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## Is the "Cinderella Effect" Controversial?

### *A Case Study of Evolution-Minded Research and Critiques Thereof*

MARTIN DALY AND MARGO WILSON

Skepticism is a useful faculty for any scientist, and one that the social sciences afford their practitioners abundant opportunities to exercise. However, the work of evolutionary psychologists has elicited more than its share of skepticism, for reasons that seem to have little to do with the quality of either the theorizing that motivated the researchers' hypotheses or their evidence. An example is provided by critical reactions to our own research on the "Cinderella effect."

The phenomenon at issue is parental discrimination against stepchildren, relative to how parents treat their birth children. It is manifest both in reduced levels of investment in stepchildren and in elevated rates of mistreatment, up to the extreme of lethal abuse. Given the ubiquity of abused stepchildren in folklore and the pervasive negative stereotyping of stepparents (e.g., Fine, 1986), any child-abuse researcher might have wondered whether steprelationship is a genuine risk factor, but in fact, those whose imaginations were uninformed by Darwinism never thought to ask. We conducted the first comparison of abuse rates in stepfamilies versus intact birth families, and the difference turned out to be large (Wilson, Daly, & Weghorst, 1980).

Abundant confirmatory research has followed, such that the disproportionate victimization of stepchildren is now the most extensively documented generalization in the family violence literature. The establishment of this epidemiological fact has of course raised further questions, such as what explains variability in the magnitude of Cinderella effects between maltreatment types and locales, and whether the individual-level predictors of abuse are the same for fathers, mothers, stepfathers, and stepmothers. Unfortunately, progress on these important issues has been hindered

by a relentless distraction: the manufacture of "controversy" about whether Cinderella effects exist at all. We suspect that the reason for this nay-saying resides largely, though not entirely, in antipathy to the Darwinian worldview and/or its application to the study of *Homo sapiens*, but before getting into that, we should explain our rationale for investigating this issue and review some relevant evidence.

Why would a Darwinian hypothesize that stepchildren might be discriminated against? Hamilton's (1964) inclusive fitness theory suggests that the social inclinations of organisms can be understood as "nepotistic": because natural selection favors that which promotes the proliferation of a focal individual's genes in competition with their alleles, it favors actions that contribute to the production, well-being, and eventual reproduction of the actor's genetic relatives. If the psychological underpinnings of parental care have evolved by natural selection, we may thus anticipate that parental feeling and action will not typically be elicited by just any conspecific juvenile. Instead, care-providing animals may be expected to care selectively for young who are (a) their own genetic offspring, and (b) able to convert that care into improved prospects for survival and reproduction. This is the kernel of the theory of *discriminative parental solicitude*, which (notwithstanding some interesting twists and *caveats*) has been abundantly verified in a broad range of care-giving species (Clutton-Brock, 1991; Daly & Wilson, 1980, 1988a, 1995).

From an evolutionary perspective, care of young other than the caretaker's own requires explanation. In nonhuman animals, such adoptions can usually be understood as failures of discrimination, either because encounters with unrelated conspecific young were not regular features of the species' environment of evolutionary adaptedness ("EEA"; e.g., Birkhead, 1978) or because that EEA was characterized by an "evolutionary arms race" between discriminative parents and those (of the same or other species) who could gain fitness by overcoming parents' evolved defenses and parasitizing their efforts (e.g., Davies & Brooke, 1991; Yom-Tov, 1980).

Even before Hamilton, astute animal behaviorists had twigged both the Darwinian rationale for expecting parents to care selectively for their own young and the idea that exceptions to this rule warrant EEA-based explanations. Here, for example, is Lack (1943) on the subject:

The comparative failure of the gull to recognize its eggs and the robin to recognize its young is related to the ways of life of the two species. There is considerable survival value to the robin but none to the gull in detecting egg-substitutes, since the robin is parasitized by the cuckoo, but in nature strange egg-like objects are unlikely to get into the gull's nest, and it does not usually matter if they do. On the other hand, while owing to the territorial system young robins rarely come into contact with the young of another pair, this frequently happens to young gulls in the crowded gullery, hence there is considerable survival value to the gull but not the robin in distinguishing its own young. (pp. 92-93)<sup>1</sup>

In the human case, adoption by unrelated persons is a recent cultural invention rather than a recurrent aspect of ancestral environments, and it was probably not a significant feature of the human EEA (see Silk, 1990). However, stepparenting is cross-culturally ubiquitous and almost certainly ancient. It is furthermore not peculiar to human beings, and its distribution in the animal kingdom suggests a reason for its existence: helping raise a new mate's young of prior unions is a component of "mating effort" in species in which suitable mates are scarce and couples often stay together for more than a single breeding attempt (Rohwer, Herron, & Daly, 1999). Thus, pseudoparental care of a predecessor's offspring can be favored by selection.

However, although stepparental investment is not, after all, an evolutionary anomaly, a stepchild must rarely have been as valuable to a stepparent's expected fitness as a child of one's own

<sup>1</sup> Lack was clearly aware that the issue is not the literal "survival" of the behaving organism, but the persistence of the trait (parental discrimination in this case) and its heritable basis over generations. "Survival value" is, in this passage, a euphemism for selective advantage.

would be, and we may therefore anticipate that stepparents will not, in general, feel such wholehearted, self-sacrificial love for their wards as genetic parents so often do. It is on these grounds that we hypothesized, many years ago, that all sorts of abuse and exploitation would occur at higher rates in steprelationships than in genetic parent-child relationships, and that differences between family types would persist when possible confounds such as socio-economic status were controlled (Daly & Wilson, 1980, 1998). Note that this is a "by-product" hypothesis: discriminative parental solicitude is the hypothesized adaptation, not child abuse. It is implausible that abusing or killing stepchildren would have promoted assailants' fitness in the human EEA, but a general preference for their own offspring surely would have.

