

Dedicated to the memory of William Donald Hamilton

# An assessment of some proposed exceptions to the phenomenon of nepotistic discrimination against stepchildren

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Stepparents commit child abuse and homicide at much higher rates than genetic parents. Proposed exceptions, including a recent claim that there is no such “Cinderella effect” in Swedish homicides, are shown to be mistaken. The hypothesis that only “mothers’ boyfriends” abuse children excessively, whereas married stepfathers do not, is tested and rejected in an analysis of Canadian homicides. *De facto* marriage and steprelationship are confounded, but each is a major risk factor when the other is controlled. Abuse is a rare and presumably non-adaptive manifestation of discrimination, but recent research confirms that stepchildren are more generally disadvantaged with respect to positive investments. There are no known exceptions to the ubiquitous phenomenon of parents discriminating, on average, against stepchildren, but there is cross-national variation in the magnitude of these effects, and the determinants of this variability warrant investigation.

“Doubtless I was seeing these problems from a somewhat different perspective when I was twenty-seven, but one thing has not changed — this is my dislike for the idea that my own behaviour or behaviour of my friends illustrates my own theory of sociality or any other. I like always to imagine that I and we are above all that, subject to far more mysterious laws. In this prejudice, however, I seem, rather sadly, to have been losing more ground than I gain.”

W. D. Hamilton (1995), p. 2

## The Cinderella effect: discriminative mistreatment of stepchildren

William Donald Hamilton, whose legacy we commemorate, was the principal architect of the modern view that the evolved social psychology of any species should be comprehensible as nepotistic by design, that is, as possessing functional organisation for promoting a focal individual's inclusive fitness in environments sufficiently like those in which its ancestors evolved. More than twenty years ago, this Hamiltonian perspective inspired us to wonder how human stepparental care compares with that provided by (putative) genetic parents, and whether stepchildren might be disproportionately mistreated (Daly & Wilson 1980). To our surprise, child abuse researchers had never addressed this question, so we undertook to investigate it ourselves (Daly & Wilson 1998). What we found is that stepchildren in Canada, Great Britain, and the United States indeed incur a greatly elevated risk of child maltreatment of various sorts, especially lethal beatings (Wilson *et al.* 1980, Daly & Wilson 1985, 1988a, 1988b, 1994).

Might this "Cinderella effect", although large and consistent, nevertheless be artifactual? For example, a high incidence of abusive stepfamilies could, in principle, be a spurious result of biased detection or reporting. However, this hypothesis cannot account for a recurrent feature of the data: stepparental overrepresentation does not shrink, but actually increases, as one's criterion of child maltreatment becomes more extreme and unequivocal. Other artifact hypotheses invoke statistical "confounds": variables hypothesized to be genuine risk factors for child abuse while incidentally associated with steprelationship. One such hypothesis is that excess abuse in stepfamilies might be an artifact of economic differences between family types, but this has proven not to be the case (Bachrach 1983, Daly & Wilson 1985, 1988b, 1994, Creighton & Noyes 1989). Other confound hypotheses that have been tested and rejected include the idea that the differences between stepfamilies and genetic parent families might be byproducts of differences in parental age and family size; such differences are in fact small and unimportant

(Daly & Wilson 1985).

Another class of confound hypotheses concerns the "personality traits" of stepfamily members. Suppose, for example, that the population of adults in stepfamilies includes unusually large numbers of disturbed, violent or otherwise abuse-prone people. This situation would elevate victimization rates among those living in (or encountering) such families, regardless of how victims and assailants were related. Such biases may exist, but one line of evidence speaks against their relevance for explaining the Cinderella effect: abusive stepparents seldom harm their own children. In a study of abusive families in the rural United States, for example, only the stepchildren were abused in every one of 10 homes containing both stepchildren and children of the current marital union (Lightcap *et al.* 1982); similarly, in Canadian urban samples, the stepchildren were selectively abused in 9 of 10 such cases in one city (Daly & Wilson 1985), and in 19 of 22 in another (Rodney 1999). This tendency for stepchildren to be discriminatively targeted is especially striking in light of the following additional facts: (1) when child abuse is detected, it is often found that all the children in the home have been victimized; and (2) stepchildren are almost always the eldest children in the home whereas the general (albeit slight) tendency in families of uniform parentage is for the youngest to be the most frequent victims (Rodney 1999).

