Responses to Daly's (2022) Criticisms of Nobes et al. (2019)

In his recent Commentary, Daly (2022) makes many criticisms of Nobes et al. (2019). In our reply (Nobes, 2023) we focus on the substantive issues on which Daly and we agree and disagree regarding the magnitude and explanation of the Cinderella effect. Here we respond to his criticisms, first by discussing errors in Nobes et al. (2019), and then errors and misrepresentation in Daly (2022). In the third section we consider Daly's often combative language.

1. Errors in Nobes et al. (2019)

1.1. Regarding whether the Canadian and US homicide data included non-cohabiting stepfathers (which would exaggerate the increased risk to stepchildren), we wrote that a "problem with all four studies *suggests* that increased risk *might* have been overestimated… the perpetrator data includes both cohabiting and noncohabiting fathers" (p. 1093, para. 5, emphases added). We should have said that, while with Daly and Wilson's (1994) and our British data this is definitely the case, for the same reasons the Canadian and US perpetrator data *probably* include both cohabiting and non-cohabiting stepfathers. Similarly, our point that "none of the previous studies (Daly & Wilson, 1994; Harris et al., 2007; Weekes-Shackelford & Shackelford, 2004) took account of the high proportions of nonresidential perpetrators" (p. 1099, para. 3) was overstated, as at this stage we cannot be certain. We discuss Daly's (2022) claim that the Canadian homicide data definitely did *not* include non-cohabiting stepfathers below (2.2.1).

1.2. "Despite their extensive output on this issue over three decades, Daly and Wilson tested for only three possible confounding variables in only one study (1985)" (p. 1093, para. 8). In fact, they tested for four in that study (the other being personality traits, which they mentioned in the Discussion), and also for marital status in Daly and Wilson (2001). We should therefore have said that they have tested for only five possible confounding variables in only two studies. See the section "Confounding Factors" in the Reply.

1.3. Another error was our claim that "There seem to be no selectionist grounds for predicting any substantive change in stepparents' increased risk with child age. Indeed, the only explanation they offer implies that the increased risk to older children would be even greater than to 0- to 4-year-old children" (p. 1099, para. 1). However, as Daly (2022, p. 2973, para. 4) points out, he and colleagues have suggested another possible explanation of the reduction in increased risk to stepchildren as they grow older, and so our statement is incorrect. We discuss this possible explanation in the section "Child's Age" in the Reply. See also 2.5.3 below.

2. Errors and misrepresentation in Daly (2022)

2.1. Daly and Wilson (1994) and Nobes et al.'s (2019) replication thereof[1]

2.1.1 Daly (2022) criticizes our reading and interpretation of Daly and Wilson (1994) and states that "the computation and comparison of abuse and homicide rates are things that we published elsewhere… not in the 1994 paper… Nobes et al. created a space for criticizing us for omitting details spelled out in the relevant primary reports…" (p. 2969, para. 4). In fact, both Daly and Wilson (1994) and Nobes et al. (2019) include computation and comparison of homicide rates, as well as analyses of methods of killings. They had not reported the British data before – and so there were no "relevant primary reports" – and only part (1974-1983, rather than 1974-1990) of the Canadian data set (Daly & Wilson, 1988a, 1988b).

2.1.2 Daly continues his criticism of our construal of the 1994 paper by arguing that it focused on stepfathers' and genetic fathers' methods of killing (from which Daly and colleagues infer motives), rather than on their relative rates of killing: "Our interest was in whether these two categories of child killings [i.e., those perpetrated by stepfathers and genetic fathers] had distinct distributions of motives, over and above any differences in rates" (p. 2969, para. 5). This attack is puzzling because both Daly and Wilson (1994) and Nobes et al. (2019) consider both. We disagree with Daly's representation of his own 1994 paper as somehow lacking relevance and influence in the debate about the increased risk to stepchildren, and we are in good company: In what Daly (2022) calls "the most thorough review of relevant human research (Daly & Wilson, 2008)" (p. 2968, para. 3), the very first source of evidence for the Cinderella effect discussed is the Canadian homicide rate data from Daly and Wilson (1994) (though they cite Daly and Wilson, 2001), and the second source is the British homicide rate data, also from Daly and Wilson (1994).[2] On this basis they claim that "as in Canada, the difference in per capita rates of such fatal assaults [in

[1] These level 2 subheadings (2.1, 2.2, etc.) refer to those in Daly (2022). The level 3 subheadings (2.1.1, 2.1.2, etc.) follow the order in which the relevant points occur there.