### THE EVIDENCE

Fatal batterings of small children exhibit differences of the greatest magnitude: in several countries, stepparents beat very young children to death at per capita rates more than 100 times higher than do genetic parents. The most thorough analyses are from Canada, where data in a national archive of all homicides known to police indicate that children under 5 years of age were beaten to death by their putative genetic fathers at a rate of 2.6 deaths per million child-years at risk (residing with their fathers) in 1974–1990, while the corresponding rate for stepfathers was over 120 times higher at 321.6 (Daly & Wilson, 2001). Note that because few small children *have* stepfathers, this rate differential does not, in itself, convey anything about the absolute numbers of victims; what these rates represent are 74 fatal batterings by genetic fathers in 28.3 million child-years at risk, and 55 by stepfathers in 0.17 million child-years at risk.

Fully comparable estimates have not been made elsewhere, but it is clear that this immense risk differential is not peculiar to Canada. In England and Wales, for example, 117 children under 5 years of age were beaten to death by putative genetic fathers and 103 by stepfathers in 1977–1990 (Daly & Wilson, 1994); as in Canada, fewer than 1% of age-matched British children dwelt with stepfathers and over 90% with putative genetic fathers, and so, as in Canada, the difference in per capita rates of such fatal assaults is well over 100-fold. Australian data indicate an even larger Cinderella effect. Wallace (1986) reported that perpetrators of fatal baby battering in New South Wales in 1968–1981 included 11 putative genetic fathers and 18 stepfathers, although the victims' median age was only 12 months. Strang (1996) reported that comparable cases for the entire country in 1989–1993 included 11 children killed by putative genetic fathers and 12 by stepfathers, and the median victim in these cases was even younger. For both samples, less than 0.5% of a random sample of same-age children from the general population would be expected to have had a stepfather, according to *Australian Family Characteristics Survey* data, and the estimated relative risk from stepfathers versus genetic fathers exceeds 300-fold.

There are no high-quality national data on fatal batterings in the United States, but the available evidence implies a similar situation. Weekes-Shackelford and Shackelford (2004) used the FBI's *Supplementary Homicide Reports* (SHR) to estimate that stepfathers beat children under 5 years old to death at a rate of 55.9 per million children at risk per annum versus 5.6 by genetic fathers. A 10-fold risk differential, albeit substantial, is surprisingly low in light of the Canadian, British and Australian data, but the true figure is surely much higher. The SHR database restricts the stepparent code to persons in registered marriages, whereas genetic fathers are coded as "fathers" regardless of marital status, so the comparison is one of married stepfathers versus married *plus* unmarried genetic fathers; moreover, 13 boys under 5 years of age who were beaten to death by adult men were miscoded in the SHR as their killers' stepfathers rather than stepsons and were therefore omitted from the estimates. Most importantly, there is direct evidence that Weekes-Shackelford and Shackelford's 10-fold estimate of excess risk is low. Wilson et al. (1980) analyzed child-abuse data from jurisdictions representing about half the U.S. population, and found 279 cases of "fatal physical abuse" (a broader category than lethal battering) in 1976; 43% of the victims (whose median age

was under 2 years) dwelt with stepparents, and these data in combination with population surveys suggest that young stepchildren incurred such mortality at about 100 times the rate for those living with two genetic parents (Daly & Wilson, 1988b). Other more localized studies likewise imply a very large Cinderella effect among murdered U.S. toddlers (e.g., Hicks & Gaughan, 1995; Lucas et al., 2002; Lyman et al., 2003; Stiffman, Schnitzer, Adam, Kruse, & Ewigman, 2002).

Swedish data indicate a smaller, but substantial, Cinderella effect for parental homicides *in toto* (i.e., not just fatal batterings). Temrin, Buchmayer, and Enquist (2000) initially reported that there was no excess risk to Swedish stepchildren, but this conclusion was based on an analytical error: rates were estimated relative to a population at large that was much older than the victims were. When the analysis was done correctly, toddlers were found to have been killed by genetic parents at a rate of 3.8 per million coresiding parent-child dyads per annum, while the corresponding rate for stepparents was 31.7 (Daly & Wilson, 2001). Because these estimates include all parental and stepparental killings, many of which have different typologies and risk factors than fatal batterings, they are not strictly comparable to the Canadian, British, and Australian numbers previously discussed, but they suggest that the magnitude of Cinderella effects may vary considerably across countries (see also Temrin, Nordlund, & Sterner, 2004).

How and why Cinderella effects vary in magnitude are important questions that can only be answered by cross-national research that differentiates homicide typologies. A fatal battering is very different, for example, from a murder-suicide by a depressed parent, who may construe killing her children as a "rescue," and both Canadian and British stepparents are overrepresented as killers to a much lesser extent in murder-suicides and family massacres than in fatal batterings (Daly & Wilson, 1994; Wilson, Daly, & Daniele, 1995). In regard to the specific case of Sweden, Daly and Wilson (2001, p. 294) speculate that "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." Whether social policy indeed has such effects on rates of family violence is a crucial question for future research.

Evidence for Cinderella effects in nonlethal abuse is even more extensive. One kind of evidence comes from case data collected by child protection agencies, in which stepfamily households and stepparent perpetrators are greatly overrepresented relative to their prevalence in the population at large (e.g., Craissati & McClurg, 1996; Creighton, 1985; Creighton & Noyes, 1989; Cyr, Wright, McDuff, & Perron, 2002; Daly & Wilson, 1985; Gordon, 1989; Gordon & Creighton, 1988; Klevens, Bayón, & Sierra, 2000; Rodney, 1999; Sirls & Franke, 1989; Trocmé et al., 2001; Wilson et al., 1980). Another source of evidence is victimization surveys, from which comparisons can be made between the responses of those who live or formerly lived with stepparents and those raised by genetic parents; the former routinely report much higher rates of both physical and sexual abuse (e.g., Kim & Ko, 1990; Russell, 1984; Sariola & Uutela, 1996). Surveys of runaway youth combine the features of the criterion case study and the victimization survey, and provide further evidence: a very large proportion of runaway and homeless adolescents report that they have fled stepfamilies in which they were subject to abuse (e.g., Powers, Eckenrode, & Jaklitsch, 1990; Tyler & Cauce, 2002).

