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The Duluth model: A data-impervious paradigm and a failed strategy

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Abstract

Gondolf has criticized our review as being selective and suggests that the Duluth model is more promising than we had concluded. We note that his own outcome study showed a failure rate for a Duluth program of 40% – identical to the mean rate of the studies we reviewed. We see his critiques as representative of the very mindset we described in our original paper – that of the gender paradigm. We review some of the shortcomings of Gondolf's critique as representative of this mindset. Our conclusion about the failure of the Duluth program remains unchanged.

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“Never argue with a man whose job depends on not being convinced.” — H.L. Mencken.

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Although Gondolf takes us to task for “selectively” reviewing literature in our description of the failure of Duluth “psychoeducational” interventions, we reviewed all studies available. The only “new” study he presents is his own but it was already included in the meta-analytic studies we reviewed. Furthermore, it presents a result that is typically ineffective (40% recidivism rate) and does not change the results or conclusions. We reiterate the shortcomings of psychoeducational models; they were designed by and are promoted by persons with no therapeutic expertise. Gondolf’s critiques are arguments we have already answered, stemming from the gender paradigm that remains the chief problem in developing a more evidence-based approach to IPV intervention. Indeed, Gondolf’s approach provides yet another example of practices endemic to the gender paradigm: selective citing of research, and avoidance of assessment of female violence or male victimization.

In our paper, “Transforming a flawed policy” we reviewed empirical evidence that contradicted the feminist “gender paradigm” claim that intimate partner violence (IPV) was primarily a gender issue. Incidence statistics, we argued, demonstrated that in North America, personality disturbance and not gender, was a better predictor of IPV. All policies that were based on the gender paradigm were, *ipso facto*, flawed by being improperly premised (Wilson, 1983).

Gondolf has taken issue with our critique of psychoeducational models for court-mandated treatment of assaultive persons. He does not comment on the broader indictment we made of the gender paradigm as simplistic and wrong. We will confine ourselves here to the more serious mistakes in the Gondolf paper so as not to repeat the 160+ studies we cited in our original article, an article that Gondolf calls “highly selective” (p.23). The “distorted caricature,” as Gondolf calls it, of the Duluth model was cited directly from the Duluth manual (Pence & Paymar, 1993) with pages referenced — it is literally “from the horse’s mouth.” We will attempt here, so much as is possible, to not get dragged into a debate on gender-political agendas that Gondolf appears to want to initiate by citing web pages who refer to our article. That is irrelevant — it is a public article and people can do with it as they want. His claim of “the first author’s role as a founding member of a legislative advocacy group advancing his goals” is factually incorrect. Our agenda is simply this — to correct the outrageous misconstruing of intimate partner violence (IPV) by social scientists who should know better.

We will focus here on the research methodology and epistemology of the arguments. We reiterate that the Duluth program was based on an initial sample size of only 9, is a monolithic model of intimate partner violence and is contradicted by numerous large sample methodologically sound studies that we cited in our article. These include a meta-analytic study of 65,000+ respondents by Archer (2000) that found women to be slightly more violent (in terms of intimate partner violence (IPV)) than men. They also include a cross cultural studies of dating violence ($n=6900$) by Douglas and Straus (2003) that found college girls to be more violent than college boys across 17 countries. We could add to that the recent US National Survey (Gaudioisi, 2006) that found mothers were the most violent group in terms of physical abuse toward and lethality of children ($N=718,000+$) or Laroche’s (2005) ($n=25,876$) finding (in a nationally representative sample) that women used “intimate terrorism” (instrumental abuse) nearly as much as men. We wonder why Gondolf does not provide a “gender analysis” of these data (apart from rationalizing of women’s violence as self-defensive (even to their children?), or the measurement instrument as being flawed whenever it reveals female violence), or tell us how to reconcile them with the Duluth view that family violence is a “male power problem” (see pages 41–48 especially of the Duluth manual).

