

## The Distinction Between Type 1 and Type 2 Batterers— Further Considerations: Reply to Ornduff et al. (1995), Margolin et al. (1995), and Walker (1995)

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The authors address three comments on J. M. Gottman et al. (1995). The authors' Type 1 batterers engage in more severe violence than Type 2 batterers. Type 2 batterers are more likely to have witnessed unilateral husband-to-wife violence in their families of origin. The greater emotional abuse in Type 1 batterers is a robust finding. At certain critical moments of conflict interaction, Type 1 batterers' heart rates do indeed decrease, whereas Type 2s' increase. Type 1s may be vagal reactors (i.e., the heart rate reduction may be parasympathetically driven). Moreover, heart rate deceleration in Type 1s functions to focus their attention. Despite correlations involving physiological reactivity, no empirical finding could in any way exonerate batterers from moral responsibility. Finally, the authors discuss the political, clinical, and research implications of their work.

Since we first reported on the distinction between batterers based on physiological reactivity both the public and our colleagues have responded with a great deal of interest. Now, with three thoughtful comments from Ornduff, Kelsey, and O'Leary (1995); Margolin, Gordis, Oliver, and Raine (1995); and Walker (1995), the discussion has broadened. We are grateful for the opportunity to discuss further our findings. In responding to the comments by these three distinguished colleagues, we focus on the findings themselves, their possible meaning, and their implications.

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### The Distinction Between Type 1 and Type 2 Batterers

As Margolin et al. (1995) note, even though we do not yet know why heart rate reactivity was such a powerful discriminating variable in our sample, we have seldom dealt with a variable that has accounted for as much. After some reanalyses, we now realize that it does even more than we originally thought. We reported in Gottman et al.'s (1995) article that Type 1 batterers were more belligerent and contemptuous (i.e., more emotionally abusive) during marital conflict interaction than Type 2 batterers, more likely to be violent outside the relationship, more likely to have reported violence between their own parents, more likely to be antisocial and drug dependent, and were married to women who were sadder, more fearful, and less angry during the marital interaction. Furthermore, the couples with a Type 1 batterer were less likely to be separated or divorced 2 years later. All of these differences were associated with one variable: whether heart rate increased or decreased (from a baseline) during the first 5 min of a marital interaction.

### *Severity of Violence*

One of the initial surprises, in light of this set of findings, was that the two subgroups did not

differ in frequency of violence. This conclusion was based on analyses of the Conflict Tactics Scale (CTS; Straus, 1979). Using instructions from Straus, we weighted Items K through R by frequency and summed them into a violence scale score. What we were really focusing on by using the scale was the number of abusive acts committed during the year. In this manner, we were treating all acts of abuse the same, for example, a slap was given the same weight as being threatened with a knife or a gun.

We have now reanalyzed the data, focusing on each item of the violence scale separately, without a weighting procedure. The CTS data were examined by severity, item-by-item, for the two types of batterers. Using wives' reports, each item was dichotomized: "Did your husband commit this abusive act during the year, yes or no?" Then simple  $2 \times 2$  (domestic violence: Type 1 vs. Type 2; abusive act: yes vs. no) chi-square tests were conducted. These analyses answered the question of whether Type 1 husbands committed different abusive acts toward their wives than Type 2 husbands.

Type 1 and Type 2 husbands appear to commit different types of abusive acts, with Type 1 husbands committing the most serious types of offenses. Significantly more Type 1 husbands (38% compared with 4% of the Type 2 husbands) threatened their wives with a knife or gun,  $\chi^2(1, 56) = 5.66, p = .017$ , and Type 1 men were more likely to have actually used a knife or gun (9% compared with 0% of the Type 2 men),  $\chi^2(1, 56) = 4.17, p = .041$ . We also found that Type 1 husbands were more likely to have kicked, bitten, or hit their wives with a fist (91% compared with 62% of Type 2 husbands),  $\chi^2(1, 56) = 3.34, p = .068$ . More Type 2 husbands (80% compared with 55% of Type 1 husbands) slapped their wives,  $\chi^2(1, 55) = 2.90, p = .089$ . The Type 1 batterers were more severely violent, as we expected.

