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Anthropologist Brian Ferguson Challenges Claim that Chimp Violence is Adaptive

By John Horgan | September 18, 2014

In my last post, I critiqued “Lethal aggression in *Pan* is better explained by adaptive strategies than human impacts,” a new paper in *Nature* that represents a broadside in the old debate over whether war is innate. Although the paper does not mention human warfare, two of its lead authors, primatologists Michael Wilson and Richard Wrangham, told reporters that their data “bolster the theory that we humans have a tendency toward violence rooted deep in history,” [as the Boston Globe put it](#).



The Globe also quotes psychologist Steven Pinker, a long-time proponent of the deep-

roots theory of war, dismissing critics of the theory as “romantics.” This is a favorite tactic of deep-roots advocates, to accuse skeptics of being driven by wishful thinking rather than science. Of course, from my perspective, it is the deep-rooters who are guilty of profound confirmation bias, which leads them to ignore overwhelming contradictory data.

Fortunately, some coverage of the *Nature* report, such as [an excellent piece by James Gorman in The New York Times](#), quotes critics of the deep-roots theory, notably R. Brian Ferguson, Professor of Anthropology and Director of Peace and Conflict Studies, Rutgers University. Ferguson, who happens to be working on a book on chimpanzee violence and human warfare, just sent me his detailed critique of the *Nature* paper. If any of the *Nature* authors want to respond to Ferguson, I'd be happy to post their responses. Here is Ferguson's critique:

Wilson et al.'s “Lethal aggression in *Pan* is better explained by adaptive strategies than human impacts” critiques the human-impact hypothesis (HIH) of chimpanzee killing. The idea that deadly violence among chimpanzees is related to human-induced changes was first suggested by Margaret Power, and developed by others, notably Robert Sussman. I outlined my own version and gave it the HIH label in a few pages of an article written in 2009, “Born to Live: Challenging Killer Myths” (in R.W. Sussman and C.R. Cloninger, *Origins of Altruism and Cooperation*). There the HIH was included as an alternative to one of several types of biological explanations of war.

That presentation painted with a broad brush in arguing that intergroup adult male killings among chimpanzees, said to be the evolutionary template for human warfare, are associated with resource competition and other disturbances brought about by the actions of people. Five more years of research and writing on the topic has made it abundantly clear that human impact must be approached in historical detail. To make generalizations about chimpanzee, or bonobo, violence, evidence must be contextualized and examined in its local specifics.

My book manuscript, *Chimpanzees, “War,” and History: Are Men Born to Kill?*, does just that, across all African research sites.

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Human impact is not simple, and often anything but obvious in its long-term effects. It varies by studied group, and always interacts with local ecology, demographics, and social organization before it has behavioral consequences. It should be approached “holistically,” as anthropologists say. In several cases, the key effect is to increase competition over food resources, natural or provisioned. Competition over food resources is broadly accepted by researchers who study chimpanzees as a factor leading to intergroup conflict. My approach does not contradict their perspective; rather, it provides a new way to look at it.

The three measures Wilson et al. created to test for human impact are questionable. One, artificial provisioning of food, is good, where it applies, though the impact of provisioning varies by how it is carried out and other conditions of food availability. Looking at Figure 1 of their paper, sites marked P for provisioning cluster (4 out of 7 cases) toward the high end of the killing distribution. Provisioning’s statistical association with killing, however, is diluted by two other sites. Mahale’s K-group was provisioned and never observed to kill a grown neighbor, but its adult males are suspected—and suspected only—of having been killed by neighboring M-group. Provisioned bonobos at Wamba have not killed, but bonobos rarely engage in serious violence. They are a different species.

