
Homicide and Cultural Evolution

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Homicide rates are suitable materials for the study of "cultural evolution," because they vary dramatically between societies (hence, are "cultural") and change gradually rather than saccadically (hence, "evolve").

Sociological models of the sources of variation in homicide rates ("subcultures of violence"; demographic change; "legitimation of violence"; mass media effects) are criticized for inattention to the social context of violence and to the individual motives of the protagonists. Models of culture change that emphasize "transmission" are criticized for treating the culture-bearing person as a passive vessel rather than an active strategist. A satisfactory theory of the "cultural evolution" of violence awaits satisfactory theories of how people apprehend their interests and how they pursue them.

KEY WORDS: Cultural transmission; Homicide; Imitative violence; Subculture of violence

How and why does the aggregate behavior of the individuals within a society change over time? This is the question of "cultural evolution." We refer to "the aggregate behavior of individuals" for two reasons: first, because social scientists have unduly reified and thereby mystified culture, instead of treating it as a summary description of the statements, actions, and mutual impacts of self-interested individuals (see Alexander 1979; Tooby and Cosmides this issue); and second, because it is only by the aggregation and analysis of individual behavioral data that we can hope to arrive at objective descriptions of culture change and at tests of explanatory hypotheses. Culture is manifested by the relative homogeneity of behavior within societies, as compared to cross-cultural and historical variation. Before one can say what "constrains" cultural evolution (or better, what generates it), one needs to explain why the behavioral homogeneity that is culture even exists. Theory here is underdeveloped; most social scientists treat culture as irreducible—as an explanation rather than an entity to be explained. Others make arm-waving al-

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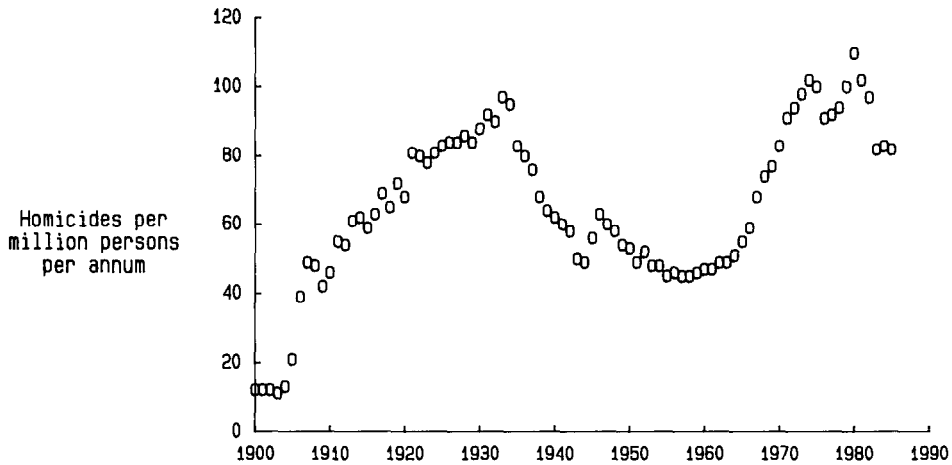


FIGURE 1. Annual homicide rates in the United States, 1900-1985 (data from U.S. Vital Statistics).

lusions to "learning" and "imitation." We shall argue that more attention must be paid to the evolved psychology of the culture-bearing organism.

Homicide is an excellent subject for the analysis of culture change. Because homicides are such drastic acts, the cross-cultural and historical data on homicide are numerous and are relatively uncontaminated by reporting biases as compared with data on almost any other sphere of human activity. And not only are the data exceptionally good, but they exhibit precisely the sort of cross-cultural and historical variation that students of culture and culture change require. Per capita rates of homicide vary enormously between nations: Iceland and Norway, for example, experienced less than one homicide per million persons per annum for the first half of this century, whereas there are presently about 27 per million per annum in Canada, almost 100 in the United States, and over 400 in certain American cities. Attitudes and values surrounding homicide are no less variable: To revenge a murder in kind, for example, is a moral obligation in some social milieus and an abomination in others. Most importantly, homicide rates exhibit substantial, measurable change within societies over historical time. In Britain, for example, the homicide rate has declined more or less steadily over several centuries, until the modern Englishman faces a risk of murder not 5 percent of that which confronted his thirteenth-century counterpart (Gurr 1981). In the shorter term, homicide rates change with a gradualism that inspires one to refer to a process of "evolution" rather than "revolution" (e.g., Figure 1).