1. Duluth is not CBT

Gondolf refers to “Duluth-CBT” models. Let’s be clear: Duluth “psychoeducational models” are philosophically incompatible with cognitive-behavioral therapy (CBT) — the two are contradictory. Duluth believes in gender shaming as an intervention technique. We believe that it is based on an extreme, negative, and polarized view both of men and abusive men. For a review of the bedrock fundamentalist ideology of radical feminism, see Corvo and Johnson (2003). As Pence and Paymar put it in the Duluth manual:

“Using slavery, a colonial relationship, or an oppressively structured workplace as an example, the facilitator can draw a picture of the consciousness of domination.” Pence and Paymar, 1993 (p. 49).

We repeat this here because we feel it is important to know how gender relations are depicted in the Duluth model. This gender paradigm divides the world, in a Manichean fashion into “good” (female) and “bad” (male) based on Marxist notions of the bourgeoisie and proletariat (see Towards a feminist theory of state, Mackinnon, 1989, p.1). A type of socio-cultural “original sin” is imposed on males and all individual motivation and circumstances are rendered trivial in the face of irresistible instructions to serve the patriarchy. The result is a persistent differential attributional set (Kelley & Michela, 1980) for human action; all female transgressions are attributed to previous victimization by a

male, all male transgressions are viewed as originating with the male. There are two totally different *gestalts* for male and female violence. We reviewed studies affirming this in our original article. This is not a “caricature;” this is the philosophical basis of the gender paradigm and the Duluth model.

Psychological “treatment” assumes something is awry with an individual and therefore is incompatible with a sociopolitical view that IPV is essentially normative. We have already reviewed the overwhelming data showing this normative assumption is wrong and that attitudes supporting IPV are not normative. Only 2% of North American males agree that it is “OK to hit your wife/girlfriend to keep her in line” (Simon, Anderson, Thompson, Crosby, Shelley & Sacks, 2001).

The Duluth concept of gender shaming is anti-therapeutic and more consistent with political forms of “thought reform.” Lifton (1961, 1989) describes how shame was used to “reform” the bourgeoisie by having them “publicly admit” their advantages. In Duluth groups this comes to admitting “male privilege.” Since Duluth is based on a Marxist model of gender relations that replaces work with sexuality (see Dutton, 2006a,b, p. 95), this parallel is not coincidental. These “thought reform” techniques should not be confused with therapeutic techniques such as CBT which accept the clients’ personhood and where the therapist joins forces with the clients to change problem behavior. One finds no mention of CBT or Aron Beck in the Duluth manual. CBT was initially developed long before Duluth by Beck (1976) and begins with a measure of respect for the clients, enabling a therapeutic bond to develop. For the importance of this alliance in BIP groups, see Murphy and Eckhardt (2005) on “motivational interviewing.” CBT has been mixed in with psychoeducational models, usually by therapists frustrated with the Duluth approach (see also Babcock, Canaday, Graham & Schart, 2007; Babcock, Green & Robie, 2004). The Duluth program was not developed by persons with any therapeutic knowledge or experience. Gondolf’s attempt to invoke Paolo Friere as an influence on Duluth thinking is risible. Friere gets “tacked on” in one paragraph in the original Duluth manual to add some philosophical *gravitas* to an otherwise bankrupt position. His main idea, that of reducing the differential between teachers and students and valuing the reality of the student, is contradicted by the Duluth practice of negating the reality of “participants” and imposing the Duluth sociopolitical worldview onto them. Paolo Friere would not approve of the Duluth method.

In our view, assessment of attitudes consistent with abuse is a proper target of CBT; however, these may vary with individual male perpetrators and should be explored on an individual basis. One should not assume that sexist attitudes pervade male perpetrators. The research results on patriarchal beliefs and IPV are far from clear (see our original paper).

2. No elevated anger in abusers

One tenet of the Duluth philosophy is that anger does not cause violence (Pence & Paymar, 1993). Duluth proponents want to believe this because they insist that male IPV is always instrumental, in the service of female domination. This belief makes the Duluth perspective dismissive of impulse management and of CBT in general, which it reduces to “anger management,” though CBT has never focused primarily on anger, and anger management would be one of about two dozen treatment objectives in CBT (Dutton, 2006a,b; Maiuro & Avery, 1996).

