

Beyond Minneapolis: A Preliminary Theoretical Model for Alleviating Conceptual Ruts in Domestic Violence Intervention Research

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ABSTRACT

Despite five replications, subsequent synthesis of data from domestic violence arrest studies, and years of active and often impassioned debate, in the twenty-three years since the end of the Minneapolis Domestic Violence Experiment, domestic violence research suffers from a conceptual rut. Individual-level interventions still ground domestic violence response, and questions remain regarding the efficacy of arrest for a suspect with a low “stake in conformity.” This paper proposes two alternative theoretical models based on advances in feminist scholarship, batterer treatment, and the emergence of a literature on collective efficacy. The first model emphasizes an individual’s stake in conformity while the second focuses on community-level, informal social controls. A preliminary look at the relationship between individual- and community-level treatments, using data collected over thirty months from 474 subjects arrested for domestic violence offenses, suggests that more research is needed to address the interplay between these types of interventions and the role they may play in reducing domestic violence.

KEYWORDS: domestic violence; mandatory arrest, stake in conformity, informal social control.

On August 1, 1982, the first-ever field experiment on police response to domestic assault was completed in Minneapolis (Sherman and Berk 1984a), and it seemed to provide support for the deterrent effect of arrest (Sherman and Berk 1984b). The study suggested police officers should arrest suspects during a domestic violence call, because the arrested suspects recidivated at rates significantly lower than suspects who were either warned and separated from their partners or received police officer-led mediation and counseling at the scene. The findings helped bolster the case for pre-arrest policies in domestic violence situations or what commonly became known as mandatory arrest for batterers (*see*, Sherman 1992; Stark 1993).

Shortly after the publication of Sherman and Berk’s findings, a lively and impassioned debate ensued regarding the strength of the theoretical orientation of the Minneapolis Domestic Violence Experiment (MDVE) and whether it was appropriate to use limited experimental results to inform policy decisions (Binder and Meeker 1993; Manning 1993; Sherman 1993; Stark 1993). Arrest deterred recidivism in Minneapolis, but the five replications in other cities did not provide resounding support for Sherman and Berk’s findings. Replications showed arrest to actually increase repeat

violence among unemployed and unmarried suspects (Schmidt and Sherman 1996) in what Sherman (1992) called a “backfire effect,” and violence increased among employed men who were not arrested (Berk et al. 1992).

Sherman and Smith (1992) tried to account for the variable effects of arrest by relying on a theoretical model emphasizing social bonding and labeling theory. They hypothesized that batterers with a low “stake in conformity” (unemployed and unmarried—which tended to correlate with suspect ethnicity) were less likely to respond to the deterrent effect of an arrest. Berk et al. (1992:705) were “uneasy with the social control and/or labeling framework,” because marriage and employment might serve as weak proxies for stake in conformity or social bonds. According to Berk et al. (1992:705), “From a rational choice perspective, then, employment is really an indirect measure of the income a suspect would lose if sanctioned by the criminal justice system. Social control has nothing to do with it.” Sherman (1995: 210) has admitted that, “neither labeling theory nor deterrence theory can account for the diversity of punishment effects on different kinds of people.”

In light of the variable effects of arrest on recidivism, Sherman (1992) called for either repealing

mandatory arrest policies or employing a discretion-based approach to domestic violence arrests (Sherman 1997). Such a policy, however, was seen as untenable by some (Berk et al. 1992) and counterproductive to the philosophical aims of mandatory arrest policies (Bowman 1992; Durham 1998; Stark 1993; Zorza 1994). The domestic violence arrest research has also been criticized for its lack of “a clearly stated theoretic rationale derived from philosophical analysis or criminological theory that links arrests to prevention or deterrence” (Manning 1993:643) and for what was seen as an “orchestrated” effort to influence police policy without proper replication and theory testing that might reveal the effects the policy would have on people’s lives (Binder and Meeker 1993:886). Even feminists, who had generally supported the movement toward pro-arrest policies, questioned the theoretical validity of the research (Bowman 1992; Frisch 1992) and pointed out that the studies “were designed and carried out with little knowledge of existing evidence and theoretical positions regarding violence against women” (Dobash and Dobash 2000:254).

Whether or not policymakers should have relied on the MDVE and its replications now seems the quintessential academic issue, because the federal government, through the 1994 Violence Against Women Act contained in Title IV of the Violent Crime Control and Law Enforcement Act (Pub. L. 103-322, 108 Stat. 1796), earmarked considerable resources to encourage pro-arrest policies (Uekert et al. 2001). Meanwhile, in more than two decades since the first experiment was completed, researchers have focused much of their efforts on determining whether or not there really ever was a deterrent effect of arrest. Although recent findings from a rigorous synthesis of the Spousal Assault Replication Program (SARP) data support the idea that there was a modest, but consistent preventive effect from arrest (Maxwell, Garner, and Fagan 2002), suspect characteristics still present a modifying and confounding influence on such effects.

The value of the synthesis conducted by Maxwell et al. (2002) is its resourceful and meticulous analysis of data to ensure the deterrent effect of arrest received its best possible test. The limitation of this work, as with most recent and past work on arrest in domestic violence cases, is the lack of parsimonious but robust theoretical models that are able to contend with the context in which arrest may vary, including police application of the policy, suspect characteristics, and environments in which suspects reside.

Limitations with the SARP, even in light of two decades worth of research and debate, illuminate important areas of inquiry that remain unexplored. Although Maxwell et al. (2002:73) suggest that further research is needed to test variations in sanctions such as, “why and when sanctions deter and whether secondary

consequences of arrest exist,” perhaps a more fruitful avenue for research comes from Sherman’s (1997:26) claim that elements of the specific and general deterrence model remain untested and that social elements such as, “Churches, employers, landlords, and neighbors may all play roles that are not yet well understood.”

