

Reply to David Buller by Martin Daly & Margo Wilson

The substantial excess risk of abuse and homicide incurred by stepchildren has been abundantly documented in dozens of studies using diverse methodologies (see companion document “The Cinderella effect”). Nevertheless, philosopher David Buller (2005a,b) has recently attempted to call the existence of this phenomenon into question by proposing “that *all* of the evidence cited in support” of it (Buller 2005b: 282; emphasis in original) can be explained away as products of a “reporting bias” such that abusive behaviour by stepparents is more likely to be detected and recorded than identical abusive behaviour by genetic parents.

What the papers that Buller cites in support of this conjecture actually contain is very different from what he says they contain. But before going into those details, it is first worth noting how big this imagined bias would have to be in order to make Buller’s speculation work.

Why reporting bias cannot possibly explain Cinderella effects

Daly & 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 (based on 74 deaths in 28.3 million child-years at risk) *vs* 321.6 per million child-years at risk for those residing with and killed by stepfathers (55 in 0.17 million child-years at risk). 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 4 that were detected. It’s time for a reality check: there aren’t even enough dead children! According to Vital Statistics, *fewer than 400* Canadian children under 5 years of age died annually in 1974-1990 from *all* causes other than diseases and congenital abnormalities (i.e. homicides *plus* accidental injuries *plus* unknown causes). Thus, Buller’s fantasy requires that in each of the 17 years, all the accidental and unknown-cause deaths, plus more than 100 others that were attributed to diseases or congenital conditions, were really successfully covered-up paternal beatings.

As regards nonlethal abuse, Buller’s conjecture is again implausible, 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 *vs* two-genetic-parent families, indeed just as large or larger than those that would be inferred from the child abuse case reports that Buller imagines to be fraught with recording bias.

Consider just three examples taken from the pages of *Child Abuse & Neglect: an International Journal*. 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 girls raised by their fathers was 2%.¹ Example # 2: on the basis of a survey of Korean primary school children, Kim &

¹ Russell (1986) elaborates on her stepfather/biological father comparison by noting regretfully that she failed to ask for greater detail about having lived with stepfathers, and that the incidence of reported abuse by more transient and later-appearing stepfathers was sufficiently high to indicate that the observed Cinderella effect would “probably” be even larger if she had collected full household composition histories.

Ko (1990) reported that 40% of those living with mother and stepfather and an identical 40% of those living with father and stepmother were severely beaten, compared to 7% of those living with two “natural” parents. Example # 3: Sariola & 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.

The estimates from these victimization surveys don’t 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. Buller can, of course, resort to further speculation and propose that responses to victimization surveys are also biased against stepparents. However, 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. Unfortunately for Buller, there is no evidence-based rationale for giving credence to such an insulting blanket dismissal of the validity of victims’ reports.

The alleged evidence for recording biases

Is there in fact *any* evidence for the existence of recording biases against stepparents of the sort that Buller imagines? The supposed evidence that he cites in support of this speculation is not at all as he portrays it. For example, Buller (2005a,b) cites papers that merely speculated that such biases might exist as demonstrations that they do exist. More importantly, he discusses at length and with quantitative specificity several reports by Child Fatality Review Panels that have addressed “underascertainment” of child abuse mortality in the United States, yet misrepresents their findings in multiple details. What these studies do in fact report is that when all childhood deaths that were not clearly disease-related were reviewed and classified with respect to whether there was an element of parental maltreatment or serious negligence in the precipitating events, then 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 does indeed suggest that there may be serious problems with U.S. mortality statistics.² However, it falls far short of the level of underascertainment required by Buller’s argument, for reasons laid out above. What is more interesting,

