the case of chimpanzees, this state of hostility means that the two groups must be from different communities. In the case of pre-state humans, they can be from different tribes.

S&M's confusion on this point is reflected by their further questions along the same lines. They ask: "Do humans normally desire to do so [i.e. kill the weak], but are they restrained by laws and regulations and the fear of punishment?" It is nonsensical to think that killing has normally to be restrained "by laws and regulations and the fear of punishment". Because the imbalance-of-power hypothesis invokes intergroup hostility as one of its conditions for attempts to kill, it is an inadequate explanation of cases where both parties are subject to the same laws and therefore necessarily part of the same social network.

S&M continue with a provocative question: "Is this why it is easier to compare primitive, pre-state human societies with chimpanzees, since such societies are less constrained by laws and regulations because they are closer to "nature"?"" The answer is

"No." In my view it is indeed easier to establish comparisons about patterns of intergroup killing between chimpanzees and pre-state societies than between chimpanzees and state societies, but this is not because pre-state human societies are closer to "nature", whatever that means. Instead, similarity in a particular pattern of violence between pre-state societies and chimpanzees (namely unprovoked killing of vulnerable members of a neighboring group) is easier to detect partly because the patterns of killing are less variable in each case than those among state societies. In intergroup hostility among state societies, by contrast, the roles of specialized military units, hierarchical leadership, huge groups, diverse weaponry, elaborate alliance systems and other features specific to state organization are significant complicating factors.

There is a second and equally important reason why comparisons among pre-state societies are particularly apposite for comparisons with chimpanzees: relationships among pre-state societies were often "anarchic", meaning that each society was so independent politically that its success depended solely on its own military resources. Among the tribes of hunter-gatherers in the Andaman Islands, for example, there were no alliances and a permanent state of war. Kelly (2000: 118-119) states that "peace was unattainable in external war (between cultural groups that speak mutually unintelligible languages)." Under these conditions the imbalance-of-power hypothesis does predict consistent efforts to attack and kill vulnerable members of neighboring groups. Such a pattern (attacking members of neighboring tribes whenever they were encountered and vulnerable) was indeed reported by Kelly (2000).

S&M's questions, asking under what conditions the imbalance-of-power hypothesis accounts for attempts to kill, are useful in opening up an important topic, which is why

there is enormous variation in the rates of inter-group killing among human societies, including hunter-gatherers. There is not sufficient room here to explore in detail the problems raised by surveys of intergroup violence and war among hunter-gatherers, but two points are crucial. First, the majority of surveys of war among hunter-gatherers do not distinguish cases of people living surrounded by other hunter-gatherers, from cases where they are part of a socio-political system involving dominant groups of farmers. The imbalance-of-power hypothesis predicts consistent attempts to kill only in the former systems, where political relationships are anarchic. Second, the difference between internal and external war is vital. In the extreme form of this distinction, external war is war between tribes speaking mutually unintelligible languages, whereas internal war is between villages or other groups within a tribe (Kelly 2000). The imbalance-of-power hypothesis predicts consistent attempts to kill only in external war. In internal war, by contrast, opponents are socially linked to each other via a variety of allies and affines. Under these conditions the imbalance-of-power hypothesis is only one part of the explanation: other ideas are needed to account for the complex effects of witnesses, reputations, alliance dynamics etc.

The third of S&M's "serious flaws" was that my concept of a "dominance drive" was "entirely inappropriate." I used "dominance drive" to refer to the striving of male chimpanzees to dominate members of neighboring communities. S&M's objection was primarily that the "the word *drive* is problematic because it has been used in so many different ways." I agree that the word "drive" had a confusing history in ethology in the 1960s. But I do not think my use of the term "drive" created much confusion, since it should have been obvious that I was merely trying to express the notion that chimpanzees

spontaneously show much behavior directed towards eagerly dominating others. Evidence for this phenomenon in chimpanzees is seen within communities. Thus males give many spontaneous aggressive displays to each other when rising in rank, followed by a cessation of displays when the target gives formal signals of submission. De Waal (1986: 474) calls this behavior "status-striving", defined as an "intentional striving caused by an appetence for dominance". ("Appetence" is a "strong craving or desire".) S&M may find the concept of status-striving or a dominance drive inappropriate, but they offer no alternative account. Whatever we call it, I believe that all observers of chimpanzees would agree that males exhibit an intense competition for status, which we may call "status-striving", "victory appetite" or a "dominance drive" depending on our preference.

