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*DOMESTIC VIOLENCE:  
DIFFERENT PERSPECTIVES*

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**Domestic Abuse Assessment  
in Child Custody Disputes:  
Beware the Domestic Violence  
Research Paradigm**

Donald G. Dutton

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Donald G. Dutton received his PhD in psychology from the University of Toronto in 1970. In 1974, while on faculty at the University of British Columbia, he began to investigate the criminal justice response to wife assault, preparing a government report that outlined the need for a more aggressive response, and subsequently training police in “domestic disturbance” intervention techniques. After receiving training as a group therapist at Cold Mountain Institute, he co-founded the Assaultive Husbands Project in 1979, a court mandated treatment program for men convicted of wife assault. During the 15 years he spent providing therapy for these men, he drew on his background in both social and clinical psychology to develop a psychological model for perpetrators of intimate abuse. This model views intimate abusiveness as emanating from a trauma triad and comprised of witnessing abuse, being shamed and experiencing insecure attachment. He has published over 100 papers and three books, including the *Domestic Assault of Women* (1995), *The Batterer: A Psychological Profile* (1995), and *The Abusive Personality* (1998). *The Batterer* has been translated into French, Spanish, Dutch, Japanese, and Polish, and Dutton has provided numerous workshops to professionals based on this work, including talks at the Sorbonne in Paris, the U.S. Army at Walter Reed Hospital, Navaho Nation in Winslow, Arizona, the joint ABA-APA Taskforce on Domestic Violence in Washington, DC, and psychiatrists in New York City. Dutton has frequently served as an expert witness in civil trials involving domestic abuse and in criminal trials involving family violence, including his work for the prosecution in the O.J. Simpson trial (1995). The latter led to an interest in spousal homicide and to “abandonment killing.” He is currently professor of psychology at the University of British Columbia, Vancouver, BC, Canada.

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**ABSTRACT.** In some states, custody assessors are now required to become familiar with the dynamics of prevalence of domestic abuse since the presence of one or more abusive parents in the house has an impact on the “best interests of the child.” The domestic abuse literature is misleading in setting a framework for abuse incidence and threat source for children. Males are represented as primary perpetrators of physical abuse although data from meta-analytic studies show otherwise. Indirect aggression is scarcely mentioned in the literature, although prevalent in research on aggression. Physical violence directed towards children is actually more likely to be mother-perpetrated. Child safety may be compromised if attention is focused solely on the possibility of abuse from a male perpetrator. [Article copies available for a fee from The Haworth Document Delivery Service: 1-800-HAWORTH. E-mail address: <docdelivery@haworthpress.com> Website: <<http://www.HaworthPress.com>> © 2005 by The Haworth Press, Inc. All rights reserved.]

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Domestic violence is very much an issue in custody assessments. In high conflict custody cases, interparental physical abuse rates run to 72-80% (Johnston & Roseby, 1997; Newmark, Hartnell, & Salem, 1995). Two recent publications (Jaffe, Lemon, & Poisson, 2003; Bancroft & Silverman, 2002) have linked domestic violence and custody assessment. Both are written with professional audiences in mind, and both cite research studies on domestic violence with a view to expanding the awareness of “professionals, therapists, child protective and court personnel, battered mothers and to anyone else who is in a personal or professional position to touch the lives of children of battered women” (Bancroft & Silverman, 2002, p. xiii). Both provide one-sided analyses of domestic violence based on self-selected and non-representative samples. It is the purpose of this paper to alert custody assessors to a more accurate data set on domestic violence incidence with a view to improving protection from threats to child safety.

Jaffe et al. (2003) begin by citing lifetime incidence of domestic abuse against women: 29% in Canada, 22% in the U.S., 23% in Australia (p. 5). Jaffe et al. (2003) acknowledge that men are abuse victims also but that the quality of violence directed toward women was more serious. Citing a study by Johnson and Bunge (2001), they concluded that women were “four times as likely to experience the most serious

and potentially lethal violence, such as threats, assault with a gun or knife, choking or sexual assault . . . three times more likely to report suffering a physical injury and twice as likely to report chronic ongoing assaults” (p. 6).