Arrest Plus

There is a paucity of carefully controlled studies engendering concepts of informal and community-level controls in the investigation of domestic violence recidivism (Snider 1998). Given this largely unexamined vein of inquiry, the current study seeks to explicate a more theoretically sound examination of arrest and domestic violence recidivism that focuses on deterrence, stake in conformity, and informal social control. The inability to effectively capture any influence that community-level variables might play in the efficacy of arrest may stem from the incorrectly specified and rather unimaginative informal social control constructs used in past studies. For example, the Colorado Springs (Berk et al. 1992) and the Miami Metro-Dade (Pate and Hamilton 1992) replications of the MDVE provided crisis counseling to suspects as an informal response. During replications in Omaha (Dunford, Huizinga, and Elliott 1990) and Charlotte (Hirschel, Hutchinson, and Dean 1991), police offered informal or nonpunitive treatments in the form of on-site mediation or separation, even though such interventions and other similar informal responses are poorly received in batterer treatment literature (*see*, Healy, Smith, and O’Sullivan 1998; Kaufman 2000). More recent field research offers little resolve. Such research is often method-driven (Dobash and Dobash 2000), pragmatic, a-theoretical (Manning 1993), or focused on increased cooperation among criminal justice agencies late in the stages of case processing—such as prosecutors, courts, and correctional institutions (e.g. *see*, Dakis 1995; Lerman 1992; Polsby 1992; Tolman and Weisz 1995).

By considering a broad spectrum of the most recent studies, we attempt to develop a better conceptualization of community-level interventions intended to deter domestic violence. We provide descriptions of two plausible and theoretically sound treatments, as well as, a preliminary test of these treatments in the field. The first theoretical model is based on the premise that if specific deterrence, as operationalized by arrest, works because it deters batterers who have a stake in conformity, might we then craft treatments to stimulate and thereby test a suspect’s stake in conformity? The second model is based on recent studies that point to an association between collective efficacy or community-level informal controls and domestic violence prevalence (Browning 1999) and domestic violence

recidivism (Marciniak 1994; Woolredge and Thistlethwaite 2002). This second model allows for an exploration of how to possibly stimulate informal social control mechanisms in communities; evoking collective efficacy and thereby impacting the occurrence of domestic violence offenses independent of individual-level characteristics.

Our primary desire is to advance the theoretical debate, but we do attempt to apply as much rigor as possible using a quasi-experimental design. Nonetheless, there are important data limitations. This research represents a case study on a working partnership between victim advocates and scholars. As part of that arrangement, however, victim advocates who managed the delivery of the individual-level and community-level treatments for the study, did not want victims of domestic abuse contacted for follow-up surveys for purposes of measuring recidivism or suspects contacted if they still resided with their victims. Instead, we had to rely on measuring recidivism by official police records on domestic violence-related offenses. Advocates were reluctant to provide researchers with contact information for survivors for fear that a survey might incite more violence from a batterer, but also because advocates wanted to emphasize the use of official statistics as a way to evaluate police practice. An additional constraint was that we did not have complete influence over the theoretical models employed. Although we could suggest changes to the models to assure construct validity, we still found the implementation was guided in large measure by the advocates and, as such, did not necessarily offer the strongest link between the treatments and the theoretical models. It is hoped that the line of inquiry established in this study will stimulate improved empirical tests and theoretical advancements in future studies assessing the effects of arrest on domestic violence recidivism.

THEORETICAL MODEL 1: THE INDIVIDUAL, SPECIFIC DETERRENCE REFINED

A weakness with the deterrence models formulated in previous domestic violence studies has been the models' reliance on a stake in conformity as defined by marital and employment status. Stake in conformity as first postulated by Toby (1957) means that an actor faces a risk of losing something if caught. For instance, an employed suspect who is arrested may face the loss of a job or scorn from his employer. More recently, researchers have used residential stability, education, and economic status to measure stake in conformity (Woolredge and Thistlethwaite 2002). There are several worrisome weaknesses with such models. First, they lead to the type of logic employed by Sherman (1997) in calling for a discretion-based arrest policy. Police in the field might find it extremely burdensome if asked to

determine recidivism risk based on a set of measures that would have to take into account various characteristics about the suspect's current life (Berk et al. 1992).

Second, a more troubling weakness is that some batterers seem readily willing to risk not only losing their jobs, but much more, including killing their partners and themselves, rather than relinquish a perceived loss of control over their victim (Johnson, Li, and Websdale 1998). To underscore the disjunction between deterrence theory and the current research on batterers and feminist scholarship, consider that threat assessment tools do not focus on employment or the status of an offender in the community. Instead, such tools emphasize the degree of physical power the subject maintains over the victim, drug abuse history, and the degree of control the suspect attempts to maintain over his victim (see, Campbell, Sharps, and Glass 2001; Websdale 1999). The question for deterrence theory is to determine whether a batterer's desire to maintain control over his victim is mitigated by such variables as employment status. The deterrence literature has not adequately addressed how losing a job or status in the community is a threat, or a cost, to suspects.

A third weakness with deterrence is that it creates a paradoxical assumption. Deterrence depends on a stake in conformity, which includes attachment and commitment to conventional behaviors and opportunity structure (Briar and Piliavin 1965). Feminist theorists maintain that conventional norms *encourage*, not discourage, spousal abuse (Eigenberg 2001; Garske 1996). Any analysis of attachment to conventional belief systems must recognize that batterers likely receive some normative messages signaling the acceptance of domestic violence in conventional society (*see also*, Block and Skogan 2001).

Creating a Stake

Perhaps previous research has focused on answering the wrong questions when it comes to deterrence and domestic violence. Prior studies have steadfastly addressed questions such as, "Is arrest working?" and "Why is arrest failing to work?" Instead, maybe the focus should shift to, "What can be done to make arrest work, especially for suspects who may have nothing to lose?" To answer this question, we begin with research outside the domestic violence literature to help provide some insights about the efficacy of arrest on batterers. For instance, how is it that deterrence was reported to have worked quite dramatically among urban gang members in Boston (Braga et al. 2000; Kennedy 1998; Kennedy, Braga, and Piehl 2001), when social control theorists would consider such individuals are weakly tied to legitimate opportunity structures with little or no stake in conformity? The Boston study offers three

valuable concepts: (1) the deterrent message was transmitted by multiple actors, with what amounted to a unified voice that encouraged and assisted desirable behavior and discouraged and resulted in action against undesirable behavior; (2) the message was delivered consistently; (3) punishment, or reactions to violence, were delivered fairly—inasmuch as the risks for violations were clearly stated and equally applied.

