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Why Straus's "Reanalysis" of Physical Tactics Used by Female Partners Is Wrong: A Response to "Addressing Violence by Female Partners Is Vital to Prevent or Stop Violence Against Women: Evidence From the Multisite Batterer Intervention Evaluation," by Murray Straus, *Violence Against Women, 20*, 889-899

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Abstract

This article refutes Straus's reinterpretation of our study, "Physical Tactics of Female Partners Against Male Batterer Program Participants," drawing on our extended follow-up of batterer program participants and their partners in four cities (Gondolf, 2002). Straus claims that the rate of women's violence in the sample is "high" and asserts the need to address women's violence to reduce the men's violence, which is the opposite of our findings and interpretation. We contend that our focus on the men and women who both used tactics in the total sample addresses the research question. We elaborate why our regression analyses to "explain" the women's violence are sound, despite Straus's unsubstantiated speculations. We argue further that the evidence points to women's "violent resistance" against severe, repeated violence, and that those cases do not fit the "both victim" dyad type that Straus promotes. Moreover, they are inappropriate for couples counseling. Finally, we

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revisit the limitations of the Conflict Tactics Scales (CTS) as a sufficient measure of the women's experience of male violence and raise concern about the implications of Straus's claims.

Keywords
batterer intervention evaluation, female partners, violence

Introduction

Not surprisingly, Straus's reanalysis of our study of the female partners of batterer program participants warrants a response (Gondolf, 2012b). Our findings suggest (a) that the extent of the women's use of physical tactics is "low," especially in contrast to claims from those promoting a "gender-neutral" position; and (b) that women's use of physical tactics is reduced as the men stop their physical tactics, except in cases of men who are repeatedly and severely violent (Gondolf, 2012b). Straus claims that the violence of the women is of great concern, is highly associated with the men's violence in the program follow-up, and must be addressed to prevent violence against women. His assertions, however, appear more as a reinterpretation than a reanalysis in which Straus superimposes his assumptions onto our results. In the process, he dismisses the major results, or bends them to fit his recommendations that are themselves in question.

While his assertions may be applicable in some cases, we found that they do not characterize the cases in our sample of batterer program participants, even when we used the controversial and limited physical tactics of the Conflict Tactics Scales (CTS) as a measure. While approximately 20% of the female partners reported using *any* type of physical tactic during a 15-month follow-up, nearly all of these women were with men who physically attacked them during that period. The contextual variables associated with the women using those tactics suggest more "violent resistance" than "mutuality or symmetry," as Straus and others have promoted in their gender-neutral perspective (Straus, 2011). Our results raise questions about the utility or appropriateness of couples counseling with that subgroup of couples, and about the implications for intervention in general.

A major concern among many practitioners is the implications of Straus's reinterpretation for batterer programming. First, does Straus's representation of women's violence provide a means to justify gender-neutral programming and couples counseling for the majority of cases, despite the many cautions surrounding those approaches? Second, does his representation inadvertently "blame" women and promote more justification among the men who batter ("She made me do it!")? Third, does it lead to more women accepting responsibility for the men's violent behavior, leaving them further entrapped in a possibly dangerous situation?

Rate of Assault by Female Partners

Straus's first point is that the 22% "assault rate" (as Straus refers to it) of the female partners during the 15-month follow-up should not be considered "low"; depending on what standards are used, it could be considered "high." In either case, the rate suggests, at face value, that the batterer program couples are not predominately involved in "mutual combat," as gender-neutral proponents would claim. Second, and perhaps more importantly, the rate does not indicate the extent, severity, impact, and context of the physical tactics, nor other forms of accompanying abuse, as mentioned in our article (Gondolf, 2012b, p. 1034). When these aspects are considered, the men's abuse and violence conform much more to "intimate terrorism." The women's tactics are generally less severe, the duration of their use is shorter, the resultant injury or distress is much less, and the reasons and issues associated with the tactics differ from those of the men. None of this means to excuse women's violence or overlook it, but rather to suggest that the "low" and "high" standards remain complicated by these factors. They leave us with a sense of "low" compared with other characterizations of couples associated with all-male batterer programs (e.g., Straus, 2011).

Straus also turns the women's report of "self-defense" into a question about the dynamics of the women's use of physical tactics. If 44% of the women using physical tactics reported doing so in self-defense, that means that most did so for other reasons and likely initiated the violence, according to Straus. As Straus acknowledges, as do we, the notion of self-defense is "often complex and ambiguous" and ultimately subjective. If fear and other cues are considered in the mix, the percentage using "self-defense" would probably be even higher. It is not clear why Straus fixes on this one descriptive response among so many other descriptors, except to move to a "less ambiguous" descriptor of "who hit first." He indicates here, and again in the Discussion section, that 40% of the women reported hitting their partners first. Straus references a Gondolf (1996) article published online that in fact does not have this statistic. That article discusses the men's reports of violence and also exposes the men's underreporting of abuse and violence, as do two other quantitative articles drawn from the database (Heckert & Gondolf, 2000a, 2000b).