The relevance of these data for custody resides in the fact that, according to these authors, the co-occurrence of wife abuse and child abuse is high. Based on a study by Edelson (1999), Jaffe et al. (2003) put the co-occurrence of these types of abuse at 30-60%. That is, “30-60% of children whose mothers had experienced abuse were themselves likely to be abused” (p. 30). Jaffe et al. (2003) also note that direct abuse is not the only concern because the child becomes a pawn, used by the batterer to maintain abusive power and control in the relationship after separation. The authors then go on to define abuse using the “Duluth Power and Control Wheel” that includes “Using Male Privilege” as a part of an octant of abusive strategies used against women (p. 31). Jaffe et al. (2003) then list three categories of “whom to assess”: victimized mothers (p. 44), battering fathers (p. 46), and “war torn children” (p. 49). Jaffe et al. (2003) suggest using an Abuse Observation Checklist (Dutton, 1992) and asking the victimized woman to describe the “first, worst, and last” incident, followed by allowing the “alleged perpetrator an opportunity to respond.” It is not clear what response, apart from denial, might be expected from an accused male. Indeed, the authors warn an assessor that the male perpetrator may “minimize their abusive behavior by blaming their victims or proclaiming that the abuse was uncharacteristic” (p. 42). It seems that, once accused, the male can only use responses that the evaluator is already primed to see as disingenuous. In addition, this view blinds assessors to another source of threat to children: their mother. As we will see below, severe physical child abuse is more likely to be perpetrated by mothers than fathers.

What is problematic about Jaffe et al.’s (2003) analysis, and the others we will review below, is that while their description of the actions and consequences of abuse on the child are accurate, there is a priming of assessors to look only at the male as the abuse perpetrator, and having done so to suspect his denial of abuse. Denial of abuse will not exonerate him because highly abusive men deny abuse as well. Although Jaffe et al. (2003) tell evaluators to “review allegations with each party and give each side an opportunity to explain what happened” (p. 47) or to “have the alleged perpetrator complete a standard inventory about the abuse to engage him in a discussion about what transpired during the course of the relationship,” they provide this suggestion to a reader who has already been informed that males are the perpetrators and that per-

petrators lie. No algorithm is provided through which the truth might mystically emerge. Essentially the authors develop skepticism about male accounts and then advise the evaluator to use a clinical judgment already primed to disbelieve the alleged perpetrator. There is a substantial literature on the problems with clinical judgments (Kahneman, Slovic, & Tversky, 1982; Grove & Meehl, 1997), the essence of which is that actuarial judgments outperform clinical judgments consistently. Subjective “engaging in a discussion” with an evaluator, who is already primed to disbelieve the male respondent, is the very type of situation that forensic assessment has sought to eliminate.

Both Jaffe et al. (2003) and Bancroft and Silverman (2002) use “he” to refer to perpetrators of abuse and both are convinced that male abuse is by far the more serious. Jaffe et al.’s (2003) section on battering fathers has no counterpart called “battering mothers” (p. 46). These authors estimate the likelihood of mutual abuse in custody cases as only 9% (p. 54). Cases where the mother is the sole abuser are not considered or reported. Actual data from the U.S. National Survey, reported by Stets and Straus (1990,1992), showed that 28.6% of married couples were female violent (with a non-violent male) and 48.2% used mutual physical abuse. Here we get the first glimpse of the misleading focus of the female-victim orientation of the domestic-violence paradigm.

Jaffe et al. (2003) devote one paragraph of their entire book to describing men as victims of female violence, pointing out that, in a study done by the Canadian Centre for Justice Statistics, rates of spousal victimization were only slightly higher for women than for men. This finding is quickly dismissed because the violence is “qualitatively different”; women reported more serious violence, three times as much physical injury, and more chronic violence. Women were also more likely to fear for their lives (p. 6). As we shall see below, more extensive analyses of violence also dispute the claim that women are substantially more injured or that male violence is more severe or chronic.

Bancroft and Silverman (2002) express many of the same concerns about batterers as parents as do Jaffe et al. (2003). Both books have an awareness of the deleterious effects of a battering personality on vulnerable children; however, throughout both books, the terms “batterer” and “he” and “victim” and “mother” or “she” are used interchangeably. The eventual mindset is that abuse perpetrators are almost always male, and when they are not, the abuse is not serious. What Kahneman and Tversky (1982) call a “representative heuristic” is developed; batterers have the attributes of maleness, and they alone pose a risk to the child.