What changes and why? These questions are of course the province of historians, few of whom have derived much inspiration from evolutionary theory. What can a Darwinian perspective contribute to their efforts? The successes of neo-Darwinism in reductionistic theorizing about organic ev-

olution inspire an analogous attack on the problem of cultural "evolution," but the interrelated matrix that is "culture" promises to be even more resistant to useful analysis into component "traits" than is the individual phenotype. Whether such theorizing will eventually make an important contribution to historical studies remains to be seen. Where a Darwinian perspective unquestionably *can* contribute to historical studies is by providing a cross-culturally and historically stable basis for theorizing about the things that change, i.e., by providing a model of human nature that identifies the bases of human "interests."

A simple example of the relevance of Darwinism for the study of culture concerns intrasexual competition. To a Darwinian, the morphological and life historical traits of *Homo sapiens* transparently bespeak a history of effective polygyny, and hence lead one to expect a species-characteristic preponderance of male participation in confrontational, violent competition. One might think that such an intuition is so obvious an induction from experience as to require no evolutionary perspective at all, but it is an intuition that many social scientists apparently lack.

"In our culture," writes Marvin Wolfgang, the dean of American criminologists, trying to explain the enormous sex difference in his study of Philadelphia homicides, the female is "less given to or expected to engage in physical violence than the male" (Wolfgang 1958, p. 163); elsewhere, he attributes the same phenomenon to "the theme of masculinity in American culture" (Wolfgang 1978, p. 87). The clear implication is that things are otherwise in other cultures. But they are not.

A Canadian sociologist (Chimbos 1978) who interviewed murderers echoes Wolfgang:

The marked differences of violent crimes . . . between the sexes . . . can be partly explained in terms of cultural conditioning. For example, in North America aggressiveness and physical prowess are important to male development while softness and gentleness are emphasized in female development. (p. 21)

But why invoke North American culture to explain a sex difference that is universal? Ironically, the sex difference in homicidal violence is *smaller* in modern North American than in any other human society yet described (Daly and Wilson 1988)!

Historians seem generally to espouse the same sort of naive cultural determinism as social scientists. James Given (1977), for example, seeks a historically particular explanation for sex differences in thirteenth-century English homicide, and attributes them to "the different social roles that contemporaries expected the sexes to play. The use of violence was regarded as inappropriate for women." Given shows that female killers were more likely than their male counterparts to be hanged, and he considers this fact to be evidence of "the strong social and cultural inhibitions against the use of force by women." These "social and cultural inhibitions" he presumes to have been the causes of the sex difference in violence. But precisely the

same sex difference persists where women are penalized *less* severely than men, e.g., in modern North America (Daly and Wilson 1988). That men kill men far more frequently than women kill women is indeed cross-culturally universal, but no non-Darwinian student of homicide seems to have thought to inquire whether this might be the case. Wolfgang, Chimbo, Given, and many others have wasted their efforts positing culturally and historically particular explanations for a general phenomenon. A Darwinian view of human nature could help students of culture and history to formulate better hypotheses.

Let us return to cultural "evolution." Why do rates of violence vary so hugely between societies, and why do they change gradually and smoothly? No doubt there are important effects of economic, legal, and institutional changes over historical time. The weakening of kinship ties by vassalage, by urbanization, or by industrialization, for example, has eliminated blood revenge and family feuding from most societies. But analyses of the effects of economic and institutional factors upon year-to-year variation in violence have told us remarkably little. Attempts to associate rates of violent crime with economic indices, for example, have yielded mixed and unimpressive results. And even drastic changes in legal penalties have little demonstrable impact on trends (see, e.g., Archer and Gartner 1984, Chapter 6). If we wish to predict the present level of violence in a given society, our estimate will not be greatly improved by knowing the unemployment rate or the modes of production or how Draconian are the laws or even the population's demographic structure; far and away the best available predictor of that present level of violence is the level in the recent past. The likely implication is that present violence is itself a causal determinant of future violence. But how might such causal influence be mediated?

“IMITATIVE VIOLENCE”

Some of the causal impact of present violence upon future violence seems to be achieved through some sort of short-term "contagion," "imitative," or "modelling" effect. This has long been an attractive idea to social psychologists (e.g., Berkowitz and Macauley 1971; Bandura 1973), but the evidence offered in its support has been shaky, consisting mainly of plausibility arguments, anecdotes (some of which were of dubious origin and likely to be apocryphal "urban myths"), and laboratory studies of questionable ecological validity. Recently, however, some ingenious time-series analyses by sociologist David Phillips have given the old idea a fresh empirical basis.