Returning to a study on domestic violence with somewhat similar implications, Paternoster, et al. (1997) found reductions in domestic violence recidivism were positively associated with a suspect's perceived fair and judicial treatment by the justice system. For those who are more often alienated from societal institutions (e.g., marriage, employment, justice, and education) arrest and poor treatment may come as quite expected and rational outcomes. For such members of society, a psychology of injustice is at work when arrest occurs (Miller 2001). Arrest, when carried out in ways perceived to be unjust, may serve to cultivate feelings of anger and injustice, especially in alienated members of society, rather than produce fear of punishment or a desire to conform.

Sherman (1995) explains the “backfire effect,” with defiance theory. Accordingly, the *process* of justice can produce the desired outcome sought by sanctions and deterrents if the process is viewed as fair and just by the offender. Because social control depends on the internalization of norms (Hirschi 1969; Aronson 1988), the defiant/alienated individual can easily dismiss the normative message of an arrest and thus disassociate himself from the internalization process. Batterers are practiced in denial, minimization, and shifting blame (Shepard 1991), and the process of arrest can either facilitate or block further denial. By stimulating a stake in conforming to non-violent norms, and thus beginning a process in which batterers internalize norms that are opposed to the use of violence, it may be possible to enhance the violence-suppressing effects of arrest.

In addition to mediating batterers' perceptions of fairness regarding criminal justice responses to domestic violence and reducing the opportunity for denying blame, Sherman (1995) suggests bringing suspects back into society's fold, especially through a ritualized process. This approach offers a potential method to stimulate batterers' stake in the institutions of society (*see also*, Braithwaite and Daly 1995). Furthermore, a process-oriented or reintegrative approach (Braithwaite 1989), may allow for stimulation of a stake in conformity even in instances where none exists. Among those with a stake in conformity, arrest may be perceived as fair and may transmit a message that domestic violence is not acceptable.

Drawing on this literature, any individual-level model of deterrence that stimulates a stake in conformity must include: (a) a multitude of voices from

legitimate sources in agreement (such as a suspect's peers and police) to block denial and form an accepted norm against battering; (b) an emphasis on fair treatment of the suspect, and/or confronting the suspect's desire to blame his predicament on unfair justice to block defiance; and (c) an effort to encourage and welcome the suspect back into the fold of a society that shares anti-battering norms.

MODEL 2: THE COMMUNITY, COLLECTIVE EFFICACY AND INFORMAL CONTROLS

Sociological research in general, “... has neglected two crucial issues: the macrosocial determinants of community social organization and the contextual effects of community structure on individual behavior” (Sampson 1988:766). Similarly, examinations of the affect of arrest on batterers assumes social bonds and ties exist between suspects and their community based on some characteristic of the suspect (employment, marriage) rather than on the reverse: a community's role in creating ties and networks in which individuals are bonded.

Social disorganization theory, especially as refined and advanced more recently in collective efficacy literature (Morenoff, Sampson, and Raudenbush 2001a, 2001b; Sampson 2000; Sampson, Raudenbush, and Earls 1997, 1998) and feminist perspectives, offers similar analyses of a community's role in the control of deviance. Consider how Morenoff et al. (2001a, 2001b) and Sampson et al. (1997) define collective efficacy to include a community's shared expectations of, and tolerance for, the social control of deviance. In accordance with this perspective on collective efficacy, Garske (1996) notes that two widely cited theories regarding violence against women rely on some variant of the cultural norm explanation for such violence. Voigt et al. (1994) identify six explanations for spouse abuse, four of which rely on cultural influences to explain behavior. According to these explanations, if a society condones violence against women (Eigenberg 2001), stopping such violence requires a social environment that is aware of it, exposes it, and confronts it (Garske 1996, Syers et al. 1992). As Gondolf (1998) reports, men who undergo treatment for battering are more likely to end their violence if their communities support a nonviolent norm.

In socially disorganized communities, there is neither cohesion between residents, nor a shared consensus on social control, which “...refers generally to the capacity of a group to regulate its members according to desired principles—to realize collective, as opposed to forced, goals” (Sampson, et al. 1997:918). Such a perspective is consistent with feminist theories that suggest domestic violence exists in part because there is little basic agreement on what constitutes

intimate partner abuse. Men who abuse are either isolated from other men who might counter their notions about power and control over women (Kaufman 2000) and/or exist in social environments (work, neighborhood, society) in which collusion and silence are the norm.

Suspect and Victim Context versus Suspect Status

One interesting aspect of the community-based viewpoint is that it offers an explanation for how marriage and employment may influence recidivism, irrespective of any specific deterrent effect. In addition, a second provocative feature of this perspective is that it introduces the victim to the outcome equation. According to Sampson (1988:768), the effects of community-based ties, such as decreased alienation and greater social bonds, are "hypothesized to be independent of urbanization and other social factors," such as SES. Hence, we might predict that the *context* of married and employed batterers is important, not their status. Batterers for whom arrest works may exist in social arrangements with a multitude of ties to resources that either transmit disapproval for the abusive act or help a batterer stop his violence. Arresting these batterers may serve to signal disapproval of the batterer's actions, as well as expose the batterer to services that can address his abusive behavior. Similarly, arrest may create a social response from the social network of these victims, setting in action more protective factors for victims or bringing about rewards for victims help-seeking behaviors (Browning 1999; Ferraro and Johnson 1983).

