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Addressing Violence by Female Partners Is Vital to Prevent or Stop Violence Against Women: Evidence From the Multisite Batterer Intervention Evaluation

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Abstract

This article presents a reanalysis of data from Gondolf's (2012) article in this journal on reoffending by men in the 15-month period subsequent to participation in batterer intervention programs. Gondolf concludes that violence by the female partners "was relatively low and does not appear to influence the program outcome in terms of men's reassault" (p. 10). The reanalyzed data lead to the opposite conclusion. The policy and practice implications are that the high rate of assault by women, including initiation of violence by female partners, needs to be addressed to enhance the effectiveness of programs to prevent and stop violence against women.

Keywords

assault, gender symmetry, partner violence, prevention, treatment

Gondolf's (2012) article, "Physical Tactics of Female Partners Against Male Batterer Program Participants," in this journal is a unique contribution to a critically important issue: women who physically assault a marital or dating partner. It is unique because it examines the relation of violence by women to violence by their male partners. Just how unique it is can be gauged by comparison with other studies of female partner violence. For example, there have been four special topic issues of this journal focused on partner violence. The 20 articles in those four issues provide a great deal of

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information explaining why women assaulted, but none except Gondolf's address the relation of violence perpetrated by women to violence by their partner. In addition to the articles in those four issues, there have been many others on female perpetration of partner violence, but only one has been located that provides empirical findings on the relation of violence by women to subsequent violence by their male partners (Feld & Straus, 1989).

The present article reports the results of a reanalysis of the data in Gondolf's (2012) article and identifies some of the implications for the effectiveness of batterer intervention programs, theories explaining violence against marital and dating partners, and for primary prevention of violence against women.

Gondolf presents data from his comprehensive study of batterer intervention programs in four U.S. cities. This is a major study that has resulted in many journal articles and a book, *Batterer Intervention Systems: Issues, Outcomes, and Recommendations* (Gondolf, 2002). The sample is large, and it covers programs in four cities. It is one of the few with data on the behavior of both partners. Thus, the study is highly suited to the central issue stated by Gondolf: "Does 'untreated' women's violence contribute to the 'failures' of batterer programs?" (p. 3). Both Gondolf's analysis and this reanalysis examine the relation of assaults by female partners to the probability of the male partner reoffending during the 15 months following participation in the program. The data are for 563 couples who participated in a batterer intervention program in four cities and their partners.

The "Low" Rate of Assault by the Female Partners

Gondolf asserts that violence by the female partners "was relatively low and does not appear to influence the program outcome in terms of men's reassault" (p. 10). He apparently bases this on his Table 3. This table cross-tabulates assaults by male offenders with female offenders during the 15 months subsequent to their participation in batterer intervention programs in four cities. It shows a 22% rate for women.

What Is a Low Rate of Assault?

Evaluating a 22% assault rate as "low" is somewhat like evaluating a glass as half-full or half-empty. There is no question that the glass is half-full, and no question that it is half-empty. The question is whether half-full is good or bad, or a lot or a little. To answer that question requires using criteria or standards. One can use a relative or an absolute criterion. One relative criterion is to compare the 22% assault rate of the women in the study with the 33% rate of the men. Using that criterion, the rate for women can be evaluated as low because the rate for men is about 50% greater. A difficulty with using this criterion is that it implies that because the 33% group commits more crimes than the group with a 22% assault rate, it justifies classifying a 22% rate of assault as "low." An interpretation that seems closer to the evidence is that 22% is the *lower* of two extremely *high* assault rates.

An absolute criterion would be to consider *any* assault as indicating violence. Few would disagree with this criterion in principle. In practice, though, this criterion is

applied sporadically. However, no published study has suggested, nor should any suggest, avoiding the erratic enforcement of the no-assault criterion by setting a threshold number of assaults beyond which the case is judged to be one of violence, or a threshold percentage of members of a group who assault for the group to be identified as violent. Readers will have to evaluate these criteria for themselves to decide whether the 22% assault rate of the women in this study is a low rate or a high rate.