This view is not supported by the extensive evidence we reviewed. Maiuro, Cahn, Vitaliano, Wagner and Zegree (1988) found that domestically violent men had significantly higher levels of both anger and hostility than controls. The authors concluded that their findings supported the “idea that anger dyscontrol is a key issue in the profile of domestically violent men” and noted that both depression and anger were elevated in this group. Margolin, John and Gleberman (1989) found that physically aggressive husbands reported significantly higher levels of anger than husbands in three control groups. When Dutton and Browning (1988) showed a videotaped husband–wife conflicts to wife assaulters and control males, the assaultive males reported significantly higher levels of anger than controls, especially in response to an “abandonment” scenario, suggesting that assaultive men may have specific anger triggers residing in attachment issues. Dutton and Sonkin’s (2002) application of attachment theory to intimate violence also contradicts this notion. Dutton and Starzomski (1994) found elevated anger scores for assaultive men on an anger inventory. They related the anger to certain personality disturbances, all of which have anger as a component. Dutton, Saunders, Starzomski and Bartholomew (1994) found elevated anger in assaultive males to be related to certain attachment disorders. Citing Bowlby’s (1977) work on attachment, which viewed anger as having a first function of reuniting with an attachment object and dysfunctional anger as further distancing the object, Dutton et al. (1994) explored the developmental origins of elevated anger in assaultive males, finding it to be produced by paternal rejection, exposure to abuse, and a failure of protective attachment. Failure to address these underlying issues therapeutically, while focusing on symptomatic beliefs and “male privilege,” would stand in the way of treatment success.

Jacobson, Gottman, Waltz, Rushe, Babcock and Holtzworth-Munroe (1994) recruited physically aggressive and maritally distressed non-violent control couples to discuss “areas of disagreement” in a laboratory setting. Maritally violent husbands and maritally violent wives both displayed significantly more anger than controls. (Although the study focused on husbands, 50% of the wives committed severe acts of abuse as well.)

In his reply, Gondolf claimed that Eckhardt, Samper and Murphy (in press) found “a relatively low proportion of men with “high level of anger” (p. 11). In fact, Eckhardt et al. actually found that 56/190 men clustered into High anger-expressive category on the state-trait anger expression inventory. Furthermore, Murphy, Taft and Eckhardt (in press) argued that state-trait anger inventories (such as used by Eckhardt et al.) may miss the relationship-specific anger in court-mandated men and should be worded more specifically to as to better capture this anger. These scales currently focus on non-intimate anger. Eckhardt, Barbour and Stuart (1997) reviewed several anger measures and argued that anger and hostility were both elevated in maritally violent men. Eckhardt, Barbour and Davis (1998) used an “articulated thoughts, simulated situations” technique that found maritally violent men articulated more anger-inducing irrational thoughts and cognitive biases than non-violent controls. In short, numerous studies from several independent sources have found anger to be prominent in physically assaultive males. It is misleading for Gondolf to attempt to cite one mislabeled aspect of one scale (which may not measure intimate anger) as “evidence” for his thesis that men in BIPs do not have elevated anger levels in their relationship functioning. As is clear above, the main thrust of Murphy and Eckhardt’s work has been on anger issues in BIP clients. It is hard to believe that Gondolf would not have known this and could cite their work as supporting his position when it has consistently not done so. For a review of additional studies on anger in intimate violence, see Eckhardt et al. (1997). In rebuttal of the substantial body of scholarship linking anger and violence, Gondolf cites an unidentified article in *US News & World Report* as a source on the efficacy of anger management.

3. Surveys

According to Gondolf, we didn’t deal with the questions of “responsible researchers” (by whom he means the gender paradigm group) re the survey data. In fact, this has, as they say in court, been “asked and answered.” We did answer these “questions” (see p. 459–460). The Conflict Tactics Scale (CTS) is sixteen times as sensitive as crime victim surveys (Straus, 1999). The only reason it is disparaged by gender paradigm adherents is because it produces results that are not ideologically acceptable: that women are often as violent as men in intimate relationships.