Based on an integration of the literature, a community-level treatment model using informal social control must include these important specifications: (a) Arrest is viewed in the Durkheimian sense as a normative message acting on the community in which the offender resides rather than an instrument of achieving specific or general deterrence. As noted by Berger (1963), Durkheim saw society as the external shell that pre-determines human actions. Change in the structure--from one that supports violence against women to a zero tolerance paradigm--is facilitated by the normative message of arrest (Stark 1993). (b) Members of a civil society are strongly linked to resources, those capacities and collective technologies of human populations (Durkheim 1933; Weber 1968), but resources cannot exist in a vacuum. They are linked via social networks, which form a necessary condition for the influence, transmission, and communication of shared expectations and the development of mutual trust across networks (*see*, Morenoff et al. 2001). If the deterrence message requires legitimacy and a wide communication axis, a shared consensus on violence against women delivered through trusted and legitimate voices is also an important context in which batterer

recidivism may decline (Gondolf 1998). (c) Other social network resources have a preventive effect on helping batterers stop their violence (Edelson and Tolman 1992), and stable resource networks reduce both domestic violence and child abuse (Kurasha 1994). This third specification is to recognize and clarify existing linkages, not to necessarily implant or superimpose new ones.

THE CURRENT STUDY

The current research permits a test of actual treatments that serve as very tentative proxies for aspects of the theoretical models described above. Results will necessarily be quite tentative, because of important limitations associated with the treatments themselves and with these data, including sample size. However, the analysis conducted is an important step in advancing tests of alternate theoretical models and can provide findings that will help refine future research questions and future research designs. Subsequent research can build upon the treatments described and tested here in order to provide for a more rigorous test of new theoretical models. Specifically, future research should consider testing the way that specific interventions, grounded in the proposed theoretical models, effect intermediary outcomes, like changing stakes in conformity and community norms.

Research Design

The current study was conducted in a West Coast county among 15 police jurisdictions over the course of 30 months.¹ The focus of the research was to test the independent and combined effects of individual-level treatments, community-level treatments, and arrest on domestic violence recidivism. Treatments were implemented in diverse and distinct communities, consisting of four primary types: high-density housing populated by impoverished migrant Latinos; subsidized housing populated mainly by African American residents; lower- to middle-class suburbs with Latino, African American, and Anglo residents; and communities of well-to-do Anglos living in some of the most expensive real estate in the country. The sample consisted of 474 male suspects arrested for domestic violence offenses during the 30-month study period.²

Due to ethical and practical considerations, a longitudinal, quasi-experimental design was utilized, because random assignment of suspects to treatment conditions was not possible. Based on a review of the literature, however, it was concluded that a quasi-experimental longitudinal design could still offer a rigorous research approach and actually carried with it distinct advantages. For instance, the six-month trial allowed for ironing out potential bias, and the 24-month longitudinal design allowed for control over historical events, program changes, and other extraneous factors.

In addition, we were not implementing arrest, but testing treatments combined with arrest as arrest was currently practiced in a variety of agencies. Given the fact data were drawn from arrests made by 15 law enforcement jurisdictions, randomized arrests were not feasible and would have led to a missed opportunity to subject the policy *as practiced* to a rigorous test. We believed allowing implementation and practice to go ahead based on whatever natural pressures, handicaps, and motivations existed and then controlling for issues in analysis would lead to findings that were robust and externally valid.

True field experiments, especially with regard to mandatory arrest for domestic violence, seldom work in practice as they are designed, which requires researchers to correct for potential limitations later on in analysis (Sherman and Berk 1984b; Weiss and Boruch 1996). The actual practices of police officers in the field did present challenges to the MDVE design (Weiss and Boruch 1996), even though statistical analysis found no bias from the officers' actions (Sherman 1992, Appendix 1). Given that previous arrest experiments also showed weaknesses in internal and external validity (Binder and Meeker 1993) and other shortcomings, Manning (1993) actually refers to them as quasi-experiments because of implementation difficulties.

Another feature of the current study was its control over prosecution policy. Past studies were plagued by poor controls over prosecution (Sherman and Smith 1992), because they took place in a variety of cities with differences in prosecutorial policies regarding domestic violence offenses. The arrangement for this study allowed for the test of treatment effects in conjunction with arrests that took place in cities from within the same county where prosecution was the responsibility of the county district attorney.³

Theoretical Models as Treatments

To test elements of the Individual-Level Theoretical Model, an individual-level treatment consisting of arrest combined with an attempt to stimulate a stake in conformity was developed. To test the Community-Level Model, a second treatment was developed: a community-level treatment consisting of efforts to stimulate collective efficacy with regard to domestic violence norms.

Using a quasi-experimental design, offenders received one of four treatments: (1) arrest by itself; (2) arrest with an individual-level treatment designed to stimulate a stake in conformity; (3) arrest with a community-level treatment in the neighborhood in which the suspect resided; and (4) arrest with both individual-level and community-level treatments.

Individual-level treatment. The individual-level treatment was an intervention similar to the one detailed by Gamache et al. (1988) in which volunteers met with

arrested male domestic violence suspects as they went through the booking process. The volunteers were former domestic violence offenders who had been recruited to deliver the point-of-booking intervention. All volunteers completed a 52-week batterer intervention program. When a volunteer was paged to the jail, and an arrestee agreed to meet with him, the treatment lasted approximately 30 minutes. During the meeting, the volunteer confronted the suspect's use of violence and provided the suspect with information about community services that could help the suspect.

The point-of-booking intervention used at the jail corresponds with the desired elements of an individual-level treatment discussed in the first theoretical model. First, a multitude of voices was used to deliver an anti-domestic violence message to suspects through the arrest, administered by law enforcement, and through the point-of-booking intervention, administered by community volunteers.

Second, even though volunteers were not allowed to discuss guilt or innocence with arrestees, they did refer to police reports to find evidence to confront a batterer's denial and his views regarding the acceptability of the battering. ("Look, it's right in the report, she had bruises on her neck and a black eye.") If suspects tried to shift blame or minimize their actions, the volunteers might further legitimize the anti-domestic violence message and confront the batterers' denial by telling the arrestee, "I used to say the same things". Additionally, the message sent to suspects may have been viewed as more legitimate since it came from former batterers familiar with domestic violence offending. Any claim that the volunteers were perceived as legitimate bearers of an anti-domestic violence message is speculative and further research in this area is needed, but the theoretical models advanced here suggest there are multiple intermediate outcomes that are important for future research to consider.