This pattern throughout the literature linking domestic violence and custody assessment is a misleading mindset to provide to evaluators who must enter into a custody evaluation from a neutral perspective and without preconceptions. The problem with both writers is their focus on males as batterers. This becomes problematic in a custody assessment where a mindset or paradigm drawn from working exclusively with battered women victims (Jaffe et al., 2003) or male perpetrators (Bancroft & Silverman, 2002) is now applied to a broader population where, despite Jaffe's attempts to dismiss it, female abuse is a reality and either can be detrimental to the best interests of the child.

### ***CHILDREN OF BATTERED WOMEN***

Both the Bancroft and Silverman (2002) and Jaffe et al. (2003) books describe the developmental problems that occur in children of battered women. Indeed an entire literature has developed chronicling psychological problems of children from abusive families. This literature consistently assumes that the source of the abuse is the father. Almost without exception this literature is based on samples drawn from battered women shelters or from treatment groups for men who batter (Bancroft & Silverman, 2002, p. xiii) and then inappropriately generalized to the general population.

For example, McCloskey, Figueredo, and Koss (1995) describe their study as examining "the link between different forms of family aggression and children's symptoms of psychopathology" (p. 1239). They then recruited 365 women from women shelters and the community. Even in the latter sample, women "were screened for the presence of violence in the home in the year prior to the interview" (p. 1242). In other words, the entire sample was based on battered women. Mothers and children were then asked to list abusive acts by both the mother and father. The authors stated that collecting samples of battered women from the community would "avoid bias in our sample" (sic). They also used a community control group that was "solicited without reference to family or partner violence" (p. 1244). Not surprisingly, in the battered women sample, children reported more violence from the fathers; the control sample children reported as much violence from their mothers as from their fathers. Mothers' reports of their own violence were not published.

The McCloskey et al. (1995) literature review on effects of family violence on children focused exclusively on children of battered women

and cited works by Jaffe amongst others. I mention this because it appears to me that a scholarly paradigm has developed where the same group of authors mutually cite one another's work and generate a model of family violence; the father is the batterer, the mother is the victim, the child is victimized by observation of the father's violence. This is the essence of an academic paradigm. A social reality is created that directs belief and focus of future research and disregards conflicting data (Dutton, 1994).

Despite their claim of examining "different forms of family violence," McCloskey et al. (1995) go on to say, "our study examines domestic violence and psychopathology from the child's as well as the mother's perspective" (p. 1240). The extent of the mother's role in any problems arising in the child is that "if there are deleterious physical and psychological consequences (of being battered) for the mother, she will be less equipped to care for her children" (op. cit., p. 1241). Despite the fact that their own data imply similarities in father to child and mother to child violence (p. 1252), the authors construct structural equation models that omit mother to partner violence and then describe the "different forms of family violence" exclusively in terms of behaviors used by fathers (p. 1255). McCloskey et al. (1995) state the following: "We found a particularly strong relation between men's use of physical abuse of women and the more extreme forms of abuse (sexual and escalated) in the mothers' reports, reflected as well in the children's reports of their fathers' violence against their mothers" (p. 1255) and conclude with "we believe that wife abuse sets the stage for paternal child abuse" (p. 1259). Although the authors refer on occasion to maternal aggression toward the child, this is viewed as a result of the mothers' own victimization. Starting with a sample of battered women, the parameters of violence within the family are already set, but this appears not to have been recognized by the authors. Straus (1997) alludes to this tendency as follows: ". . . mode of denying the bad news about assaults by female partners is to explain it away as a result of frustration and anger at being dominated by men" (p. 82). Dutton (1994), citing data from Stets and Straus (1992), showed that only 9.4% of marriages self reported as male dominant.

Another example of the problem in applying the woman as victim paradigm based on shelter samples to the general public can be seen in a paper by Appel and Holden (1998). These authors found in a review of 31 studies that wife assault and physical abuse of children occurred from 20% to 100% when the sample selection basis was either battered women or abused children (average of 40%). However, in "representa-

tive community samples,” the overlap was only 6%. In other words, the assumptions drawn from a shelter sample or a male perpetrator sample do not apply to community samples. Overlap rates diminish when items like “physically coerces” (as the authors point out, a legal form of punishment) were dropped from the study. The item “pushed, grabbed or shoved” generated the highest overlap, followed by “slapped and spanked.” While these actions too are problematic (Douglas & Straus, 2003), they do not constitute “battering,” and they inflate overlap rates for apparent abuse. However, they do so by including corporal punishment of children in the equation even though this corporal punishment is not legally or technically abuse (so long as it does not injure the child as is done for correction). As the authors put it, “some of the highest rates of overlap came from reports of children of battered women but these reports included slap/spank” (p. 585). This argument creates an erroneous impression that spouse assault is mainly husband to wife and that such assault has a high likelihood of being accompanied by physical child abuse.