S&M summarize their objections to the imbalance-of-power hypothesis by saying that the hypothesis is not "sufficient to explain violent behavior in chimpanzees or humans." I agree whole-heartedly: there are vast complexities in the nature and patterning of violence, and of course the imbalance-of-power hypothesis does not capture all of them. But in its favor the imbalance-of-power hypothesis represents a start at solving the extraordinary, important and chilling problem of explaining why chimpanzees make deliberate attacks on victims from neighboring communities who are outnumbered and over-powered, in a manner evocative of various human practices and contexts.

Competing hypotheses have been examined but receive little support (Williams et al. 2004; Wilson et al. 2004). First, chimpanzees could in theory have a generalized tendency to attack unfamiliar individuals. However, the likelihood of an attack clearly depends on the relative balance of power (Crofoot and Wrangham, 2009). Second,

specific individuals might be particularly prone to violence. However although individual variation has been shown for predatory aggression by chimpanzees (Gilby et al. 2008) and for rank-related frequencies of intra-community aggression (Muller and Wrangham 2004), Wilson et al. (2001) found equally strong responses to playbacks of strangers among all seven adult males in their study. Third, attacks could be provoked by immediate competition over resources. Relevant stimuli could include the presence of sexually active females, the presence of preferred food patches, a season of ecological stress or a long-term shortage of land or females. None of these has yet been demonstrated to be important, however (Wilson et al. 2004). Only the imbalance-of-power hypothesis has been strongly supported, since much evidence shows that chimpanzees are sensitive to power imbalances, tend to reduce the number of males in neighboring communities, and gain fitness advantages by doing so (Crofoot and Wrangham 2009).

Finally S&M complain that *Demonic Males* pays too little attention to an idea presented by Power (2001), who argued that human interference in Gombe may have exacerbated a natural tendency to violence. "It seems possible," she concluded, "that quite unintentionally the Gombe feeding methods brought about the stressful emotive atmosphere of rare, acute food crisis such as might be brought about through either overpopulation or prolonged natural disaster, which may have made adaptive, under the special circumstances, a change to the mode of resource defense Krebs and Davies (1981) refer to as 'despotism': exclusion of others from resources." (Power 2001: 243). This quote shows that whether or not the Gombe killings were promoted by the provision of some bananas, Power thought that chimpanzees also exhibit such behavior in relation to

natural events. She did not, however, present a theory for why they should do so.

Power (2001) speculated that the provision of large numbers of bananas in a small area (particularly in 1966-68) could have contributed to the killing of adults several valleys away in 1974-77. It is difficult to test her idea, but it clearly involves a large number of assumptions. Power (2001) also suggested that Ngogo (Kibale) would be a more informative site, on the basis that it was undisturbed. The Ngogo chimpanzees have indeed never been provisioned, but now that they have been habituated they prove to have had a rate of death from coalitionary killing that exceeds even that reported from Gombe (Table 2 in Wrangham et al 2006).

The relevance of coalitionary killing in chimpanzees to warfare in humans.

Demonic Males suggests that inter-group killings could have occurred in the ancestral lineages of both chimpanzees and humans, all the way back to their common ancestor. S&M make two main objections to this idea.

First they are skeptical that chimpanzees (strictly, the genus *Pan*) and humans are each other's closest ancestor. As a result, they view behavioral reconstruction on the basis of similarities in behavior between *Pan* and humans as a misguided enterprise. The data on ape phylogeny are strong however. The trichotomy among chimpanzees, humans and gorillas has been resolved in favor of chimpanzees and humans as sister clades (with gorillas as an outgroup) on the basis of the following types of evidence: total single-copy DNA hybridization (Caccone and Powell, 1989), mitochondrial genes (Ruvolo et al. 1991), the entire mitochondrial genome (Horai et al. 2005), multiple independent DNA data-sets (Ruvolo 1997; Chen and Li, 2001), non-coding DNA (Xq13.3) (Kaessmann et

al. 2001), *Alu* elements (Salem et al. 2003), and most notably, ~ 10 million aligned base pairs (Patterson et al. 2006). No large data-sets contradict these results. Data suggesting alternative gene trees can occasionally be found, and is ascribed to lineage sorting of ancestral polymorphisms (Ruvolo 1997; Chen and Li, 2001). The evidence for *Pan* and humans being each other's closest relatives is now so overwhelming that the scientific community and NIH decided to spend millions of dollars sequencing the chimpanzee genome before that of any other primate.

