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# Aggression and Violent Behavior



## Gender and types of intimate partner violence: A response to an anti-feminist literature review

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### ABSTRACT

This article presents a feminist perspective on domestic violence that is rooted in an explication of the differences among three major types of intimate partner violence (Johnson, 2008). Theory and research from this perspective is then reviewed to rebut recent attacks on feminist scholarship and policy regarding intimate partner violence.

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## 1. Introduction

The most recent of a series of anti-feminist attacks from Dutton, Hamel, and their colleagues is “The gender paradigm in family court processes: Re-balancing the scales of justice from biased social science” (Dutton, Hamel, & Aaronson, 2010), an ironic title, given the panoply of biases with which it itself is riddled. In this particular article they claim to expose two recent papers (Jaffe, Johnston, Crooks, & Bala, 2008; Kelly & Johnson, 2008) as biased and unsupported by research evidence. Responding to this particular attack is useful in itself, but their article also serves as a good example of the substance and tactics of their more general anti-feminist critique. In the process of responding here to their allegations about feminist theory and research, I hope to accomplish two goals. First, I will present a feminist perspective on domestic violence that is rooted in an explication of the differences among the major types of intimate partner violence (Johnson, 2008). Second, theory and research from this perspective will be used to rebut the Dutton et al. claims about what they call “the gender paradigm,” which includes my own work.

## 2. A feminist perspective on domestic violence

It is probably useful to begin by saying that there is more than one feminist understanding of the nature of domestic violence, more than one “gender paradigm,” just as there are multiple feminist perspectives on anything. What I will present here is my feminist perspective on the nature of intimate partner violence, a perspective formed primarily from a wide reading of over thirty years of research on “domestic violence,” and informed by feminist perspectives from my home discipline of sociology.

### 2.1. A feminist perspective on types of intimate partner violence

The core proposition of this perspective is simple: there is more than one type of intimate partner violence, and the major types differ dramatically in almost all respects (Johnson, 2008). The typology that I began developing in the early 1990s is organized around the concept of coercive controlling violence, a pattern of behaviors identified by feminists working in the battered women's movement as the type of intimate partner violence that was reported by women coming to shelters to seek help (Pence & Paymar, 1993). There are three major types.

#### 2.1.1. Intimate terrorism

This is the pattern of violent coercive control that comes to mind for most people when they hear the term “domestic violence”. Although it probably represents a small part of all of the violence that takes place between partners in intimate relationships, it is the type of violence that predominates among the cases that come to the attention of law enforcement, shelters and other public agencies, and that therefore has been the prototype of domestic violence for the battered women's movement (see almost any shelter Web site). It involves the combination of physical and/or sexual violence with a variety of non-violent control tactics, such as economic abuse, emotional abuse, the use of children, threats and intimidation, invocation of male privilege, constant monitoring, blaming the victim, threats to report to immigration authorities, or threats to “out” a person to work or family.

Although this is the type of violence initially identified by the battered women's movement as characteristic of the male violence encountered in shelters and law enforcement, it is not exclusively male-perpetrated, having been identified in lesbian couples (Renzetti, 1992) and among some women who terrorize their male partners (Cook, 1997; Hines & Douglas, 2010). The data are clear, however, that the primary perpetrators in heterosexual couples are men (Graham-Kevan & Archer, 2003; Johnson, 2006a, 2008). It is also clear from the

research of Holtzworth-Munroe, Meehan, Herron, Rehman, & Stuart (2000) and from a major literature review (Sugarman & Frankel, 1996) that misogyny and gender traditionalism play an important role in heterosexual intimate terrorism.<sup>1</sup>

#### 2.1.2. Violent resistance

Many victims of intimate terrorism do respond with violence of their own. For some, this is an instinctive reaction to being attacked, and it happens at the first blow—almost without thought. For others, it doesn't happen until it seems that the assaults will continue forever if something isn't done to stop them. For most women in heterosexual relationships, the size difference between them and their male partner ensures that violent resistance won't help, and may make things worse, so they turn to other means of coping. For a few, eventually it seems that the only way out is to kill their partner.

#### 2.1.3. Situational couple violence

This is violence that is not part of a general pattern of coercive control, but rather occurs when couple conflicts become arguments that turn to aggression that becomes violent. It is by far the most common form of intimate partner violence, and also the most variable. Somewhere around 40% of the cases identified in general surveys involve only one relatively minor incident, but many cases do involve chronic and/or serious, even life-threatening, violence. In contrast to intimate terrorism, situational couple violence does not involve an attempt on the part of one partner to gain general control over the other, and unlike intimate terrorism and violent resistance it is roughly gender-symmetric in terms of perpetration. The violence is situationally-provoked, as the tensions or emotions of a particular encounter lead one or both of the partners to resort to violence.