Contrary to this view, Margolin and Gordis (2003) found that cumulative stress impacted more on co-occurrence between spouse and child abuse for women perpetrators, although their measure of child abuse was scores on the child abuse potential scale rather than actual abuse. They concluded “*wife to husband aggression, in contrast, is not linked to husbands’ child abuse potential, although it is linked to the wives’ own child abuse potential*” (p. 252). By comparison, in a sample drawn from “entrenched disputes over custody and visitation,” Johnston and Campbell (1993) described a “female-initiated violence” group who reacted to “internal states of tension and stress” (p. 195). Finally, in a study of co-occurrence of spouse and child abuse based on police reports of child abuse, Browne and Hamilton (1999) found that the main perpetrator (in 255 cases) was the mother 33% of the time and the father 23% of the time.

Even after examining the data on co-occurrence and noting that all came from women’s reports and that shelter samples were not representative of community samples, Appel and Holden (1998) outline five models of co-occurring spouse and physical child abuse. Not one of these models depicts the wife as a sole perpetrator of abuse to the husband, although the Margolin and Gordis (2003) and Browne and Hamilton (1999) studies indicate this is a prevalent category. To their credit, Johnston and Campbell (1993) include a “female-initiated violence” category in their five profiles of violence in divorcing families disputing custody (p. 289). However, their “profiling” is qualitative, not a formal

cluster analysis. They describe outcome severity in the female-initiated model as minor until the male loses control.

### ***INCIDENCE OF SPOUSAL VIOLENCE BY GENDER***

In contrast to the depictions of domestic violence by Jaffe et al. (2003) and Bancroft and Silverman (2002), numerous studies indicate a high incidence of female intimate violence. Using a self-report scale called the Conflict Tactics Scale (CTS), Straus, Gelles, and Steinmetz (1980) and Straus and Gelles (1992) found comparable intimate violence rates for men and women. These results were dismissed by feminist critics as being due to women reporting differently on the CTS or to reporting violence used self-defensively. However, Straus (1999) showed that overall incidence rates were quite similar whether reported by men or women. Recent studies by Morse (1995) and George (2003) indicate no gender differences in reporting using the CTS. Both genders underreport both commission of and victimization by serious acts of violence. However, results cited demonstrate consistencies whether reported by men or women.

Straus (1999) has also shown that the CTS is about 16 times more sensitive than “crime victim” surveys (such as the National Violence Against Women Survey [NVAWS]; Tjaden & Thoennes, 1998) to assault.<sup>1</sup> Crime victim surveys find very low annual assault rate of only 1.1% (compared to a 16% average on the CTS studies). The NVAWS found male annual rates of 1.3, 0.9 for females. The CTS studies found male annual rates of 12.2% and female rates of 12.4%.

The argument is also made by advocates that women’s violence is self-defensive. However, Stets and Straus (1990, 1992), reporting the results of the 1975 national U.S. incidence survey, found a pattern of female severe violence with a non-violent male occurred in 11.8% of couples, and the reverse pattern (male severe violence–female non-violent) occurred in only 4.4% of couples.<sup>2</sup> For all kinds of violence across relationship types, *females were unilaterally more violent than males to non-violent partners* (32% versus 18%). Fear did not seem to be an immediate female response to male violence. More females struck first (Bland & Orn, 1986) and more females than males hit back in response to an assault by the other (Stets & Straus, 1990).

The Stets and Straus (1989) study, which was based on a large sample and is methodologically sound, generates results that contradict feminist explanations for female intimate violence. Since the female vi-



olence observed was committed against a non-violent male,<sup>3</sup> it could not have been self-defensive violence. Also, since the same ratio of male/female initiated violence was found regardless of whether the respondent was male or female, the results could not be due to differential reporting by gender on the CTS. These data were initially reported in 1989 and were reemphasized in Dutton (1994). They are typically ignored by the paradigm that holds to a male perpetrator-female victim model. These data are not anomalous; other studies have produced the same result for violence incidence by gender.

Studying university dating couples in 17 countries, Douglas and Straus (2003) found that more females than males assaulted their partners. Furthermore, the female assault patterns were more chronic than the males (overall means = 7.1 vs. 6.2 assaults). Males inflicted injury 8.1% of the time compared to 6.1% for females. In younger (typically dating samples), female violence is greater than male violence.