Homicides in the United States are sufficiently numerous—about 60 cases per day—that one can seek transient effects of publicized events upon daily killings. Phillips (1983), for example, found a significant increase in homicides occurring a few days after heavyweight championship boxing matches. Moreover, the magnitude of this increase was significantly higher

for more publicized fights (those whose results were reported on network television newscasts) than for less publicized fights (those not so reported). Most remarkably, deaths of whites, but not blacks, increased significantly after fights with white losers, while the reverse was true after fights with black losers.

The results (and those of similar studies of imitative suicide by Phillips and his colleagues) remain controversial. The much higher rates of homicide on weekends and holidays complicate the analyses; these effects have been "controlled" by statistical procedures whose assumptions are not strictly satisfied. Baron and Reiss (1985a) proposed that Phillips's results are complicated artifacts, and supported their claim with a demonstration of similar but surely meaningless "effects" of prize fights upon short-term fluctuations in daily homicide rates a year later. Phillips and Bollen (1985) countered with further analyses of an enlarged data set, in which elevated homicide rates occurred in the immediate aftermath of a prize fight, but not a year earlier or a year later. Baron and Reiss (1985b) replied with further criticisms: They found, for example, that a different measure of publicity than that used by Phillips, i.e., the Nielsen television audience estimate, was negatively associated with the magnitude of the homicide increase, and they complained that several of Phillips's statistical procedures were arbitrary, ad hoc, or illegitimate.

For the present discussion, the most interesting aspect of this controversy is the conceptual vacuum that it has underscored: Neither Phillips nor anyone else has proposed a specific psychological theory of the alleged "imitation" that can explain what sort of publicized event will produce what sort of imitative violence, by whom, and after what delay (Baron and Reiss 1985a,b). Truly imitative homicides, in which a killer apes the details of a television show or news story, are occasionally reported, but these are far too few to exert a measurable impact upon rates of violence. Phillips's use of the word "imitation" implies, apparently intentionally, that the prize fight effect is somehow the same sort of phenomenon as these rare "copycat" killings. But this is just whistling in the dark.

Most American homicides arise out of social conflicts between acquainted people. The commonest variety involves two unrelated men in a dispute over status or "face" (Wilson and Daly 1985). Many similar but nonfatal disputes occur for every one that ends in death. Both Phillips and his critics, operating from only the scantest statistical data, have unwittingly adopted a naive view of homicide that ignores its social context. Both sides of the controversy assume, for example, that any genuine effect of the losing fighter's race could only imply that the spectacle of a white man losing (or perhaps his being victimized in socially approved violence) specifically inspires observers to set out to kill other whites; likewise for blacks. But one can envision a completely different, and to us more plausible, process involving the well-known dynamic of "victim precipitation" (Wolfgang 1958). This is that young men who are already of a competitive, combative mindset

identify with prizefighters (especially those of their own race), are rendered somewhat more belligerent by the macho banter surrounding the fight (perhaps by the defeat of a hero whose victory they had publicly predicted, perhaps by lost wagers) and are therefore a little likelier to end up dead in an escalated face dispute during those days when the fight is still a popular subject of barroom analysis. To call such a chain of causality "imitation" is to stretch the word beyond recognition.

Killings by the police afford another example of the blinkering effects of the assumption that any positive feedback of violence upon further violence must be "imitative." Phillips (1983) excluded legal killings from his prizefight analysis on the assumption that they would not and should not be subject to the same imitative effects as criminal homicides. Baron and Reiss (1985b) agreed with this logic and then gleefully pointed out that killings by the police also increase after fights, a result that they construed as evidence that any apparent effect of prizefights must be artifactual. Again, both sides in the controversy seem to have a naively idealized view of the causes of homicides, i.e., that whatever processes inspire criminal killings could not conceivably have a similar impact upon the police. In fact, like other American killings, those perpetrated by the police commonly arise out of face-threatening confrontations with belligerent antagonists (see, e.g., Toch 1969). Whatever psychological process mediates the prizefight effect could easily be operating in the policeman or his victim or both.

Our point is that Phillips's ingenious and widely publicized demonstrations of "imitative violence" explain much less than one may at first suppose (see also Baron and Reiss 1985a). Even if it is indeed the case that each highly publicized prize fight causes a brief flurry of homicides, the mechanism of this causal influence is simply not known.