In sum, there is a large overrepresentation of stepchildren among child abuse victims, especially those violently slain, and the evidence to date suggests that steprelationship *per se* is the relevant risk factor, rather than some correlate thereof. Moreover, this Cinderella effect is clearly not confined to Canada, Britain and the United States. Since our initial studies, for example, other researchers have reported that children incurred excess risk of various sorts of mistreatment and/or mortality at the hands of stepparents among Ache hunter-gatherers (Hill & Kaplan 1988) and in Australia (Wallace 1986, Fleming *et al.* 1997), Colombia (Klevens *et al.* 2000), Finland (Sariola & Uutela 1996), Korea (Kim & Ko 1990), Malaysia (Kassim & Kasim 1995), and Trinidad (Flinn 1988).

### Three alleged failures to replicate the Cinderella effect

Three published studies have been presented by their authors as failures to replicate our finding that stepchildren are relatively often abused. However, each of the three had serious methodological flaws, such that none of them provides even a local exception.

In the first case, Gelles and Harrop (1991) estimated the rates at which U.S. stepparents and genetic parents assault children on the basis of voluntary disclosures to telephone interviewers, who called and asked respondents a series of questions such as whether they had “slapped” certain family members (considered one by one) within the last year, had “punched” them, had “used a knife or gun on” them, and so forth, when they “had a disagreement or were angry with them”. Unsurprisingly, the 117 stepparents who agreed to complete the interview were no more likely to profess to have assaulted the children under their care than were genetic parents. Exhibiting no concern about the validity of such data, Gelles and Harrop asserted that because their study was based on a “large nationally representative sample,” it constituted the first test of differential abuse rates “that has met the normal standards of social scientific evidence.” Sadly, this may be so, but there are obvious grounds for doubting Gelles and Harrop’s assumptions that (1) self-selection for interview was unbiased with respect to the relevant behaviour, and (2) the telephone interviewees spoke the truth. In this regard it is perhaps noteworthy that in another interview study, U.S. stepparents did admit to striking the children substantially more often than genetic parents when the question was framed with a more defensible rationale of “discipline” rather than with respect to being “angry” (see Hashima & Amato 1994).

The second alleged counter-demonstration was presented by Malkin and Lamb (1994), who analyzed an archive of US child abuse reports maintained by the American Humane Association and concluded that “biological parents were more rather than less likely than nonbiological parents to abuse severely and to kill rather than cause major physical injuries to their children. These findings thus failed to replicate previous

findings about the risks associated with step-parenthood” (p. 129). Malkin and Lamb went on to speculate that our prior findings of higher abuse rates by stepparents in data from the same source might somehow be an artifact of analysis at the household level. Curiously, however, the analyses by Malkin and Lamb consisted entirely of cross-tabulations within the abuse cases (hence the confusing “rather than” construction quoted above); no estimates of abuse rates at the hands of stepparents or genetic parents were even attempted. In fact, in the data archive that Malkin and Lamb analyzed, 39% of the abuse victims who resided with “two parents” had a stepparent, compared to an expected value for a same-age sample of US children of less than 5%, and most of the identified abusers in those homes were indeed the stepparents; according to the data in this archive, *every* form of abuse was perpetrated at massively higher rates by stepparents than by genetic parents.