### 2.2. A feminist perspective on sampling biases

Here is another simple proposition: all of our major sampling methods are biased, with the result that they yield samples that differ dramatically in the representation of the major types of intimate partner violence. So-called random sample surveys are biased because of high rates of non-response, beginning with non-response to the brief screening interview for eligibility that often precedes the request for a full interview. Response rates often do not reflect that initial refusal to answer even the screening questions. For example, the National Family Violence Surveys that report an 82% response rate actually have a 60% response rate if non-response to the screening questions is included (Johnson, 1995). Because intimate terrorism and violent resistance have low base rates to begin with, and because perpetrators and victims of intimate terrorism are highly likely to refuse to respond to surveys – perpetrators because they do not wish to implicate themselves, victims because they fear reprisals from their partner – the violence in general surveys is heavily dominated by situational couple violence.

Agency studies are biased not by non-response as much as by the nature of the sampling frame itself. Because only serious or chronic violence tends to come to the attention of law enforcement, shelters, hospitals, and other such agencies, the violence in agency data or in surveys conducted in these settings is heavily biased in the direction of intimate terrorism and violent resistance. Similar biases are found in help lines, voluntary on-line databases, and other sources of information that involve *safe* self-reporting, but the general point here

<sup>1</sup> Although the Sugarman and Frankel meta-analysis found a strong relationship between gender traditionalism and male intimate partner violence ( $d = .54$ ), more telling is an important interaction effect that they do not include in their major conclusions. The relationship between gender traditionalism and intimate partner violence is quite strong in samples that are likely to be dominated by intimate terrorism ( $d = .80$ ) and tiny for samples that are likely to be dominated by situational couple violence ( $d = -.14$ ).

is that the sampling frame of every study in a specific institutional setting has a specific set of processes that shape the balance of types of violence that enter it.

The biases of these major approaches to sampling in intimate partner violence research are the major source of the seemingly contradictory data that continue to maintain the gender symmetry debate. Those who believe in gender symmetry cite hundreds of general survey studies that show that women perpetrate intimate partner violence at least as often as men. On the other side, believers in male perpetration of intimate partner violence cite hundreds of agency studies that show that men are the primary perpetrators. Studies with mixed samples that give access to all three major types of intimate partner violence, and that make distinctions among the types, find that intimate terrorism and violent resistance are heavily gendered, and that situational couple violence is perpetrated about equally by men and women—and it is this pattern, combined with sampling biases, that explains the dramatic differences among various studies with regard to the issue of gender symmetry. Surveys, dominated by situational couple violence, show rough gender symmetry in perpetration. Agency studies, dominated by intimate terrorism and violent resistance, show a pattern of (primarily) male violent coercive control and female resistance.

### 3. The anti-feminist backlash

The Dutton et al. (2010) paper to which I am responding exemplifies all of the general strategies of recent attacks on the progress of the battered women's movement and on the research that confirms that the feminist analysis of "domestic violence" (intimate terrorism) is largely correct.<sup>2</sup> In this particular paper they begin with an attack on their own caricature of the feminist analysis (which they call "the gender paradigm"), then move on to a rebuttal of what they allege has been said in two papers published in a 2008 issue of *Family Court Review* (Jaffe et al., 2008; Kelly & Johnson, 2008). I will focus here on their general misrepresentations of feminism and on their attack on the paper I wrote with Joan Kelly.

#### 3.1. Misrepresentations of the general feminist analysis of domestic violence<sup>3</sup>

##### 3.1.1. General allegation #1: feminists say that only men do it

The misrepresentations begin in the first paragraph of the paper: "This view [what Dutton et al. label as "the gender paradigm"] holds that consequential IPV is an exclusively male-perpetrated crime against female victims and children" (p. 2).<sup>4</sup> This caricature continues at the beginning of the "analysis" section: "...IPV is viewed as mainly male perpetrated against female victims" (p.3). Perhaps the authors are confusing the early rhetoric of the battered women's movement with contemporary feminist analyses of intimate partner violence. It is important to remember that feminist analyses began with an understanding of what was happening to women victims who came to the attention of law enforcement and who contacted shelters and help lines in the 1970s. The pattern of violent coercive control that dominated those cases came to be labeled by the movement as "domestic violence," creating considerable confusion because family sociologists continued to use that term for *any* violence between intimate partners, not just the coercive controlling violence that I have labeled as intimate terrorism.