Moreover, we continue to not know the dynamics, situation, and context of that "hit first," leaving it no less "complex and ambiguous" than "self-defense." A woman's "hit first" could be in the context of a partner trying to take her children, block the door of her leaving, throwing something at her, or threatening to kill her with a knife—all of which give very different meaning to "hit first."

Straus (2014) apparently raises these points to lead to the main assumption throughout:

> . . . [The women's violence] 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. (p. 891)

This assumption goes beyond the question and data in our study and, in itself, seems an overgeneralization that is open to debate and exceptions. Sages have long struggled with what ethically constitutes a "just war," and politicians continue to argue over the legality of police or military violence as a legally protected necessity.

Relationship of Female Violence to Male Violence

Straus claims our cross-tabulations of women's and men's tactics used during the batterer program follow-up (Gondolf, 2012b, Table 3, p. 1036) are "incorrect to answer [the] crucial main question." His Table 1 presents the percentage of "men who assault-edby female partners' assault" to demonstrate a high relationship between the women and men assaulting each other (Straus, 2014, p. 892). In fact, the principal results of this table are reported in the text of our article. However, our Table 3 addresses the "crucial main question" of what percentage of the *whole sample* are women and men both using physical tactics during the follow-up, which turns out to be 18%. This is the subgroup of concern: those couples that appear to have not been sufficiently helped by the batterer intervention. It would appear on the surface that this group might benefit from the couples counseling that Straus promotes. We acknowledge, along with Straus, that these findings, however, do not identify the sequence or interaction in the use of tactics, as well as the extent and severity. We add that they neglect a fuller context needed to interpret them and conduct two exploratory regression analyses to do so.

Straus dismisses these regressions as conceptually incorrect in one case and procedurally suspect in the other. Our aim in the first regression is to identify other contextual variables associated with the women's violence identified in the decontextualized cross-tabulations mentioned above. As our introduction pointed out, there are competing notions of partners who both use physical tactics: Are they best characterized as "mutual combat" or women using "violent resistance" amid men's "intimate terrorism"? In an effort to better "explain" the women's use of physical tactics, we use women's tactics as the *dependent* variable, rather than the men's violence as the dependent or outcome variable, which Straus claims is essential. Our regression results reveal predictors that would suggest "violent resistance" and associate the women's use of physical tactics with the most severely violent and unresponsive men: that is, the men's antisocial tendencies, prior arrests, verbal abuse, threats, repeated use of physical tactics, and injury caused, as well as the women's help-seeking in terms of prior shelter contact and welfare assistance. That leads us to question the claims of "mutual combat" in this sample and the recommendation for couples counseling.

Straus draws on general population studies to confirm the disproportionate injuries to women in general and suggests that the greater injuries sustained likely account for their heavier use of services. The implication is that these variables are not really part of the "explanation" of the women's use of physical tactics in our sample. However, in context with the other variables, injuries and help-seeking appear as part of the women's experience that would understandably lead them to desperately resist or attempt to cope in what some refer to as a "survivor" mode. Straus (2014) concludes, "The regression 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" (p. 895). That is what we were trying to do in this regression: "explain" it.

Our second regression showed women's previous and follow-up use of physical tactics was not a significant predictor of the men's physical tactics in the program follow-up. The conventional procedure of controlling for the men's background characteristics in a stepwise procedure reflects the study of risk factors used to develop risk assessment tools and previous "predictive" analyses we have conducted with our extensive data set, as identified in the article (e.g., Heckert & Gondolf, 2005). The results reflect the absence of women's tactics in risk assessment instruments. Straus concedes that our results are "literally correct," but then *speculates* at length on the possibility of alternative results. He suggests the control variables are not sufficiently defined (they are noted in the "Sample Characteristics," as well as the cited article above), the stepwise procedures are not fully defined (again, detailed in the referenced article and fairly straightforward and conventional), and mediators and interactions were not explored (Straus offers no hypotheses in this regard nor explains why such interactions would substantially alter the results). Admittedly, there is always further "data dredging" that can be done, but the regression results appear consistent with a backdrop of practitioner observations and prior risk factor studies with clinical samples.

