

Comment on Fry and Soderberg “Lethal aggression in mobile forager bands and implications for the origins of war.”

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19 July, 2013

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The authors of a recent paper (1) study 148 episodes of lethal violence in 21 mobile forager band societies (MFBSs) from the Standard Cross Cultural Sample, finding that ‘most incidents of lethal aggression can aptly be called homicides, a few others feud and only a minority warfare’ which they maintain is “incongruent with the assertion by Bowles ... that war is prevalent in MBFS..” and what they term my conclusion “that war has been pervasive during human evolution.”

I did not make the two claims that the authors attribute to me for the simple reason that I was answering a different question. The paper to which they refer (2) used archaeological and ethnographic data on mortality in intergroup conflicts to calibrate a model of the evolution of altruism, in order to pose the question in its title: “Did warfare among ancestral hunter-gatherer groups affect the evolution of human social behaviors?” To answer the question I needed data on the fraction of all deaths that were due to intergroup conflict, not the evidence that Fry and Soderberg present, namely data on whether “war” is “prevalent” or “pervasive” or the major source of violent deaths.

The Fry and Soderberg paper (FS) contains valuable information for the study of violence in mobile foraging bands. But it does not support the broader implications that it claims. Here I demonstrate two things. First, their evidence is not “incongruent” with my conclusion, namely that mortality in intergroup conflict could indeed have had a major impact on the evolution of human social behavior. And second, their data allow only a limited and biased assessment of their main hypothesis, namely that “most lethal events would stem from personal disputes rather than coalitionary aggression against other groups (war).” I also comment on the relationship between their data set and mine and on their usage of the term ‘war’.

*The question.* Because I considered mortality data in an explicit model of biological evolution I was able to determine that even quite modest levels of mortality in

intergroup conflicts could have sufficiently great effects to alter the evolutionary dynamics of human social behavior. These results reproduce the conclusions of an earlier paper (3) based on entirely different methods (agent based simulation models rather than calibration of an analogue to Hamilton's rule.) In neither paper did the evolutionary importance of warfare require that a substantial fraction of lethal violence is war related, or that war had been "pervasive" during the Late Pleistocene.

For the conclusions of my papers to hold it is sufficient that some (not most) groups sometimes (not most of the time) engage in lethal conflicts with other groups and that those groups with greater numbers of altruistic cooperators tend to win these conflicts. Since Lee's work on the !Kung (4) it is certainly not news that levels of interpersonal within group violence unrelated to war are high in some forager groups, and the fact that this kind of violence may occur with greater frequency than lethal intergroup violence has no bearing on my conclusions.

Being able to determine exactly what numbers are needed to evaluate a well defined hypotheses is the great advantage of formally modeling the evolutionary process, so that the meaning of the data collected can be accurately assessed. The FS paper fails to provide a conceptual framework in which to assess the implications of their data, relying instead poorly posed questions about how "pervasive" "war" was in the past.

*The evidence.* While ethnographic evidence on nomadic foragers is a valuable source of information, conclusions about the extent of warfare during the Late Pleistocene cannot be convincingly addressed using the KS data. There are four reasons why this is true.

First, the FS data set curiously excludes sedentary hunter gatherers, arguably a population type that was quite common during the Late Pleistocene, before agricultural populations ousted these groups from their highly productive sites during the Holocene. Sedentary groups of both egalitarian and hierarchical hunter-gatherers occupying coastal, riverain, and other resource rich sites (e.g. Klasie's River Mouth and other southern African sites at 90,000 years ago (5), Huaca Prieta at 13,000 years ago (6)) were common during the Late Pleistocene, and we learn very little about them by studying nomadic foraging bands. Indeed some of the evidence (on British Columbia sedentary fishers, e.g (7, 8)) suggests extraordinary levels of intergroup violence in sedentary

hunter gatherer groups. The sample also excluded hunter gatherers with “class distinctions” because these (KS claim) “arose within the last 12,500 years.” But the claim motivating this exclusion is incorrect in light of clear evidence of hierarchy dating even to 26,000 years ago (9-14).

The exclusion of sedentary and unequal foragers produces a biased estimate as it ignores data on hunter gatherer societies that are known to have engaged in significant levels of intergroup lethal conflict. Indeed the authors’ nine reasons to expect that mobile foraging bands would have low levels of intergroup lethal conflict should have led them to conclude that other hunter gathers (sedentary populations with class distinctions) would have substantial levels of intergroup conflict and should have been included in the sample so as to have a balanced picture of Late Pleistocene human societies.

Second, in light of the authors’ interest in “early human behaviour” it is surprising that they make no use of the archaeological evidence, which suggests that warfare among hunter gatherers was frequent(2, 15, 16). In my data set level of mortality in intergroup conflicts in the archaeological evidence is very substantial.

Third, all of the societies studied by the authors were (at the time the ethnographic data was collected) living under the (at least formal) authority of states (some colonial some not) which invested substantial military and other resources so that the state itself would monopolize the use of violence within its territory. While these efforts were not entirely successful, they surely reduced intergroup violent conflict among hunter-gatherers living under the state’s authority. For example, we know from a detailed ethno-historical study of the Inupiaq in Alaska that prior to the establishment of state authority intergroup violence was extraordinarily frequent (17) and from archaeological evidence that mortality rates in warfare were very high among pre-contact British Columbia Indians (8). But the northern North American populations in the authors’ sample show virtually no intergroup violence

Fourth, the authors’ data all come from a period of remarkable climatic and environmental stability (19<sup>th</sup> and 20<sup>th</sup> century), conditions allowing foragers to establish between group practices for minimizing conflict. Climate volatility during the Pleistocene was extraordinary(18), often inducing long distance migrations, and hence bringing into contact groups competing for survival and with no established practices for peaceful

interaction. Adverse or volatile weather is a condition shown to predict elevated levels of external conflict(19, 20); and this surely was a more serious problem during the Late Pleistocene than for the living foragers who are the data base for this study.