Magdol, Moffitt, Caspi, Fagan, and Silva (1997) examined partner violence in a community-representative sample of young adults, collecting data from a birth cohort of young adults “to avoid clinical selection bias” (p. 70). In a sample of 941 participants in intimate relationships, women perpetrated violence 37.2% of the time and men 21.8% of the time. When measures of severe violence were taken (CTS-Severe), women perpetrated 18.6% of the time and men 5.7%. As the authors point out, “the expectation that rates of partner violence by men would exceed rates by women may stem from the sampling choices of previous studies. Many of these studies relied on samples of men who had been adjudicated or mandated to treatment programs because of the injurious consequences of their violence. Other studies used samples of women who sought shelter or treatment for the injurious consequences of their husband’s violence” (p. 75).

Feminist advocates argue that the Conflict Tactics Scale used in all of these dating sample and community sample studies is a poor measure of intimate violence because it does not assess consequences. Archer (2000) published a meta-analytic study based on 82 studies of gender differences in intimate aggression that assessed not only incidence of aggression by gender but medical consequences. Using the conventional  $d'$  statistic that estimates effect size, Archer found that women were both slightly more violent than men ( $d' = .05$ ) and slightly more likely to be injured by male partners ( $d' = .08$ ). Neither difference is large;  $d'$  of .08 is less than one-tenth of a standard deviation difference in distributions of injuries by gender. The size of this meta-analytic study ( $N = 64,487$ ) gives it great weight in determining gender differ-

ences in intimate violence. The results suggest they are minimal. Archer describes an overlooked norm, lost in the feminist paradigm: that men should restrain themselves from physically aggressing towards a woman. Also, the Archer (2000) studies measure consequences of intimate violence that are not assessed by the CTS per se, such as injury and use of hospital services (reported as addenda to the CTS). Hence, these measures fall outside any criticisms of the CTS.

Despite powerful findings from the meta-analytic studies reported above, Sudermann and Jaffe (1999) claim, "the overwhelming majority (95%) of instances of marital violence involve women as victims and men as perpetrators, according to police and court reports" (p. 26). As evidence for this claim, these authors report an out-of-date statistic based on police reports in my 1988 book. However, as Stets and Straus (1992) have shown, men are only one-tenth as likely to call police when assaulted as are women. This occurs, in part, because police refuse to take violence against males seriously (Buzawa, Austin, Bannon & Jackson, 1992; Brown, 2004; see below) and are less likely to arrest or to issue a report. Hence, Sudermann and Jaffe (1999) and Jaffe et al. (2003) use police and criminal justice statistics as the criterion for their claim when these systems arrest differentially and with a gender bias. A better criterion would be to use the community self reports described above that show much greater equality by gender.

### ***FEAR OF VIOLENCE***

Jaffe et al. (2003) use female fear of being killed as proof that male violence is more severe. There are two problems with this argument. First, as evidenced by female aggression cited above, female aggression contradicts fear. Furthermore, even if they were afraid, many fears are overblown and based on lurid media reports (Slovic, Fischhoff, & Lichtenstein, 1982). We are more terrified of death by terrorist attack than of drowning, although our chances of death by drowning are higher. Abandonment homicide occurs in about three of 100,000 marriages (Wilson & Daly, 1993), and any form of male perpetrated partner homicide at less than 0.6 per 100,000 women over age 15. Female perpetrated homicide rates are 0.5 per 100,000 (Browne, Dutton, & Williams, 1998). Females may feel more fear but they are not at greater risk. Spousal homicide is extremely rare regardless of the gender of the victim. Despite this fact, the domestic violence literature treats female fear

as though it were a veridical representation of danger and can be extended to custody disputes (Jaffe et al., 2003).

After several logistic regressions with a variety of variables, Gondolf and Heckert (2003) reported that women's "strongest determinant of perceptions of safety was the perceived likelihood of reassault" (p. 383). But this is merely a tautology, two measures of the same thing used in a "predictive" fashion. Gondolf and Heckert (2003) go on to cite de Becker's book, *The Gift of Fear*, as evidence for a "bio-anthropological survival view" (p. 373). De Becker never conducted empirical research and provided no evidence that any fears were valid. Subjective measures of fear must be compared to actual outcomes. In fact, prospective studies on actual risk had not been published at the time of Gondolf and Heckert's (2003) paper. The first, Williams and Houghton (2004), reports only moderate success at risk prediction, in part because domestic violence outcome measures are extremely skewed distributions. The paradigm of the domestic violence literature has literally made female subjective consciousness into reality. Clearly there will be some relationships where fear of an aggressor is justified, but to generalize this on the basis of gender alone is not warranted by the data.