Also relevant for evaluating Gondolf's labeling these women as low in violence is an analysis by Gondolf of pre-program violence by the women in the four batterer intervention programs. He found that "The majority (66%) of women reported being physically aggressive toward their partners prior to the initial arrest" (Gondolf, 1996, p. 27). Gondolf probably considers those to have been acts of self-defense and, therefore, not indicative of a high violence group. The next section evaluates that perspective.

Self-Defense

Gondolf points to the data in the right column of his Table 2 to show that for the relevant time period (the same 15 months used to measure assaults by men in their program), 44% of the women gave self-defense as the major reason for their violent acts during the period under review. This large percentage needs to be recognized. However, it also indicates that for slightly more than half the women (56%), self-defense was *not* the main motive. That also needs to be recognized.

Self-defense is a complex and often ambiguous phenomenon. For example, someone who initiated violence could a minute or two later feel a need to protect themselves. The incident then involves self-defense by both parties. The ambiguity of self-defense is also illustrated by assaults by women acting in fear for themselves or their child, or as a response to cues that an assault is about to happen. If these circumstances are counted as self-defense, the percent acting in self-defense would probably be higher.

Given these problems, a less ambiguous alternative may be to focus on who hit first. Gondolf (1996) did this in a previous publication and found that 40% of the women were the first to hit in the 12 months following participation of their partner in the program. This is similar to the 56% who did not act in self-defense mentioned in the previous paragraph. Eight other studies also report data for both men and women on initiation of partner violence in the form of hitting. The percent of women who said they were the first to hit ranged from 25 to 61, with a median of 46 (Straus, 2012b).

What prompted assaults by these women is related to the self-defense issue. The women in this study were all in a relationship with a history of violence. Many were probably responding to a history of abuse. Horrible and understandable as that is, it is not self-defense and also increases the risk of further attacks by the male partner. It is also morally and legally wrong. Violent responses do not end violence unless the opponent is killed or totally subjugated, both of which are rare and, when they occur, are usually abhorrent.

Table 1. Percent of Men Who Assaulted (in Bold Type) By Whether Female Partner Had Assaulted (Calculated From Gondolf, 2012, Table 3).

Assault by male partner	Assault by female partner	
	No assault	Assaulted
No assault	81% (<i>n</i> = 354)	18% (<i>n</i> = 23)
Assaulted	19% (<i>n</i> = 85)	82% (<i>n</i> = 101)
Total	100% (<i>n</i> = 439)	100% (<i>n</i> = 124)

The Cross-Tabulation

Important as the data on the rates of assault by the female partners are, the crucial issue posed by Gondolf is whether women's violence contributes to *reoffending by men* who have been in the program. To answer that question, Gondolf cross-tabulated assaults by men with assaults by their partners and presented the results in his Table 3. These assault data were obtained by interviews with the female partners of men in the program. They completed the Conflict Tactics Scales (CTS; Straus, Hamby, Boney-McCoy, & Sugarman, 1996), which ask about assaults by the respondent and by the partner.

Unfortunately, the percentages in Gondolf's Table 3 were computed in a way that is incorrect to answer his crucial main question. This is because he did not follow the principle that Hirschi and Selvin (1967) call "percentaging in the causal direction." This requires that the categories of the hypothesized cause be the basis for computing percentages. The percentages for the dependent variable must sum to 100% of each category of the hypothesized cause variable. (See Hirschi & Selvin, 1967, for further explanation and examples.) How does that apply to Gondolf's article?

Relation of Female Violence to Violence by Men

The issue that Gondolf's Table 3 is intended to address is whether violence by women adversely affected the success of the program to end violence by men. His implicit hypothesis is that it does not, that is, that assaults by female partners are not related to the probability of assaults by male partners. Thus, assault by a female partner is the hypothesized cause. Because in Gondolf's Table 3, the categories of women who did and did not assault are the columns, the principle of "percentaging the causal direction" requires computing the percentages so that they sum to 100 in each column.

To accomplish this, the number of cases in each cell was calculated to provide the basis of computing the percentages in a way that examines the causal theory. These cell *ns* were obtained by multiplying the proportion of cases in each cell by the total *N* of 563. Second, the resulting cell *ns* were used to compute the percentages of male perpetrators in the column for women who did not assault and in the column for women who assaulted their partner in this 15-month period. Table 1 presents the results of this reanalysis of Gondolf's Table 3 data.