<sup>2</sup> I will focus on the research critique here, but the attacks also include attempts to undermine funding for services for battered women by arguing that they discriminate on the basis of gender (Dragiewicz, 2008; Dragiewicz & Lindgren, 2009).

<sup>3</sup> There have been a number of other responses to these attacks on the feminist analysis (e.g., DeKeseredy & Dragiewicz, 2007; Dragiewicz & Lindgren, 2009; Frieze, 2000; Holtzworth-Munroe, 2005; Johnson, 2005a, 2005b, 2005c; Kimmel, 2002).

<sup>4</sup> Page numbers without a reference all refer to Dutton et al., 2010.

More "recent" feminist analyses (since the early 1990s) have stressed the differences among the three major types of intimate partner violence that I have just discussed above. First, it should be noted that this framework does not dismiss the existence of female-perpetrated intimate terrorism. Second, violent resistance, which involves mostly women, is acknowledged to lead sometimes to very serious violence, including homicide. Third, I and others have always noted that situational couple violence (a) is far and away the most common form of intimate partner violence, (b) is perpetrated about equally by men and women, and (c) can be extremely consequential.

Of course, we feminists, and all other family sociologists for that matter, have always noted that male violence is more likely to produce injuries and fear than is women's violence. That observation of a relationship between gender and the consequences of violence certainly does not translate into Dutton et al.'s cartoonish version of the feminist analysis as arguing that all consequential intimate partner violence is male-perpetrated.

#### 3.1.2. General allegation #2: feminists say violent men are evil, violent women are good

Here is how Dutton et al. present this pair of ideas, which they attribute to the so-called gender paradigm: "Men are presented as intentionally perpetrating domestic violence in order to maintain power and control in family relationships. In contrast, female violence is rationalized as the result of external circumstances—primarily as a reaction to male oppression" (p. 3). Of course, the point of the analysis I presented above is that most intimate partner violence does not involve an attempt on the part of either partner to exercise coercive control—it is situational couple violence. The feminist analysis does argue that in heterosexual relationships most of the intimate terrorists are men, but also that most of the violent men are not intimate terrorists.

As with the men, most women are involved in situational couple violence—no control issues, no reaction to male oppression, just arguments that escalate for a variety of reasons that differ from couple to couple. The feminist analysis also recognizes the existence of a few female intimate terrorists, and they certainly are not characterized as reacting to male oppression. The authors have purposely or inadvertently expanded the relatively small number of women who are involved in violent resistance into a group that encompasses the entire feminist analysis of women's violence.

#### 3.1.3. General allegation #3: feminists say that the only cause of intimate partner violence is the patriarchy

Here's how Dutton et al. put it: "Various empirically demonstrated etiological contributions to IPV (e.g., learning, attachment, and personality) are ignored, as are correlates of IPV perpetration such as alcohol abuse, depression, reported interpersonal dominance between partners (regardless of gender), and dyadic communication deficits" (p. 3). Well, let's start where one might expect the feminist analysis to be most single-minded—intimate terrorism. For more than a decade (Johnson & Ferraro, 2000) my feminist colleagues and I have incorporated Holtzworth-Munroe's work into our analysis of intimate terrorism, work that centers on matters of personality in general, and attachment in particular. And my work with Alison Cares demonstrates the relationship between violence in one's childhood home and male intimate terrorism (Johnson, 2008; Johnson & Cares, 2004), a central tenet of the learning approach to understanding intimate partner violence. As I have noted above, and in a number of published pieces, substance abuse and couple communication issues are central to any analysis of situational couple violence (Johnson, 2006b, 2007), and my analyses of situational couple violence have always emphasized the extreme variability of its causes.

The authors go on in this section to cite my concept of violent resistance as a major example of how the feminist analysis even attributes *women's* violence to men's coercive control. They neglect to

point out that violent resistance accounts for only a small part of women's intimate partner violence in my typology, in which most of women's violence is situational couple violence with much the same causes as men's situational couple violence.