### ***THE RIPPLE EFFECT OF THE PARADIGM***

Researchers who focus on women from shelters or batterers groups samples have, unfortunately, had influence on the laws and legal process. The Judicial Council of California has a policy document called "Parenting in the Context of Domestic Violence," which was written by Edelson in 2003. This document is available on a government Website, and although it reads in fine print that the views "do not necessarily" represent the official position of the Judicial Council of California, it is presented to the public without any contradictory information. It represents a source of information on domestic violence for the public. Edelson's prior work was on the connection between woman battering and child abuse, finding that women reported more threats of violence against them than did their male partners (Edelson & Brygger, 1986). The sample was again taken from a treatment group for violent men but was reported under the title "Gender Differences in Reporting of Battering Incidences," leaving the impression that men in general underreported their own violence and that women's reports were, by default, more accurate. As described above, Edelson's (1986) study found a high degree (30-60%) of overlap between wife abuse and child abuse,

although as we mentioned above, this overlap is much lower in community samples.

In the California Judicial Council (2003) report, victims of domestic violence are “battered mothers”; perpetrators of domestic abuse are “controlling and authoritarian compared to that of nonviolent fathers” (p. 3; i.e., perpetrators of abuse are male). One study is cited to support the parenting ability of battered mothers. Its results are dramatically different from the Margolin and Gordis (2003) study reported above. Edelson (2003) cites Bancroft and Silverman (2002, p. 5, another court mandated male sample) to support his checklist for continuing risk to the children (from the father). Edelson does a literature review but raises none of the issues I raise here. Appel and Holden’s (1998) work is cited uncritically and the following conclusion is cited; “often the perpetrating male beats the woman, who then abuses the child, or that both parents abuse the child” (p. 10). No possibility of female-initiated abuse is raised. Victims’ (who are battered mothers) parenting skills are reviewed (p. 13) and a concern is raised that mothers’ skills may be devalued because of a lack of information about male perpetrators. The male perpetrator-female victim model is now enshrined at the policy level.

A study of police response in Detroit found that when men asked for police assistance in domestic disputes, the incident was trivialized and the men were belittled by the officers (Buzawa et al., 1992). This happened regardless of the degree of injury. As Buzawa et al. described it, “. . . for example, one male reported requiring hospitalization for being stabbed in the back, with a wound that just missed puncturing his lungs. Despite his request to have the offending woman removed (not even arrested), the officers simply called an ambulance and refused formal sanctions against that woman, including her removal” (p. 265). A similar result was found by Brown (2004). Brown found that women were only arrested and prosecuted on domestic calls when they inflicted high levels of injury on male victims. Women were more likely than men to use weapons, typically a knife. This finding was replicated by Henning and Renauer (2005). It is possible to trace the pathway of misinformation here: unwarranted generalizing from a non-representative sample creates the view that only females are abusers; this becomes enshrined in policy and eventually in practice. Female violence is simply not perceived nor treated comparably to male violence. Hence, basing conclusion on “police statistics” is itself misleading.

The work cited above by Jaffe et al. (2003), Bancroft and Silverman (2002), and Edelson (2003) reflects the “groupthink” (Janis, 1982) character of the domestic violence paradigm. Conceived with the well-

intended notion of improving women's safety, it lost sight of its original scientific obligation: to objectively tell the truth. Over the years a paradigm developed that in the domestic violence literature, disparaged individuals who reported female violence (see Straus & Gelles, 1992, p. 11-14; Straus, 1997, p. 218), explained away such violence (Saunders, 1988), or simply ignored it (Yllo & Bograd, 1988).

### ***RELATIONSHIP TO CUSTODY ASSESSMENT***

In the original National Survey on Family Violence, it was found that 17.7% of female respondents reported committing severe violence against their children (compared to 10.1% of men) and 67.8% reported committing any violence (compared to 57.9% of men; Straus et al., 1980). As described above, in a study of co-occurrence of spouse and child abuse, Browne and Hamilton (1999) found that the main perpetrator of child abuse in 255 cases was the mother 33% of the time and the father 23% of the time. Despite these statistics, the "domestic violence paradigm" described above would direct custody assessors to look exclusively at males as a threat source for children.