#### *Relationship Between Physiological Reactivity and Emotional Abuse*

As we reported in Gottman et al. (1995), the observational data were coded in two ways, both involving the Specific Affect Coding System (SPAFF; Gottman, in press). The first set of coding was summarized into frequency counts of particular behaviors (or affects), and on the basis of that analysis, Type 1 men were found to

be more emotionally abusive (belligerent and contemptuous) than their Type 2 counterparts. Because SPAFF coders were only moderately reliable the first time around, the data were recoded by the use of Affect Wheels that generated duration codes for each behavior. Despite the fact that, as Ornduff et al. (1995) note, duration data are not really comparable to frequency counts, in most observational studies duration data and frequency counts are correlated (as was the case in our study). We again found that Type 1 husbands were more belligerent and contemptuous than Type 2 husbands.

We are impressed with the replication, despite the conceptual differences between duration and frequency. Given that the primary purpose of the reanalysis was to see whether these results would replicate, we do not consider them to be post hoc analyses, and we are not concerned about the lack of significance in the overall multivariate  $F$  test; in fact, the multivariate  $F$  test has come under increasing scrutiny by statisticians in recent years for not being a viable way to protect against Type 1 errors (Huberty & Morris, 1989). Nor do we think that the moderate reliability during the first pass through the SPAFF makes our findings more tenuous. Our confidence in these findings is based in part on the fact that they were replicated with a more reliable method of SPAFF coding. In some respects, however, the moderate reliability in the initial set of codes makes the findings more impressive. Reliability minimizes error variance and makes effects easier to detect. We had to overcome a lot of noise to find these group differences. Moderate reliability is a much bigger concern when group differences are not found, because the null findings could be due to the large amount of error variance. In short, moderate interrater reliability does not make findings that do emerge more spurious, especially if they are predicted. Thus, the combination of replicating across methods of coding and the finding of group differences despite only moderate reliability adds confidence to the robustness of the differences between Type 1 and Type 2 husbands on belligerence and contempt.

#### *Relationship Between Physiological Reactivity and Violence Between Parents*

Margolin et al. (1995) note that we reported an unexpectedly low rate of witnessing parental

violence in Type 2 men; these authors' observation is based on previous research (cited in Margolin et al., 1995) that 20%–25% of children have witnessed such violence. In reexamining the analyses that we reported in Gottman et al. (1995), we discovered that they were misleading: Parental violence in that study refers to bidirectional parental violence, that is, both violence from father toward mother and violence from mother toward father. Absence of bidirectional parental violence does not mean that there was no parental violence reported in the home, because this category includes unilateral violence, whether inflicted by the father toward the mother or by the mother toward father, as well as no parental violence.

To clarify the parental violence results, we conducted additional chi-square tests, with more specific categorizations of parental violence: unilateral father-to-mother violence; bidirectional parental violence; unilateral mother-to-father violence; and no parental violence. The resulting chi-square still showed significant differences between Type 1 and Type 2 batterers,  $\chi^2(3, 56) = 9.87, p = .0197$ . However, the results are much more complicated than we thought. First, 22% of the Type 1 men reported no violence between their parents, compared with 49% of the Type 2 men. Thus, both groups reported much higher levels of parental violence than the general population. Second, 23% of the Type 2 men reported unilateral father-to-mother violence, compared with 0% of the Type 1 men. Third, and in stark contrast to the findings just reported, 78% of the Type 1 men reported bidirectional or unilateral mother-to-father violence, compared with 28% of the Type 2 men. In short, there was an association between type of parental violence and physiological reactivity in the offspring. If we make the assumption that unilateral husband-to-wife violence is most like the phenomenon of battering, the Type 2 men were significantly more likely to have witnessed battering than the Type 1 men, even though parental violence was generally less common for the Type 2 men. Moreover, parental violence was very high in both types. These reanalyses actually support a hypothesis suggested in Gottman et al. (1995) that the battering among Type 2 men has more to do with issues specifically reflecting attitudes toward women than the battering exhibited by Type 1 men. The Type 2 men may have been more likely to have had batterers as role models, and

one might speculate that the attitudes toward women inherent in battering (and the emotional abuse that invariably accompanies it) were passed on by the fathers to the sons.