Broader Research Debates

In the opening of his Discussion section, Straus (2014) draws on an isolated statement from the National Institute of Justice webpage asserting that "batterer intervention programs have little effect on offending" (p. 896). The implication is that this "little effect" is due to the failure to address women's use of physical tactics in the intervention, through couples counseling in particular. The "little effect" statement is likely based on the three experimental evaluations of batterer programs that have been widely debated and qualified as a result of conceptual issues and methodological shortcomings (e.g., Smedslund, Dalsbo, Steiro, Winsvold, & Clench-Aas, 2007). We offer several references supporting this point at the beginning of our article (p. 1029) and discuss it fully in our recent book on batterer program research (Gondolf, 2012a).

Straus (2014) further draws on longitudinal studies of general population samples to reinforce his assertion that "violence by women is one of the risk factors for violence against women" (p. 896). The extent to which these general population studies can be generalized to intervention or clinical samples is questionable for several reasons. As Johnson (2008, 2011) suggests in his analysis of population and clinical samples, the dynamics and severity of the violence in the clinical samples appear more as "intimate terrorism" and "violence resistance," than the less severe "situational couple violence" that typifies the population at large. Also, the general population findings do not answer the questions related to our intervention sample: What portion of batterer program partners use physical tactics, how do we best characterize those cases, and what do the results imply for intervention?

Moreover, as most in the field are well aware, there has been a long debate over the interpretation of results from general population samples using the CTS. Our article includes a long section of qualifications about these measures and their interpretation that Straus overlooks (Gondolf, 2012b, pp. 1038-1039). We acknowledge that the literature using clinical samples emphasizes the motives, impacts, dynamics, and context of events in what is termed the "constellation of abuse" (Dobash & Dobash, 2000). In our previous studies, the women's narratives of events show that physical tactics are typically just a part of the abuse that the women experience and fail to account for the severity and extent of that abuse (Gondolf & Beeman, 2003; Heckert, Ficco, & Gondolf, 2000). As Stark (2007) further argues, the focus on violent events is, in fact, a distraction from the loss of agency and justice for many battered women. This loss is too often compounded by limited resources and alternatives to their relationship situation.

Dyadic Types?

Straus finishes his critique by promoting research on the Dyadic Concordance Types: Female-Only Victim, Male-Only Victim, and Both Victims. He asserts that his CTS is a reliable way to identify the prevalence of these static types in clinical as well as population samples and, by implication, can lead to more appropriate intervention, including couples counseling. The field admittedly has been exploring various "types" of domestic violence cases to differentiate treatments and intervention, and it has learned a few things in the process. First, discrete typologies in general have been difficult to establish as reliable and consistent over time and are not very predictive of intervention outcome (Gondolf, 2011). Second, obtaining the information in a program intake interview or with a checklist to reliably establish a "type" can also be problematic (Campbell, 2005). Third, the CTS falls far short of the numerous risk assessment instruments and ongoing risk management that have been developed for domestic violence intervention and are shown to be much more effective in reducing domestic violence in clinical samples (Hanson, 2005).

In our study, even the physical tactics of the CTS alone point to a subgroup of women who might best be characterized as "violent resisters" to severely violent men. The context of those cases breaks down the tidy "dyad types." The types or categories that do emerge are not good candidates for couples counseling. The severely violent and repeatedly violent men are the most likely to drop out of programs, as are their partners from gender-mixed counseling (see Gondolf, 2012a, pp. 147-152). This "type" of couple warrants a greater concern for protection and support of the women, and more intense supervision, extensive treatment, and ultimately containment of their severely violent male partners.

Finally, Straus makes much of his opening point that "violence by female partners needs to be addressed to enhance the effectiveness of programs to prevent and stop violence against women" (Straus, 2014, p. 889). As we have argued, this assertion seems to be a misinterpretation of or overgeneralization from our data. However, our article does not deny that treatment and support for women who have been violent against their partners is warranted and helpful. As we mention in our article, previous articles in this journal bring out the development and utility of specialized programming for women who "use force against their partners," including physical tactics. These articles report that the experience, situation, and needs of these women tend to differ from those of their violent partners; these women, therefore, are best served in gender-based groups (see also Houry et al., 2008). We also acknowledged that some cases may respond to couples counseling, but a careful review of the couples counseling studies shows such counseling is appropriate for an exceptional subgroup of "low-level abuse" and intact couples and needs to follow extensive assessment and monitoring (Gondolf, 2011, p. 350).

There is no doubt that further research and program development are warranted in this area. We end up, however, with a very different set of recommendations in this regard and a very different research agenda than Straus. As we point out in our article, those directions are underway in the broad array of studies related to women's violence in prison, among youth, in ethnic and racial neighborhoods, and within intimate couples—and this research also extends to treatment and intervention efforts in response to such violence. Our hope is to have that work appreciated and acknowledged more fully by apparent detractors promoting their own entrenched biases.

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