The third alleged failure to replicate was provided by Temrin *et al.* (2000) who analyzed Swedish national data on child homicide victimization and summarized their findings as follows: “In contrast to the Canadian data, children in Sweden living with a step-parent were not at an increased risk compared with children living together with two parents to whom they were genetically related. In addition, there were no other indications that step-parents are overrepresented as offenders” (abstract). Unfortunately, these conclusions are predicated on an analytic error. As in prior analyses demonstrating excess numbers of stepparents among child killers in Canada, the United States, and Great Britain (Daly & Wilson 1988a, 1988b, 1994), Temrin *et al.* (2000) used the numbers of children living in various situations in the population at large as a basis for computing homicide rates in different household types and by different categories of perpetrators. Unlike prior analyses, however, Temrin *et al.* computed these rates without regard for the fact that the proportion of children who reside with a stepparent is near zero at birth and increases steadily with age. Their erroneous affirmation of the null hypothesis derives from this oversight.

The appropriate measure by which to compare homicide risk from stepparents versus ge-

netic parents is the homicide rate per million coresident parent-child dyads per annum, for each age category of children (Daly & Wilson 1988a). Temrin *et al.* tabulated only the information for all children aged 0–15, ignoring the fact that the average child in the population at large was substantially older (and therefore more likely to have had time to acquire a stepparent) than the average homicide victim. In the paper's text, however, Temrin *et al.* noted that within the narrower age range of 1 to 4 years, 53 children were killed by genetic parents and 4 by stepparents, and in response to our request, Dr. Temrin has supplied information on the family situations of 1–4 year olds in the Swedish population-at-large: 1.67% lived with a genetic parent and a stepparent, 10.81% with a lone genetic parent, 86.88% with two genetic parents, and 0.64% with neither genetic parent. From these frequencies, we can compute that 99.11% of step- or genetic parent-child dyads were in fact genetic relationships, while just 0.89% were steprelationships. The 53 genetic child victims represent a homicide rate of 3.8 per million parent-child dyads per annum, while the 4 stepchild victims represent a homicide rate of 31.7 per million stepparent-child dyads per annum. A binomial test of the chance that 4 (or more) of 57 randomly selected cases would have an attribute with a base rate of 0.89% yields  $p < 0.002$ . Thus, Temrin *et al.*'s conclusion that there were no "indications that step-parents are overrepresented as offenders" (abstract) is mistaken.

In addition, Temrin *et al.* assert that "The Swedish material is also very different in that there is no bias towards very young children being killed" in stepfamilies (p. 945). This too is clearly false: the rate of 31.7 slain stepchildren per million stepparent-child dyads per annum for those aged 1–4 is more than 20 times higher than the corresponding rate for those aged 5–15.

### Stepfathers or mothers' "Boyfriends"?

In our published research on child abuse and murder, we have defined steprelationship without regard to marital registration: one is deemed

a stepparent if one cohabits in a registered or *de facto* ("commonlaw") marital union with the focal child's (putative) genetic parent. This raises the question of whether the Cinderella effect might be due primarily, or even solely, to the actions of relatively uncommitted "mothers' boyfriends", rather than those "real stepfathers" who commit themselves to stepfamily life by formalizing the marriage. (This proposition has been advanced in a personal communication to us by Professor Mary Ann Mason of the University of California.) In this regard, it is worth stressing that our published risk estimates for "stepfathers" exclude all assaults and murders perpetrated by "mothers' boyfriends" who were *not* co-residing with their victims. Thus, the question to be resolved is that of excess risk at the hands of *de facto* versus registered-marriage stepfathers living with the children and their mother, in comparison to the risk from their respective genetic father counterparts.