The parental violence to which the Type 1 men were exposed was more common but also harder to interpret, because it seems to be a mixture of many different types. As Margolin et al. (1995) note, although previous research has shown that children do not habituate to such intraparental conflict, it may be that the Type 1 men do indeed habituate; that is, they may learn that the best way to cope with a stressful family environment is to not respond physiologically.

### The Meaning of These Findings

Much of the discussion in the comments by our esteemed colleagues revolves around how the findings of Gottman et al. (1995) should be interpreted. We have a bit more information now than we had at the time that article was written, and this additional information helps us to interpret the findings. However, complete explanations remain within the realm of speculation, and several interesting possibilities were suggested by the various authors who responded to our article. Space limitations compel us to be selective in our reply.

#### *Heart Rate Reduction or Hyporeactivity in Type 1 Batterers*

We believe that the interrupted time-series analyses reported in Gottman et al. (1995) confirm that our classification system was not corrupted by baseline artifacts. The time-series data provided solid evidence that the Type 1 men were lowering their heart rates rather than simply manifesting hyporeactivity. Where Ornduff et al. (1995) suggest other possibilities, they seem to be ignoring or minimizing the importance of the time-series data.

#### *Type 1 Men May Be Vagal Reactors After All*

The fact that the Type 1 men were lowering their heart rates does not explain how or why they were doing so; nor does the lowering imply a particular mechanism. In Gottman et al. (1995), we were puzzled by the absence of group differences in vagal tone, which is com-

puted as the proportion of variance in the heart period time series (time between R-spikes of the electrocardiogram) that is in the respiratory range. The *spectral density function* is simply the estimate of the proportion of variance accounted for as a function of the frequency bands in the data (determined uniquely by the overtone series of frequencies). The programs we used, developed by Porges (cf. Gottman, 1981), attempt to estimate the proportion of variance in the heart period time series that is within the respiratory range. Actually, the estimate is obtained by an algorithm that takes the heart period time series and presumably filters out all the frequencies in the data that are not in the respiratory range.

There is a potential problem with the Porges filter because Porges used a local cubic filter proposed by Kendall (1973) that is actually a nonlinear filter of the entire time series. The problem with a nonlinear filter is that it can distort the amount of variance accounted for by frequencies that are and are not filtered out from the data. Only a linear filter will prevent this potential data distortion. In revisiting this issue, we considered that a much simpler and more direct approach to computing vagal tone would be to compute an estimate of the spectral density function. In our reanalysis of the vagal tones of Type 1 and Type 2 men, we directly computed the percentage of the total variance in the heart rate time series that was in the respiratory range by taking the area under the curve within the respiratory range. There was no filtering of the data. Thus, our reanalysis avoided potential problems that could be encountered using a nonlinear filter. We computed the correlation between our estimate of vagal tone and the Porges method, and the correlation was .62 ( $p < .001$ ) for the interaction and .67 ( $p < .001$ ) for the baseline. We found no significant differences between the two types of batterers in baseline vagal tone,  $F(1, 58) = 2.45$ , *ns*. However, the differences between groups was significant for the change in vagal tone from baseline to interaction,  $F(1, 58) = 4.14$ ,  $p = .046$ . Type 1 men increased their vagal tone, whereas Type 2 men showed a decrease (for Type 1s,  $M = 1.09$ ; for Type 2s,  $M = -3.93$ ). Thus, we have some tentative support for the original hypothesis that the heart rate reduction was driven by parasympathetic activation. It may be that our original inclination to call the Type 1 men *vagal reactors* was justified.

### *Type 1 Batterers Versus Type 2 Batterers: Focused Attention Versus Defensiveness*

As we mentioned in Gottman et al. (1995) and as Margolin et al. (1995) amplified, heart rate deceleration in Type 1 men might very well be in the service of focused attention. That is, vagal activation during stress is associated with focused attention. Our best guess at this point is that the Type 1 batterers focus their attention to maximize the impact of their verbal aggression. Whether reductions of heart rate are voluntary or involuntary, they are probably learned, and they are certainly functional if the aggression is effective at controlling the behavior of the battered woman.