[2] When reporting both the Canadian and British data, Daly and Wilson (2008) report only the rates of fatal beatings, for which the increased risk to stepchildren is higher than for killings by other means.

Britain] is well over 100-fold." (Daly & Wilson, 2008, p. 385).[3]

In any case, the actual focus of Daly and Wilson's (1994) studies has no bearing on the debate about the data and analyses of relative rates of filicides by step and genetic parents that they report, and with which Daly (2022) and we (Nobes et al., 2019; Nobes, 2023) are primarily concerned.

2.1.3 Daly writes that Daly and Wilson (1994) reported that, "Of the 131 children slain by stepfathers, 89% were killed in this manner [i.e., beating] versus just 48% of the 244 slain by birth fathers" (p. 2969, para. 6). The stepfathers' percentage was actually 78.6%.

2.2. Who are classified as "stepfathers" in homicide archives?

2.2.1 Daly repeatedly and categorically states that our view that the Canadian perpetrator data probably include non-cohabiting stepfathers (which would lead to overestimates of the magnitude of the Cinderella effect) is wrong. He asserts that we "erroneously insist that a coding practice peculiar to the British data also invalidates findings from other countries" (p. 2968, para. 1); "They were mistaken, however, in their repeated assertions that the same problem applies to research that uses data from Canada and the United States" (p. 2970, para. 5); and "Nobes et al.'s (2019) claim is exactly backward in this case" (p. 2970, para. 7).

Daly makes these assertions because, he claims, Canadian recording officers are provided with clear guidelines, and these state that only cohabiting perpetrators should be classified as stepfathers. On these grounds he asserts that "The Canadian case is unequivocal" (p. 2970, para. 6), and that "for stepfathers, both numerator and denominator include only men who resided with the child at the time of the killing" (p. 2970, para. 7); "In fact, no such cases [of misclassification of non-cohabitees as stepfathers] were included in the Canadian analyses" (p. 2971, para. 5).

To prove his case, Daly quotes the *Scoring guide for the Homicide Survey* (Statistics Canada, n.d.) that advises officials who complete Homicide Survey questionnaires (usually investigating officers) on definitions, such as those of father, stepfather and spouse. Daly says that these guidelines "explicitly exclude 'noncohabiting' killers from the stepfather category" (p. 2970, para. 6) because stepfathers must coreside with the victim's biological or adoptive

[3] As Daly (2022, p. 2969, para. 6; p. 2972, para. 2) points out, Daly and Wilson (1994) did not estimate the magnitude of the increased risk to British stepchildren. Daly (2022, p. 2972, para. 2) reports that this 2008 estimate is based on a comparison of the 1994 homicide rate data and Wadsworth et al.'s (1983) population data.

parent, and therefore with the child. If correct, this would mean that we were wrong to suggest that the issue of non-cohabiting stepfathers is likely to apply to Daly and Wilson's (1994) Canadian data.

However, until 1991 the victim-perpetrator relationship question in the Homicide Survey provided no categories, definitions of the types of relationships were not provided, and recording officers wrote in their own descriptions (P. Walsh[4], personal communication, November 23, 2021). In 1991 categories were provided for the first time, with category 09 being "Father", and category 10 being "Stepfather", but there were still no definitions. It was not until 1999 that any were provided. These definitions are those (slightly mis-) quoted by Daly (p. 2970, para. 6), which actually come from the 1999-2004 version of the Scoring guide.[5]