The arguments about “context” and “motive” have already been discussed in Dutton and Nicholls (2005) and in Dutton (2006a,b) (chapters 6 and 7) as well as by Dutton and Corvo (2006, p. 466–467). We cited evidence that suggests that there are only minor differences in motive or consequences by gender (e.g., Follingstad, Wright, Lloyd & Sebastian, 1991; Laroche, 2005). Here is our question to Gondolf, since Stets and Straus (1992) found, in a nationally representative sample, that women were three times as likely to use severe violence against a non-violent male than the reverse gender pattern (reprinted in Dutton & Nicholls, 2005, p., 687), how do you explain that? Does “severe violence” by women not hurt? Are these non-violent men not victims? How can this be “self-defensive” violence? Furthermore, if you cannot explain this, how can you then proceed with a Duluth view of BIP’s that argues all female violence is self-defensive?

4. Unilateral violence

Duluth programs still operate on the gender-political assumption that male violence is always unilateral and any mention of female violence is “victim blaming.” Research carried on in transition houses does not allow questions about female’s own use violence. Yet, as we cited, the Stets and Straus’ national survey data that assessed for gender of the perpetrator and level of severity found mutual violence, matched for level of severity, to be the most common form of IPV (followed by “husband battering,” then by “wife battering”). When a male client in a Duluth group makes the mistake of mentioning his wife’s use of violence, he is accused of “victim blaming” or being “in denial.” In fact, he is likely to be describing reality and without a proper assessment of the woman’s own reported use of violence, the “facilitator” just does not know. The Duluth model has already discounted the client’s reality.

This limited view is reflected in Gondolf’s own research, reported in a series of papers on his multi-site study (Gondolf, 1996, 1999, 2000a,b; Gondolf & Jones, 2001; Jones & Gondolf, 2002) through which he repeatedly implies that there is something superior about a “multi-site” study but admits “the four sites reflected regional differences in

demographics, but had relatively similar portions of men with mental and drinking problems” (Gondolf, 1996, p. 4). As is typical with the gender paradigm, his research questions focus on male violence, and whether or not the men in these programs have violent female partners is de-emphasized. However, even through this filter the following data emerged: “66% of the women reported being physically aggressive toward their partners prior to the initial arrest” and 15% were also arrested when the man was, 25% were heavy drinkers (1996, p. 39–40). Sixty percent of the women said the man had hit them first. Hence, forty percent had hit the man first. Gondolf’s focus exclusively on characteristics of the male “perpetrators” is reminiscent of our critique of the Jacobson et al. (1994) same focus on male “cobras” and “pitbulls” when half of their female “victims” “would have qualified for the DV group if wife violence had been the criterion” (see Dutton & Corvo, p. 467). Gender paradigm studies routinely ignore and discount female violence. Was female violence included then in the Gondolf’s structural model used to predict recidivism (Jones & Gondolf, 2002)? No it was not.

Just as with the criticisms we made of the Reitzel-Jaffe and Wolfe study (2001) in our original paper, Gondolf’s recidivism “predictions” are entirely based on an unwarranted assumption of unilateral violence by the males.¹ In fact, gender politics typically rules out asking research questions about female violence. Any feminist “researcher” operating with a transition house sample is *prima facie*, prevented from even asking the question. When, however, it is asked, the results indicate high levels of female violence, including samples of women partnered with men in BIPs (Stacey, Hazelwood & Shupe, 1994). If we really wanted to predict recidivism, would it not matter if a group-client was returning to a relationship with a violent woman? It does not matter how sophisticated a statistical design is, if crucial variables are left out, the results will misrepresent reality. Forty percent of Gondolf’s assessed “batterers” return to women who had hit them first and who, according to Gondolf’s own research, have nothing more to fall back on than leaving the situation (Gondolf, 2000b, p.1213). Is it surprising that Duluth recidivism rates are around 40%?