### *Are Cinderella Effects By-Products of Other Risk Factors Associated With Stepparenthood?*

The fact that stepparents abuse and kill children at much higher *per capita* rates than genetic parents does not necessarily implicate the steprelationship as a causal factor. It could instead be correlated (confounded) with some other factor of more direct relevance. One obvious possibility is socioeconomic status: perhaps the stresses of poverty make the poor especially likely both to abuse their children and to divorce and remarry, making steprelationship an incidental correlate of abuse. This initially plausible hypothesis has been tested and rejected with respect to Cinderella effects in Canada and the United States, where poverty is indeed a risk factor for child maltreatment but

weakly or not at all associated with steprelationship, with the result that having a stepparent and being poor are more or less independent, additive predictors of the risk that a child will be abused (Daly & Wilson, 1985; Wilson & Daly, 1987; Wilson, Daly, & Weghorst, 1980).

Other confound hypotheses that have been tested and rejected are that differences between stepparent and genetic parent families might be by-products of differences in parental age or family size; such differences are in fact small and make negligible contributions to Cinderella effects (e.g., Daly & Wilson, 1985). A final confound hypothesis is that the effect is a by-product of the traits of those who become stepparents: in principle, the population of remarried adults might include disproportionate numbers of violent people, elevating victimization rates for those living in remarriages regardless of relationship. However, although those who become stepparents may indeed be atypical of parents in general, one fact speaks against the idea that this could account for Cinderella effects: abusive stepparents typically spare their own children. In a U.S. study of abusive families, for example, only the stepchildren were abused in every 1 of 10 households containing both stepchildren and children of the current marital union (Lightcap, Kurland, & Burgess, 1982); similarly, in Canada, only the stepchildren were abused in 9 of 10 such families in one study (Daly & Wilson, 1985), and in 19 of 22 in another (Rodney, 1999). This tendency for stepchildren to be targeted is especially striking in light of two additional facts: (1) when child abuse is detected, it is often found that all children in the home have been victimized, and (2) an abused stepchild is almost always the eldest child in the home, whereas the general (albeit slight) tendency in genetic-children-only families is for the youngest to be the most frequent victims (Rodney, 1999).

### *Stepfathers or "Mothers' Boyfriends"?*

Here and in our research papers, we call coresiding partners of genetic parents "stepparents" regardless of marital registration. This raises the question of whether Cinderella effects are due primarily, or even solely, to abuse by *de facto* stepparents. The answer is that such effects are large regardless of marital registration.

Both registered-marriage stepfathers and *de facto* stepfathers (also called "common-law stepfathers," "mothers' boyfriends," "cohabitees," and, in older literature, "paramours") are overrepresented as abusers in many of the studies previously cited. Weekes-Shackelford and Shackelford (2004) analyzed U.S. homicides using a database that restricts "stepparent" to persons in registered marriages and found large Cinderella effects even though the comparison group of "parents" included both the married and the unmarried. Creighton and Noyes (1989) estimated rates of child abuse by married stepfathers versus mothers' cohabitees in Britain, and actually found the former to be significantly higher than the latter, a unique result that is likely to prove exceptional.

The most thorough examination of the simultaneous relevance of steprelationship and marital registration is that conducted by Daly and Wilson (2001) with respect to fatal batterings in Canada. What they found was that both steprelationship and common-law status were strong predictors of homicide risk, and that neither variable's influence could be explained away as an artifact of the other's. In other words, stepfathers were greatly overrepresented as killers within both registered and *de facto* unions considered separately, and *de facto* fathers were greatly overrepresented within both genetic and stepfathers considered separately.

### *Stepparents or Stepfathers?*

Many of the analyses previously discussed contrast stepfathers versus (putative) genetic fathers, but this cannot be taken to mean that excess risk of abuse derives only from male stepparents. Step-mothers are often omitted from the data presentation only because small children reside with them so infrequently that in all but the largest databases, the cases are few and any estimate of abuse rates would have very wide confidence intervals.

Although the evidence on this point is limited, it is consistent in indicating that excess risk from stepmothers (relative to genetic mothers) is roughly on the same order as excess risk from

stepfathers (relative to genetic fathers). In an interview study of Korean schoolchildren, for example, Kim and Ko (1990) reported identical rates of child beating in stepmother and stepfather households, both far in excess of the rate in two-genetic-parent homes. More substantial evidence comes from large child-abuse databases such as those analyzed by Daly and Wilson (1981) and Creighton and Noyes (1989). Both studies included large numbers of stepmother cases and provided evidence that rates of physical abuse in stepmother and stepfather households are roughly similar and far in excess of those in two-genetic-parent households. Stepmothers are also substantially and significantly more likely to kill young children than genetic mothers according to the analyses of U.S. data by Weekes-Shackelford and Shackelford (2004), despite the facts that (1) as with stepfathers, the code "stepmother" was restricted to those in registered marriages, and (2) the genetic mother cases included neonaticides, a distinct category of homicides that is sometimes quite numerous. Finally, stepmother households tend to be even more extremely overrepresented than stepfather households among adolescent runaways who say they are fleeing abusive homes.

### *Mundane (Nonabusive) Discrimination Against Stepchildren*

It is important to stress that although stepchildren incur elevated rates of abuse and homicide, these dire outcomes are by no means typical. Many, perhaps most, stepparents make positive contributions to the well-being of their stepchildren, and most stepparents and stepchildren evaluate their relationships at least somewhat positively. Nevertheless, steprelationships are difficult, and those who make it their business to help stepfamilies in distress are unanimous in cautioning that it is a mistake to expect that a stepparent-stepchild relationship is, or will with time become, psychologically equivalent to a birthparent-child relationship (e.g., Johnson, 1980; Turnbull & Turnbull, 1983). Research tells the same story. Duberman (1975), for example, interviewed a select sample of well-established, "successful," middle-class, registered-marriage U.S. stepfamilies, and reported that only 53% of the stepfathers and 25% of the stepmothers felt able to say that they had any "parental feeling" (much less love) for their stepchildren. There are literally hundreds of self-help manuals for stepfamily members, with a single focus: how to cope with the characteristic conflicts of stepfamily life.