Margolin et al. (1995) also note that heart rate acceleration has been associated with stress, and they call this response a *defensive reaction*. They suggest that the Type 2 men may be overwhelmed by their emotional discomfort, and if not in the lab might very well withdraw from the situation; these men may resort to battering when withdrawal is impossible. We suspect that these speculations are incorrect. If anything, the Type 2 men, rather than being conflict avoidant, are the kind of men who confront their partner, restraining their wives from exiting and responding to anger with physical abuse that escalates when the wife tries to withdraw (Jacobson et al., 1994). In contrast, we found no evidence that wives kept Type 2 husbands from withdrawing, that these husbands withdrew from conflict in the natural environment any more than nonviolent husbands did, or that Type 2 husband withdrawal was correlated with any relevant wife behaviors. However, to address these issues more fully, we plan to conduct content analyses of argument descriptions with codes that are more sensitive to conflict engagement and conflict avoidance.

### *Physiological Causes, Physiological Correlates, and Questions of Moral Responsibility*

Margolin et al. (1995) raise several interesting questions regarding mechanisms, and they correctly point out that there is an important distinction between discovering a physiological marker, on the one hand, and positing a biological cause for battering, on the other hand. In

addition to the possibility that the Type 1 men may have learned this mechanism (heart rate reduction) as children, it could be that heart rate reduction is learned in the process of current or other recent relationships with women. To the extent that men believe that they are entitled to be in control of their relationships with women, and to the extent that calming down during arguments helps establish that control, it is not hard to imagine how heart rate deceleration could become well established in the process of becoming a batterer.

### *Miscellaneous Issues*

*The Type 1–Type 2 typology versus treating heart rate as a continuous variable.* Margolin et al. (1995) note that when continuous variables are dichotomized, they often lose some of their predictive power. This is usually true, but there are also times when dichotomous classifications are more predictive, namely in those instances where nature seems truly to be cut at its joints. The Type 1–Type 2 typology seems to be one of those cases. In other words, the relevant variable in our findings seems to be whether heart rate goes up or down, not the degree of increase or decrease. When heart rate change was treated as a continuous variable and correlated with relevant criteria, differences between the types were no longer statistically significant, although they were in the right direction. For example, the correlation between heart rate change (from baseline to marital interaction) and emotional aggression was  $-.24$  ( $p = .065$ ). In short, there do appear to be two types of batterers, and these types differ in a number of ways relevant to both theory and practice.

*The Type 1–Type 2 distinction is not unique to batterers.* Although the Gottman et al. article (1995) was limited to a domestic violence group, we also have data on nonviolent couples, both maritally distressed and happily married. Interestingly, the ratio of Type 1 to Type 2 men is very similar in these latter two conditions: In the maritally distressed group, 28% were Type 1, and 72% were Type 2; in the happily married group, 24% were Type 1, and 76% were Type 2. Given sampling error, these percentages were remarkably similar. Furthermore, within the maritally distressed group, just as within the domestic violence group, Type 1 men were more emotionally aggressive than their Type 2 counterparts, and the women married to the

Type 2 men were sadder, more frightened, and less angry than their counterparts in the maritally distressed group.

## Political, Clinical, and Research Implications

### *Political Implications*

We agree with Margolin et al. (1995) and with Walker (1995) that any examination of individual differences between types of batterers must be viewed within the context of cultural practices and political realities that, in various ways, perpetuate violence against women. In fact, we have made this argument ourselves in attempts to reconcile psychological research with advocacy (Jacobson, 1994a, 1994b). However, we have also noted that these sociocultural factors cannot possibly be a sufficient explanation for battering, because not all men are batterers. Moreover, because batterers do not batter 24 hr per day, 7 days per week, it is essential to understand the context in which arguments occur. Thus, scientific inquiries into battering must include examinations of individual differences between male batterers and the interactions of these men with their partners. The models that identify characteristics of men that make them prone to battering can only serve the cause of advocacy; ignoring individual differences results in the stagnation of knowledge that, in the end, perpetuates the risks to battered women.