Although the following the principle of percentaging in the causal direction does not always make an important difference, Table 1 shows that in this case it did. The No Assault *column* is for relationships in which the female partner did *not* assault. It shows that when their partner was not violent, 81% of the men also did not assault. The lower cell in that column shows that 19% did assault. Nineteen percent is still a high rate. It indicates that when the women in this study refrained from violence, although it greatly increased their safety, it did not guarantee safety.

However, the column on the right headed "Assaulted" shows that when the women in this study did assault, it almost guaranteed they would be attacked. The lower cell indicates that when women assaulted, 82% of their male partners also assaulted. Thus, violence by the female partner was associated with an 8 out of 10 chance of the male partner reoffending. Comparing the 82% attacked by their partner among the women who assaulted with the 19% who were attacked among the women who did not assault shows that in the 15 months subsequent to the program, violence by the female partner was associated with a 4 times greater probability of being assaulted by a male partner.

Does this mean that assaults by women are one of the *causes* of assaults by men? Not necessarily, because the data analyzed are cross-sectional; we do not know the sequence of events. It could be that the women were responding to assaults by the men, or it could be that the men were responding to assaults by the women, or some combination of the two sequences. In the absence of sequential data, the previously cited data from an earlier publication on this study can throw some light on this issue. In that article, Gondolf (1996) reports that 40% of the women were the first to hit in the 12 months following participation of their partner in the program. This suggests that in about 40% of the 82% of cases where both the male and female partners were violent, the causal direction runs from the female partner hitting first to the male partner also hitting, and that in about 60% of these cases, the causal direction runs from the male partner hitting first to which the woman responded. If that is correct, an assault by either the male or the female partner is associated with an increased probability of the partner assaulting.

The Relation of Male Violence to Violence by Women

Because the data in Gondolf's Table 3 are cross-sectional, the data can be used to provide information on the relation of male violence to violence by the female partner. This tests the theory that assaults *by men* are one of the many causes of assaults by women. That is both very plausible and probably the most widely asserted theory to explain assaults by women. As was the case in the previous analysis to test the theory about the effects of assaults *by women*, this test required recomputing the percentages in Gondolf's Table 3 to obtain the percentage of women who assaulted when the men did not, and the percentage of women who assaulted when the men did assault in the period after the program. The results show that if men who had been in the program did *not* subsequently assault their partner, only 6% of the women attacked them. However, if the male partner did assault, 54% of the women also assaulted. This large

difference is consistent with the theory that being attacked by their partner is one of the causes of partner violence by women (Stith, Smith, Penn, Ward, and Tritt (2004), just as the previous analysis was consistent with the theory that assaults by female partners is one of the causes of men assaulting their partners.

Does the finding that only 6% of the women assaulted when their partner did not assault contradict the conclusion that these women have a high rate of assault? It does not because it does not alter the 22% rate of assault by the women in this sample. Nevertheless, it affects the interpretation of what those 22% did. For example, the acts of those who assaulted when the male partner assaulted can be interpreted as self-defense. However, as was pointed out in the previous section, these are cross-sectional data and we do not know who attacked first. It could be the male partner or it could be the female partner. The 40% of women who had initiated violence during the study period also cited in the previous section suggests the self-defense explanation does not apply to 40% of the women who were attacked by their partner and were violent. These are extremely rough estimates. Therefore, the important point is that *perpetration* of assault by either the female or male partner is associated with an increased probability of the perpetrator being a *victim* of assault.

