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RE: A critique of the current Couples Therapy Study

Phyllis,

Thanks for sending the couples article: "Treating intimate partner violence within intact couple relationships: Outcomes of multi-couple vs. individual couple therapy by Stith et al. (*Journal of Marriage and the Family Therapy*, Vol. 30, 2004, pp. 305-318). The experimental study compares the 6-month outcomes in terms of re-assault and aggression, marital satisfaction, battering acceptance for completers of a couples group (n=16), individual couple counseling (n=14), and a smaller comparison group without treatment (n=9). The counseling both in the group and in individual couples treatment is characterized as "family violence focused" much like Geffner, Mills, and O'Leary have outlined. The proponents argue that it importantly attends to the interactions of the couple and the women's abuse or violence, as well as the man's.

The couples group produced the best outcomes compared to the apparently less effective individual couples counseling and the comparison group. One possible interpretation that is not fully discussed is that the individual couple counseling appears to have no affect over the comparison group, and may of itself offer some direct challenge to such counseling. The study consequently may be more an endorsement for group work and the apparent group effect that fosters public disclosure and monitoring of a private and often concealed problem. It does not, however, provide evidence for group couples counseling as an option over conventional batterer counseling as the authors suggest. For one, the sample of intact couples with low-level violence is so highly selective—largely self-identified with high recruitment attrition and extensive screening—that it applies to only the smallest subset of batterer program participants.

Unfortunately, this kind of research is being heralded as objective and in turn being used to dismiss the more conventional group programs for male batterers. The results are however over-interpreted or mis-interpreted in the face of several obvious research shortcomings and limitations. I outline below those shortcomings which apply to the few previous couples counseling studies as well.

1) The conceptual framework has a number of leaps that don't add up and are not what is really tested. The introduction lays out batterer program studies that apparently fail to impact

some men and batterer typology studies that appear to warrant different kinds of treatment for some “types.” It also suggests that violence among some women warrants couples counseling. These assumptions don’t apply however to the exceptional sample of intact, highly selective, apparently motivated, and low violence couples. The men in the conventional batterer programs who are “unresponsive” are clearly not the men eligible for couples counseling. They are the most violent and dangerous men who tend to be in very “un-intact” relationships. The violence among women that is referred to in the introduction also tends to be in response to the most violent and volatile men—again totally outside of the study’s parameters. The introduction also fails to address the limitation of the research on batterer programs, meta-analyses, and typology research which it uses as rationale for the couples counseling.

The findings in the couples counseling study are used to endorse outright couples counseling despite the acknowledged concerns in the introduction. By implication it also questions batterer program standards mentioned in the introduction as a constraint against couples counseling: “...the study calls into question standards that prohibit the use of couple therapy to treat domestic violence” (p. 316). The article will be used no doubt by opponents of established domestic violence intervention to further criticize it. The couples study appears particularly authoritative because of its experimental design (despite all its limitations and qualifications) which is promoted these days as the “gold standard.” There is fortunately more being written exposing the limitations of experimental designs in criminology and what one expert author refers to as, at best, the “bronze standard”. However, these writings are largely neglected in the current preoccupation with “evidence-based” treatments and interventions.

2) The most glaring and obvious limitation is the very highly selective sample in the couples study. Out of 700 calls responding to recruitment efforts, 364 completed an intake interview, 89 completed a pretreatment screening, and only 39 were included in the final analysis. That is less than 6% of the initial self-selected pool. In a study we did many years ago, we found that 20% of the women in a court sample of batterers indicated some interest in a couples education program, yet out of over 1000 women contacted about the option only a handful ever pursued it and none when for more than a few sessions. A similar result appeared in the experimental San Diego Naval study in which only 2 out of 10 partners attended any one session of a randomly assigned couples group, and this was a more highly motivated and stable sample than those from civilian batterer programs. The sample of the couples study, as a result, is much more highly motivated and less violent, and has a much more positive set of demographics, than any set of batterer program participants.

The sample is so exceptional, I’m not sure why one would put so much energy and resources into recruiting and identifying such a group, especially since this group of low-level intact couples appears to have a similar positive outcome when the man is in an all-male batterer program. The selection includes a 2-hour individual interview with each partner along with other screening devices. According to our study and others, these are the men who are most likely to have, qualitatively and quantitatively, the best outcomes in batterer programs. The real danger is that this article will be used to endorse and encourage

more couples counseling, but without the highly-selective screening and safety precautions. The conclusion does not emphasize the pre-conditions and high selectivity enough.

I would doubt that most couples therapists would have gotten the training and supervision that those implementing the treatment in this study. The domestic violence field has argued that a great deal of sensitivity and experience is needed to identify the denial, projection, and manipulation of many domestic violence perpetrators. Also couples may come forth because the man coerced the woman into treatment—because he claims it's her fault too. Moreover, clinical psychologists and family therapist are trained to focus primarily on interaction dynamics or psychopathology and as a result safety and justice may be neglected.

3) The results are based not only on a highly selective sample but a very small one: an untreated comparison group of 9 couples, 14 couples who completed the individual couples counseling, 16 couples who completed group couples counseling at the 6-month follow-up. There was no comparison to conventional batterer programs yet the implication is that couples counseling should replace it or at least be an option or alternative. The authors make the claim that the small sample reinforces the statistically significant differences in favor of the group couples counseling, and both couples approaches over the comparison group. I think you could argue the opposite with such an exceptional sample. However, I'm not convinced this is actually the case. The significance applies only to those in the highly selective sample, and the results could be shifted with only one or two couples reporting a different outcome. The actual "effect size" would be negligible because of the small sample size as well. Moreover, while the examined characteristics of the subsamples are similar, they may differ for the respective follow-up respondents.

4) A further example of the leaps is the comparison at the conclusion of the couples study outcome to the outcomes in our multi-site evaluation of batterer programs. The authors claim that their lower levels of reassault further endorse the utility of the couples approach. Yet there is no comparison here. The samples are dramatically different. Most striking, the batterer program participants are much more violent, the women in more danger and, most significant, the men are all coerced by the courts into the batterer programs. The couples sample is not only voluntary but highly selective. Also the outcome follow-up procedures and measures are different. The introduction of the couples counseling study in fact notes that the batterer program outcomes are different to compare because of methodological and sampling differences, yet the conclusion makes such a comparison.

Ed

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