There is no contradiction between identifying a physiological marker for Type 1 batterers and holding them personally responsible for battering. We have argued that the distinctions between correlation, causation, and moral responsibility must be maintained in order for our work to be interpreted correctly by the public, the media, and even by our colleagues (Jacobson, 1994b). Similarly, we agree, and have said in numerous contexts, that batterers should be held responsible for their battering, regardless of what its correlates are (Jacobson, 1994b; Waltz, Babcock, Jacobson, & Gottman, 1995). We have identified a correlation between physiological reactivity and certain characteristics of batterers. However, this correlation provides no basis for asserting that heart rate deceleration causes battering episodes. Even if we could establish causality, this would not exonerate the batterer from moral responsibility for the bat-

tering, nor would it in any way make violence more acceptable.

Margolin et al. (1995) also make the point that interaction research is problematic from a feminist perspective, because it is easy to jump from the observation of the interaction to the conclusion that women are somehow being blamed for the battering. Once again, we assert that questions of moral responsibility are not empirical. There is no empirical finding that would mitigate the unacceptability of battering, nor is there any empirical finding that would limit the batterer's responsibility for the violence.

We found Walker's (1995) sensitivity to the need for factual knowledge, regardless of whether it is consistent with current claims made by advocates, refreshing. Walker phrases her remarks in terms of political correctness versus incorrectness. However, we have discovered from speaking to advocacy groups that pundits may not always be accurate spokespeople for advocates. For example, when we reported that battered women were as belligerent and contemptuous during verbal arguments as were batterers (Jacobson et al., 1994), most advocates told us that this was consistent with their experience. And it makes good sense. If a person has been subjected to a history of emotional and physical abuse, regardless of how afraid that person is, he or she is also likely to be extremely angry. Being a trauma victim does not imply passivity, submissiveness, or docility. In fact, as Walker notes, even though we were selecting for husband-to-wife violence, in about half of our sample the couple would have met criteria for the domestic violence group had the selection been based solely on the woman's violence, according to the woman's self-report. However, as we also reported in Jacobson et al. (1994), we found no evidence of female battering in our sample. Battering is more than physical aggression: It is the systematic use of physical aggression to intimidate, subjugate, and control another human being. It is rarely something that women do to men (Jacobson, 1994a, 1994b).

### *Clinical Implications*

Ornduff et al. (1995) state that it would be premature to make clinical decisions on the basis of our findings. We could not agree more.

However, they proceeded to describe a population of batterers that could easily be mistaken for our Type 1s, and they suggest that this group of batterers would be less likely to respond to treatment. This suggestion is similar to the point we made at the end of the article (Gottman et al., 1995). If our findings are replicated, we may find that the Type 1 batterers will be less likely than Type 2 batterers to respond effectively to any form of psychotherapy.

We do not believe that any batterer should be denied treatment. However, we are critical of offering psychotherapy as an alternative to prosecution and punishment. Such alternatives are not offered to those who commit violence against strangers, and this double standard implies that family violence is less of a crime than violence against strangers. If batterers, whether they can be classified as Type 1 or Type 2, seek therapy on a voluntary basis, and if therapy seeking is unrelated to how the criminal justice system treats them, then we are all in favor of psychotherapy for batterers. However, batterers are fundamentally criminals, not clients, as Margolin et al. (1995) note. Battering is primarily a public health problem, not a problem for psychotherapists.

Although the Gottman et al. (1995) article was not about therapy and only a paragraph was devoted to discussing clinical implications, all three commentators made at least a passing reference to the apparent claims we were making about the clinical implications of our work. Walker (1995) particularly broadened the playing field by bringing in anecdotes about how to match certain types of batterers with specific psychotherapies. Given the sociopolitical context in which battering occurs, we are struck by Walker's apparent faith in psychotherapy for batterers. The evidence to date does not make one optimistic about the role of psychotherapy in reducing recidivism. The most recent reviews of the treatment outcome literature (e.g., Holtzworth-Munroe, Beatty, & Anglin, in press) have suggested that recidivism rates following currently existing treatments may not be lower than those that would have occurred in the absence of treatment and that those who complete treatment programs are no more likely than dropouts to desist from violence in the future. Our guess is that if psychotherapy is going to be successful in the treatment of battering, it will have to be integrated with a community-wide response so that there is coordination between therapists,