Daly's argument on this point is therefore based on quotes from the *Scoring guide* that *did not exist* until nine years after the last homicides in their 1974-1990 data set took place, and five years after Daly and Wilson (1994) was published. Before then, as with the British Homicide Index data, whether the perpetrator was recorded as being a father, stepfather, or as having another relationship to the child, depended on the views of the recording officer, regardless of whether the accused and victim were co-habitants. It is therefore entirely possible and, we would argue, very probable, that some perpetrators who, rightly or wrongly, were identified as "stepfathers" in Daly and Wilson's 1974-1990 Canadian data were actually mothers' partners who did not live with the victims. If so, just as with their own and our British data, the proportions of homicide perpetrators recorded as stepfathers in the Canadian data, and hence the increased risk to stepchildren, are likely to have been substantially inflated.[6][7]

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[4] Manager of Client Services and Dissemination, Canadian Centre for Justice and Community Safety Statistics, Statistics Canada

[5] Even these 1999-2004 definitions are open to interpretation: The final sentence of the definition of "stepfather" is, "In the case of a homicide committed by a current or estranged lover of a biological or adoptive parent, score "34 – Other", unless the accused had a relationship with the victim that is better described by one of the other values" (p. 86). Since "Stepfather" is one of these "Other" values, reporting officers might (rightly or wrongly) record that the accused was the stepfather even when he did not live with the child.

[6] The possibility remains that Canadian reporting officers are different from their British counterparts and, regardless of the lack of guidance, have never classified non-cohabitees as stepfathers. We hope it will be possible to test this possibility empirically by comparing the addresses of "stepfathers" and their victims.

[7] Daly and colleagues have long recognized the importance of including only cohabiting perpetrators when calculating increased risk to stepchildren. Daly and Wilson (2001) asserted that "our

2.2.2 Daly then goes on to discuss the US data. He accepts that, “In principle, noncohabitants might thus have been miscoded as stepparents, as Nobes et al. (2019) allege” (p. 2970, para. 9), and reports that the relevant SHR data file was “riddled with errors, especially with respect to victim–offender relationships” (pp. 2970-2971). As he says, “there is no evidence that they [miscodings of mothers’ non-cohabiting partners as stepfathers] contaminate any of the U.S. analyses” (p. 2971, para. 5). But neither is there any evidence that they do not, and in our view they probably do.

2.2.3 Daly states that “In our original child abuse study based on American Humane Association data for 1976 from 29 states, we estimated that fatal abuse occurred at approximately 100 times higher rates in birth-parent-plus-stepparent households than in those with two birth parents (Wilson et al., 1980), even though our denominators were derived from survey-based estimates by Glick (1976) which were later shown by Bachrach (1983) to have substantially exaggerated stepparent numbers in the population at large” (p. 2971, para. 3). In fact, Wilson et al. (1980) said nothing about fatalities. Instead, it was Daly and Wilson (1988) who reported on the 279 cases of fatal abuse in Wilson et al.’s data set (pp. 88-89), and they based their estimate of increased risk to stepchildren not on Glick’s (1976) population data, but on Bachrach’s (1983). Daly’s suggestion that even their estimated increased risk to stepchildren of “approximately 100 times” was a substantial underestimate is therefore incorrect.

2.2.4 Daly claims that “Nobes et al. (2019) asserted that the results of ‘all’ studies prior to their own were biased by miscodings” (p. 2971, para. 5). (He goes on to repeat this point; see 2.7.1 below). Daly does not give a page reference, but we do not claim this, and we did not use the word “all” in this sense. He seems to be referring to our sentence that occurs in the context of a discussion of four studies (two in Daly & Wilson, 1994; Harris et al., 2007; Weekes-Shackelford & Shackelford, 2004): “However, another (previously unrecognized) problem with *all four* studies suggests that increased risk might have been overestimated” (p. 1093, emphasis added). Daly’s assertions are incorrect.