For these four reasons, it is likely that the sample used presents a downward biased estimate of the extent of warfare among hunter gatherers in the past.

*Comparison with my estimates (2)* . There is but a single overlap between the ethnographic evidence I used (restricted to those societies on which data were sufficient to estimate the fraction of all mortality that was due to intergroup conflict) and the data in (1). This is the Tiwi in Arnhem Land, Australia, who stand out in FS as exhibiting exceptionally high levels of intergroup lethal violence. My estimate of the fraction of all mortality due to intergroup conflict among the Tiwi (not the same statistic that Fry and Soderberg report) places this population a below the mean; only two of the 8 ethnographic groups have lower rates of wartime mortality by my measure. These contrasting estimates from the Tiwi population show that the quantity appropriate to test my hypothesis that warfare was an important force in the evolution of human behaviour (fraction of all mortality due to inter group conflict) can be quite different from the quantities that FS measured. Using both mortality and genetic data from Arnhem Land aboriginal Australians I show (Table 3 of (2) that the Tiwi mortality rate is more than sufficient so that a quite costly form of altruism could proliferate under these conditions. Were I to eliminate from my data set the group that Fry and Soderberg consider to be “exceptional” in its extreme levels of intergroup mortality, my estimate of average wartime mortality would increase.

Another possible comparison concerns their ethnographic evidence of the very limited extent of lethal intergroup interaction among peoples of northern North America, which as I have pointed out above, is inconsistent with the archaeological record of other groups in the region in the past included in my data set (30 sites in northern British Columbia(8), with a intergroup conflict mortality rate almost twice that of the other archaeological estimates.

KS use data from the Andaman Islanders, the ethnographic evidence on which I studied carefully and but could not use because I could not get a reliable estimate of total morality (from all causes) of the groups involved. But close inspection of the KS data on

the Andaman Islanders raises concerns about this and perhaps other data points. We find in the FS data set just two cases (18 and 19) and both are identified as “same tribe” and neither is said to have involved more than a single perpetrator. But in Radcliffe Brown’s celebrated ethnography of the islanders we find that between 1872 and 1902 the Jawara (an Andaman Island tribe) made a series of attacks on other tribes killing 4 (21). I do not see how the authors could have missed these cases (especially given the prominence of Radcliffe Brown’s research, and the fact that they cite him as a source). The Jawara also killed a much larger number of islanders in separate raids, but these attacks were directed against the British and may be rightly excluded as not pertaining to conflicts among hunter-gatherer groups.

FS object that my data includes some deaths in conflicts of indigenous peoples with non hunter-gatherer groups. This is the case for just two of my 23 estimates. Even if these estimates are entirely removed from the data set, the mean fraction of all deaths due to between group conflict falls from 0.14 to 0.11 for the ethnographic sample and from 0.14 to 0.13 for the combined archaeological and ethnographic sample. From the evidence in Figure 2B in (2) it is clear that changes of this magnitude have no effect on the conclusions of that study, namely that warfare could have had a substantial effect on the evolution of human social behaviours.

*Intergroup conflict, war, and human evolution.* The term “war” is not really apt for the kinds of intergroup conflicts likely to have occurred during the Late Pleistocene. A value of the FS paper is that it provides details on deaths, allowing us to determine the number of perpetrators, the relationship between the perpetrator and the victim, and other important aspects of the lethal event. In my models of the evolution of human behaviour, the appropriate usage of the term is “events in which coalitions of members of a group seek to inflict bodily harm on one or more members of another group;” and I have included “ambushes, revenge murders and other kinds of hostilities” analogizing human intergroup conflict during the Late Pleistocene to “boundary conflicts among chimpanzees’ rather than “pitched battles of modern warfare.” (2)

My usage is similar to the definition of Wrangham and Glowaki (22) cited by in FS, but apparently quite different from that adopted in FS, where the motives of the killing matter. FS partition killings into “interpersonal” and “intergroup,” and regard “feud” as

something different from ‘war.’ From their Table 1 we see that motives such as “revenge” or killing “over a particular man” or the fact that a killing was “interpersonal” mean that the event did not fall under the heading of ‘war’. From the standpoint of evolutionary biology, these aspects of the killing are irrelevant: what matters for the dynamics of population composition is that members of a group (more than one) cooperated in killing a member of another group, for any reason whatever. Thus FS exclude from their definition of ‘war’ a possibly large class of events that from the standpoint of evolutionary biology should be included.

We may learn something about intergroup conflict in the Late Pleistocene by analogizing it to modern urban gang warfare. The killings that occur in this setting would certainly qualify as mortality in intergroup conflict. But the motives are often personal, or over a particular man, or feud- or revenge-based(23). Moreover the high level of gang mortality raises questions about many of the characteristics of mobile foraging groups that KS claim militate against intergroup violence, namely i) small group size, iii) fluctuating group membership, iv) multilocal residence, and v) egalitarianism and “no one has the authority to order others to fight.”

But these speculations and concerns take us well beyond the purpose of this note, namely both to point out that the evidence in FS in no way contradicts my claim that intergroup warfare affected the evolution of human behaviour, and to enumerate the ways in which their evidence probably understates the extent of intergroup conflict mortality during the Late Pleistocene.

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