police officers, probation officers, prosecutors, judges, and advocates. In this respect, we agree with Walker (1995) that the so-called *Duluth model* (Pence & Paymar, 1993) seems particularly promising, because treatment includes attention to the entire social context of battering through the kind of community organizing we just described. Although this model has not yet been subjected to empirical scrutiny, it certainly merits such attention.

To summarize, we would never suggest that batterers not receive psychotherapy or that psychotherapists not continue to try to find helpful strategies for dealing with violence against women. However, therapists must confront the possibility that psychotherapy may not be the primary solution to a problem that begins in childhood, is supported by the culture, leads to drug dependence and antisocial personality disorder, and culminates in severe battering and general violence.

### *Research Implications*

Most fundamentally, we recognize the need to replicate our findings. We suspect that Type 1 men actually constitute more than 20% of the battering population involved in the criminal justice system. The profile of a Type 1 batterer is much more similar than that of a Type 2 batterer to the type of batterer who gets arrested. As one anonymous reviewer pointed out, Type 1 men might be more reluctant to volunteer for a research project. Thus, in addition to replicating these findings, we would like to know how common the Type 1 pattern is in the criminal battering population. Given that criminal batterers are often the ones who are referred to court-mandated treatment programs, the programs may be clogged with those batterers who are least likely to benefit from the treatments.

Many other suggestions for future research are offered, in particular by Margolin et al. (1995). Unfortunately, their most intriguing research question is probably unanswerable: To what extent do these physiological differences between Type 1 and Type 2 batterers hold up during violent altercations? We suspect that the distinction will break down during violent arguments, because physical force and all of its psychophysiological underpinnings pull strongly for sympathetic arousal in all men.

For us, one of the most intriguing sets of

questions involves the way in which our findings relate to the literature on criminality. We recognize that the connection is tenuous at best thus far, but we would like to know more. We also recognize that Type 2 batterers are criminals as well. However, they may not look as much like criminals on standard laboratory tasks, because their violence may be more circumscribed and have more to do with attitudes toward women, fear of abandonment, and emotional dependency than the characteristics that drive the Type 1 batterers. Margolin et al. (1995) point out that criminals show lower resting heart rates than noncriminals, whereas we did not find differences between Type 1 and Type 2 batterers in lowered resting heart rates. However, it is possible that batterers in general have lower resting heart rates than the general population. As Margolin et al. point out quite correctly, our baseline cannot be considered to be a resting heart rate, just a baseline that either increased or decreased as the interaction began. In short, it is still possible that either Type 1 men, Type 2 men, or both will show lower resting heart rates than nonviolent men.

As to the comparison between batterers and anticipatory heart rate responding in psychopathic individuals, Margolin et al. (1995) cite the work of Hare, who found that psychopathic individuals showed increased heart rate as they waited for an aversive stimulus. Margolin et al. suggest that this findings is in conflict with the heart rate decrease shown by the Type 1 men we studied. However, as they also note, the paradigms are not comparable; thus, neither are the findings. Furthermore, under certain conditions, psychopathic individuals do indeed show heart rate deceleration in anticipation of an aversive stimulus (Hare, 1982). We heartily agree with Margolin et al. that the context is extremely important when comparing groups on heart rate reactivity, and as yet there is little basis for comparing either psychopathic or nonpsychopathic antisocial men with either Type 1 or Type 2 batterers. Currently, we have an intriguing set of possible parallels awaiting rigorous investigation.

We also think that it is quite important to understand the mechanisms underlying heart rate reactivity and its developmental course before spinning elaborate theories about the role that it plays. There is a tendency in our field to equate a biological finding with a biological cause. Yet, we have already mentioned at least

two viable competing explanations for heart rate reduction in batterers where the reactivity is a consequence of learning history rather than a cause of violence. In the end, issues such as these can only be resolved by prospective studies.

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