Gordon and Creighton (1988) noted that whereas "non-natal fathers" were greatly overrepresented as perpetrators of sexual abuse in Britain, the rates for married stepfathers were actually closer to those for genetic fathers than to the much higher values for unmarried "father substitutes", which they attributed to "a more profound involvement in the household than is true of [unmarried] father substitutes and a greater commitment to the father role" (p. 104), concluding that "for non-natal fathers marriage appears to be associated with a greater commitment to the father role" (p. 105). However, as Whelan (1994) then commented, "With regard to this last observation it would seem logical to ask, could not the same be said of natal fathers?" (p. 26). In other words, step- versus genetic parenthood and registered versus *de facto* marriage must both be coded and their effects must be unconfounded before we can say whether these two variables interact, as Gordon and Creighton imply and as Mason assumes in proposing that "real" (i.e., married) stepfathers impose no risk.

To address this issue, Whelan (1994) presented the proportions of British children dwelling in various "parental situations" in samples of child abuse victims, homicide victims, and the population at large. The results implied that both

variables are major risk factors: married *and* unmarried stepfathers were both much more likely to abuse or kill than their “natal father” counterparts, and, to an even greater degree, unmarried step- *and* “natal” fathers were both much more likely to abuse or kill than their married counterparts. Whelan concludes that “It becomes increasingly difficult to accept the old mantra that marriage is ‘just a piece of paper’. It clearly has a protective value of its own for children in the household” (p. 29). Leaving aside the question of whether this is really evidence for marriage’s “protective value”, however, Whelan’s analysis suffers from small Ns (just 23 homicide victims in two-“parent” households) and a lack of statistical tests, as well as from inadequate consideration of the fact that the victim groups were younger, on average, than the population sample with which they were compared, a factor which should have led Whelan (like Temrin *et al.* 2000) to underestimate excess risk from stepfathers.

Our Canadian child homicide data provide an opportunity for a somewhat better test of the same issues. Daly and Wilson (1994) confined analysis to killings of children under 5 years of age, both to reduce the complications engendered by age-related change in the distribution of family circumstance and to eliminate any possibility of mutual combat or self-defensive

killings. Here, we analyze the same 129 lethal beatings summarized in Daly and Wilson (1994)’s table 1, which were all those known to have been committed by Canadian “fathers” in 1974–1990: 74 pre-school children beaten to death by their putative genetic fathers (representing 2.6 deaths per million same-age children living with their fathers per annum) and 55 beaten to death by stepfathers (321.6 per million same-age children living with stepfathers per annum). Data on these homicides, including the perpetrators’ current marital status and relationship to the victim, were obtained from Statistics Canada’s “Homicide Survey”, an archive of case data for all homicides investigated by Canadian police; estimates of household compositions in relation to marital status in the Canadian population-at-large over the same time period were derived from census data and General Social Surveys. The results are presented in Table 1.

These Canadian data, like Whelan’s British data, indicate that steprelationship and marital status are both relevant to the risk of lethal beatings. Neither variable’s influence can be explained away as an artifact of the other’s: despite the small Ns, stepfathers are significantly overrepresented as killers within both registered and *de facto* unions considered separately, and *de facto* fathers are significantly overrepresented within both genetic and stepfathers con-

**Table 1.** Numbers and rates at which Canadian children under five years of age were beaten to death by genetic fathers versus stepfathers in 1974–1990, in relation to whether the killer was married to the victim’s mother or cohabited with her in a *de facto* (commonlaw) union. Numbers of same-age children in the population at large (Row 1) were derived from Canadian census data, and were allocated among co-residing “father” types in proportion to Canadian “General Social Survey” data. Victim numbers, their relationships to their killers, and the killers’ marital statuses (Row 2) were obtained from case data in Statistics Canada’s “Homicide Survey”.

	Genetic fathers			Stepfathers		
	Total	Registered marriages	<i>De facto</i> marriages	Total	Registered marriages	<i>De facto</i> marriages
Average annual number of coresiding “father”-child pairs in the population at large (thousands)	1665	1615	50	10	5	5
Children beaten to death by “fathers” (total, 1974–1990)	74	48	26	55	6	49
Beating death rate (per million such “father”-child pairs per annum)	2.6	1.8	30.6	321.6	70.6	576.5

sidered separately ( $p < 0.0001$  by binomial test for all four comparisons).