To an evolutionist, these facts are unsurprising. Becoming a stepparent may be a tolerable price to pay for a desired mate, but how much will then be invested in stepchildren remains negotiable. The extent to which a couple's combined resources will be devoted to such children is therefore likely to be a source of persistent conflict, an expectation that is abundantly confirmed by studies of marital discord (see Daly & Wilson, 1996; Wilson & Daly, 2001, 2004). Children of former unions enter into (re)marriage negotiations as costs, not benefits (e.g., White & Booth, 1985), and their presence reduces the custodial parent's value on the marriage market. Moreover, children of former unions increase the marital-duration-specific probability of divorce, whereas children of the present union reduce it (Becker, Landes, & Michael, 1977; Hall & Zhao, 1995). Having children of former unions also elevates the risk that wives will be assaulted (Daly, Singh & Wilson, 1993) and killed (Campbell et al., 2003; Daly, Wiseman & Wilson, 1997).

In light of the theoretical ideas that we espoused at the beginning of this review and facts like those reviewed above, we long ago proposed that violence against stepchildren is best understood as the atypical and extreme "tip of the iceberg" of a more ubiquitous discrimination (Daly & Wilson, 1980). Recent research in diverse disciplines has now confirmed this proposal. Econometric analyses of large data bases such as the U.S. *Panel Study of Income Dynamics* provide one sort of evidence: controlling for family income, stepchildren receive reduced investment in the form of support for higher education, routine medical and dental care, and even food (e.g., Case, Loh, & McLanahan, 2000; Case & Paxson, 2001; Zvoch, 1999). Surveys that ask directly about parental support tell the same story: according to both the parents and the children, stepparents wither investment relative to genetic parents (e.g., Anderson, Kaplan, Lam & Lancaster, 1999; Anderson, Kaplan, & Lancaster, 1999; White, 1994). Also of interest in this context is Ferri's (1984) finding

that both the mothers and the stepfathers in British stepfamilies express low aspirations for the children's education, lower even than those of single mothers of lesser means.

Other evidence comes from anthropological studies using observational sampling methods. In one study of Trinidadian villagers, Flinn (1988) found that stepfathers spent significantly less time with their children than genetic fathers, and that a significantly higher proportion of their interactions were "agonistic." In another such study of Hadza hunter-gatherers in Tanzania, Marlowe (1999) reported that although stepfathers baby sit their stepchildren, they are unlike genetic fathers in their behavior toward them; for example, they never play with them. Stepchildren also suffer elevated rates of accidental injury, both lethal and nonlethal, apparently because they are less assiduously monitored and protected (e.g., Fergusson, Fleming, & O'Neil, 1972; Tooley, Karakis, Stokes, & Ozanne-Smith, 2006; Wadsworth et al., 1983), and they suffer elevated mortality in general (e.g., Hill & Kaplan, 1988; Volland, 1988).

In view of all these factors, it is no surprise that stepchildren find their home lives stressful. Many studies have reported that they leave home at a substantially younger age than children from intact birth families (e.g., Aquilino, 1991; Davis & Daly, 1997; Kiernan, 1992; White & Booth, 1985), and not only do they leave earlier, but they are also far more likely to cite family conflict as the reason (Kiernan, 1992). More direct evidence of the stress associated with being a stepchild comes from a remarkable long-term study of child health in Dominica, where stepchildren exhibit reduced growth (Flinn, Leone, & Quinlan, 1999) and have chronically higher circulating levels of the stress hormone cortisol (Flinn & England, 1995; Flinn, Quinlan, Decker, Turner, & England, 1996) than their age mates living with only their genetic parents under similar material circumstances in the same village.

### THE CONTROVERSY

Despite the immense body of consistent evidence, dismissals of the Cinderella effect persist, even in refereed journals. Three published studies, in particular, have been packaged by their authors as failures to replicate our results, and despite obvious flaws, each of the three has been enthusiastically cited by skeptics.

In the first of the three, Gelles and Harrop (1991) estimated rates at which U.S. stepparents and genetic parents assault children on the basis of 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 be interviewed were no more likely to say they had assaulted children under their care than were genetic parents. Never questioning the validity of such data, Gelles and Harrop assert that because their study was based on a "large nationally representative sample," it constitutes 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 whether (1) self-selection for interview was unbiased with respect to the relevant behavior, and (2) the telephone interviewees spoke the truth. In the latter regard, it is 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 defensible rationale of discipline rather than with respect to being angry (Hashima & Amato, 1994).

The second alleged counterdemonstration is a paper by Malkin and Lamb (1994), who analyzed data on U.S. child-abuse reports 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" (p. 129). Curiously, however, these authors presented only cross-tabulations within the abuse cases (hence the convoluted and confusing double "rather than" construction quoted above), and made no estimates of abuse *rates* at the hands of stepparents or genetic

parents. The analyses entailed no failure to replicate prior findings because no prior paper had ever presented such an (uninformative) analysis. In fact, in the data archive that Malkin and Lamb analyzed, *every* form of abuse was perpetrated at massively higher rates by stepparents than by genetic parents, and yet this study has been repeatedly cited as proving the converse.

The third alleged failure to replicate was provided by Temrin, Buchmayer, & Enquist (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). As we have already mentioned, however, Temrin and his collaborators computed their homicide rates without regard to the fact that the proportion of children who reside with a stepparent is near zero at birth and increases steadily with age, and their erroneous affirmation of the null hypothesis derives from this oversight. Their data, like Malkin and Lamb's (1994), actually exhibit a large Cinderella effect (Daly & Wilson, 2001).