Third, the reintegrative element of the individual-level treatment consisted of volunteers encouraging suspects to attend a batterer support group. The volunteers' presence at the jail demonstrated that formerly violent men could serve an important role in the community, working with law enforcement and victim advocates, to help others change their behavior. The claim that the point-of-booking intervention could stimulate a stake in conformity among male arrestees is based on the notion that the volunteers who delivered the treatment symbolize the improved community status that can be achieved if one conforms their behavior in accordance with the law. Thus, the message sent to a suspect is that he can also gain a new and important role in society if he accepts the anti-domestic violence message presented in the individual-level treatment.

Community-level treatment. This treatment involved community education and organizing (*see*, Appendix for

complete description). The sessions were used to clarify local norms and resource networks.⁴ Three elements in the treatment were essential for construct validity as defined in the community-level theoretical model. Here it is worth clarifying that collective efficacy has been defined as shared expectations and mutual trust that forms cohesion, not something stimulated by shared expectations and cohesion/mutual trust. Therefore, for purposes of this study, “stimulating” referred to changing existing expectations regarding domestic violence to coincide with formal social control aims, expanding the legitimacy of anti-domestic violence messages, and identifying and clarifying resources to help batterers and victims.

The treatment’s construct validity was maintained by the following elements: (a) a focus on confronting existing norms and working toward clarifying shared norms and expectations for social control with regard to domestic violence; (b) legitimizing the process of arrest and the nonviolent norm; and (c) identifying and clarifying important resources within the community for dealing with domestic violence (*see*, note 5 and Appendix). It is not central to the community-level theoretical model or the intervention discussed here that the batterers themselves be exposed to community activities, like outreach and education meetings. Rather, the intervention seeks to stimulate a shared consensus among many members of the community. Thus, it is not necessary that batterers receive anti-domestic violence messages from formal community education and events. The assumption is that batterers will receive messages informally through the actions and messages of other members in their community who have been exposed to such education and activities. The potential of the community-level theoretical model is that it invokes resources and power that are indigenous to the larger community.

Data and Measures

Outcome measure: Repeat domestic violence. Because of limitations placed on the study by victim-advocates, we relied on measuring recidivism by referring to official police records on domestic violence-related offenses. One limitation of relying on official arrest data is that such data may under-represent the prevalence and incidence of domestic abuse or assault when the victim and offender know each other (Manning 1993; McDermott 1979). Maxwell et al. (2002:63) found that “23.1% of the suspects in the research sample had one or more reported offenses after the experimental incident,” whereas 42.5% of the victims reported “at least one new victimization by the suspect.” Such findings suggest that data gathered from victims may provide a better measure of the incidence of re-offending. In addition, some treatments implied by the theoretical models discussed here might lead to

changes in arrest data that are unrelated to actual changes in the behavior of intimates. For example, community-level treatments may lead to increased reporting to police, which can, in turn, lead to increased arrests in the aggregate.

Gathering data from survivors would have allowed for measurement of unique and valuable outcomes and intermediate processes that result from interventions. Survivors are in a distinct position to say something about evolving community norms toward partner abuse. There is some disagreement in the literature, however, regarding whether or not such surveys provide reliable measures of repeat violence, especially given problems identified in earlier studies (*see*, Sherman and Smith 1992:684; Berk et al. 1992).

Our recidivism measure was any domestic violence-related offense during a six-month follow-up. As it turns out, 72 percent of these repeat offenses involved felonies, which by the state’s penal code, requires evidence of a physical injury (“whether of a minor or serious nature”) to make an arrest. Hence these data do reflect real events occurring in the community as measured by an observer (a police officer) trained to a legal standard. In addition, these data were collected systematically (Weis 1989) and measure recidivism among subjects likely to have had contact with the criminal justice system (Berk and Newton 1985). Thus, the measure of recidivism employed offers some assurances of capturing valid and conservative representations of treatment effectiveness. Finally, rearrest data have been used frequently in replication studies and have become a standard metric (*see*, Woolredge and Thistlethwaite 2002).

Dependent variable. A subject entered the study when he was arrested for a domestic violence-related offense at any time during the project. The first arrest acted as the beginning of the study period for each offender. A subsequent arrest for any domestic violence-related offense occurring during the study period was recorded and counted as recidivism.

Independent variables. The first independent variable (IV) was the individual-level treatment at the jail. A suspect brought to the jail for booking was assigned the treatment when a volunteer was available to respond. This variable was coded as dichotomous (treatment or no treatment). Volunteers kept logs of each individual who received the point-of-booking intervention and these data were cross-referenced with the list of every suspect booked for a domestic violence-related offense in the county. For data analysis purposes, offenders receiving the individual-level treatment were coded as Treatment 1 YES. All others received a Treatment 1 NO code.

The second IV was the community-level treatment. This variable was dichotomized to designate whether or not community-based activities occurred in the

Table 1. Recidivism within Six Months by Level of Arrest, Treatment 1, & Treatment 2 (N=474).

Level	N	Percent who Recidivated
Arrested, Treatment 1 Yes, Treatment 2 Yes	120	1.6 (n=2)
Arrested and Treatment 2 Yes only	138	4.3 (n=6)
Arrested and Treatment 1 Yes only	78	14 (n=11)
Arrest only	138	8 (n=11)

Notes: Recidivism was measured by rearrest for at least one domestic violence related offense during a 6-month follow-up; Treatment 1 Yes=received individual-level treatment at the jail; and Treatment 2 Yes=suspect’s area of residence was exposed to community-level treatment.

suspect’s place of residence up to 30 days before or 30 days after the arrest that brought the suspect into the study. A suspect’s place of residence was defined as the suspect’s street address surrounded by a two-block catchment area.⁵ If a community-based activity took place within two blocks of a suspect’s residence, 30 days before or after his first recorded arrest, that offender was coded as having received the community-level treatment (Treatment 2 YES code). All other offenders were coded as not having received the community-level treatment (Treatment 2 NO code).