### 3.1.4. General allegation #4: the feminist mindset has a lock on a variety of institutions, especially the law

Certainly the feminist analysis has had an important impact on major institutions (Buzawa, 2003; Dobash & Dobash, 1992), institutions that until the 1970s had generally assumed that all intimate partner violence was situational couple violence, a private matter for the couple to work out themselves. The feminist analysis produced a major shift in perspective, one in which most, if not all, domestic violence was seen as intimate terrorism. In presenting that out-of-date kernel of truth, however, the authors seem to have missed the major paradigm shift that has been sweeping those institutions in the last ten years: differentiation is the new catchword in the courts and law enforcement (see the bibliographies in Jaffe et al., 2008; Kelly & Johnson, 2008). This oversight is puzzling, because two major examples of that paradigm shift are the very articles that they criticize in Dutton et al. as examples of the death grip of the feminist focus on patriarchy (Jaffe et al., 2008; Kelly & Johnson, 2008). Both of those articles are rooted in the idea that not all intimate partner violence is intimate terrorism, and that the courts and other institutions need to use all of the assessment tools at their disposal to identify what type of intimate partner violence is involved in each particular case in order to decide on an appropriate course of action.

## 3.2. Misrepresentations of specific work: the case of Kelly and Johnson

### 3.2.1. Specific allegation #1: Kelly and Johnson ignore the data on gender symmetry

The authors accuse Joan Kelly and me of purposely ignoring the data on gender symmetry: "By thus omitting significant similarities in the actual incidence of male- and female-perpetrated domestic violence, victim advocates and allied researchers [this is in the section on my typology] present truncated, empirically skewed and data-poor, sometimes emotionally charged, stereotypic interpretations of IPV..." (p. 6). What we actually say is the following (Kelly & Johnson, 2008, pp. 486–487): "Situational Couple Violence is initiated at similar rates by men and women, as measured by large survey studies and community samples.... Overall, these and other survey data support claims that women both initiate violence and participate in mutual violence and that, particularly in teenage and young adult samples, women perpetrate violence against their partners more frequently than do the men." We had already stated clearly that situational couple violence is the most common type of intimate partner violence.

### 3.2.2. Specific allegation #2: Kelly and Johnson misrepresent female situational couple violence

"For the most part Johnson... relegates female IPV to the category 'situational couple violence'.... Female-instigated, conflict-engendered SCV is cast as an understandable reaction to male SCV..." (p. 6). There are no quotes to back up this distortion of what we allegedly said, and there are two important misrepresentations in this two-part characterization. The first distortion is somewhat subtle in that the authors simply chose not to note that for the most part we relegate *all* IPV, female *and* male, to situational couple violence. The second distortion is more direct: far from excusing women's situational couple violence, we do not distinguish between men's and women's situational couple violence in terms of causes (Kelly & Johnson, 2008, p. 485): "Situational Couple Violence results from situations or arguments between partners that escalate on occasion into physical violence. One or both partners appear to have poor ability to manage their conflicts and/or poor control of anger." We

never (nor have I anywhere else) characterized women's situational couple violence as an "understandable reaction" to anything.

### 3.2.3. Specific allegation #3: Kelly and Johnson deny female intimate terrorism

Here Dutton et al. first damn with faint praise: "As Johnson occasionally acknowledges (Johnson [2006a], fn 2), most but not all severe IPV is perpetrated by men" (p. 7). Of course, the choice of a footnote seems to imply that I hide even those few times that I acknowledge that women sometimes perpetrate intimate terrorism. The truth is that there is not a single piece among the dozens of my papers and my book in which I did not acknowledge that there are female intimate terrorists, beginning with the earliest published paper on the typology, in which I was still using the term "patriarchal" terrorism and was focused almost entirely on men's violence (Johnson, 1995, p. 292): "Although it is indisputable that some men are terrorized by their female partners (I have worked with some at my local shelter), [the argument] that men are terrorized as frequently as women produces a dangerous distortion of reality." Not satisfied with this subtle distortion, Dutton et al. follow with a more blatant lie, alleging that in my work "...by definition, male violence is internally caused by the conscious intent to dominate women" (p. 7). No—I have repeatedly stated that most male violence is situational couple violence.

## 3.3. Dutton et al.'s review of research that allegedly contradicts the feminist analysis

### 3.3.1. Reality check for the gender paradigm

Throughout much of this section Dutton et al. "refute" our argument that intimate terrorism is primarily male-perpetrated by citing survey data on gender symmetry. As Kelly and I noted, research conducted by me and my colleagues, and by other scholars (Ansara & Hindin, 2010; Graham-Kevan & Archer, 2003; Johnson, 2006a; Johnson, Leone, & Xu, 2008) has demonstrated that the violence in survey research is so heavily dominated by situational couple violence that it provides no information regarding the gender symmetry or asymmetry of intimate terrorism.