The two independent variables are not orthogonal, however. The overall step- versus genetic father odds ratio of 123.0 is substantially greater than that for either registered marriage fathers (40.3) or *de facto* fathers (18.8) considered separately, because these two “independent” variables are highly associated: about half of all Canadian pre-school children living with *de facto* couples are children of former unions, compared to just 3% of those living with registered-marriage couples (see Table 1, row 1). One could thus conclude that the dramatic overall odds ratio of 123.0 is misleading, because it exploits differential risk associated with the type of marital union. However, this reasoning cuts both ways: the overall odds ratio by which *de facto* unions are riskier than registered marriages (40.9) exceeds the corresponding odds ratios for stepfathers (8.2) and genetic fathers (17.5) considered separately, and one might equally well say that the 40.9 value is misleading because it exploits differential risk associated with step- versus genetic relationship. But however we look at it, each of the two confounded variables is predictive of risk when the other is controlled, and step- versus genetic relationship is the more powerful predictor of the two, at least in these Canadian data. A further, rather obvious, conjecture is that the main reason *why* so many stepchildren live with couples who have not registered their marital unions is because men are reluctant to assume full parental responsibility for stepchildren.

### Why has there been so much controversy about the Cinderella effect?

Despite obvious rationales, both from Hamiltonian theory and from folk knowledge, for predicting that stepchildren might be at risk of abuse, and despite abundant evidence that this prediction is upheld, the Cinderella effect has encountered a steady stream of incredulity and indignation for more than 20 years. As Daly and Wilson (1998) summarized the situation, “There is something about the association between step-

parenthood and child maltreatment that appears to be uniquely unpalatable, and we have witnessed some curious attempts to make it vanish” (p. 48).

An interesting aspect of the three alleged failures to replicate the Cinderella effect that were discussed on p. 289–290 is that each was presented by its authors not merely as a local null result, showing that the effect is of limited generality, but as reason to doubt that it exists at all, anywhere. Gelles and Harrop (1991) maintained that all prior studies were biased by their reliance on “official report data”, and that only their telephone survey could be considered “free” of bias. Malkin and Lamb (1994) proposed, more modestly but no more persuasively, that alleged differences between their results and those of Wilson *et al.* (1980) might result from some unexplained artifact engendered by our focus on household composition rather than individual perpetrators, implying that the whole phenomenon might be illusory. Finally, in a popular report on the Temrin *et al.* findings (Motluk 2000), one of its co-authors, Magnus Enquist, is quoted as explaining that the study was conducted because the Swedish researchers found the Canadian evidence “suspicious”, and near the end of their paper, Temrin *et al.* (2000) write, “Daly and Wilson (1988b) have found an overrepresentation of stepfathers in studies of child abuse in North America. Confounding variables are, however, at hand and have not been thoroughly investigated” (p. 945). The intent is clearly to arouse doubt that steprelationship has been persuasively linked to child maltreatment anywhere, but although we are glad to agree that hypotheses about possible confounds should be addressed more “thoroughly,” Temrin *et al.*’s remarks are highly misleading. In the first place, they documented no confounds, although they implied that psychiatric illness and drug abuse may be more prevalent in stepfamilies than in genetic parent families. Secondly, they ignore the fact that such factors cannot explain why abusive stepparents in North American families of mixed parentage are usually selective in whom they target, as discussed above. Thirdly, in raising the issue of confounds, Temrin *et al.* provide no indication that several confound hypotheses have been

articulated, tested, and rejected in papers that they cite (*see* page 288).