FINDINGS

To explore the possibility that delivery bias occurred in the distribution of the individual- and community-level treatments, an examination was conducted to assess the relationship, if any, between a suspect’s exposure to the treatments and variables, such as, the suspect’s ethnicity, offense seriousness, and area of residency (ZIP and city). Less than half of all suspects (n = 198, 42%) received the individual-level treatment after the arrest that brought them into the study, and slightly more than half of all suspects (n = 258, 54%) lived in an area exposed to a community-level treatment within the measured time frame of their first arrest.⁶

Most of the suspects in the study were Anglo (54%), over a quarter were Latino (28%), 14 percent were African American, and 3 percent belonged to Asian, Native American, and other ethnic groups. There was no significant relationship between ethnicity and treatment delivery. A large number of the suspects (41%) were charged with a felony plus some additional charge. Thirty-one percent were charged with a single felony or a misdemeanor plus an open charge. The remaining suspects (28%) were charged with misdemeanor offenses only. There was no significant relationship between offense seriousness and delivery of the individual-level treatment. These data do show, however, a relationship between offense seriousness and delivery of the community-level treatment ($\chi^2 = 7.58$, d.f. = 2, $p = .02$).⁷ Those who were initially charged with a single felony were less likely to have received the community-level treatment than those charged with a misdemeanor only or those charged with multiple felonies or felonies plus misdemeanor offenses. Overall, the lack of significant relationships between

individual characteristics and treatment delivery means that several potential spurious relationships can be ruled out with a degree of confidence.

Outcome Measures

Recidivism was measured as any arrest occurring within six months after the arrest that brought the suspect into the study. Six months is the standard used in previous research and there is evidence that treatment effects may deteriorate and backfire beyond six months (Sherman et al. 1991). Of the 474 suspects included in the current analysis, six percent (n = 30) were rearrested within six months of entering the study. On average, there were 217 days between a suspect’s initial arrest and his first rearrest. Of the suspects that were never rearrested, they were at risk for an average of 566 days.⁸

The treatment effects on suspect’s recidivism are shown in Table 1. Keep in mind that all men had been arrested at least once and either received subsequent exposure to Treatment 1 only, Treatment 2 only, Treatment 1 and Treatment 2, or neither treatment. As Table 1 shows, approximately two percent of the suspects exposed to both treatment models were rearrested at least once in the six months following the arrest that resulted in their entry into the study. Four percent of the suspects who were arrested and received only the community-level treatment (Treatment 2) were rearrested. Both of these results were lower than recidivism among those who received only arrest and the individual-level treatment (Treatment 1) (14%) and those who were arrested and received neither treatment (8%).

Treatment Effects (Survival Analysis)

One methodological difficulty with following suspects after their initial arrest is the variable time at risk. In other words, suspects arrested at the start of the study are subject to data collection for a longer time period, meaning they have a better chance of being rearrested than suspects entering the study towards the end of the study period. To address this issue survival analysis, as used in the Omaha experiment (Dunford et al. 1990), was employed to analyze recidivism data. Life tables allowed for the control of variable time at which suspects were at risk for rearrest and thus allowed for an examination of treatment effects over a longer

period of time.

A life table comparing each treatment is shown in Figure 1, which reports a Wilcoxon (Gehan) Statistic. This statistic represents a chi-square for the probability that the survival distributions are the same for compared treatment conditions. Unlike the findings from the six-month follow-up, the survival comparison revealed that over the long run no intervention by itself performed any better than arrest by itself. Table 2 summarizes the differences in survival likelihood between the treatment conditions. The community-level treatment did perform

well by itself, but only in comparison to the survival rate of suspects who received the individual-level treatment.

The largest number of suspects surviving until the end of the study period (95.83%) were exposed to arrest and a combination of both treatments; a significantly larger proportion than the 83.33 percent who survived with an arrest only. Similarly, the combination of interventions was more effective than the individual-level treatment, which performed worse than arrest by itself.

Figure 1. Survival Functions for Rearrest by Treatment Models.

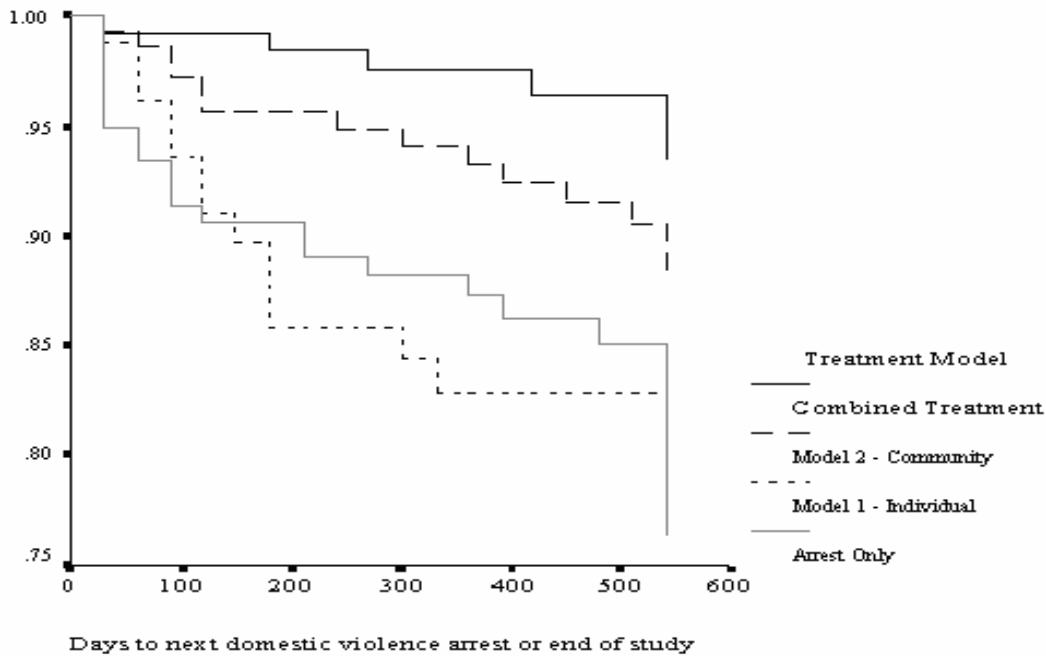


Table 2. Summary Results of Survival Analysis.