The authors also claim that there is no evidence that men are the major perpetrators of intimate terrorism. My colleagues and I have presented such evidence from Irene Frieze's Pittsburgh study (Johnson, 2001, 2006a) and from data on previous marriages from the U.S. National Violence Against Women Survey (Johnson et al., 2008). Graham-Kevan and Archer (2003) have presented confirming data from their study in Britain—different researchers, different measures, and different populations. And of course there are decades of research from law enforcement, shelters, hospitals and other agencies (in which the data are dominated by intimate terrorism) that show that men are the primary perpetrators of intimate terrorism in heterosexual relationships.

Next, Dutton et al. cite Graham-Kevan and Archer (2003) as evidence for the gender symmetry of intimate terrorism, and later they argue in a particularly nasty way that Kelly and I have misinterpreted the Graham-Kevan and Archer data: "[Kelly and Johnson] misrepresent Graham-Kevan and Archer's (2003) findings... and cherry pick and distort the data..." (p. 13). The alleged "distortion" to which they refer is that we do not point out that, although 87% of the intimate terrorism in Graham-Kevan and Archer's data is male-perpetrated, most of these cases come from their shelter sample. Here is how Dutton et al. summarize the gender and intimate terrorism findings in the Graham-Kevan and Archer study: "... all [non-shelter] groups, including a group of men court-mandated for spouse assault treatment, exhibited gender symmetry in incidence of [intimate terrorism]..." (p. 13, my emphasis). Here are the actual data for all of the groups. In the shelter sample (n = 68), there were 36 male and one female intimate terrorists. Among students (n = 56),

there were seven male and two female intimate terrorists. Among prisoners ( $n = 105$ ), there were two male and five female intimate terrorists. For the batterer intervention program ( $n = 10$ ), there were no male or female intimate terrorists. Do they really consider this to be evidence for the gender symmetry of intimate terrorism?

Finally, they cite Laroche's study (2005) using Canadian General Social Survey data. The problem with the Laroche study (much as I admire it) is that he made the mistake of using the cutoffs for intimate terrorism that Janel Leone and I had mistakenly adopted in our earlier research on current relationships in the NVAW (Johnson & Leone, 2005). Our mistake was relying on a cluster analysis in a survey sample of current relationships, a sample that essentially includes no intimate terrorism. When we corrected our analysis by applying the cluster analysis to previous marriages (where there is a reasonable number of cases of intimate terrorism), we obtained a different cutoff and found that 83% of the intimate terrorism in previous relationships is male-perpetrated, and for current marriages only 6/10 of one percent of respondents report intimate terrorism Johnson et al., 2008.

### 3.3.2. *Old wine in new bottles*

The gist of this section is that this is "the typology of one researcher (Johnson), whose linking of gender and [intimate terrorism] ...has been disconfirmed by relevant empirical research finding female as well as male [intimate terrorists]" (p.8). Of course, it is not I, but Dutton et al. who conflate type of violence and gender in their caricature of my work. I have never said there were no female intimate terrorists, only that in heterosexual relationships intimate terrorism is primarily male-perpetrated. In fact, my own studies cited above include female intimate terrorists (3% of the intimate terrorists in the Pittsburgh data, 17% among NVAW previous marriages).

Dutton et al. then go on to present largely irrelevant data on the distribution of bilateral, male-initiated, and female-initiated violence in survey data—data which make no distinctions among types. Of course, those data are dominated by situational couple violence, which is gender-symmetric.

### 3.3.3. *The myth of equivalent methodological bias*

In this section Dutton et al. claim that there is no evidence that so-called representative sample surveys are heavily biased in the direction of finding primarily situational couple violence and very little intimate terrorism. Well, in the general sample of the Pittsburgh study, the male violence was 89% situational couple violence (Johnson, 2006a), among current marriages in the NVAW it was 85% situational couple violence (Johnson et al., 2008). For Graham-Kevan and Archer's British general sample, the male violence was 75% situational couple violence. For Ansara and Hindin's (2010) Canadian data, the male violence among current partners was 81% situational couple violence. The point is two-fold: (a) general survey data without distinctions will show patterns that are characteristic of situational couple violence, and (b) therefore we need to develop standard operationalizations of types that will allow us to make distinctions in various types of samples.

### 3.3.4. *Shelter to general population extrapolation*

"...Johnson based his typology solely on self-reports from women in shelters [and] erroneously generalized his findings to the distribution of IPV in the broader community" (p. 9). Wrong! The Pittsburgh sample is self-reports from women, but from shelters, courts, and the general community. The differences between intimate terrorism and situational couple violence have also been documented in a U.S. national sample (Johnson et al., 2008; Johnson & Leone, 2005), in a Chicago health service sample (Leone, Johnson, & Cohan, 2007; Leone, Johnson, Cohan, & Lloyd, 2004), in a large multi-city study in the U.S. (Frye, Manganello, Campbell, Walton-Moss, & Wilt, 2006), in two studies of different Canadian General Social Surveys

(Ansara & Hindin, 2010; Laroche, 2005), and in a multi-sample study in Britain (Graham-Kevan & Archer, 2003).