"LEGITIMATION OF VIOLENCE"

A more subtle conception of the causal influence of present violence upon future violence than simple "imitation"—and one seemingly better suited to the explanation of gradual changes of the sort illustrated in Figure 1—is that of the legitimization of violence. When acts of violence occur, and more particularly when at least some such acts are seen to be socially acceptable or even lauded, then general attitudes toward the use of violence shift in the direction of acceptance, and thresholds for resorting to violence fall.

Archer and Gartner (1984) consider some such legitimization process to be the likeliest explanation for postwar increases in homicide rates. Using a large set of data on homicide rates in various countries before, during, and after various wars, these authors have provided the first truly convincing demonstration that such increases are indeed the rule. They then consider various candidate explanations, finding fault with each one other than the "legitimation of violence model." For example, they reject theories that

postwar crime waves are the consequence either of sociopolitical disintegration in defeated nations or of devastated economies, by demonstrating that homicide rates increase (i.e., postwar minus prewar) by at least as much in victorious as in defeated nations and by at least as much in nations with improved postwar economies as in those with worsened economies.

These and other analyses in Archer and Gartner's (1984) volume constitute the most sophisticated and imaginative uses yet made of multinational homicide data. Nevertheless, their interpretations suffer to some degree from the same flaw that bedevils the imitation controversy: inattention to the psychological processes underlying aggregated sociological data. We will provide two illustrations of this shortcoming.

One of the explanations for postwar murder waves that Archer and Gartner find unsatisfactory is the "violent veteran model." This is the notion that "the experience of war resocializes soldiers to be more accepting of violence and more proficient at it," with the result that nations participating in wars experience a postwar increase in violent crimes "as a result of the actions of returning war veterans" (p. 75). The evidence that Archer and Gartner deploy against this model is the fact that there is a postwar increase in killings both by veterans and by nonveterans, and in particular that killings by women and by men increase by comparable factors. But when women kill, their victims are usually men, and they are most typically men who have assaulted them; these men may or may not include disproportionate numbers of combat veterans (nor need combat per se be a necessary element of the "resocializing" of men in the army). Archer and Gartner claim to be testing the proposition that "postwar increases in homicide would be due to the acts of veterans" (p. 91), but by overlooking the possibility of victim precipitation, they actually test a much narrower claim, namely that the prewar-postwar differential must consist of cases in which veterans are themselves the killers.

Another of Archer and Gartner's analyses exhibits similar inattention to the social psychological context of homicide, leading again to an unwarranted conclusion. It so happens that variations in homicide rates can sometimes be related to demographic variations. If, for example, there is a change in the proportion of the population that falls within the most homicidal age-sex class, then the overall homicide rate will change, even though age-sex-class-specific rates remain constant. Archer and Gartner produce the figure that is reproduced here as Figure 2. They conclude that the maturation of a "baby boom" cohort accounts for a large part of the increase in American homicides between 1963 and 1972. But the rise in curve (B) inspires a second conclusion as well: "despite this demographic effect, there were homicide rate increases during this period that cannot be explained by demography alone" (p. 144). The unstated assumption here is that "demography" would constitute an "explanation" only if age- and sex-specific rates of homicide were invariant, and all change were attributable to changing proportions of