² Buller asserts repeatedly, on no basis other than his own intuition, that there is no reason to doubt that the shortcomings of official U.S. records will apply to Canada, too. If he had asked anyone who knows anything about the subject, he would have discovered plenty of reason to doubt this. It is well known that U.S. national homicide archives are the worst in the developed world, in terms of omissions and random data entry errors, whereas those for Canada are excellent. And *why* child fatality review panels in the U.S. have uncovered large numbers of additional “maltreatment deaths” is because the U.S. is unique among developed countries in initially recording the cause of death as “undetermined” for large numbers of child fatalities. It is particularly ludicrous that Buller (2005a: 404) should cite the incompleteness of the FBI’s Uniform Crime Reports with innuendo implying that this constitutes an argument against us, since we have discussed their shortcomings in print more than once (Wilson 1989; Daly *et al.* 2001), and, unlike many other homicide researchers, have therefore eschewed their use.

and is ignored by Buller, is that the very studies he cites provide additional confirmation of the existence of very large Cinderella effects, after the Fatality Review Panels had done their work and the “underascertainment” had been rectified.

The first such paper that Buller (2005a: 403-406) discusses at length is a report by members of the Missouri Child Fatality Review Panel (Ewigman *et al.* 1993), in which all 384 cases in which a child under 5 years of age died of external causes in 1983-1986 were re-examined. Only 58 of these deaths had been recorded as “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 children in a subsequent “case-control” study (Stiffman *et al.* 2002): after Child Fatality Review of all injury-related deaths had identified the fatal maltreatment cases, there were 15 stepfathers (married and *de facto*) among the identified killers of children under 5 years of age (plus 2 non-coresiding mothers’ boyfriends) *vs* 11 putative genetic fathers. Comparing the maltreatment cases to natural deaths (the “case controls”), 22% of the former dwelt with a stepfather compared to 4% of the latter (a percentage that is still, interestingly, higher than would be expected for the population-at-large).

The Missouri Child Fatality Review Panel team cites previous analyses by ourselves (Wilson *et al.* 1980; Daly & Wilson 1985) and emphasizes that their own results, using a different statistical method, are fully supportive of ours. We know that Buller did not overlook the Stiffman *et al.* report, because he cites it, and he could not have read the paper and failed to notice its documentation of a large Cinderella effect, because as far as the authors are concerned, that was their main finding: the abstract’s “Results” and “Conclusions” section refer to nothing else. It takes considerable chutzpah to cite this Child Fatality Review Panel’s work in support of an argument against the reality of Cinderella effects, while avoiding mention of the fact that further documentation of such effects, after death review and using a novel case-control methodology, was the Panel’s primary empirical finding!

The next paper that Buller (2005a: 406) discusses is a North Carolina study by Herman-Giddens *et al.* (1999), which identified 220 “child-abuse homicides” in which the victims were under 10 years old, of which only 68 had been recorded as “homicides” in the state’s Vital Statistics. Characteristically, Buller again neglects to mention that the paper reports the identities of the perpetrators, who included 67 genetic fathers and 45 stepfathers or mothers’ boyfriends. This represents a lesser proportion of stepfathers than in the Missouri study, but still far higher than in the age-matched population-at-large, and so this study, too, demonstrates a very large Cinderella effect after child fatality review.

Next comes the paper that Buller (2005a,b) most stresses, and most blatantly misrepresents: a Colorado study by Crume *et al.* (2002), which is unique among the Child Fatality Review Panel investigations that he cites 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: 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 startlingly at odds with the actual data. One way in which Buller’s statement is a falsehood is that “(commonlaw) stepfathers” were not even distinguished from neighbours,

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. More remarkably, when one reads the paper one discovers that 43% of the cases ultimately attributed to genetic parents were initially “recorded as such”, making it hard to imagine what “more than eight times more likely to be recorded as such” could possibly mean!