As far as I know, I have never argued that "...community sample methodology yields data as selective and biased as that collected from shelter houses" (p. 9). Again, Dutton et al. offer no quotes, but in the 2006 paper that they cite for this statement I actually said, "Finally, let me nail down my sampling argument, that general survey samples tap primarily situational couple violence, whereas agency samples give access primarily to intimate terrorism.... [M]ost couples who experience violence, including those in our audiences, are involved in situational couple violence.... [O]ne can err by assuming that the patterns observed in agency samples describe all partner violence" (Johnson, 2006a, pp. 1010–1011). In the same article, here is how I spoke about the issue of extrapolation: "If my arguments regarding the biases of various types of sampling strategies are correct, it is almost impossible to develop precise estimates of the incidence of the various types of violence. I come to the conclusion that most partner violence is situational couple violence in the following way, based on figures in my 1995 article. First, accepting my evidence that almost all of the partner violence in general surveys is situational couple violence, we can use the figures from the National Family Violence Surveys to estimate the incidence of situational couple violence. Second, extrapolating from agency data in two states that keep excellent shelter statistics, we can develop an estimate of the incidence of intimate terrorism. Those figures, which may be found in the 1995 article, suggest that there is probably 3 times as much situational couple violence as intimate terrorism, which would mean that 75% of women experiencing violence from their male partners are experiencing situational couple violence" (Johnson, 2006a, p. 1016, fn. 11).

### 3.3.5. *Non-selective sample studies*

In the next, long section the authors cite studies that are alleged to show that (a) women are as violent as men, (b) women are as controlling as men, and (c) women are as likely to be intimate terrorists as men. I intend to try to go through this section study by study, but before I go there, let me simply tell you what the three major problems are with the uses to which Dutton et al. put the studies that they cite. First, almost all of the studies they cite are general surveys, studies that have been demonstrated to represent situational couple violence, a type of violence that we all agree is roughly gender-symmetric in terms of perpetration.

Second, a number of the studies they cite show that men and women are equally controlling or dominant in relationships. That, of course, is irrelevant to the issue of whether men and women are equally likely to use a combination of violence and non-violent control tactics to attempt to take complete control over their partner—intimate terrorism.

Third, a few of the studies they cite are alleged to show that women are as likely as men to combine violence and control, i.e., to be intimate terrorists. There are two responses to these studies. On the one hand, some of them involve general surveys that show similar correlations between control motive and violence for men and women. With respect to these, I need to remind Dutton, his colleagues, and the other anti-feminists, that this typology is a typology—for a reason. I believe that what is involved here is *not* a simple correlation between control motive and violence, for either men or women. What we have is a small group of people, men and women but mostly men, who attempt to take total control over their partners with a combination of violence and other control tactics, i.e., violent coercive control. These are the intimate terrorists. *Among other couples*, we will sometimes see a relationship between control motive and situational couple violence because those with a stronger need for control will be more likely to escalate arguments when they feel they are not winning. And the studies that Dutton et al. cite are general

surveys that are dominated by situational couple violence, the other couples.

In one important case Dutton et al. do misrepresent a study's findings regarding control and violence. They say that [Felson and Outlaw \(2007\)](#) find that "...the relationship between use of control/jealousy and physical violence exists equally for both male and female respondents...." (p. 11). What Felson and Outlaw actually found was a strong interaction effect in which there is no relationship between control and violence for current partners (as I would predict because survey data on current partners include no intimate terrorism), but a strong relationship between control and violence *only for men* for previous partners (as I would predict for a sample that includes intimate terrorism). As my colleagues and I have recently argued, this is actually compelling evidence for our feminist analysis of intimate partner violence ([Johnson et al., 2008](#)).

Now for the specific studies.<sup>5</sup> Dutton et al. start with four studies that find no gender differences in emotional abuse and control. These are general survey studies—situational couple violence. Then come two articles that show similar correlations between dominant personality, the need to control, and physical violence for men and women. One is a general survey, the other a convenience sample of undergraduates—situational couple violence. Next are two studies that allegedly show that males and females are equally likely to combine physical violence with emotionally abusive and controlling behaviors. As far as I can tell, neither of the studies presents evidence on that matter. Perhaps Dutton et al. are under the mistaken impression that similar correlations between those variables indicate a similar likelihood of combining them. It does not. But more importantly, one of the studies is of high school students, the other of college students—situational couple violence. Then comes Straus's big study of dating university couples, showing that dominance scores for men and women are equal and that female dominance predicts female physical aggression. Another general survey—situational couple violence.

