

Comment

A REPLY TO GELLES:
STEPCHILDREN ARE DISPROPORTIONATELY ABUSED,
AND DIVERSE FORMS OF VIOLENCE CAN SHARE
CAUSAL FACTORS

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**DIVERSE FORMS OF VIOLENCE SHARE
CAUSAL FACTORS**

A long-held and traditional assumption in the study of violence, family violence, and especially violence towards children is that a continuum of violence spans from the mildest form of physical punishment to the most severe and deadly forms of violence.

Thus began a recent article in this journal by Gelles (1991:59–60), who went on to repudiate the concept of such a continuum and to argue that distinct forms of violence require distinct explanations. To our surprise, the researchers most lengthily criticized for subscribing to this false notion of continuity were ourselves.

We too are skeptical of approaches that treat the difference between “the mildest form of physical punishment” and “the most severe and deadly forms of violence” merely as a matter of degree. There is no reason to suppose that mild punishment and deadly assault have a

Received February 25, 1991; accepted March 1, 1991.

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Human Nature, Vol. 2, No. 4, pp. 419–426.

1045-6767/91/\$1.00 + .10

significant commonality of motivation, intent, or eliciting circumstance. Mild punishment, for example, can be facilitated by its perpetrator's concern for its recipient's well-being, whereas assault is inhibited by that same concern. The notion of a "continuum of violence" can indeed be misleading if one is so doctrinaire as to deem all spankings instances of a low-level form of child abuse. But one throws out the baby with the bathwater if one overlooks the partial overlap of causal factors among distinct acts. Diversity of social behavior is partly to be understood as a reflection of variation along underlying psychological dimensions, including relationship-specific malevolence or affection.

Gelles concludes that "homicide is a distinct form of behavior that requires a distinct explanation" from other sorts of violence. In a way, this claim does not go far enough: "homicide" incorporates many distinct forms of behavior united by the (often inadvertent) fact of lethality (Daly and Wilson 1988c), so homicide is no more "a distinct form of behavior" than is "violence." But in another way, the claim goes too far: homicides unquestionably manifest many commonalities of causation with sublethal assaults when both are divided into meaningful categories, including commonalities of demographic risk patterns, prevalent motives, facilitating and inhibiting circumstances, and discourse of the antagonists (e.g., Daly et al. 1982; Luckenbill 1984; Pokorny 1965; Toch 1969; Wilson 1989). Sound explanations of lethal and sublethal assaults are certain to overlap considerably, notwithstanding the importance of understanding what distinguishes them; both must deal, for example, with the sources of relationship-specific and situation-specific antagonism.

STEPCHILDREN INCUR MULTIPLE RISKS

Evidence from many countries and many sources (including child welfare agency records, homicide investigations, hospital admissions, cohort studies, ethnographic fieldwork, victimization surveys, and vital statistics) indicates that children living with one genetic parent and one substitute (step-) parent incur vastly greater risks than their counterparts in two-genetic-parent homes. These differential risks include gross mortality, homicide victimization, sexual abuse, physical assaults, various sorts of neglect, illness, and injuries both accidental and nonaccidental (e.g., Creighton 1985; Creighton and Noyes 1989; Daly and Wilson 1981, 1984, 1985, 1988b, 1988c; Fergusson et al. 1972; Ferri 1984; Flinn 1988; Gordon 1989; Gordon and Creighton 1988; Hill and Kaplan 1988; Kim and Ko 1990; Lightcap et al. 1982; Russell 1984; Scott 1973; Voland 1988; Wadsworth et al. 1983; Wallace 1986; Wilson et al. 1980).

Why should steprelationship be associated with greater probability of so many different negative outcomes? Because in spite of all their differences, they share at least one major causal determinant: if parents lack concern for an individual child's well-being, then all manner of risks to that child are likely to increase. *Child-specific parental solicitude* varies among parent-child dyads (and, through time, within dyads), ranging from intense loving concern through indifference to active hostility. Stepparents do not, on average, love their children as much as genetic parents, and they are more likely than genetic parents to feel indifference or hostility instead (e.g., Anderson and White 1986; Duberman 1975; Ferri 1984; Perkins and Kahan 1979; Santrock and Sitterle 1987; White and Booth 1985). The abundant evidence on this point is independent of the evidence of differential risks and has for the most part been collected by researchers oblivious to the Darwinian rationale for predicting this difference. Variable child-specific parental solicitude is one partial determinant of many different sorts of risk. Greater love motivates greater parental protection and vigilance against extrinsic threats, and it inhibits parents from damaging children recklessly or in anger.