5. Patriarchal beliefs and violence

Another shibboleth of Duluth is that male intimate violence can be stopped by altering “patriarchal beliefs.” The problem is that the evidence that patriarchal beliefs cause violence has very little empirical support. A recent meta-analytic review by Stith, Smith, Penn, Ward and Tritt (2004) assessing evidence for “traditional sex role ideology” found mixed results for patriarchal ideology and IPV (reviewed in Dutton & Corvo, p. 276).

Duluth proponents fail to understand that thoughts, emotions, and behaviors are inter-related and that this interaction is multidirectional. For example, under the influence of strong emotions, thoughts or beliefs may become distorted, which in turn will lead to particular behaviors. Pre-existing thoughts or beliefs may exacerbate emotions (such as “I can’t depend on anyone for love”), also leading to problematic behaviors. Behaviors can trigger thoughts or emotions that were not present before the action was taken. For example, one can smile and shortly afterward begin to feel happy. Additionally, many behaviors can occur in a mindless way, with little awareness in thought or emotion (walking or riding a bike, for example). In other words, the Duluth model of cognition and behavior is inaccurate and simplistic and fails to capture the true complexity of the multi-layered dyadic interaction of individuals (Burman, Margolin & John, 1993; Leonard & Roberts, 1998; Margolin et al., 1989).

6. Externalizing Duluth failure onto other causes

By any reasonable empirical treatment-outcome standard, Duluth intervention is a failure. Duluth interventions cannot succeed because they lack the essential elements for therapeutic success. If one reads the works of experienced therapists (Schore 2003a,b; Yalom, 1975), one sees that the development of a therapeutic bond is essential for treatment to have an effect. The outcome studies we reported made it clear that the Duluth model had zero effect. However, as with the issue of women’s violence, advocates with an ideological mindset do not accept refuted hypotheses; they simply redefine the issue to keep the ideology alive. This is what happened with the failure of Duluth. Gondolph (2001) interprets the failure of Duluth as a failure of the “coordinated community response” that supports the system. He says that “the program’s success; consequently, reflects the effectiveness of the system in establishing consistent police and

¹ We reviewed evidence showing that judgments of whether an action was abusive differ by gender of perpetrator. These judgments are made by police too. We also reviewed evidence that police arrest males more frequently, even in cases where the male was the injured party. The “designated perpetrator,” called by the police, is used to slot many mutually violent relationships into “perpetrator and victim categories that have little bearing on the mutuality of violence” (Stets & Straus, 1992).

court action for domestic violence cases... and providing ongoing outreach and support to victims.” A recent evaluation of the psychoeducational group in Quincy, Massachusetts found a 50% recidivism rate for 342 completers based on police reports up to ten years after completion (Wilson & Klein, 2006).² The reports authors also saw this not just as a failure of a psychoeducational program (in this case, EMERGE) but of the whole system. However, only a quarter of the men arrested did not attend EMERGE but 60% recidivated. The program success is preserved in these cases, by spinning the interpretation of the outcome. Criminal justice system effectiveness may be important, but it is equally important for CBT programs that outperform Duluth. Gondolf, like Dobash, Jaffe, Kimmel, Dekeserdy, and other feminist advocates, are unwilling to recognize when a hypothesis has been disproved but are willing to find confirmation in any supporting data whatsoever, no matter how scant. One is reminded of the actions of the members of a doomsday cult reported by Festinger, Riecken and Schachter (1956) who sought publicity after their prediction of doomsday had been disconfirmed.