Documentation of a local exception to the Cinderella effect could be illuminating, but these studies deliver much less than that. Remarkably, however, their authors claim to have delivered much more: Each of the three studies was touted not simply as a local null result, showing that the Cinderella effect is of limited generality, but as reason to doubt that it exists at all, anywhere. Gelles and Harrop (1991) assert that all prior studies are biased by their reliance on "official report data," and that only their telephone survey can be considered free of bias. Malkin and Lamb (1994) subtitle their paper "a test of sociobiological theory" (needless to say, the "theory" fails the "test"); maintain that their results constitute a failure to replicate ours (they do not); and conclude vaguely that our findings might result from some unspecified artifact. Temrin and colleagues (2000) subtitle their paper "new data contradict evolutionary predictions," and write "Daly & Wilson 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 instill doubt that steprelationship has been persuasively linked to child abuse anywhere, but although we are glad to agree that hypotheses about possible confounds should be investigated more "thoroughly," Temrin et al.'s remarks are deceptive: They present no evidence of confounds (merely suggesting that psychiatric illness and drug abuse might be prevalent in stepfamilies); neglect to mention that the obvious confound hypotheses were tested and rejected in papers that they cite; and ignore the fact that confounds with family type cannot explain why abusive stepparents usually spare their own children selectively, as previously discussed.

The structure of these arguments appears to be motivated by something other than a humble search for the truth. Gelles and Harrop's passionate defense of their survey data is understandable when one realizes that Gelles's stature as a leading family violence researcher was built on such surveys and that their validity was already under attack from other quarters, especially feminists (see Dobash, Dobash, Wilson, & Daly, 1992). However, the Malkin and Lamb (1994) and Temrin and colleagues subtitles and discussion sections suggest another agenda: these authors evidently believe that a sociobiological or 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 the data were actually to contradict some *particular* model of the evolved human psyche, what would be required is a *better* model of the evolved human psyche.

### *Could Cinderella Effects Be Artifacts of Biased Reporting?*

Gelles and Harrop (1991) raise one point that deserves serious consideration: to what extent might Cinderella effects be artifacts of biased detection or recording, such that stepparental abuse is more likely than comparable acts by genetic parents to end up in official records? It is precisely this issue that initially led us to shift focus from child abuse in general to lethal abuse, on the presumption



that the most extreme cases should be relatively immune to such biases, and our general finding has been that the overrepresentation of stepparents as perpetrators is actually higher in child homicide cases than in nonlethal abuse cases, just the opposite of what one would expect if Cinderella effects were mere artifacts of biased recording.

Gelles (1991) has conceded that the Cinderella effect is real and large in the case of homicide, while clinging to the view that his telephone survey data are valid and that Cinderella effects in nonlethal abuse must therefore be artifacts of biased reporting. More recently, this idea has been taken up and extended even to lethal abuse by a philosopher, David Buller (2005a, 2005b), as part of a vehement, mendacious general attack on evolutionary psychology that has garnered considerable attention, much of it positive. According to Buller (2005b, p. 282; emphasis in original), "all of the evidence cited in support" of Cinderella effects can be explained as being products of a reporting bias, and he argues his case at length. What the papers Buller cites in support of this conjecture actually contain is very different from what he says; however, before going into those details, it is first worth noting how big this imagined bias would have to be.

Recall that Daly and Wilson (2001) estimated rates of fatal batterings of Canadian children under 5 years of age in 1974–1990 at 2.6 deaths per million child-years at risk for those residing with and killed by their (presumed) genetic fathers versus 321.6 per million child-years at risk for those residing with and killed by stepfathers. The latter rate is more than 120 times higher than the former. To give Buller's argument its best chance, suppose for the moment that stepfathers were always caught whereas genetic fathers often got away with murder. Even so, for the true rate of fatal batterings by genetic fathers to equal that for stepfathers, there would have to have been more than 500 undiscovered paternal murders each year in addition to the annual average of four that were detected. It's time for a reality check—there aren't even enough dead children! According to Canadian Vital Statistics, fewer than 400 children under 5 years of age died annually in 1974–1990 from any cause other than disease and congenital abnormality (i.e., homicides plus accidental injuries plus unknown causes). Thus, Buller's fantasy requires that in each of the 17 years, all accidental and unknown-cause deaths, plus more than 100 others that were attributed to specific diseases or congenital conditions, were actually successfully covered-up paternal beatings.

In regard to nonlethal abuse, the hypothesis that recording biases might explain Cinderella effects is again untenable, notwithstanding the fact that such abuse must often go undetected. The principle evidence justifying this conclusion comes from victimization surveys, which consistently reveal large differences in the experiences of persons raised in stepfamilies versus two-genetic-parent families. Consider just three examples published in the journal *Child Abuse & Neglect*. Russell (1984) surveyed a representative sample of San Francisco women and reported that 17% of those who said that a stepfather had been their primary father figure before age 14 also said they had been sexually abused by him by that age, whereas the comparable figure for women raised by their fathers was 2%; Kim and Ko (1990) reported that 40% of surveyed Korean primary schoolchildren living with one genetic and one stepparent reported receiving severe beatings, compared to 7% of those living with two genetic parents; and Sariola and Uutela (1996) reported that 3.7% of 15-year-old Finnish schoolgirls currently living with a stepfather affirmed on a questionnaire that he had abused them sexually, compared to 0.2% of those living with their genetic fathers. These estimates do not depend on anyone other than the victims themselves detecting and recording the abuse, so Buller's conjecture that stereotypes induce professionals to expect abuse in stepfamilies and to overlook it in genetic-parent families is irrelevant. One can, of course, resort to further speculation and propose that responses to victimization surveys are also biased against stepparents, but a modest bias will not suffice: in order to equalize the highly disparate rates, it would have to be the case that the vast majority of stepchildren who claim to have been abused are lying, or the vast majority of those who are actually abused by their genetic parents are lying when they deny it, or both. There is no evidence-based rationale for giving credence to such an insulting blanket dismissal of the validity of victims' reports.