The Regression Analysis

Gondolf conducted two logistic regression analyses from which he concludes that “neither the women’s tactics ‘ever in the past’ [nor] ‘3 months prior to program intake’ was a significant predictor of the men’s tactics during the follow-up (p. 1036).” The first of the two analyses does not address what Gondolf states as the issue of concern: whether assaults by female partners in the post-program period were related to reoffending by the male partner. To do so, the analysis needs to use female violence as an independent variable, but it does not. It uses male violence as an independent variable. The results, therefore, show the effects of *male* violence, such as injury, use of services by the female partner, and so on. Those adverse effects of male violence have been found by many studies to be greater than the adverse effects on men of female violence (Archer, 2000; Gelles & Straus, 1988). Studies of injuries usually find that about two thirds of the injuries are sustained by women, including two thirds of the deaths that are at the hands of a partner. Therefore, *services* for victims, in contrast to programs to prevent and treat perpetration, need to continue to focus on female victims. However, that does not mean exclusive attention to women victims. There is a simultaneous need to recognize that the statistics on gender differences in injury and deaths also indicate that a third of those injured or killed by a partner are men. Those men also need help (Douglas & Hines, 2011). In relation to the main issue of Gondolf’s article—harm sustained by women—important as it is, does not address, much less contradict, the evidence presented up to this point showing that assaults by the women in the study are associated with an increased probability of assaults by their male partners.

The second regression analysis does address the main issue because it used assaults by female partners as an independent variable. Gondolf concludes from this analysis that after controlling for other characteristics of both partners, women’s violence was

not a significant predictor of the men's tactics during the follow-up period. That is literally correct, but there are reasons for skepticism about his implication that the results support his view that violence by women is *not* one of the causes of violence by the male partners.

First, there is inadequate information about the variables in the model tested. We are told they included variables for demographics, relationship status, the men's prior behavior, psychological tendencies, the men's abuse of their partners, the women's service contact and help seeking, and the women's perceptions of safety and quality of life. However, it is difficult to evaluate the appropriateness of some of those variables because contrary to Gondolf's statement that all the variables are explained in a prior publication, that publication does not adequately describe some key variables. What, for example, was used to measure men's "psychological tendencies" and "men's abuse of their partners"?

Second, the customary table showing the regression coefficients in stepwise order is not provided. This is needed to adequately evaluate the results, including, for example, to determine at what point adding other variables produced no further increase in explaining assaults by the male partner.

Third, because an independent variable becomes nonsignificant when other variables are controlled does *not* demonstrate that there is no relationship between X and Y. This fundamental point can be illustrated by imagining a study of race differences in income, which found that the average income of African Americans did not significantly differ from that of Euro Americans after controlling for differences in parents' education, quality of schools attended, years of schooling completed, and social capital of the neighborhood of residence. It would be erroneous to conclude from those results that race had no effect on income in the United States. The correct interpretation would be that those variables fully "mediated" (i.e., "explained") the race difference in income. The hypothetical results showing no significant difference would be extremely important because they suggest that steps to reduce those mediating variables are likely to reduce the income disparity, but they do not show that race is not related to income. Returning to Gondolf's regression results, it is erroneous to imply that the lack of a significant relationship after introducing the controls shows that assaults by female partners are not related to assault by male partners. The regression results may help *explain* the extremely strong relationship of assaults by female partners to the probability of assault by male partners, but they do not contradict it.

Discussion

This reanalysis of the data on recidivism by men in four batterer intervention programs found that when their female partner was violent in the 15-month post-program period, 80% of the men reoffended, compared with 19% when their female partner was not violent. Thus, assault by female partners was associated with a 4 times greater probability of violence against the women in this study. Although caution is needed in drawing causal conclusions from this finding because it is based on a cross-sectional analysis, it is consistent with what may be the only longitudinal study of this issue

(Feld & Straus, 1989) that found that assault by the female partner was associated with an increased probability of assault by the male partner 1 year later.

The greatly increased probability of men in these four batterer intervention programs reoffending if their partner assaults them is likely to be part of the explanation for the conclusion stated on the National Institute of Justice (2010) webpage, *Intimate Partner Violence*, which states that batterer intervention programs “do not change batterers’ attitudes toward women or domestic violence, and that they have little to no impact on reoffending.”

The results reported in Gondolf’s article show the applicability to a male-offender population of the theory that *violence by women* is one of the risk factors for *violence against women*. The results are consistent with other empirical studies that have found that a woman’s perpetration of violence is a strong predictor of her being a victim of partner violence. This includes longitudinal studies by [Feld and Straus \(1989\)](#); [Kuijpers, van der Knaap, and Winkel \(2012\)](#); and [Lorber and O’Leary \(2011\)](#) and cross-sectional studies by [O’Keefe \(1997\)](#) and [Whitaker, Haileyesus, Swahn, and Saltzman \(2007\)](#). Yet other studies are reviewed in the meta-analysis by [Stith, Smith, Penn, Ward, and Tritt \(2004\)](#) who concluded that violence by the female partner is the largest single risk factor for victimization of women.