Treatment Comparison	N	Percent Surviving	Gehan Statistic	Probability
Individual-level only	78	83.3		
Community-level only	138	90.6	4.43	.04
Arrest only	138	83.3		
Individual & Community	120	95.8	9.83	.00
Individual-level only	78	83.3		
Individual & Community	120	95.8	11.13	.00

Notes: Overall model statistic = 14.40, 3 df, p=.00.

Post-Hoc Analysis

Although a total of 198 suspects did receive the individual-level treatment, some did not receive it either because jail staff did not always page a volunteer, volunteers failed to answer pages, or a volunteer was not available during the time one was requested. Because either jail staff or volunteers failed to fully implement the treatment for reasons discussed above, we initially grew concerned with potential variability, and thus bias, in treatment delivery. An analysis failed to show any evidence that delivery of the point of booking intervention was biased due to suspect characteristics, such as, ethnicity, area of residency, or offense seriousness.⁹ While there was some variability in these data due to factors like volunteer availability, in effect the individual-level treatment became randomly distributed due to programmatic constraints.

One potential threat to the validity of conclusions about the effects of the community-level intervention is that treatment delivery may have been associated with the socioeconomic status of the location. Thus, the relationship between re-arrest rates and the community treatment might be spurious since these effects may be due to socioeconomic factors rather than to any treatment effects. In order to better understand this possibility, an examination was conducted of 1990 Census tract data on median household income for areas in which the community-level treatments were delivered. Just over 83 percent of interventions took place in tracts with median household incomes of \$29,999 and below (43.3%) and incomes between \$30,000 and \$59,999 (40.0%). Areas with median incomes of \$60,000 and above experienced 16.7 percent of the intervention sessions. Given these findings, further exploration of the impact that economic resources may have played on the relationship between the community treatment and outcomes did not seem warranted and was not pursued.

Although the evidence suggests that treatment groups were equivalent on important variables, the community-level model was significantly associated with offense seriousness. This relationship likely exists because the largest and most sophisticated agencies have protocols that encourage more stringent charging. As we might expect, the community-level intervention was delivered more frequently in the two largest cities. Hence, it is likely that charging practices in those two departments are reflected in our finding of a relationship between the delivery of the community-level intervention and the seriousness of the offense. Still, the effect of the community-level model held even within these cities.

DISCUSSION

Following the ambition and vigor that marked the initial domestic violence arrest experiments and the

subsequent interest in the phenomena, more recent field research has fallen short of providing authoritative findings than can forward the debate on the efficacy of arrest. What remains is research that is often method driven (Dobash and Dobash 2000), pragmatic, atheoretical (Manning 1993), or focused on increased cooperation among criminal justice agencies late in the stages of case processing, such as prosecutors, courts, and correctional institutions (*e.g., see*, Dakis 1995; Dobash et al. 1996; Lerman 1992; Polsby 1992; Tolman and Weisz 1995).

Recent evidence suggests that the deterrent effect of arrest fares no better when combined with formal criminal justice responses such as prosecution and court sanctions (Davis, Smith, and Nickles 1998) or court-mandated counseling (Feder and Forde 2000). Such studies raise doubts about the efficacy of what Sherman (1997:25) refers to as the “full enforcement” hypothesis.

There is an important message in all of this research: domestic violence is a complex problem. It is unlikely influenced by the relatively unrefined and unimaginative model offered by arrest and prosecution (Egger 1995; Hutchinson and Hirschel 1994). Instead, policymakers and scholars might endeavor to design and investigate new strategies for assessing relationships between arrest and informal and formal social controls (Dunford et al 1990). As Berk et al. (1992:703) concede, “If we could construct a more plausible model, we should offer it up.”

In this study, the attempt was to offer a new, more plausible test of interventions that were theoretically parsimonious but compelling. Unlike earlier studies on pro-arrest policies, comparisons were not made between the efficacy of arrest as compared to alternative treatments, such as cite and release or mediation. Instead the relationship between arrest and a combination of treatment models considered more imaginative and theoretically defined than those used in the past was examined.

The findings reveal that getting deterrence right, even with an effort to stimulate a stake in conformity, is a tough bet. Over the long term, arrest, combined with a focused and well-designed treatment, delivered directly to subjects at the individual level may be less effective at reducing recidivism rates than delivery of a community-level treatment. In short, the findings support further evidence of disconfirmation for a specific deterrence model by itself. However, if some community-level treatments are combined with arrest and with individual-level responses to battering, measurable reductions in recidivism rates do occur. Keep in mind, this is with relatively modest community-level treatments dispersed in neighborhoods.

The research design employed does not guarantee that the treatment groups were completely equivalent, however strengths of the design employed which create

more confidence in the study findings include: (1) the length of the study, (2) the fact that suspects entered the study with a presenting offense at different points in time, (3) the assignment of the community-level treatment at different points in time, and (4) statistical controls for possible bias.

It is encouraging that our recidivism rate (11%) is the same found in Omaha (Dunford et al. 1990) and close to those reported in other studies (*see*, Sherman and Berk 1983b). Still, over 75 percent of the cases were censored in the survival analysis, meaning a large number of suspects across all treatments did not experience a rearrest. Furthermore, as with all research on such a controversial topic, these findings only offer the best recent attempt to study elusive theoretical themes in light of a now institutionalized and widespread police practice.

Nonetheless, we can raise two important points in light of our findings. The first concerns the difficulties with deterrence; the second underscores the fairly unexplored realm of community-level interventions.