7. Power and control

According to the Duluth model, “power and control” are the prime motives for IPV but only in males. Females do not suffer from this motivational affliction; according to the Duluth model, they have no needs for power or control. The essential psychology of males is completely different from that of females. In fact, males and females describe using “power and control” motives equally (Follingstad et al., 1991) and dominance in marriages is about equal by gender (Coleman & Straus, 1986). The Duluth model, which was not written by psychologists, lacks a basic psychological insight yet claims to be able to change human behavior. We reviewed the in-depth work on the power motive by McClelland (1975) in our paper. If Gondolf understood this, he would see that human motivation does not differ so much by gender. Power motivation generates such diverse actions as running for political office, stamp collecting, and seeking a relationship with an incarcerated killer. Advocates who insist that only their model of intervention can be state sanctioned and funded are also operating on the same power motive. The persistent failure of the Duluth model to appreciate psychological insights is the key bulwark to establishing dialogue with Duluth activists. Their “us–them” approach to the work included a false dichotomy about human motivation. Dutton (2007) argued that anxiety, specifically attachment anxiety, is primary to power motivation in intimate relationships and provides a universal motivation which requires therapeutic attention in BIPs. Several studies support this view: Mauricio, Tein and Lopez (in press), Fonagy, Target, Gergely and Jurist (2002) and Schore (2003a). Nowhere is a sociopolitical model more absurd than when it tries to describe human motivation in terms it clearly does not understand. It is for this very reason that Duluth programs generate superficial compliance in court-mandated groups followed by high recidivism rates upon group completion.

8. We were selective

The meta-analytic studies³ we cited included all treatment outcome studies available at the time. We cited Shepard’s (1987) finding of a 40% recidivism rate within 6 months following a Duluth program as well as other failures to find effects from Duluth (Babcock et al., 2004; Davis, Taylor & Maxwell, 2000; Feder & Wilson, 2005 meta-analysis). Gondolf argues that, if only we had consulted his study, we would have a different view of this matter. But Gondolf’s study was already included in the meta-analytic study we cited (Babcock et al., 2004). It made no difference to her results. Duluth still failed. A more recent meta-analytic study (Feder & Wilson, 2005) also included the Gondolf results (Jones & Gondolf, 2002). The authors concluded “the mean effect for victim reported outcomes was zero” (p. 239). So much for the “selectivity” problem.

Gondolf (2000a) reports 41% recidivism rate based on wives reports in a 30 month follow-up. The Quincy psychoeducational group, cited above, had a 50% failure rate. Our CBT group had a 21% rate for completers, nationwide for up to 11 years after treatment completion based on police reports (Dutton, Ogloff, Hart, Bodnarchuk & Kropp, 1997). We found a 16% recidivism rates based on wives reports for up to 2.5 years after CBT (Dutton, 1986). Men in Duluth

² The studies’ author, Andrew Klein, points out that the study was of the entire intervention system in Quincy, not just the men who went to the psycho educational program (called Emerge). 25% of the arrested men were not prosecuted, the rest were arrested and went to Emerge. Ten year follow-ups revealed a 60% re-arrest rate for all initially arrested men (personal communication).

³ Gondolf’s specious argument that meta-analytic studies are not perfect (a “bronze standard”) notwithstanding. Of course they are not perfect, just superior to the biased single samples (taken from transition houses and male treatment programs, then generalized to the entire population) passed off as “representative” by the “responsible researchers” of the gender paradigm for the last 20 years (Dutton, 2005, 2006a,b) on this matter.

programs quickly learn how to comply with the gender-political demands of the “facilitator” but there is little or no real change. Gondolf’s own recidivism rate is average for Duluth outcomes. How then could it change our conclusion?

Against this consistent body of work showing little or no positive effects from Duluth-type interventions, Gondolf places a great deal of importance on his own “multi-site” study. It is beyond the scope of our response, but readers are urged to critically review the methods and findings from this study with particular attention to the exclusion of one of the four sites from analysis, the effects of attrition, and the calculation of propensity scores to allow for “comparability” of program completers to drop-outs. Gondolf discredits experimental designs in favor of this quasi-experimental design with the proviso: “This more sophisticated analysis with quasi-experimental design controls for contextual factors (e.g., referral source, availability of services, local unemployment rate), as well as an array of batterer characteristics, and is arguably better than results from a poorly implement experimental design with intention-to-treat assumptions” (p.21).