Second, S&M say that "even if the chimpanzee were a good model for the ancestral hominid, this would not mean that humans would necessarily share specific behavioral traits." I agree, and I have made the same point myself several times. The fact that chimpanzees and humans share a rare behavior (coalitionary killing of male neighbors) raises the possibility that the behavior has occurred continuously in both lineages since their common ancestry, but it does not prove it. The hypothesis of common ancestry for such behaviors will be testable with genetic and neurobiological data.

Concluding comments.

Demonic Males represents an early effort at understanding the taxonomic distribution and functional significance of coalitionary killing, and its essential evidence and arguments have been well supported by subsequent observations and ideas (e.g. Wilson et al. 2004, Williams et al. 2004, Watts et al. 2006, Boesch et al. 2007, Sherrow and Amsler 2007, Boesch et al. 2008). I agree with S&M that there are many questions left unanswered, and I believe that continuing research offers the prospect of ever more important insights about the biological, environmental, social and cultural influences on

violence. As we probe such topics, I hope that various distractions can be avoided.

First, a theory about a particular set of behaviors (whether it is about killing, or any other behavior, such as feeding, or dancing) should not be criticized for failing to explain "much of human behavior." Contrary to S&M's implications with their dancing parody, *Demonic Males* is focused on violence as a specific problem, not as a prime mover underlying human behavior in general. The fact that *Demonic Males* does not review affiliation, trust, sexual behavior, foraging and so on does not mean that Peterson or I think these topics unimportant. We both consider them very important. It simply means that *Demonic Males* had a particular focus. *Demonic Males* is completely compatible with the obvious fact that men are often caring, loving, cooperative and moral in the best sense. I regret that anyone should have thought otherwise.

Second, superficial similarities with past ideas are no basis for dismissing contemporary theories. To my mind S&M exaggerate the similarities between the imbalance-of-power hypothesis and early Christian beliefs about human ethics. But even a strong resemblance would merely justify caution in evaluation, rather than rejection.

Third, we should strive to avoid false dichotomies. To S&M human behavior is determined by socialization and culture, and not by nature. But behavior is always the product of both nurture and nature. To S&M, male violence stems either from male competition or female choice. But the behavior of both males' and females' behavior affects the course of evolution. To S&M, australopithecines were the hunted, not the hunters. But chimpanzees are both hunted and hunters, and australopithecines were probably the same. As we seek to explain complex behaviors we must be open to an interacting set of influences.

Finally, I hope that those of us involved in the search for a helpful theory of violence can avoid trivializing each other's efforts. I would be the first to admit that the imbalance-of-power hypothesis does not give us a working prescription for non-violence. But by stressing the particular dangers of male coalitionary behavior *Demonic Males* contributes to an ongoing debate about the prospects for promoting non-violence through the education of women and their increased representation in legislative bodies. Since *Demonic Males* was published I have participated in annual seminars with such programs as *Women Waging Peace*, in which participants represent conflict zones from around the world. I have repeatedly found that they cherish the optimism represented in *Demonic Males* by its identification of some sources of violence that we can do something about – namely, the appalling ease with which men are induced to violence under some circumstances.

Peterson and I ended our book with this challenge: "If we are cursed with a demonic male temperament and a Machiavellian capacity to express it, we are also blessed with an intelligence that can, through the acquisition of wisdom, draw us away from the 5-million-year stain of our ape past." I agree that whether the stain is 5 million years old remains an open question, but however old it is, we can all agree about the urgency of leaving it behind. William James, Jane Goodall, Robert Hinde and David Hamburg have written constructively about a brighter future. The aim of the nascent theory represented in *Demonic Males* is to follow in that important tradition.

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