Gelles questions both the reality of an overrepresentation of stepchildren among child abuse victims and our rationale for having predicted it. His quarrel with our rationale (which he calls "Daly and Wilson's sociobiological explanation of abuse") is that whereas killing a child might constitute an act of adaptive parental divestment, sublethal abuse "might actually require *more*, not less, parental investment in the abused but surviving offspring" (Gelles 1991:68). "But what," he demands, "of the majority of cases of maltreatment that injure children but do not place them at grave risk of death? How do Daly and Wilson's propositions apply to cases of moderately severe violence toward children?" (Gelles 1991:68).

Gelles's assumption seems to be that if abuse per se is not adaptive (fitness-promoting), then "sociobiological explanation" fails. The assumption is unfounded. Maladaptive behavior requires explanation in terms of the functioning of evolved psychological mechanisms no less than does adaptive behavior; indeed the malfunctions often provide the best clues about the mechanisms (Daly and Wilson 1986). We have stressed that child abuse is evidently maladaptive in material Gelles cites, so we find it odd that in echoing this point he should imply that it represents a novel challenge to our analysis.

What, after all, can most usefully be considered to have evolved by natural selection? Certainly not "child abuse"! . . . Abusive parents commonly persist in inflicting damage upon their wards, while continuing to invest in them. This is hardly an efficient strategy of parental effort allocation (Daly and Wilson 1985:207).

We have elaborated on this point elsewhere:

Some parents chronically abuse children in sublethal ways that simultaneously damage the children's life prospects and raise the investment costs for the abusers themselves. There are no grounds for supposing that such parental behavior can be rationalized as adaptive. It is not. But though the actions in question be maladaptive, evolutionary models of parental motives remain relevant for the prediction and explanation of patterned variation in the incidence of these extreme lapses of parental solicitude (Daly 1989:36; see also Daly and Wilson 1987:302).

Neither have we represented child homicide as an adaptation:

The specific act of infanticide may or may not benefit the actor's fitness, whether in an individual case or on average, but the act need not contribute to fitness for a sociobiological analysis to be illuminating. Infanticide can be viewed as one (rare) manifestation of variations in more abstract motivational states such as child-specific parental love and solicitude. Adaptation may then be sought at the level of these more abstract states (Daly and Wilson 1984:487-488).

Evolved social adaptations can only be characterized at a psychological level of description (Symons 1987:136); excessive reliance on behavioral acts as the units of analysis leads to such absurdities as the conflation of aggressive shoving with shoving someone out of harm's way (e.g., Straus and Gelles 1990). The behavior of child abuse does not exhibit the requisite design features to be deemed an evolved adaptation; the psychological mechanism of discriminative child-specific parental solicitude does (Daly and Wilson 1988a; Wilson and Daly 1987).

Gelles's empirical challenge to the reality of elevated risk in step-families depends on two studies: an unpublished analysis of American Humane Association child abuse archives (Malkin and Lamb 1989), and a telephone survey (Gelles and Harrop 1991). He presents the former in these words:

it is plausible that . . . the official report data used by Daly and Wilson are flawed and biased. Wilson, Daly, and Weghorst's (1980) analysis of the American Humane Association data uses household composition as the unit of analysis and not relationship of the perpetrator to the child. Malkin and Lamb's (1989) analysis of the 1984 American Humane Association data, which examines the relationship between perpetrator and child, failed to find that stepparents were more likely to abuse their offspring than biological parents (Gelles 1991:66).

This characterization is incorrect on all counts. Malkin and Lamb (1989) made no assessment of rates of abuse by stepparents vs genetic parents. It is clear from the absolute numbers of victims, however, that had they performed these analyses, they would have replicated our

results; in fact, the overrepresentation of stepparent-plus-genetic-parent families as compared to two-genetic-parent families is even more extreme in the 1984 data set analyzed by Malkin and Lamb than in the 1976 data set analyzed by Wilson et al. (1980). Gelles also errs in suggesting that comparing step- vs genetic parents as perpetrators of abuse abolishes the differences obtained when one compares household compositions; in fact, the perpetrator analysis produces even larger differences, in both data sets. We prefer the more conservative household composition analysis because household type is a more reliable datum in the AHA archives than is perpetrator identity (Daly and Wilson 1981).

The Gelles and Harrop (1991) survey is thus the only empirical study cited by Gelles (1991) or known to us in which (as Gelles describes the result) “non-genetically related caretakers were no more likely than biological parents to exhibit mild, severe, or very severe violence toward their children” (pp. 65–66). An alternative characterization of this finding is that when telephoned by a stranger and asked whether they have committed various assaultive acts against their children within the past year, stepparents were no more likely than genetic parents to profess to have done so. Gelles feels that this single exceptional result is unusually likely to be valid because it used a “national probability sample” rather than “clinical” cases. Others may feel otherwise.

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