He concludes: “In light of the implementation problems in the experimental designs and the contradictory evidence from the multi-site study, a definitive dismissal of the Duluth program based on program evaluations is unwarranted” (p.21) This seems to mean that since the Duluth model cannot be definitely refuted in *every* study, it should retain its hegemony in shaping domestic violence policy and practice. This is an astounding premise to maintain a mandated practice. In essence the burden of *disproving* efficacy replaces the burden of *demonstrating* efficacy. The premise runs counter to the basic principles of evidence-based practice, evaluation research, and professional codes of ethics.

Much of Gondolf’s work was on predicting recidivism in male perpetrators (Gondolf, 2000a,b; Gondolf & Jones, 2001). He credits Duluth with the “first ‘Lethality Checklist’” (p.14) but does not tell the reader that it was never validated. Gondolf reported that main procedure that clients used to “avoid re-assault” in the program he evaluated was leaving or taking a time out. That these men neither acquired nor used any skills beyond these procedures is telling.

We would argue that it is Gondolf and the gender paradigm adherents who have been selective. This is a group that holds beliefs outside the normative beliefs of the host culture (e.g., sexuality is to feminism what work is to Marxism, only males are domestically violent), believes its ideology is “correct” and is resistant to disconfirming information. The gender paradigm selects out any and all research that finds females to be violent (Archer, 2000; Stets & Straus, 1992), unilaterally violent (Stets & Straus, 1992), or violent towards children (Gaudioisi, 2006; Trocme, 2001). It disregards research that shows developmental trajectories for violent females or how such females select or create violent men (Moffitt, Caspi, Rutter & Silva, 2001; Serbin et al., 2004). It disregards studies on dyadic generation of violence and the role that women play — not passive but active and aggressive (Jacobson et al., 1994).

Here is Gondolf’s reading of the Garner, Fagan and Maxwell (1995) summary of the arrest studies: “overall, the analysis of the pooled arrest studies emphasize the impressive impact of arrest by itself on domestic violence re-offense” (Gondolf’s version, citing Garner et al., 1995). Here is what Garner and Maxwell (Garner & Maxwell, 2000) actually said in 2000:

“The contemporary policy discussion surrounding the appropriate societal responses to domestic violence includes numerous suggestions for mandating arrest, coordinated legal and social service responses, the use of protection orders, offender treatment programs, intensive responses to high-risk situations and the prosecution and incarceration of offenders. These suggestions do not appear to be derived from, nor tested by, systematic empirical research that approaches the standards of the National Academy of Sciences and met by the police arrest studies. *The current discussions and policy options appear to be driven more by personal preference and ideology of the currently powerful than any real evidence about the safety of victims or behavior of suspects subjected to these plausible but untested approaches.* (Italics are ours) (Garner & Maxwell, 2000, p. 109).

In belief perseverance (Lord, Ross & Lepper, 1979), self-serving interpretation of the data (called “biased assimilation”) is a central feature. Gondolf seems stuck in the past, still going on about “anger-management techniques” (as if this was ever an issue) and citing the Johnson (1995) false dichotomy (of common couple violence and “intimate terrorism” as though there was no female generated violence) as though it were a fact. We repeat our assertion that it is time to move on and that the only way to do that is to drop the gender paradigm allowing new perspectives to solve this problem.

Gondolf’s approach to his research and critique of our paper really represent nothing new. The problems with the conceptualization and interpretation of data in his design are the same as with studies we already reviewed in our original paper: leave women’s violence out of the equation even though it is frequent and then build a “house of cards” based on illusions of males acting unilaterally, in some sort of relationship vacuum. The gender paradigm is really not social science that tries to find the truth. It capitalizes on self-selection (shelter house samples of women, criminal

justice samples of men) and tends to de-emphasize any information at odds with the ideological conclusion drawn long before the research was conducted. One could write this off as merely poor scholarship, except for the fact that all paradigm studies err in the same direction: that of the fundamental belief structure constituting the paradigm. It is the use of science for political ends — in this case gender-political.