Dutton et al. now turn to some studies of what they call clinical populations. Here, depending on the institutional setting, we may find some intimate terrorism. The first study they cite seems to me to provide support for the feminist analysis. Judge for yourself: "In the pioneering studies by [Stacey, Hazlewood, and Shupe \(1994\)](#), on men arrested for domestic violence and mandated to batterer intervention programs, one-third of the physical violence was perpetrated by the female partner (legally deemed the 'victim'), and rates of male-perpetrated emotional abuse and control were significantly higher than female partner rates in only about half of the 13 categories" (p. 11). This sounds to me like some situational couple violence and some intimate terrorism/violent resistance (the higher emotional abuse and control among the men would suggest that they were the intimate terrorists, although it is impossible to know without looking at patterns rather than average differences in individual categories). At the very least, these findings are hardly incompatible with the feminist analysis.

The second study cited in this category provides no data on the types, but finds "equivalent rates of injury-causing physical violence among couples dual-arrested for domestic violence, with men more likely to engage in isolation behaviors and women somewhat more likely to engage in verbal abuse" (p. 11). One of the most important points of my typological approach is that all of our samples are biased in terms of types and we need to ask what those biases are. These were *dual arrests*!

The final study in this trio is a 2008 conference presentation ([Hamel, Graham-Kevan, & Prospero, 2008](#)) that finds "comparable levels of controlling and emotionally abusive behaviors by male and

female clients court-mandated to batterer intervention programs across California" (p. 11 of Dutton et al.). It is possible that they have both male and female intimate terrorists in this sample, and I would like to know how many there were of each. Comparable levels do not address the question of comparable numbers. In our own study of intimate terrorism among previous spouses in the NVAW ([Johnson et al., 2008](#)) we found that most of the intimate terrorism was male-perpetrated, but that control tactics for the much smaller number of female intimate terrorists were in many ways similar to those used by the men.

The next study cited is the [Felson and Outlaw \(2007\)](#) study discussed above, that actually shows strong support for our position that intimate terrorism is mostly male-perpetrated (see also our more appropriate analysis of the same data: [Johnson et al., 2008](#)). Then comes Laroche, also discussed above ([Section 3.3.1](#)).

Next Dutton et al. cite a series of findings from general surveys—situational couple violence. They seem to believe, somehow, that this evidence that women often initiate violence against a non-violent partner is evidence that "the IPV profiles in these surveys involved significant female-perpetrated 'abusive-controlling' violence [intimate terrorism]...." (p. 13). Of course, there is no such evidence, as Kelly and I noted: "Overall, these and other survey data support claims that women both initiate violence and participate in mutual violence and that, particularly in teenage and young adult samples, women perpetrate violence against their partners more frequently than do the men. Based on knowledge available, this gender symmetry is associated primarily with Situational Couple Violence and not Coercive Controlling Violence" ([Kelly & Johnson, 2008, p. 487](#)).

Next comes the allegedly relevant finding that in one study women in lesbian relationships report more IPV perpetration by a lesbian partner than by prior heterosexual partners. My initial reaction was to be amazed that Dutton et al. somehow thought this was relevant to questions about the relative frequency of heterosexual male and female intimate terrorism. But if it were relevant, one might want to know that this was a sample of "abusive lesbian relationships" ([Lie, Schilit, Bush, & Montagne, 1991, p. 123](#)). Given this dramatic sampling bias (100% abusive relationships), how could this study possibly have found the same level of violence among prior heterosexual partners? Did Dutton et al. somehow not notice this? The title of this paper is "Lesbians in currently aggressive relationships."

As a lead-in to the discussion of [Graham-Kevan and Archer \(2003\)](#) in this section of their paper, Dutton et al. misrepresent us again: "In identifying [intimate terrorism] as an exclusively male pattern of domination over female intimates, J. B. Kelly and Johnson.... [in a] typology based exclusively on shelter sample data...." (p. 13). I guess they think that if they say it often enough, it will be true. Of course, we do not identify intimate terrorism as exclusively male, and the typology is not based exclusively on shelter data. As I have argued above ([Section 3.3.1](#)), [Graham-Kevan and Archer \(2003\)](#) essentially supports the gender asymmetry of intimate terrorism.