As regards perpetrators, 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 step-parents)”, 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”.³

There is an additional twist to this story, which is also ignored by Buller. The “underascertainment” of Colorado child maltreatment deaths documented by Crume *et al.* occurred overwhelmingly in cases in which the alleged maltreatment took the form of negligence (e.g. deaths in house fires, car accidents, and drownings while unattended), rather than assaults. The reason why this is important is that genetic parents are likely to be deemed the “perpetrators” in such cases *by definition* (i.e. by virtue of having failed in their duty of care), leaving it up in the air whether there was any detection bias at all with regards to perpetrator identity in truly comparable cases. Nevertheless, Buller (2005a: 409), after pointlessly comparing the imaginary 8-fold difference to the relative risk of abuse by stepfathers versus genetic mothers in another sample -- why? -- triumphantly concludes 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”. Of course, it would not be anywhere 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 truth of the matter is that no-one knows for sure whether there is even a slight reporting bias against stepparents in official child abuse reports, and there are reasons to suspect that such biases as exist might even run the other way. One of the reasons for suspecting this derives from studies of sexual abuse allegations that were eventually substantiated by medical examination and/or perpetrator confession: it turns out that girls who disclosed the 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 & Franke (1989) reported that 86% of girls who disclosed paternal abuse to their mothers were

³ Where the “more than eight times more likely” claim comes from is misinterpretation of an “odds ratio”: 86% *vs* 43% is translated into “odds” of 86:14 *vs* 43:57, and then divided like this: $[(86/14)/(43/57)] = 8.1$. 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 *vs* “only” 99.99% of those by genetic fathers, then by Buller’s logic, the former would be “infinitely more likely” to be detected.) More importantly, 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, which it could not.

believed, as were 92% of those who disclosed abuse by other relatives, but only 56% of those who disclosed abuse 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 who disclosed abuse by a brother, but only 61% of those who disclosed abuse by stepfathers. The samples were large and the results statistically 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 be reported to authorities, and hence to find its way into official records, than maltreatment by stepfathers.

Like other researchers, we have long been inclined to guess that reporting biases in physical abuse would 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: 27)

An ambitious Canadian study of 135,573 investigations of child maltreatment allegations conducted by child welfare workers (Trocmé *et al.* 2002) suggests, however, that reporting biases may actually operate against genetic fathers instead. After investigation, the allegations were classified as substantiated, suspected but unconfirmed, or “unsubstantiated” (i.e. conclusively *disconfirmed*: “there is sufficient evidence to conclude that the child has not been maltreated”). Disconfirmed allegations were not rare, constituting 43% of physical abuse reports and 40% of sexual abuse reports; about 10% of the disconfirmed allegations were judged to be “malicious” (knowingly false), and about 90% to be “honest mistakes”. Those sources whose allegations were most likely to turn out to be false were anonymous phone callers, followed by neighbours, and then school personnel. For present purposes, the most striking result of this massive national study is that a report of alleged abuse by a genetic father was significantly more likely to turn out to be false than a report against a stepfather: 43% of 19,486 allegations of physical or sexual abuse against “biological fathers” were disconfirmed by investigation (and 35% substantiated), compared to 34% of 5667 allegations against stepfathers disconfirmed (and 39% substantiated).

These results seem to indicate 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. But could it really be the case that there is a reporting bias against genetic fathers rather than stepfathers? A possible reason why there *might* be is that child welfare workers and teachers may “bend over backwards” in response to the steady stream of materials to which they are exposed exhorting them to not succumb to “the 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 away observed Cinderella effects, which are often more than an order of magnitude greater still.

Buller's own study

In addition to his distorted accounts of U.S. Child Fatality Review Panel studies, Buller (2005a) presents some novel tabulations from a 1993 U.S. data base, the *Third National Incidence Study of Child Abuse and Neglect*. When he tries to put case counts by age and household type (Table 7.3, p 378) together with population-at-large estimates (Table 7.4, p 379) to generate estimates of abuse rates (Table 7.5, p 379), Buller's arithmetical skills fail him and the numbers in his tables cannot be made to gibe with one another.⁴ Nevertheless, Buller does acknowledge that his data, like those in the peer-reviewed literature, exhibit a large Cinderella effect, whereupon he embarks on a distraction, stressing that his data exhibit a significantly *smaller* overrepresentation of stepparents than was found in a city-level study that we conducted in Canada (Daly & Wilson 1985).⁵ This is a red herring because no theory predicts that the Cinderella effect's magnitude should be invariant, and it is a particularly pointless distraction given, first, that a Cinderella effect of greater magnitude is what is typically found when analysis is confined to more severe abuse cases, as it was in the Daly & Wilson (1985) study, and secondly, that Buller's U.S. estimates closely match those that we reported 25 years ago on the basis of a much larger U.S. sample of similar provenance (Wilson *et al.* 1980).