It is clear that Gondolf's reply was meant to shift attention from the empirical, epistemological, and practice failings of the Duluth model to a sort of political debate: "There are, admittedly, differences in approaches to dealing with domestic violence perpetrators being played out by *political factions in turf wars*" (italics added) (p.2). This is an effort to frame what is primarily a matter of science, theory and empirical verification to one of arbitrary ideological debate. One sees similar rhetorical machinations by creationist adherents endeavoring to counter the scientific fossil record.

Finally, it is long past the time for the gender paradigm adherents to be attempting to take the moral high road ("responsible researchers"). The "currently powerful" (as Garner and Maxwell called them) misrepresent data and bully treatment providers, insisting that only they know the truth (Gelles, 2001), and using the status of "state sanctioned treatment providers" as a means to generate compliance. The gender paradigm advocates are obstructing evidence-based practice⁴ that could improve the lot of both men and women. Gondolf's recommendation that we listen to the "wisdom of activists" returns up to what Babcock et al. (2007) call the "Dark Ages" of batterer intervention and will merely repeat the mistakes of the past.

The current positions of the responsible researchers, as Gondolf calls them, remain unaltered by the tsunami of data we originally reported. Dobash and Dobash (2004) still refer to the "context of violence," avoid citing papers such as Simon's (Simon et al., 2001) demonstration of no normative support for male IPV, and publish results based on interviews with couples selected because of the man's arrest for IPV. Ignoring the selection factors this sample poses (of differential calling of police (Stets & Straus, 1992) and differential likelihood of arrest (Brown, 2004)), and stating that women arrested for IPV are too rare to study, they continue to defend the patriarchy position. They dismiss contradictory empirical studies as "act based" implying that these studies are too narrow and setting the stage for the rejection of empiricism completely, in order to preserve the ideology. After rejecting "act based" research, they then go on to count "violent events" as though that were a vastly superior method. They report that men underreport the violence, while ignoring the fact that these men had been arrested and could face more serious penalties if they disclosed repeat violence (Gondolf does this as well and attributes the under-reporting to gender not circumstances). DeKeseredy, who drew conclusions contradicting his own data (but consistent with the gender paradigm), decried the Canadian government for surrendering its "mandate" to his special interest group and conducting a survey that asked men about their victimization (DeKeseredy, 1999; DeKeseredy & Schwartz, 2003). He saw this as "backlash" and still remains convinced that governments should conduct biased surveys to perpetuate the gender paradigm. The Duluth writers (Shepard & Pence, 1999), while acknowledging that a model developed for a small town may not work elsewhere, have essentially unchanged views of IPV. It is male violence towards women. They follow the pattern of emphasizing statistics that seem to most favor the gender paradigm such as spousal homicide, a relatively rare event, calculated in rates of 3 per 100,000 marriages.

There are some reasonable conclusions that arise from a review of the data we presented in our original paper. Some sensational cases of partner abuse seem to be gender based. We do not hear of men kept prisoner in chronic abusive marriages as we may hear of women in these circumstances. The questions are whether these sensational cases are representative of typical abuse in community samples and whether they represent a conjunction of the gender of the perpetrator and extreme personality disorder.

Also, it is may be the case that abusive men are generally irremediable and only some may improve through intervention. These seem to have a stake in conformity (Sherman et al., 1992) and the personal motivation to improve and they are the ones who adhere to arrest-treatment and do improve. However, the possibility still exists that there is a larger group that could be treated successfully and that the best treatment system has not been tried yet. This treatment would use established CBT procedures and would treat abuse, attachment, personality disturbance and substance abuse. This program would require the use of professionals; psychologists, social workers, and marital therapists and would eschew the advice of non-professional advocates. Finally, now that women are being arrested for IPV and are found to have personality disorders like male perpetrators (Henning, Jones & Holford, 2003), one wonders how they could be treated by a model emphasizing their slavery in a patriarchal world.

⁴ A shelter in central New York had its BIP funding entirely eliminated by the State Office for the Prevention of Domestic Violence for incorporating modest CBT strategies into its program.

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