Antipathy to the facts about stepparental abuse is apparently behind a widespread readiness to embrace even the shakiest of counterclaims. In preparing guidelines to help physicians detect child abuse, for example, the American Medical Association endorsed Gelles and Harrop's (1991) claim that their telephone survey provided better evidence than children's injuries and deaths, and therefore omitted stepparenthood, the best available risk marker, from their check list (*see* Daly & Wilson 1998: 53–55). A more recent example lays bare the misplaced ideological underpinnings of its authors' repulsion: writing in the journal that is circulated to more clinical psychologists than any other, Silverstein and Auerbach (1999) asserted that Malkin and Lamb's (1994) research, plus additional evidence that the absolute numbers (sic) of abusive stepfathers and genetic fathers are about equal, had disproved "the neoconservative contention that stepfathers or mothers' boy-friends abuse children more frequently than biological fathers (and mothers)" (p. 402). It appears that because the facts have been linked to "biology", which is in turn assumed to be the handmaiden of conservative politics, the facts themselves have become tainted.

One might hope for better from biologists themselves, but many dislike the notion that an evolutionary perspective could shed light on human social psychology and behaviour. Temrin *et al.*'s (2000) title "Step-parents and infanticide: new data contradict evolutionary predictions" provides an apparent example. Here and in their discussion, these authors imply that an "evolutionary" approach provides a unique, falsifiable prediction that can be pitted against alternatives such as "culture". In reality, of course, culture is not an alternative to evolution, and there is no single privileged "evolutionary prediction" in a case such as this; if data "contradict" some *particular* model of the evolved human psyche, what is required is a *better* model of the evolved human psyche. Some biologists have failed to grasp the idea of non-adaptive byproducts of evolved adaptations, and have thus insisted that an evolutionary explanation of the Cinderella effect could be valid only

if stepparental abuse actually promoted fitness (Lenington 1981), or only if *all* stepparents were killers (Rose 1999)! Dawkins (1982) noted that it is a common misconception that explanations in terms of evolution by selection are more "deterministic" than the a-Darwinian causal claims preferred by most social scientists, and we suggest that this too may be relevant. Although W. D. Hamilton was a great supporter of evolutionary approaches to the study of human behaviour and served as the first President of the *Human Behavior & Evolution Society*, even he seems to have been ambivalent about the enterprise. The quotation with which we began this article appears to express a longing for human action to remain forever inexplicable and hence free.

## The way forward

In correcting their errors, we are not suggesting that what Temrin *et al.* (2000) call "cultural factors" are unimportant, nor that their Swedish data are unexceptional. Sweden has a much lower child homicide rate (and a lower overall homicide rate) than other countries in which these matters have been studied, and the odds ratio of risk at the hands of stepparents versus genetic parents (8.4 for children aged 1–4, according to our calculations on page 290) is lower, too. Temrin *et al.* (2000) are certainly correct in proposing that such cross-national differences warrant further scrutiny. They err, however, when they presume (on no explicit basis) that the existence of cross-national variability and the finding that a substantial proportion of Swedish child-killers had psychiatric histories somehow repudiate our Hamiltonian approach to discriminative parental solicitude and homicide. Instead, on the basis of our writings, they could actually have *predicted* the observed departures of the Swedish data from those reported previously, for two reasons. First, as Daly and Wilson (1988b) documented at length, wherever the homicide rate is relatively low, the proportion of cases that involve genetic relatives and the proportion of killers who are psychiatrically disturbed both tend to be relatively high, reflecting the relative invariance of

the rates of these “abnormal” homicides compared to other types. Secondly, and of greater interest with respect to the implications of the Swedish findings for child protection, it may well be the case that the modern Swedish welfare state provides a social climate in which stepparents do not experience, and thus do not resent, heavy pseudoparental obligation. Daly and Wilson (1994) suggested that the reason why excess risk at the hands of stepparents is maximal for the youngest children may be because “stepparents of very young children incur the greatest social pressure, from mates and others, to feel and act like genetic parents, a pressure they often resist and resent, sometimes violently” (p. 208). In both Canada and Great Britain, stepfathers are vastly more likely than genetic fathers to beat children to death in anger, but they are not demonstrably more likely to kill them in other ways that do not so clearly bespeak victim-directed hostility (Daly & Wilson 1994). If Sweden is a country in which such lethal rages are virtually non-existent, that is indeed interesting.