Cinderella effects typically decline in magnitude as a function of the child's age

One of the first things we discovered in our research on child abuse and homicide is that with the exception of sexual abuse, rates appear to be maximal in infancy and to decline monotonically as a function of the child's age. This may be explained in part by the fact that smaller children are more likely than older children to be seriously injured by blows of equal force, and also by the fact that older children can more readily escape, but it is noteworthy nonetheless when one considers that abuse of preschoolers must often be easier to hide and that adolescents are supposedly more overtly conflictual with their parents than toddlers.

More remarkable is the fact that this age-related diminution of both lethal and nonlethal violence against children has a much steeper slope for stepchildren than for those living with birth parents, such that the overrepresentation of stepchildren as victims is maximal in infancy and declines sharply (e.g. Wilson *et al.* 1980; Daly & Wilson 1985, 1988a). In one study, for example, children under 5 years of age were 40.1 times more likely to be registered victims of serious abuse if living with a genetic parent and stepparent than if with two genetic parents, but this ratio of risk fell to 19.4 for those 5-10 years old,

4. For example, although infants and toddlers in "two genetic parent" households outnumber those in "genetic father plus stepmother" households in the population-at-large by a ratio of 163:1 (Table 7.4) and the corresponding ratio of victims is only 33:1 (Table 7.3), Buller somehow erroneously computes a higher abuse rate in the former household type than in the latter.

5. As he consistently does when citing empirical research reports, Buller mischaracterizes the Daly & Wilson (1985) study. He asserts, falsely and on no apparent basis, that things like "failing to secure a child with a seat belt" were included as "abuse", and he complains that sexual abuse distorts the picture and that analysis should have been confined to physical abuse, yet neglects to mention that the study explicitly reported that Cinderella effects were clear and large within both categories.

and to 9.8 for those 11-17 (Daly & Wilson 1985).

As we have noted in various discussions, this result has two important implications. The first is that conflict in stepfamilies cannot plausibly be blamed merely on stropky adolescents. As Daly & Wilson (1988b: 90-91) put the argument:

“Much of the popular literature on stepfamilies takes it for granted that conflict derives primarily from the children’s rejection of the substitute parent, rather than the reverse. From [our] perspective... this interpretation rings false. Surely it is the stepparent who is likely to resent the pseudoparental obligation thrust upon him, and any rejection by the child may be interpreted as reflecting a well-founded apprehension of that lack of genuine solicitude.

“These alternative hypotheses lead to contrasting predictions about the effects of age upon the risk of violence. If it is the child who refuses the new parent, then any problems that are peculiar to strelationships may be most severe with adolescents. At the least, such problems should be absent in the very youngest infants. If, on the other hand, the problem resides in the substitute parent’s resentment and disinclination, then the elevation of risk should be worst when the anticipated dependency and obligation is maximal. We would then see the greatest difference between stepparent and natural-parent homes in the maltreatment not of adolescents, but of infants.

“The facts support the latter view.”

The second important implication of the observed age-related risk pattern is this: proposing that stepparents treat children poorly in comparison to genetic parents because they were late arrivals in the children’s lives and failed to bond with them as infants is not going to work. And other evidence supports the same conclusion. For example, Ferri (1984) reported that British children had significantly poorer relationships with stepparents than with genetic parents, and that this difference was not at all diminished in cases in which the stepparent had come on the scene when the stepchild was an infant. The most striking evidence on this point comes from an observational study in Trinidad, in which anthropologist Mark Flinn (1988) had enough cases in which a stepfather had moved in with the mother during her pregnancy, and had therefore resided with his stepchild *from birth*, to compare their relationships with those involving later arriving stepfathers as well as (putative) genetic fathers. The findings were that those stepfathers who had been present from the child’s birth were significantly *more* antagonistic to the children than later arriving stepfathers, who were in turn significantly more antagonistic than genetic fathers.