Custody assessment must, in following other forms of forensic assessment, become as actuarial as possible. Subjective judgments, including gender based expectations, must be guarded against and objective assessment checklists that include questions about parental abuse should be used to assess both fathers and mothers equally. As much corroborative information as possible must be collected, but sheer numbers of witnesses is not a substitute for impartiality. A biased evaluator will simply affirm their bias through the information gleaned from witnesses (Nisbett & Ross, 1980; Garb, 1998). Witnesses in custody disputes "choose up sides" and a biased assessor can easily collect cumulative disinformation to prove a pre-formed hypothesis. For this very reason, the biases generated by Jaffe et al. (2003) and by Bancroft and Silverman are especially troublesome. A custody assessment is an emotionally charged and difficult forensic assessment.

For these reasons, Ackerman's (2001) development of an actuarial assessment strategy for custody, complete with scales for parent evaluation, is promising. This approach ensures that all the right questions are asked and that they are asked of both parents. Furthermore, as data from the validation studies becomes available, corrective weighing of the scale items becomes a possibility; the system can auto-correct. Subjective evaluations do not have this capacity and, as research in social cog-

nitition has shown, strongly held beliefs become more entrenched, even in the face of disconfirming evidence (Lord, Ross, & Lepper, 1979). Gould (1998) in writing on custody assessment has not bought into the disinformation of the domestic violence paradigm, and includes reference to Johnston and Campbell's (1993) typology of domestic violence which includes "female initiated violence."

### *CONCLUDING COMMENTS*

Both Gould (1998) and Ackerman (2001) provide well thought out checklists for questions regarding parenting issues and general assessment. In addition to these, there are certain questions a custody assessor can investigate when allegations of physical abuse arise. The most obvious include whether emergency calls to police were ever made, whether friends were told, or whether there were eyewitnesses. If the only source for corroboration of abuse is friends, then the possibility of collusion must be considered. Are all of the friends confidants of the alleged victim? This obviously generates a conflict since confidants would be both the persons most likely to have disclosures of physical abuse made to them and the persons most likely to collude with the witness. Nevertheless, Stets and Straus (1992) found that only 8.5% of assaulted women and 0.9% of assaulted men called police. Injuries sustained by violence and reported to a doctor constitute the fourth source of primary evidence for physical assault. The context of the doctors' visit should also be examined since abusers would sometimes accompany their mate to a physician to ensure a "cover story" for the abuse was given to the doctor.

Secondary sources of evidence for physical abuse include days lost from work, classically conditioned fears, trauma symptoms, reports to psychotherapists, and injury histories reported but where no cause was recorded.

Tertiary sources should include friends' observations of the power dynamics of the couple, witnessed verbal abuse, and emotional or behavioral problems in the children (although divorce itself is linked to acting out behaviors). Distress from having a security base disrupted can trigger many of the same behaviors as exposure to abuse or identification with one parent and blaming the other (Dutton, 2000; Costello & Angold, 1995; Grych, Fincham, Jouriles, & McDonald, 2000; Yehuda, 1999). These are not flawless data sources; each

comes with an inherent deficit, but their concinnity with allegations of abuse should be assessed.

I have been an expert witness in several child custody cases where abuse was alleged. I have seen the impact on children that Jaffe et al. (2003) and Bancroft and Silverman (2002) describe. I have seen fathers, for whom independent evidence of prior abusiveness existed, use the courts and the children as pawns in an obsessive “war” with their wives. I have seen them stalk, lie, and deceive. I have also seen women where evidence of abusiveness existed both through the reports of third party witnesses and psychological tests. I have seen custody evaluations that dismissed their abusiveness as trivial. I have argued that personality disorder in parents has a strong effect on children’s behavior and personality disorders exist in both sexes (e.g., Bartlett, 2001; Dickstein, 1998; Pfiffner, 1999).