A second measure of human impact is the size of protected area, with the assumption that larger areas are less disrupted. But some chimpanzee groups living within large protected areas have been heavily impacted. Their third measure is a combination of five factors. These work well as a general index of over-all human impact, good for developing conservation measures. But they do not work as predictors of intensified violence. “Degree of harassment of study animals by people” and “whether major predators have been eliminated” have not, in my research, had any bearing on chimpanzee violence. “Degree of habituation to human observers at beginning of studies” can be relevant, but locations such as Bossou show that long term cohabitation of chimpanzees and people leading “traditional” lives is very possible. “Amount of hunting of study animals” could possibly lead to increased territorial impaction, as chimpanzees move further into protected areas, thus increasing food competition between groups. But I have not been able to establish any clear connection with violence.

“Disturbance to habitat” is definitely a human impact factor with consequences, but one that cannot be reduced to a simple measure. In some places lumber extraction has severely impacted chimpanzee foods, in others the effect seems minor, and at one location, Budongo, earlier lumbering led to an improved food base when the trees which grew back were more productive than the hardwoods they replaced. Later lumbering by pitsawyers, however, seems to be wholly negative for chimpanzees. None of this critical variation is captured by a four-point scale of degree of habitat disturbance. Nor is it clear that “islandization”—the elimination of chimpanzee rangeland surrounding protected areas, which can push chimpanzees inward—is included in this measure.

Beyond questions about their measures of human impact, a more fundamental issue is methodological. My method is to read all reports about territorial and violent behavior for all study sites, identify changes in behaviors over time, and examine them in the context of changing human activities in the area. When these complex local histories are reconstructed, parallels across different locations emerge, reinforcing inferences about the effects of human impact, which cannot be deduced from a few simple indices. Some will dismiss this method as anecdotal. The question will be, does a historical perspective bringing in human impact provide good explanations of killings, especially in those cases where human impact is said to be insignificant?

Wilson et al. claim three other variables, unrelated to human impact, do show significant predictive power for killings. One is clade, with western chimpanzees having more killings than eastern chimpanzees or bonobos. No argument there—the question is, why are they different? Another of their explanatory variables is population density. The authors assume this to “reflect natural food abundance.” It could be true that density has some statistical association with more violence. But it is also true that higher density may reflect human activities, which either pull or push chimpanzees into the same area.

Their third explanatory variable is the number of males in a group. With this I am in partial agreement. Usually, it takes a good number of males to catch, immobilize and beat and bite another grown chimpanzee to death. Parties with a higher number of males may be more likely to attack vulnerable individuals than those with less. This seem pretty elementary. The point with which I disagree is that great numerical superiority is a necessary and sufficient condition for killing. That is what the argument has been about all these years, and is expressed in the final sentence of the *Nature* article: rather than human impact, killings are “better explained by the adaptive hypothesis that killing is a means to eliminate rivals when the costs of killing are low.”

The long-standing theory is that males kill adult males of other groups to reduce the competition they represent, giving rivals “one less warrior” in intergroup resource competition. It was testably specific. Not included in that theory were killings of within-group males, or of outside females, or of infants. In Wilson et al. 2014, all kinds of killings are lumped together as evidence of an evolutionary adaptive strategy supposedly increasing fitness. Their conclusions are very broad. Most killings are done by numbers of males, infants are more likely to be killed than adults, outsiders are more likely (by 63%) to be killed than insiders. But readers need to go beyond those generalizations, and the statistical sophistication of this study, to consider for themselves what the data (in supplementary material) actually show.

Consider infant killings, listed in extended data tables 2 and 4. When chimpanzees mature, males stay in their own group, and females typically leave to settle elsewhere. If killing infants expresses an adaptive strategy to defend territory and increase mates for males, that entails expectations about what kind of infant should be killed. Not to be killed are the group’s own infant males, who are their future “warriors,” or females of neighboring groups, which potentially are future mates. Likely targets are outside male infants, future competitors; and even inside female infants, who will go off to breed with others. Look at their compiled data. Of all observed or inferred intercommunity infanticides where the sex of the victim is known (a minority of all cases), 4 are males, 4 are females. For intracommunity infanticides, where most times the sex of victim is known, 9 are males, 7 are females. 13 to 11 instances go against reproductive logic. Or look at the data for Budongo, presented in Figure 1 as having the second highest rate of killings per year of any site. Though Wilson et al. claim to find confirmation of adaptive purpose in the fact that over-all, more outsiders than insiders are killed, Budongo has 7 internal, and 7 external infant killings, no outside killing of an adult male, and one internal killing of an adult male.