The authors end this section with discussions of more findings from two general surveys, so I'll note again that such surveys are dominated by situational couple violence. One set of findings is from [Stets and Straus's \(1989\)](#) analysis of National Family Violence Survey data, an analysis in which they show that "repeat, severe violence against a non-violent intimate is symmetrical by gender" (p. 14). Dutton et al. make the mistake of thinking that this type of violence is necessarily intimate terrorism. It is not; it is simply repeat, severe, non-reciprocal violence; there is no evidence regarding control.

The other evidence is a reference to a piece in which [Graham-Kevan and Archer](#) make the mistake discussed above ([Section 3.3.1](#)), using a cluster analysis in a sample that includes little or no intimate terrorism. When this piece was presented at meetings, I contacted [Graham-Kevan](#) as follows: "I finally found time to read your 'non-selected sample' paper carefully. As usual, I love the care with which you do your work. I skimmed it when you first sent it to me and, as

<sup>5</sup> To keep my reference list manageable, in general I will not provide citations for these papers. I will proceed through them in order and the citations can be found in the Dutton et al. paper.

you might expect, I was troubled that you found gender symmetry for IT and VR, and I think I know why that happened. My interpretation of your results has general implications that I think are very important both for DV research and for the uses of cluster analysis in general. Here's what I came to think as I read your findings. Does this study really get at IT or merely at the most controlling cases of SCV? I have always argued that general samples will include very little IT. I expect that you had a very low response rate (you don't report how many emails were sent), which would exaggerate that bias. If you have a sample that in fact is mostly SCV (we had the same problem with NVAW data), then your cluster analysis will identify relatively high control, but will still include lots of (maybe even mostly) SCV. We really need to get away from the cluster analysis approach because it is almost entirely dependent on the nature of the sample (as you note in your paper, when you point out that Frieze's sample has that group of male ITs from shelters and courts). The patterns that you find with regard to the difference between IT and SCV for other variables also didn't look as strong as the patterns we've found in other work. So, I thought, 'I'll bet their high control group isn't as high as it is in their studies with other, selected samples that would include real IT.' And I went to your 2003 JIV paper. Look at the numbers for the control types below. The first column is your 2-cluster highs, the second your 3-cluster highs, and the third your 2003 highs. [Here I gave her the numbers, which showed way higher control for the 2003 intimate terrorist group.] I wonder what would happen if you re-ran your unselected data using a cutoff derived from the 2003 paper. Take your 2003 cluster analyses, crosstab it against the total control score, and choose a cutoff that comes closest to replicating the high cluster (I've done this in some of my papers, but I've never had the same measures across the various samples I've used so that I could use the same criterion to identify high control across samples). Now use that cutoff to identify IT, VR, SCV, and MVC in your unselected sample. The numbers in each type will change dramatically, and I would predict that you'll see a more dramatic differentiation between IT and SCV, including a shift in the gender symmetry. I'm really dying to find what happens if you do this. Willing to try it?" Graham-Kevan said she'd do it and get back to me, but that never happened. I'd still like to see the re-analysis. When my colleagues and I used a similar strategy to re-analyze the NVAW data (Johnson et al., 2008), we found only 34 male intimate terrorists among current partners of almost 5000 married respondents (Johnson & Leone, 2005).

#### 4. Conclusion

So, what's up with these authors? Why the comic book caricatures of the feminist analysis? Why the gross misrepresentations of what Joan Kelly and I wrote in our 2008 article? Why the single-minded focus on alleged evidence that women are as bad as men? In their determination to see what they want to see, they seem to have missed the obvious implications of the call for differentiation among types of intimate partner violence that is the heart of the Kelly & Johnson and Jaffe et al. articles. So, let me finish with a summary of the implications of the Kelly and Johnson paper and of this review.

Most intimate partner violence, both men's and women's, does not fit the power and control model of intimate terrorism. It is situational couple violence and must be treated accordingly. And even if it were to turn out to be the case that men and women were equally likely to be intimate terrorists (I of course do not believe that and the evidence does not indicate that), it would not affect what Joan Kelly and I suggested in our paper. The dramatic differences among intimate terrorism, violent resistance, and situational couple violence make it essential that the family courts make these distinctions in order to do the right thing with respect to the adults involved, and to serve the best interests of the children. In fact, the final paragraph of the Kelly and Johnson paper serves well as the correct interpretation of the literature cited by Dutton et al. in their attack on the feminist analysis

of intimate partner violence: "Current research provides considerable support for differentiating among types of intimate partner violence, and such differentiations should provide benefits to those required to make recommendations and decisions about custody and parenting plans, treatment programs, and legal sanctions" (Kelly & Johnson, 2008, p. 495).

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