The results summarized in the previous paragraph, together with Gondolf’s finding that 40% of the women had *initiated* violence during the post-intervention period and 66% had assaulted previously, suggest that an important step to increase the effectiveness of efforts to protect women from revictimization is to also provide the female partners of men in those programs with an “intervention” about violence. Obviously, there must first be empirical assessment to identify those who need such help. Gondolf’s (1998) study suggests that many of these women also need help with social and psychological problems, such as the 24% who reported a drinking or drug use problem. Treatment to meet the needs of these women would be part of a humane approach, and it would probably help reduce partner violence by both men and women.

Dyadic Types of Victimization

Many other procedures that might help to achieve a higher success rate need to be tried and rigorously evaluated. A simple but fundamental step to try is to begin treatment efforts by first empirically assessing which of the three Dyadic Concordance Types (DCT; [Straus, 2012a, 2013](#); [Straus & Michel-Smith, 2014](#)) applies to the cases being treated: Female-Only victim, Male-Only victim, and Both victims of assault by the partner. The importance of knowing this information is indicated by a review of 48 studies that obtained data on both partners and found that the typical pattern was that about half the cases were couples in the Both-Assault victims type, about a quarter were couples in the Female-Only type, and about a quarter were in the Male-Only type ([Langhinrichsen-Rohling, Misra, Selwyn, & Rohling, 2012](#); [Langhinrichsen-Rohling, Selwyn, & Rohling, 2012](#)). These results were found to apply to severe as well as minor assaults, to estimates based on data provided by women as well as by men, and to studies conducted in many nations, and regardless of the instrument used to measure

partner violence ([Langhinrichsen-Rohling, Misra, et al., 2012](#); [Langhinrichsen-Rohling, Selwyn, & Rohling, 2012](#)).

Use of DCTs is not only theoretically important but also a practical approach because DVTs are easily identified. This applies to both research cases and clinical cases. If necessary, DCTs can be identified with a single question, *provided that question is asked twice*. For example, to identify the DCTs for physical violence in a relationship, either partner can be asked whether they had hit their partner in the past year, and then asked whether their partner had hit them in that period. In clinical work, this instantly identifies the DCT of the relationship, that is, whether the pattern of assault victimization is Female-Only, Male-Only, or Both victims of assault by a partner. In research, DCTs are identified by simple cross-tabulation.

Although use of a single question is sometimes necessary because of time or other situational requirements, when possible, it is best to obtain more in-depth data, such as that provided by the CTS. The short form of the CTS takes only 3 to 5 min to complete. In addition to providing the data on prevalence of assault, the CTS also indicates frequency, and severity of physical assaults. The CTS also provides that information for four other aspects of victimization: injury, psychological aggression, sexual coercion, and intransigence by the partner. These are some of the most frequently mentioned aspects of *context* that must be determined to understand the situation of women who are victims of physical assault by a partner.

Conclusion

The results of this reanalysis provide evidence supporting the belief that the success of batterer intervention programs and the safety of women require efforts to prevent and treat violence by women as well as by men. Moreover, these results are also applicable to *primary prevention* of violence against women. Together with the results of other studies cited, they suggest that *explicitly* addressing violence by girls and women is crucial for prevention of violence against women. As the title of a 2005 article on treatment of partner abuse indicated, it is “Time For A Paradigm Shift” ([Stuart, 2005](#)). Identification of the DCT (Female-Only, Male-Only, Both assault victims) at intake is a practical step in doing that. Ultimately, there must be more program-specific evidence that a dyadic approach is more effective. This could be obtained by an experiment in which the DCT is identified at intake. Then, except for women in the Female-Only victim type, a random half of the women would be provided with an intervention that also included steps to address their violence and help for social and psychological problems as needed. The hypothesis is that providing these services to the female partner as well as the male partner will result in fewer assaults by the women and, therefore, will also result in less violence against women than that experienced by women who did not receive interventions for their violence and other problems.

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