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published risk estimates for ‘stepfathers’ exclude all assaults and murders perpetrated by ‘mothers’ boyfriends’ who were *not* coresiding with their victims” (p. 290). The fact that many of the perpetrators who were classified as stepfathers in the Homicide Index – and hence by Daly and Wilson (1994) – did not reside with their victims shows that, at least regarding the British data, Daly and colleagues did not check for co-residency, and so made no such exclusion. Daly’s (2022) claims concerning Statistic Canada’s Scoring guide indicate that Daly and Wilson (1994) did not check for co-residency in the Canadian data, either.

2.3. Age matching in the computation of expected victim numbers and relative rates

2.3.1 "Nobes et al. refuted none of this but remained unsatisfied and wrote, '[t]hough their justification for doing so is unclear, Daly & Wilson (2008) revised this [i.e., Clarke's] estimate to 'fewer than 1%'.' This assertion is incorrect in two ways" (p. 2972, para. 2). Neither of our assertions is incorrect: Daly and Wilson (2008) did, indeed, revise Clarke's (1992) estimate; and their justification for doing so was, indeed, unclear because they did not substantiate or explain this revision. Daly (2022) has now corrected this omission by providing citations: "the justification of which we have spelled out elsewhere (e.g., Daly & Wilson, 1988b, 1988c), citing Wadsworth et al. (1983)" (p. 2972, para. 2).[8] Daly continues: "Nobes et al. apparently either overlooked or misinterpreted the modifier 'age-matched' when they implied that our estimate of 'fewer than 1%' for the earlier period was arbitrary and unfounded" (p. 2972, para. 3). We did not imply this, but merely pointed out that he did not explain the source of this percentage.

2.4. Might poverty explain the prevalence of stepfathers among child abusers and killers?

2.4.1 Daly claims we were unaware that he and colleagues have frequently "addressed the possibility of a socioeconomic confound" (p. 2972, para. 8). This is quite wrong. In fact, we gave poverty as an example of a potential confound (p. 1093) because he "addresses" (i.e., discusses) it so frequently (e.g., Daly, 2022; Daly & Perry, 2020; Daly & Wilson, 1988a, 1988b, 1996, 1998a, 1998b, 2008; Wilson et al., 1980; Wilson & Daly, 1987). We actually said that "Daly and Wilson (1994) did not *analyze* the homicide data to investigate this possibility" (p. 1093, emphasis added), and that they have *tested* for it only once (Daly & Wilson, 1985).[9] Both these statements are correct.

2.5. Cinderella effects and child age

2.5.1 "It is therefore curious that Nobes et al. (2019) should assert that a "problem" with "the evolutionary psychologists' claims" is that we and others have "considered only 0- to 4-year-old children" (p. 2973, para. 5). Although our use of "they" should have been clearer, the

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[8] Daly and Wilson (1994) did not estimate the magnitude of the increased risk to British stepchildren "because population-at-large estimates of the numbers of children living with stepfathers vs genetic fathers are questionable" (p. 213). This is puzzling because they had previously cited Wadsworth et al.'s (1983) population data (Daly & Wilson, 1988a, 1988b) on which this 2008 estimate was based.

[9] Daly and Wilson (1985) measured income only indirectly, according to whether families lived in areas in which incomes were above or below the median.

sentence to which Daly refers appears in a concluding paragraph (p. 1101) in which we summarized our preceding argument, the first point of which was that "Daly and Wilson (1994) (and Weekes-Shackelford and Shackelford, 2004) limited their analyses to children aged 4 and below." (p. 1098). Daly seems not to have realized that our point therefore concerned only the Daly and Wilson (1994) studies; it would indeed be "curious" for us to claim otherwise.

2.5.2 "But having misrepresented prior research as having been limited to preschoolers, Nobes et al. created a way to report their replication of this well-known age pattern as novel" (p.2973, para. 5). This too is incorrect; we did not misrepresent prior research in this way (see previous point, 2.5.1), and we did not attempt to present the age pattern as novel.