Buller cites several sources in which we have documented and discussed the age patterns noted above, so we assume that he read them, and yet he presents the diminution in risk to stepchildren as they age as if it were his own novel discovery (Buller 2005a: 413-414). Moreover, he pretends that the observed age patterns constitute a challenge to our account of the matter, by falsely attributing to us the very “critical period” hypothesis (i.e. that stepparents differ from genetic parents merely in having missed out on early bonding experiences) which we have rejected for the reasons stated in the last paragraph.

Buller’s pervasive misrepresentation of our own writings

This last point is just one of several instances in which Buller fabricates simple-minded hypotheses and attributes them to us, knowing full well that they contradict what we have actually written in the works

he cites. Some additional examples are the following.

We have hypothesized that stepparents love their wards less, on average, than genetic parents, and therefore are more likely to mistreat them in pretty much any way you can think of. For example, Daly & Wilson (1985: 207) wrote:

“We consider such a criterion [having been registered in a provincial child abuse registry] particularly apt, because psychological constructs such as ‘child-specific parental solicitude’ afford the best level of abstraction for evolutionary theoretical analyses...

“Lapses of parental solicitude include, but are not confined to, direct violence. Neglecting a child and deliberately inflicting injury are certainly different, but both betray a failure of parental love. The caretaker who is truly concerned for a child’s welfare will furthermore not normally use that child as a sexual object.”

Citing this very discussion, Buller (2005b) nevertheless attributes to us a simple-minded theory whereby the *only* difference in the motivations of genetic parents and stepparents is that the former are inhibited against physically assaulting the child, and pretends that sexual abuse should not count as a domain in which we predicted a “Cinderella effect”. It most certainly is such a domain, and it exhibits a massive overrepresentation of stepparents, just as nonsexual abuse does.

Buller (2005a: 382-383) also pretends that our view of the evolution of parental sentiments predicts high rates of abuse in adoptive families, and tries to encompass this implication in the phrase “Daly & Wilson’s ‘most obvious prediction’”, which he lifts out of the context of a general theoretical preamble (Daly & Wilson 1988b: 83) and repeats literally dozens of times. In fact, we have repeatedly explained why we would *not* expect to see high levels of abuse in families practicing modern “adoption by stranger”, including an explicit treatment of this issue just two pages after the source of the phrase that Buller repeatedly quotes. In brief, the most important reasons are that prospective adoptive parents are screened, largely precluding the worst sorts of exploitative adoption known from other times and places; that they enter into the relationship with the explicit intention of simulating the experience of genetic parent families; that they are not comparable to genetic-parent *or* stepparent families with respect to other risk factors, especially socioeconomic status; and that whereas stepparenthood is a cross-culturally universal phenomenon that has presumably existed in ancestral environments for many millennia and exerted some selective pressure, adoption by stranger is a modern novelty. The prediction that adoptive families should exhibit high rates of abuse is Buller’s, not ours, and it is derived from his simplistic notion of what a Darwinian theory of parental motives must be.

Buller (2005a: 388-390) also pretends that our analysis of the steprelationship will somehow be damaged if it turns out that some fraction of the excess risk in stepfather families derives from the genetic mother herself rather than the stepfather. Not only is this possibility entirely consistent with our treatment of the topic, but we have explicitly explained why this is likely to be the case, in material that Buller cites. The basic idea is that single mothers who wish to re-enter the marriage market have reduced value in that market, and often face a cruel bind in which they must choose whether their primary allegiance is to their children or to a new mate who wants no part of them. An iconic case is that of Susan Smith, the U.S. mother who drowned her children in 1994 when her new partner told her that he didn’t want to see them there when he got home.