The danger in the domestic violence literature comes from the sample bias that has informed much of the research: battered women’s shelters and court-mandated male assaulters. These groups are small percentages of the general population, have exceptional physical abuse rates, and results based exclusively on such groups will paint a very distorted picture of the general population. For this very reason, some readers may be surprised at the equality of abuse incidence rates by gender taken from community samples. Our theories have not prepared us for these facts. Similarly, the relative rarity of spousal homicide is belied by an emphasis on fear of spousal homicide. This surprise is a cue that the paradigm constructed by domestic violence research has warped our perceptions. When, as professional assessors, we come to evaluate people from the general population and bring these distortions to the process, we are doing a disservice to the “best interests of the child.” We should use a balanced, self-correcting “actuarial” tool as a prophylaxis against the possibility of bias. The most thorough protection of the safety and best interests of the child derives from an assessment of all potential risks to that safety as opposed to a gender biased exclusion of maternal threat.

## NOTES

1. The U.S. National Violence Against Women Survey (NVAWS; Tjaden & Thoennes, 1998) is often cited as strong evidence refuting data indicating similar rates of aggression committed by men and women. Tjaden and Thoennes reported that “women experience significantly more partner violence than men do” (p. 2). The report

indicated that of 8,000 men and 8,000 women, 22.1% of women and just 7.4% of men reported any physical assault by an intimate partner across the lifetime; 1.3% of women and 0.9% of men reported a physical assault by a partner in the previous 12 months. This study's methodology excludes it from direct comparisons with much of the literature relevant to a discussion of women's use of aggression. Respondents in this survey had to acknowledge themselves as victims, and the study was presented as a study on victimization of women. It obtained very low reported incidence rates.

Men are less likely to report victimization than women (Straus & Gelles, 1992). Similar to most crime surveys, the study implicitly included demand characteristics because of its emphasis on "personal safety" and "violence" (Straus, 1999). Moreover, Archer (2000) noted the NVAWS was presented to respondents as "a survey of violence toward women, thus giving the message that men's victimization was not a concern" (p. 698). Mihalic and Elliot (1997) found that phrasing questions about partner assaults in the context of criminal assaults reduced reporting of serious partner assaults by 83%, compared to questions phrased as about relationships.

2. The Self Defense Debate: Walker (1984) and Saunders (1988) acknowledged the presence of female violence but argued that it was defensive. Both the Walker and Saunders samples, upon which they based this argument, came from women's shelters or women who self reported as battered and so, by definition, contained women who were physically abused but who do not appear to be representative of community samples. Saunders did not comment on this generalization problem but simply commented that as a "feminist researcher" he had an obligation to examine motives in addition to hit counts. Saunders argued that social science must be a "tool for social change." Saunders (1988) concluded that female violence is self defence, even when the woman uses severe violence and the man uses only mild violence. This, he argues, is because of the woman's smaller size and weight. He focuses this analysis on the Straus data. These data never asked who used violence first, so the question of self defence cannot be answered by this data set.

Bland and Orn (1986), in a survey conducted in Alberta, Canada, did ask who used violence first. Of the women who reported using violence against their husbands, 73.4% said they used violence first.

Stets and Straus (1992) combined the 1985 U.S. National Family Violence Resurvey data ( $N = 5,005$ ) with a sample of 526 dating couples to generate a large and representative sample of male-female relationships. Using a subset of 825 respondents who reported experiencing at least one or more assaults, Stets and Straus (1990) found that in one-half (49%) of the incidents the couples reported reciprocal violence, in one-quarter (23%) of the cases the couples reported that the husband alone was violent, and one-quarter (28%) reported the wife alone was violent. Men ( $n = 297$ ) reported striking the first blow in 43.7% of cases and that their partner struck the first blow in 44.1% of the cases. The women ( $n = 428$ ) reported striking the first blow in 52.7% of the cases and that their partner struck first in 42.6% of the cases.

Stets and Straus (1992) concluded that not only do women engage in a comparable amount of violence, they are "at least as likely" to instigate violence. The results also indicated that women were more likely to hit back (24.4%) than men (15%) in response to violent provocation by a partner. This latter result is difficult to explain, given the feminist argument that women are more afraid of male violence than the reverse. In all,



these data do not support the argument that female violence is solely defensive. Straus (1999) cites six studies that found equal rates of violence initiation by women. The Stets and Straus data contained another finding that was problematic for the self-defence argument; many women (12.5% of dating, 13.4% of cohabitating, and 9.6% of married) reported using severe violence against non-violent men. Corresponding rates for men using severe violence against non-violent women were 4.8%, 6.1% and 7.1%, respectively (Stets & Straus, 1992, p. 234). These data patterns replicated whether the respondents were male or female, making the issue of whether men and women respond differentially on the CTS irrelevant.

3. The Stets and Straus data replicate for Severe Violence-Minor Violence patterns.

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