Of course, even if Temrin *et al.*'s (2000) erroneous claim that Swedish stepfathers are not overrepresented as homicide offenders had been correct, one could hardly infer that Swedes treat their stepchildren as they do their genetic offspring. Where lethal beatings are almost non-existent, they are simply the wrong “assay” for assessing such discrimination. Other assays that might better address the question of whether discrimination against stepchildren is absent or exceptionally low in Sweden include sexual abuse (e.g. Russell 1984, Sariola & Uutela 1996, Klevens *et al.* 2000), measures of direct positive investments in the children's welfare (e.g. Anderson *et al.* 1999a, 1999b, Zvoch 1999, Case *et al.* 2000, Jaakkola & Sääntti 2000, Case & Paxson 2001), measures of chronic stress (Flinn & England 1995, Flinn *et al.* 1996), and direct behavioural observations (e.g. Flinn 1988, Marlowe 1999).

There is abundant evidence, reviewed elsewhere (e.g. Wilson & Daly 1987) that stepparents do not love their wards as much as genetic parents, on average. They are therefore more likely to resent them, to withhold investment from them, to exploit them sexually, to strike

them in anger, and so forth. In other words, excess risk to stepchildren is a predictable by-product of the fact that costly parental care can be parasitized and that parental solicitude has therefore evolved to be individualized and preferentially directed to one's own children. The abuse or murder of human stepchildren is certainly not an adaptation analogous to the “sexually selected infanticide” exhibited by some other species, for it fails the tests expounded by Williams (1966) and others: it is performed irregularly and inefficiently, often at great cost to the perpetrator, and it yields no demonstrated benefits (Daly & Wilson 1985, 1991). Instead, the human animal is one of many in which stepparents invest in their predecessors' young as a form of “mating effort” (Flinn 1988, Rohwer *et al.* 1999). As Flinn *et al.* (1999) have aptly remarked, stepchildren “are not simply unrelated parasites; they are a special kind of relative by marriage” (p. 467).

The fact that stepparental investment is normal and adaptive in a given species does not provide a theoretical basis for supposing that it will be as wholehearted as that provided by genetic parents, however, and this topic provides one case in which evolution-minded study of human social behaviour is ahead of, and should inspire, research on other species. We expect even investing stepparents to exhibit subtle discriminations, since step-offspring must only very rarely be as valuable to a parental investor's expected fitness as its own offspring would be, but this prediction has not yet been tested in any of the many nonhuman species in which stepparental investment is a regular form of mating effort (Rohwer *et al.* 1999). It has, however, been extensively tested and confirmed in a variety of human societies.

Flinn's (1988) observational study in Trinidad was the first to show that men provide less nurturant attention to stepchildren than to their genetic offspring and that the differences in how they treat the two groups of children are attenuated in the mother's presence. Marlowe (1999) made similar, although less extensive, observations among Hadza foragers in Tanzania. Anderson *et al.* (1999a, 1999b) have shown that both current co-residence and step- versus genetic parenthood influence fathers' investments of time

and money, in the United States and South Africa, and recent analyses of large U.S. national probability sample surveys have demonstrated both that stepparents of both sexes are discriminative, and that this differential treatment has appreciable impacts on the children's lives (Zvoch 1999, Case *et al.* 2000, 2001, Case & Paxson 2001).

We have long contended that in looking at violence as a relatively rare "reverse assay" of discriminative parental solicitude, we were studying the tails of the overlapping but very different distributions of step- versus genetic parental solicitude, and that measuring positive assays across a broader and potentially more adaptive range of behaviours would reveal that discrimination against stepchildren is a much more general phenomenon. The above-cited research by other investigators now provides abundant confirmation of this prediction.

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