I hasten to add that infant killing among chimpanzee is an extremely complicated topic, with great variation—whether adult males or females do the killing, whether the body is eaten, whether possibly aberrant behaviors are observed. Some cases fit one or another hypothesis, including my own HIH, but explanation is confounded by all the other cases that do not. My point is that the aggregate data—the currency of this article—when broken down by sex of victim and whether it is inter- or intracommunity, does not support the proposition that members of one group are trying to increase their reproductive success vs. neighboring competitors.

Or consider another set of numbers. The most important measure for comparing chimpanzee to human warfare is intercommunity killing of non-infants, labeled in extended data table 1 as “weaned” individuals to include three older juveniles or young adolescents, but otherwise all adults or close to it. Dealing with chimpanzee research sites (excluding those of bonobos), in 18 areas, over 426 years of observation, 23 males and 6 females were observed or inferred to be victims of intergroup killings. 5 of the males and 2 females are from the prototypical “Four Year War” at Gombe, 1974-1977. 9 males and 1 female come from the time of the Ngogo expansion, 2002-2006, in a class by itself as the most extreme episode of violence ever recorded.

Thus 59% of all intergroup killings of grown individuals come from just 9 years. (3 additional male and 2 female killings come from those same two sites a few years later). The intergroup killings of weaned individuals during the two intense conflicts at Gombe and Ngogo produce a rate of 1.9 killings per year; whereas the other 417 observation years calculate to .03 per year. If intergroup killing is an evolved adaptive strategy, it is a pretty rare occurrence outside those two clusters.

Figures 2a and 2b show that only four study sites rise above .2 killings per year. All the other data points below .2 do not suggest any apparent relationship between number of killings and the article’s claimed explanatory variables, population density and number of males. Take out those four cases and the neat diagonal indicating a relationship would be replaced by a flat line along the X axis. Three of the four >.2 points include these nine years of bloody strife (since Gombe 1974-1977 includes two studied groups, Kahama and Kasakela). The fourth is Budongo. The authors argue that merging these results with all others provides insight to evolved violent predispositions. A historical perspective asks why these cases are unusually violent. To know the answers, you will have to read my forthcoming book *Chimpanzees, “War” and History*.

Three clarifications are needed before closing. First, I am not claiming that chimpanzees never kill members of other groups in the absence of human impact. They are capable of doing so, and I have no doubt that has happened. But the cases that we know about

are best explained by bringing human impact into focus.

Second, emphasizing greater attention to human impact is not an attempt to discredit the behavioral ecology paradigm that is the basis of primate field research. When anthropologists studying “tribal warfare” questioned their older assumption that such fighting was a timeless expression of local conditions and culture, when they came to understand that wars had to be seen historically and in context of interactions with outside factors and actors, that did not end theorizing about the effects of local conditions and culture on war. It made the theories stronger by calling attention to the historical dimension, which had been neglected. My hope is that once tempers cool, *Chimpanzees, “War,” and History* will open productive avenues for behavioral ecology field investigation.

Third, whether chimpanzees do sometimes make war depends on your definition of “war.” In the minimal behavioral sense of a group from one community trying to kill members of another community, chimpanzees can be said to practice war. That makes for interesting comparisons. But otherwise, there is a huge gulf between what chimpanzees do and what humans do. Human warfare, in essence and practice, is structured by social institutions and learned behaviors, and carried out through shared symbolic understandings. Explaining why, when, and where humans go out to kill can only be done by studying these cultural realms. —*R. Brian Ferguson*

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