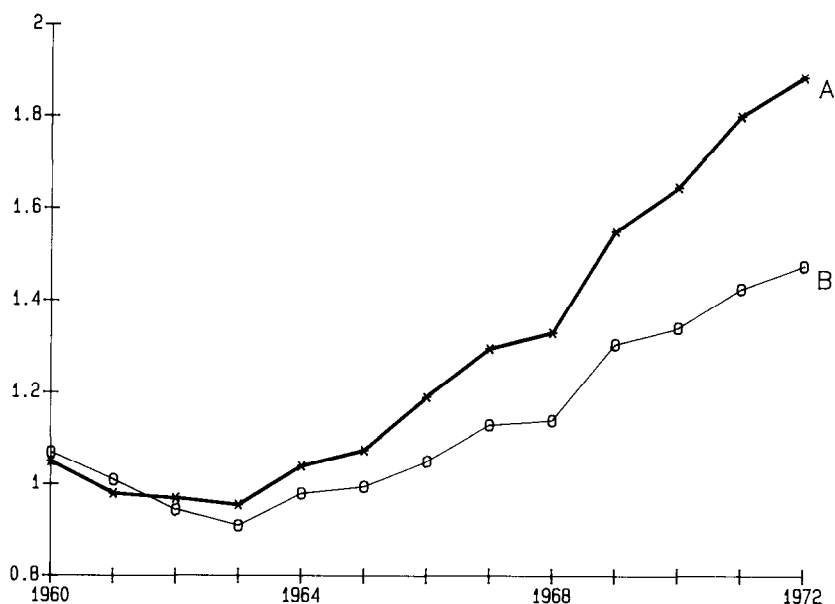


FIGURE 2. United States homicide rates expressed against two different population bases. (A) Homicides/total population, relative to the period 1955–1959. (B) Homicides/population ages 15–29, relative to the period 1955–1959. Adapted from Archer and Gartner (1984), p. 144; copyright Yale University Press; reprinted with permission.

these demographic classes. But by what theory may we expect age-sex-class-specific homicide rates to be unaffected by the demographic mix? The interpersonal violence of young men is a manifestation of competitiveness (Wilson and Daly 1985); if the proportional representation of young men in a population increases, might not the level of competition experienced by *all* men increase? Moreover, if the proportion of men who are in a violence-prone age-class doubles (for example), then even by a simplistic random model, the proportion of all dyadic encounters that will involve *two* such men will *quadruple*; might we not expect a greater than linear increase in violence with each increment in the numbers of violence-prone persons? And how does the number of men in other age-classes affect the perceived marital and economic prospects of young men, and hence their risk-prone, violent tendencies? Our point is simply that “demography alone” in fact “explains” nothing without some individual-level specification of how demographic and other effects upon violence are mediated.

The foregoing does not, of course, refute Archer and Gartner’s concept of the “legitimation of violence,” which remains appealing. Our principal quarrel with the concept is its seeming implication of the assimilation of an arbitrary social value. The socially apprehended *utility* of violence and the perceived *risks* in its use or nonuse are surely more the point.

“SUBCULTURES OF VIOLENCE”

Closely related to the idea of violence having a greater or lesser “legitimacy” is the allegation that there are “subcultures of violence.” The criminologists Wolfgang and Ferracuti (1967) have promoted this concept as an explanation for regional and ethnic differences in the incidence of homicide. Within certain reference groups, violence is frequent, and the display of one’s capacity for violence is admired or even obligatory; other groups within the same larger society condemn violence, and, indeed, they rarely resort to it.

No doubt there is truth to this conception, but its value as explanation is debatable. The cultural differences between groups are manifested primarily by the same behavioral differences that the culture concept is invoked to explain; culture or subculture is often little more than a pseudoexplanatory label. One can attempt to anchor the subculture concept by measuring attitudes and values independently of the violent acts themselves, but this does not really solve the problem: Although behavioral differences are indeed likely to be associated with differences in attitudes and values, there seems little basis for attributing the former to the latter rather than the reverse.

Insofar as the appeal to “culture” entails a genuine hypothesis about the causes of behavioral differences between groups and not just a pseudoexplanatory label, that hypothesis, although rarely explicit, is surely something like this: The present difference is a product of the distinct cumulative histories of the two groups *rather than* a product of present differences in the external forces acting upon the two groups. Thus, a “cultural” difference is generally assumed to be an “arbitrary” difference in the sense that there is no present utilitarian reason for either group’s adoption of the one cultural alternative rather than the other.

It is this implication of the culture concept that has provoked antagonism to Wolfgang and Ferracuti’s “subculture of violence” theory. Critics have maintained that the theory, by attributing violence among black Americans to a black subculture, for example, subtly implies that the social problems of disadvantaged minorities are intrinsically generated rather than being the products of exploitation and economic inopportunity and that it is mere happenstance that the poorer classes in industrial society exhibit more face-to-face violence than the privileged rather than the reverse. Proponents of the subculture theory can fairly protest that they have said no such thing, and yet the criticism has justice. If we think we can explain why poor young men behave violently in terms of the “transmission” of “values” within a “subculture,” then we are unlikely to seek more utilitarian explanations. But, in fact, poor young men with dismal prospects for the future have *good reason* to escalate their tactics of social competition and become violent (Wilson and Daly 1985). To characterize a difference as subcultural is at best a description, not an explanation; when such a description masquerades as explanation, it distracts from the quest for better ones.