Is there in fact *any* evidence for the existence of detection biases against stepparents? In an effort to show that there is, Buller misrepresents the findings of several U.S. *Child Fatality Review Panels* that have addressed "underascertainment" of child-abuse mortality. What all these Review Panels have reported is that when childhood deaths that were not clearly disease related were reviewed to see if there was an element of parental maltreatment or serious negligence in the precipitating events, the number of maltreatment-related deaths was approximately doubled over what one would have inferred from the causes of death recorded by coroners and/or pursued as criminal matters. This underascertainment is serious, but it falls far short of the level required by Buller's argument. More importantly, Buller fails to remark that the studies he cites all provide additional confirmation of the existence of very large Cinderella effects, after the *Child Fatality Review Panels* had done their work and the underascertainment had been rectified.

The first such paper that Buller (2005a, pp. 403–406) discusses at length is the report (Ewigman, Kivlahan, & Land, 1993) of a Missouri *Child Fatality Review Panel* that reexamined all 384 cases in which a child under 5 years of age died of external causes in 1983–1986. Only 58 of the deaths had been called homicides on the death certificate, and yet the panel concluded that 121 were definite maltreatment fatalities; this much Buller gets right. What he fails to mention is that the Missouri team proceeded to address the question of who actually killed these children in a subsequent case-control study (Stiffman et al., 2002): after review of all injury-related deaths had identified the fatal maltreatment cases, the identified killers included 15 stepfathers (married and *de facto*; plus two noncoresiding mothers' boyfriends) and 11 putative genetic fathers. Comparing the maltreatment cases to natural deaths (the case controls), 22% of the former dwelt with stepfathers versus 4% of the latter (still, interestingly, more than would be expected on the basis of the living arrangements of Missouri children). The authors cite our prior results, and emphasize that theirs' are fully supportive. We know that Buller did not overlook this second report, because he cites it, and one could hardly read it and fail to notice its documentation of a large Cinderella effect, since the authors consider it their main finding—the abstract's "Results" and "Conclusions" mention nothing else. It takes considerable chutzpah to cite this *Child Fatality Review Panel's* work in support of a denial of Cinderella effects, while neglecting to mention that further documentation of such effects, after death review and using a novel case-control methodology, was the Panel's primary empirical finding.

Buller (2005a) similarly miscites a North Carolina study (Herman-Giddens et al., 1999), stressing that it, like the Missouri study, uncovered many more "child-abuse homicides" than had been recorded as such in official statistics, while neglecting to mention that the findings again exhibit a very large Cinderella effect after child fatality review. However, the paper that Buller (2005a, 2005b) most stresses is a Colorado study by Crume, DiGiuseppi, Byers, Sirotinak, and Garrett (2002), which is unique among these *Child Fatality Review Panel* investigations in that it presents explicit comparisons between maltreatment-related deaths that were initially ascertained as such and those that were added as a result of the review process. Buller (2005a, p. 409) asserts that this study "found that a case of fatal maltreatment is more than eight times more likely to be recorded as such if perpetrated by a (commonlaw) stepfather than if perpetrated by a genetic parent," a claim that is wildly at odds with the actual data. For one thing, "(commonlaw) stepfathers" were not in fact distinguished from neighbors, clergymen, strangers, and other unrelated persons in the data presented, nor, according to a personal communication from the paper's senior author, can they be distinguished retrospectively. Moreover, what Crume et al. (2002) actually report is this: only 43% of 152 maltreatment deaths that the review panel attributed to "parents" were initially recorded as such, compared to 47% of 36 deemed to have been committed by "other relatives (including stepparents)," and 86% of 51 deemed to have been committed by "other unrelated (including boyfriend)." In other words, the initial ascertainment rate for the category including stepparents was scarcely different from that for "parents," and the initial ascertainment rate for the category including mothers' "boyfriends" was exactly two (not "more than eight") times higher than that

for "parents."<sup>2</sup> Buller (2005a, p. 409) concludes triumphantly that "the degree of diagnostic bias exposed by Crume and her colleagues is more than sufficient to account for the greater abuse by stepfathers in official case reports." In fact, it would be nowhere near sufficient to account for the 100-fold and greater Cinderella effects that have been documented in child maltreatment deaths, even if Crume et al. actually had "exposed" an 8-fold reporting bias, which they did not.

The simple truth is that no one knows whether there is even a slight reporting bias against step-parents in child-abuse records, and there are reasons to suspect that such biases as do exist might even run the other way. One reason for suspecting this derives from studies of sexual-abuse allegations that were later substantiated by medical examination and/or perpetrator confession: it turns out that girls who disclosed such abuse to their mothers were most likely to be disbelieved, and their allegations therefore ignored, if the abuser was a stepfather. In a U.S. study, Sirles and Franke (1989) reported that 86% of girls who disclosed paternal abuse to their mothers were believed, as were 92% of those abused by other relatives, but only 56% of those abused by stepfathers. A subsequent Canadian study (Cyr et al., 2002) produced almost identical results: 90% of girls who disclosed paternal abuse to their mothers were believed, as were 86% of those abused by a brother, but only 61% of those abused by stepfathers. The samples were large and the differences highly significant in both cases. What these data suggest is that, at least in cases of sexual abuse, maltreatment by genetic fathers may actually be more likely to find its way into official records than maltreatment by stepfathers.

Like other researchers, we had long been inclined to suppose that reporting biases with respect to physical abuse probably operate against stepfathers, thinking along the following lines:

Suppose that you lived next door to a child who exhibited recurrent, suspicious bruising, and that you (like everyone else) were familiar with the stereotype of step-parental cruelty. Isn't it possible that your likelihood of assuming the worst and calling a child protection agency might be affected by knowing that the man in the house was a stepparent? (Daly & Wilson, 1998, p. 27)

However, we may have been wrong in this intuition. An ambitious Canadian study of 135,573 investigations of child-abuse allegations (Trocme et al., 2002) suggests that reporting biases may actually operate against genetic fathers instead. After investigation, the allegations were classified as substantiated, suspected but unconfirmed, or "unsubstantiated" (conclusively disconfirmed). Disconfirmed allegations were not rare, constituting 43% of physical-abuse reports and 40% for sexual abuse; about 10% of disconfirmed allegations were judged to be "malicious" (knowingly false), and about 90% to be "honest mistakes." For present purposes, the most striking result of this massive national study is that reports of abuse by genetic fathers were significantly more likely to turn out to be false than reports against stepfathers: 43% of 19,486 allegations of physical or sexual abuse by "biological fathers" were disconfirmed by investigation (and 35% substantiated), compared to 34% of 5,667 allegations against stepfathers disconfirmed (and 39% substantiated).