Buller (2005a: 390-396) furthermore pretends that the proposition that stepparental investment may be understood as “mating effort” (i.e. as an investment in establishing a relationship with a new mate) is an alternative to our views, and is somehow damaging to them. This distortion is particularly bizarre because this proposition is central to our view of stepparenthood (e.g., Daly & Wilson 1996a,b, 1998). Indeed, we were perhaps the first to propose this with reference to human stepparenting, and one of us is a co-author on the most thorough comparative treatment of this hypothesis (Rohwer *et al.* 1999). As Daly & Wilson (1998: 64-65) put the matter:

“Step-parents are primarily replacement mates, and only secondarily replacement parents. They assume their pseudo-parental obligations in the context of a web of reciprocities with the genetic parent, who is likely to recognize more or less explicitly that the new mate’s tolerance and investment constitute benefits bestowed on the genetic parent and the child, entitling the step-parent to reciprocal considerations. And once having opted into this situation, why shouldn’t a well-appreciated step-parent be kindly, and even affectionate? After all, violent hostility is episodic and amicableness is frequent even among non-relatives. People thrive by the maintenance of networks of reciprocity and by establishing reputations that will make them attractive exchange partners, with the result that the desire to be generous and humane, and to be seen as generous and humane, is as human and as functional as more conflictual motives. There is thus no great conundrum in the fact that people treat their stepchildren quite tolerantly, nor even in their accepting some share of the costs of raising those children. But the fact of such investment cannot be taken to imply that step-parents will often come to feel the same sort of love and commitment as is ordinarily felt by genetic parents.”

A few final remarks

The accompanying document (The Cinderella effect) reviews some of the vast body of evidence that stepchildren incur elevated rates of abuse and homicide. Skepticism is a virtue in science, perhaps especially in social science, but the reality of Cinderella effects is not controversial. We hope we have conveyed how Buller’s effort to make it controversial relies on misrepresentation of the research literature that he cites; if you doubt us, you can look up the cited papers for yourself. We also hope we have conveyed how pervasively he caricatures the theory of discriminative parental solicitude that has helped guide our discoveries. What we have scarcely conveyed, however, is the tone of Buller’s attack.

Here is an exemplary passage in which we (Daly & Wilson 1988b: 83) laid out how we think about the evolutionary basis of parental solicitude and the complications of steprelationships:

“In most relationships between two people, reciprocity is carefully monitored by both parties. If either seems regularly to take more than he or she gives, the imbalance is resented as exploitative. Parental altruism is different – unique in fact – in that the flow of benefits is prolongedly, cumulatively, and ungrudgingly unbalanced. Why this should be so is evident: Organisms have evolved to expend their very lives enhancing the fitness prospects of their descendants...

“What, then, of the party who steps into a ‘parental’ relationship with a child not his or her own? With all the good will in the world, the substitute parent is likely to find the situation difficult. Child-specific parental love is the emotional mechanism that permits people to tolerate – even to rejoice in – those long years of expensive, unreciprocated parental investment. Substitute parents are less likely than natural parents to experience the emotional rewards that make the costs of parenthood tolerable.”

Elaborating on this framework to suggest why it may be the case that it is specifically with respect to the rare phenomenon of lethal angry outbursts against infants and toddlers that we see Cinderella effects of the greatest magnitude, Daly & Wilson (1998: 32) wrote:

“Little children *are* annoying after all: they cry and soil themselves and sometimes refuse to be consoled. A caretaker with a heartfelt, individualized love for a squalling baby is motivated to tenderly alleviate its distress, but a caretaker who is merely playing the part without emotional commitment – and who might even prefer that the child had never been born – is apt to respond rather differently.”

And here is Buller’s (2005a: 411-412) sneering caricature of these proposals:

“According to this picture of human psychology, we all have the inclination to throttle the screaming infant on the plane, but only its parents are sufficiently besotted by love for the despicable creature ‘to tenderly alleviate its distress’ rather than silence it permanently.”

Enough said.

Martin Daly & Margo Wilson
Department of Psychology, Neuroscience & Behaviour
McMaster University
Hamilton, Ontario, Canada L8S 4K1
<daly@mcmaster.ca> <wilson@mcmaster.ca>

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