Related to this point, Daly states that "Nobes et al. (2019) combined novel analyses of homicide victimization of British *preschool* children…" (p. 2968, Abstract, emphasis added), and "Nobes et al. (2019) presented novel analyses of killings of *young* children by birth fathers versus stepfathers in England and Wales" (p. 2968, para. 1, emphasis added). In fact, we included all ages up to 18 years, and contrasted this with Daly and Wilson (1994), who included only young children, when Cinderella effects are greatest. It is therefore Daly who has "misrepresented [our] research as having been limited to preschoolers" (p. 2973, para. 5), not we who misrepresented his.

2.5.3 "Also curious is Nobes et al.'s further assertion that this typical age pattern is contrary to our evolution-minded account, which they misrepresent as predicting that Cinderella effects should increase with age. Nobes et al. do not explain their derivation of this "prediction", and my collaborators and I have never articulated or entertained any theory that would generate it" (p. 2973, para. 6). Daly's assertion is incorrect; we did not claim anywhere that Daly and colleagues would make this prediction. We *did* explain that the reason given by Daly and Wilson (1994, p. 208) for excluding older children was that "these cases clearly cannot be construed as matters of mutual combat or self-defense" and that, "Because killings of older children *could* be construed in these ways, the implication is that the increased risk to older stepchildren should be even *greater* than to younger stepchildren" (p. 1093). Unfortunately, Daly missed this point, and his criticism is unfounded.

2.6 Conclusion: The way forward

2.6.1 "Nobes et al. (2019) concluded that excess risk to stepchildren has been exaggerated in all studies prior to their own, and may not even exist." (p. 2973, para. 8). This opening sentence of Daly's conclusion includes two errors. First, it repeats the incorrect assertion about our use of “all” (see 2.2.4 above). And second, we do not say that it may not exist. In fact, the opening sentence of our conclusion is “The picture that emerges from this study is that stepfathers are, indeed, more likely to kill children than are genetic fathers” (p. 1101).

3 Civility

Throughout his Commentary Daly (2022) attacks Nobes et al. (2019) in robust and emphatic terms. For instance, he claims that our article “contains factual errors and misrepresentations… [and] misstatements… Nobes et al.’s efforts to deny this reality are misguided” (p. 2968, Abstract). He writes that our study “minimizes the reality of stepchild disadvantage” (p. 2968, para. 1), and that “The arguments in support of [our] conclusion are spurious, and [he will] dismantle them point-by-point in the following sections” (p. 2970, para. 2). It is surprising to see such combative writing in a scientific journal.[10] We suspect it does not make his assertions any more correct.

Daly has a long and undistinguished record of attacking in similar terms researchers who disagree with him. For example, in a section titled “Scholarship and Civility”, Daly and Wilson (2008) wrote, “Buller provides an extreme example in his apparently willful distortions of the evidence that he cites” (p. 394); “H. Rose and S. Rose (2000), two tireless opponents of evolutionary psychology who are utterly unconcerned with accuracy…”; “Evolutionary biologist Deborah Charlesworth (1999, p. 987) patronizingly scolds us…”; and “Neurobiologist Steven Rose (1999) snorts derisively… Rose does not seem to have actually read the (very short) book he is ostensibly reviewing” (p. 395).

Daly and Wilson (1998a) commented that Giles-Sims and Finkelhor’s (1984) proposal that stepfathers’ overrepresentation might result from socioeconomic disadvantage “was a remarkable argument, given that this possibility had been raised and disposed of in papers that they cited” (p. 49). About Malkin and Lamb (1984) they wrote, “this incoherent claim continues to be parroted as a reason to doubt the evidence” (p. 53), and on Duberman (1975),

[10] According to the APA (2020) Publication Manual, researchers should “Use language that conveys professionalism and formality… For example, scientific writing often contrasts the positions of different researchers, and these differences should be presented in a professional, noncombative manner: Stating “Gerard (2019) did not address” is acceptable, whereas “Gerard (2019) completely overlooked” is not” (p. 115). There are at least five instances of “combative” writing in Daly’s (2022) 120-word abstract alone.

"The prevalence of such vacuous pap is largely a result of well-intentioned efforts to help stepfamilies cope" (p. 56).

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