These results suggest that when third parties suspect abuse on the basis of shaky evidence, they are more willing to blow the whistle if the suspected perpetrator is a genetic father than a stepfather. However, could it really be the case that reporting is biased against genetic fathers in this way? A possible reason why this might be so is that child welfare workers and teachers may "bend over backwards" in response to the steady stream of materials exhorting them to not succumb to "the

<sup>2</sup> Where the "more than eight times more likely" claim comes from is misinterpretation of an "odds ratio": 86% versus 43% is translated into "odds" of 86:14 versus 43:57, and then divided thus:  $[(86/14)/(43/57)] = 8.1$ . Crume et al. do indeed translate such ratios into statements of relative "likelihood," but this does violence to the ordinary English meaning of "x times more likely"; to see the absurdity of this use of "likely," consider that if 100% of stepfather homicides were detected versus "only" 99.99% of those by genetic fathers, then by Buller's logic, the former would be "infinitely more likely" to be detected. Buller deceptively uses the phrase "8 times more likely" to create the illusion that a 2-fold difference in rates of recording could account for an 8-fold difference in incidence rates.

myth of the cruel stepparent." That, of course, is speculation. What can be said with confidence is this:

1. that no one knows whether a reporting bias against stepparents even exists;
2. that Buller grossly misrepresents the research that he cites to support his claim that such a bias not only exists but is large; and
3. that even if a reporting bias were as large as Buller fantasizes, it would not be nearly large enough to explain observed Cinderella effects, which are often more than an order of magnitude greater still.

### *Scholarship and Civility*

Buller provides an extreme example in his apparently willful distortions of the evidence that he cites. However, it is not unusual for evolutionary psychology's critics to heap scorn on a caricature of the field or its findings, and it often appears that those who do so have never troubled to read the material they are trashing. In a wide-ranging attack, for example, Lickliter and Honeycutt (2003, p. 870) wrote

A second threat to validity [of evolutionary psychology] stems from a wealth of counterfactual observations. As a case in point, Daly and Wilson (1988, 1999) claimed that humans (and other species) possess a cognitive module to love and protect genetic offspring and that during humans' evolutionary history it was likely adaptive for men to engage in infanticide of the offspring of their mate(s) that were not their biological offspring. In support of this thesis, Daly and Wilson (1988) presented evidence that children who grow up in a household with a stepfather are at greater risk of abuse than those raised with their biological father. To their credit, Daly and Wilson (1999) acknowledged that their hypothesis has difficulty explaining why a majority of stepfathers do not abuse their children, but they ignored other potentially counterfactual evidence such as the lesser levels of abuse in families that adopt children.

*Nothing* in this paragraph is accurate except for the fact of excess abuse risk in stepchildren. The claims that Lickliter and Honeycutt attribute to us are not merely things we have never said, but explicit opposites of what we wrote in the works they are purportedly citing. We have consistently rejected, not advocated, the hypothesis that selection favored steppaternal infanticide during human evolution. In the short book that Lickliter and Honeycutt cite as "Daly & Wilson (1999)" (originally 1998), what we had to say about this hypothesis was this (p. 37):

Human beings are not like langurs and lions. We know that "sexually selected infanticide" is not a human adaptation because men, unlike male langurs and lions, do not routinely, efficiently dispose of their predecessors' young. Stepfathers are very much more likely to inflict non-lethal abuse than to kill, and such abuse is obviously not a "well-designed" means to hasten the production of one's own children nor even to reduce the costs of step-parental investment. Child abuse must therefore be considered a non-adaptive or maladaptive byproduct of the evolved psyche's functional organization, rather than an adaptation in its own right. Moreover, because of the complex social life of the human animal, which includes reputations and retribution, those who assault or kill children flirt with disaster. All told, we see little reason to imagine that the average reproductive benefits of killing stepchildren would ever have outweighed the average costs enough to select for specifically infanticidal inclinations. (pp. 37–38)

It is galling to be accused of advocating a hypothesis that one has carefully dissected and dismissed, but it is almost more galling to be given left-handed "credit" for something equally fantastic, namely for having "acknowledged that [our] hypothesis has difficulty explaining why

majority of stepfathers do not abuse their children." We have "acknowledged" nothing of the sort, since we have never entertained any hypothesis that would generate such a prediction. We hypothesized that steprelationship might be a risk factor for family violence as a by-product of parental adaptations, and this hypothesis has been abundantly confirmed. With identical logic, Lickliter and Honeycutt might complain that the hypothesis that smoking is a risk factor for lung cancer "has difficulty explaining" why most smokers do not get the disease. And of course, the accusation that we "ignored other potentially counterfactual evidence such as the lesser levels of abuse in families that adopt children" is also false (see Daly & Wilson, 1998, pp. 45-46).

How is it that one can write one thing, and yet be cited as having asserted its opposite? Lickliter and Honeycutt's approving citation of H. Rose and S. Rose (2000), two tireless opponents of evolutionary psychology who are utterly unconcerned with accuracy, provides a hint, for it turns out that the paragraph that we have quoted is a minimal paraphrase therefrom. The Roses' slanders evidently fit Lickliter and Honeycutt's prejudices so well that they felt no need to check whether they were true. The presumption seems to be that the evolutionary psychological slant on a given topic is a predictable "it's innate; it's adaptive," and there is a community that buys into this presumption and feeds off one another. In a column purporting to debunk the notion "that human behavior is the Stone Age artifact that evolutionary psychology claims," a Wall Street Journal columnist smugly demands, "Why, if child abuse by stepfathers is such a great evolutionary strategy, do many more stepdads love and care for their stepchildren than abuse them?" (Begley, 2005), apparently without pausing to wonder whether anyone had ever suggested that child abuse is a "great evolutionary strategy."