THE NEED FOR DARWINIAN PSYCHOLOGY

The "imitative violence," "legitimation of violence," and "subculture of violence" hypotheses all share a certain feature with models of cultural evolution that emphasize transmission processes (e.g., Cavalli-Sforza and Feldman 1981; Boyd and Richerson 1985). All of these approaches treat the culture-bearing person as a more-or-less passive recipient of cultural influences, buffeted by the statistical sum of his experiences, rather than as an active strategist who apprehends his own self-interest and his social milieu, and responds accordingly. These approaches downplay the fact that people are capable of flexible responses to their immediate readings of a social circumstance and its demands (Daly 1982).

Consider, for example, the impact of the local prevalence of violence upon a rational man's readiness to resort to violence. Such a man will be quicker on the trigger in a relatively violent milieu than in a peaceful one, although he brings the same cultural baggage to both. This expected difference in his behavior has little to do with the "legitimacy" of violence, still less with "cultural conditioning," "meme" transmission, or "learning by imitation." The difference reflects differences in the perceived likelihood of life-threatening action by one's antagonists, which in turn influence the expected costs and benefits of one's own options, including that of preemptive attack. Our point is that the positive feedback of violent behavior upon its own frequency, which is often blithely attributed to "imitation" or "contagion" and thereby seemingly explained, may often be mediated by processes quite different than these terms imply. Violence can beget violence, for example, by raising the perceived risk in behaving *nonviolently*.

A satisfactory theory of the causation of variable rates of violence must incorporate a specific account of the ways in which the effective variables influence the individual actors and interpersonal interactions that are the stuff of aggregate statistics. Such theorizing is often scorned as "psychological reductionism" by sociologists. However, without engaging in precisely such reductionism, Phillips *cannot tell* whether his "imitative" theory is supported or refuted by the data on police killings; Archer and Gartner *cannot tell* whether the data in Figure 2 imply nondemographic as well as demographic influences, nor can they tell whether the "violent veteran model" predicts elevated violence by women as well as men; and Wolfgang and Ferracuti *cannot tell* which groups are likely to develop violent subcultures.

No theory of culture will get far without explicit attention to the psychology of the culture-bearing organism. Those who model cultural change as a product of transmission dynamics (e.g., Boyd and Richerson 1985) endorse this assertion, but they consider the relevant psychology to be primarily that of learning (repertoire acquisition) and conformity (imitation). We prefer to argue that what is needed is a Darwinian psychology that uses evolutionary ideas as metatheory for the postulation of cognitive/emotional/

motivational mechanisms and strategies (see Daly 1982; Symons 1987; Cosmides and Tooby 1987). Sexual selection theory, for example, makes sense of the cross-culturally consistent effects of gender and life-stage upon individual risk-proneness and violence (Wilson and Daly 1985); the combination of such theory with game-theoretical models of competitive interactions could provide fresh insights into demographic effects upon rates of violence, of the sort illustrated in Figure 2. The evolutionary concepts of parental investment and parental manipulation provide a satisfying unitary framework for the analysis of culturally variable acts of (and attitudes toward) infanticide and filicide (Daly and Wilson 1984, 1988). The evolutionary model of man as effective "nepotist" clarifies the role of kinship in collaborative killing, in blood revenge, and in wergild, as well as illuminating the historical changes in homicide that accompanied feudalism and later social changes (Daly and Wilson 1988). The "evolved constraints on cultural evolution" are the specific adaptive psychological properties of our species: things like life-historical changes in parental feeling, sexual jealousy in heterosexual transactions, nepotistic discrimination, the desire for social status and respect, equity in vengeance, the sexual attractiveness of the healthy and well formed, attention to the successful, aversion to the putrefying, finding pleasure in genealogical knowledge, and so forth.

Culture is manifested by the relative homogeneity of behavior within a group, as compared to cross-cultural and historical variation. It seems logical to seek the bases of that homogeneity—and hence of culture itself—in imitation and in the replicative transmission of behavioral repertoires. But there are other homogenizing forces too. As we noted earlier, for example, although one's repertoire includes both violent and nonviolent acts, rational self-interest dictates a readier use of violence in a violent society.

Chronic, society-specific levels of violence *are* "culture," and they are surely instantiated in the minds/brains of individuals, but the prevalent model of individuals adopting one or another "cultural alternative" simply fails to capture this kind of cultural phenomenon. Moreover, as we hope we have shown, the idea that within-culture homogeneity is achieved by the transmission of favored alternatives from culture models is at best incomplete. A satisfactory theory of the "cultural evolution" of violence awaits satisfactory theories of how individuals apprehend their interests and how they pursue them.

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