Unfortunately, it is not just newspaper columnists who are so sure that they know what evolutionary psychologists must have said that they need not read their actual words before attacking. Two book reviews of Daly and Wilson (1998) by academic biologists provide further examples. Evolutionary biologist Deborah Charlesworth (1999, p. 987) patronizingly scolds us for equating homicide by human stepfathers with infanticide by male lions: "The differences between lion and human social systems, and even between the social lives of lions and other members of the cat tribe, are large, and we cannot pick out only the similarities." Apparently, she did not read as far as page 37 (quoted above) before writing her review. Neurobiologist Steven Rose (1999) snorts derisively that "The fact that stepfamilies are more likely to be poor" is explanation enough for the excess violence in stepfamilies "but somehow they [i.e. we] can't see the obvious." On page 28 of the work under review, we, too, described as "obvious" the hypothesis that poverty might be a relevant confound, and then explained the evidence that poverty is not in fact confounded with steprelationship and is an orthogonal risk factor. But like Charlesworth, Rose does not seem to have actually read the (very short) book he is ostensibly reviewing.

As we have discussed above, many of our critics have tried (not always honestly) to make some sort of evidence-based case against the reality of the Cinderella effect. But, most of the biologists who have taken pot shots accept that the phenomenon must be real and have different fish to fry. In addition to attributing hypotheses to us that we have explicitly refuted, both Steven and Hilary Rose have tried in several writings to convince their readers that excess risk to stepchildren was well known before our research and that we have provided nothing but an implausible post hoc explanation for it. In a similar vein, geneticist Steve Jones harrumphs:

The way sociobiology rediscovers the blindingly obvious and then packages it as scientific breakthrough makes me laugh. The great biological "discovery" ... is that stepmothers (sic) kill their children more than mothers do. OK, it is good to get the data, but is anybody surprised? You can go back to the middle ages and find that is already known. (quoted in Patel, 1999)

In sum, some critics believe that the Cinderella effect is "blindingly obvious," others that it is nonexistent, but they are united in their indignation about a simple-minded evolutionary approach that

exists in their own imaginations and in the diatribes they write for what Robert Wright (personal communication) calls "the anti-ev-psych market niche."

From the perspective of two researchers on the receiving ends of these attacks, a disturbing and sometimes perplexing element has been their incivility. Our critics do not merely question our theoretical arguments or our data. Indeed, we have seen no critique, measured or otherwise, of our rationale for hypothesizing that parents will not love their stepchildren as they do their genetic children, and our attackers have paid scarcely more attention to our empirical findings. They are not just skeptical, they are angry, and we are still not entirely sure what they are angry about.

Some of evolutionary psychology's detractors apparently believe that it denies agency and undermines personal responsibility, as witness the frequent accusations of "determinism" and the characterization of efforts to understand the roots of antisocial behavior as efforts to excuse it. But why should this brand of moralistic aggression target evolutionary psychology in particular, rather than psychological science in general? It was not, after all, some raving Darwinian who maintained that his science had rendered "freedom and dignity" obsolete; it was B. F. Skinner (1971). When the attackers are biologists, perhaps evolutionary psychology stands alone in their gun sights because they are scarcely aware that psychology is a science whose practitioners are quietly and steadily eroding the domain of an unfettered human will. Biologists like Rose, Jones, Stephen Jay Gould, and Richard Lewontin apparently dislike the idea that human beings can and should be studied within the same Darwinian framework that applies to other creatures, for their contempt for evolution-minded research on *Homo sapiens* is Catholic in its targets.

The charge of "determinism" is so philosophically naive that one must suspect that it is a rhetorical tactic of these biologists rather than a sincere accusation. A more important source of their ire may be suspicions that evolutionary psychology is a smoke screen for a reactionary political agenda. Subscribers to such a conspiracy theory were unabashed during the "sociobiology debate" of the 1970s (Segerstråle, 2000), and critics like the Roses beat this drum ceaselessly. It even surfaces in denials of the Cinderella effect. Writing in the journal that is circulated to more clinical psychologists than any other, for example, Silverstein and Auerbach (1999) cite the Malkin and Lamb (1994) study previously discussed, as well as reporting that the absolute numbers (sic) of abusive stepfathers and genetic fathers are about equal in one small sample, and conclude that they have debunked "the neoconservative contention that stepfathers or mothers' boyfriends abuse children more frequently than biological fathers (and mothers)" (p. 402)!

That these writers can see no difference between epidemiological generalizations and political positions is alarming, but it would be foolhardy to deny that political preferences influence the issues that social scientists elect to pursue and sometimes color the interpretation of results. What is transparently false is the notion that evolutionary psychology has some sort of right-wing political agenda or is even especially compatible therewith (Pinker, 2002; Segerstråle, 2000). When its practitioners feel moved to political advocacy, they are at least as likely to invoke their field's theories and findings in support of politically progressive causes as the reverse. Tooby and Cosmides (1990) argue forcefully that an evolutionary psychological view of human nature is antithetical to racism, for example, and Trivers (1981) sees the same perspective as a force against authoritarianism and sexism. We interpret some of our own research (Daly, Wilson & Vasdev, 2001; Wilson & Daly, 1997) as supportive of egalitarian, redistributive policies. But the science itself (unlike its attackers) is politically neutral.

Our experience with angry and uncivil critics is not unusual, and some of the sneers directed at the enterprise of evolutionary psychology are astounding in their breadth (see, e.g., quotes in Hagen, 2005). It is not clear whether evolutionary psychologists will be able to counter the popular caricatures of our discipline, which feed on themselves as well as on the public's ignorance, and it can be disheartening when this antipathy comes from other Darwinians, as it surprisingly often does. However, it is important not to lose sight of the fact that we have more allies than enemies in evolutionary biology. In our view, perhaps the most effective thing that evolutionary psychologists

can do, both to counter misrepresentations of our field and to foster its continuing development, is to assist our colleagues in other life sciences in their battle to ensure that children are given a genuine biological education from an early age.

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