Deterrence's Tough Bet

First, when it comes to domestic violence, efforts to focus on the offender and tweak deterrents accordingly may offer only diminishing returns for future research. As noted earlier, other investigations that tested efforts to strengthen and enhance only formal criminal justice responses to domestic violence did not provide convincing evidence for pursuing such strategies. There is, of course, always room to argue for more dosage. Perhaps the stimulated stake in conformity used in this study was too weak and incorrectly specified. While this is always a possibility, great care was taken to loosely tie the treatment to existing theory, something previous research has been loath to do. More important, this particular treatment required a tremendous programmatic effort between law enforcement and community volunteers, and thus likely represents taking a single-treatment model as far as possible within the context of the arrest policy.

Communities and Family Matters

The surprising result of the analysis was the significance of the community-level treatment. Why was this finding surprising? At first glance, the individual-level treatment seemed specific and intensive, reinforced by swift punishment in the form of an arrest. The community-level treatment was seemingly diffuse without any guarantee suspects or victims were ever directly exposed to it. A return to the literature helps to illuminate some reasons for our findings. Deterrence is born of a philosophical tradition; social disorganization and collective efficacy are scientific enterprises. Perhaps community support increases the chances that survivors will seek help,

formally or informally. Whether the community-level intervention is more likely to lead to help-seeking behavior on the part of a victim is a point worthy of greater inquiry. One recent study suggests that this assumption may not be reasonable. Block and Skogan (2001) found that neighborhood collective efficacy was not associated with increased help-seeking behaviors by female survivors of domestic violence in Chicago.

CONCLUSION

By outlining an intervention model based on a body of knowledge that is grounded in the scientific method, an attempt was made in this study to connect the individual and his deviance to the context of his social existence. Again, there is always room for improvement. In future analyses we intend to explore the mechanisms at play in the community-level intervention as it works to mitigate recidivism. The recidivism measure served as a proxy for whether or not an effort to stimulate conformity to non-violent norms and beliefs took root, but future research may actually survey suspects as to their beliefs and any change in those beliefs. Similarly, community-level surveys could help determine if beliefs and norms did in fact change in areas in which the community-level treatment took place.

With that much said, this theoretical exercise and the subsequent findings raise the prospect of an important link in the preventive effect of arrest and community. The link is important because it helps us move beyond notions of arrest and social control in domestic violence situations strictly as police-led and criminal justice enterprises. This study encourages us to look elsewhere. The evidence summons us to consider something different, mainly that there is a singular and additive power of communities to influence individual behavior. This power deserves attention when arrest is used to respond to domestic violence. It is a power that we ought to continue to observe, study, and harness in our efforts to reduce domestic violence.

ENDNOTES

1. The project began in 1997 as part of a research partnership on a Violence Against Women Office grant. This particular partnership allowed for a great deal of latitude in developing the theoretical foci for strategies that were developed and the methodological strategies utilized to test outcomes. Although the arrangement allowed for the testing of theory-guided models based on a rigorous methodology, there were some limitations because the project involved collaboration with community groups and law enforcement agencies. One of the limitations was that random assignment of suspects and neighborhoods to treatment conditions was not used in the current study. The community agency that provided the staffing and resources for the

interventions resisted randomization for two main reasons. One was ethical. Agency staff insisted on trying to provide 100 percent delivery of services whenever possible. The second was practical. The agency did not know if it would have the resources to provide interventions based on the hour, date, and place of some random design. In essence then, the delivery was to whomever, or wherever, and whenever possible. Even though this arrangement presented methodological challenges and is a potential source of weakness with the project, the research parameters were accepted, because it was important to conduct the research in the context of the reality with which most interventions would ever be delivered in a more generalizable model.

2. Subjects were admitted into the study over a 24-month time period within the 20-month study period. This was because we stopped collecting new cases after 24 months to allow for at least 6 months follow-up on all cases.

3. In this particular county, all prosecution was the responsibility of the county district attorney.

4. To clarify the message, a focus group with victims, batterers, and law enforcement officers was conducted. It was surprising to learn that officers viewed arrest as only the first step in the process of getting help for a family. The victims and offenders were more ambivalent. They supported arrest when defined as a catalyst for resources and responses, rather than punishment in and of itself.

5. Rather than census block, addresses fronting on both sides of the street bounded by the cross streets offers a more valid ecological unit of analysis for neighborhood (Perkins, Brown, and Taylor 1996). To account for the likelihood of the intervention pulling in residents from a larger area, suspect addresses were plotted with GIS software against treatment locations within the noted time frame. If a treatment took place in an area bounded by two intersections beyond the suspect's address in all possible directions (roughly two full square blocks), the suspect received an intervention.

6. These men may have received the individual-level intervention after subsequent arrests. Analysis did not control for treatments that suspects may have received after subsequent arrests.

7. Two cases were missing data on the seriousness of the charged offense.

8. Arrest data were gathered from the county. If a suspect in the study moved out of the county and was rearrested, it would appear as though he was never

rearrested simply because he was no longer living in the jurisdiction. Due to data limitations it cannot be determined how frequently this occurred.

9. For example, 28 percent of those who received and 28 percent of those who did not receive the individual-level treatment were arrested for a misdemeanor; 34 percent of those who received and 30 percent of those who did not receive the individual-level treatment were arrested for a felony; 53 percent of those who received and 55 percent of those who did not receive the individual-level treatment were white; 29 percent of those who received and 27 percent of those who did not receive the individual-level treatment were Hispanic; and 15 percent of those who received and 14 percent of those who did not receive the individual-level treatment were African American. A large percentage of participants (84%) resided in seven zip code areas, and the overall pattern shows no differences between those who received or those who did not receive the individual-level intervention. In other words, the delivery of the individual intervention does not seem to be biased in terms of where the participants resided. Just over 25 percent of those who received the intervention were from the zip code area with the largest number of study participants ($n = 134$), and 30 percent of those who did not receive the intervention were from this same area. A slightly greater percentage of those who received the treatment (20%) came from one of the seven most represented zip codes, as opposed to those who did not receive the intervention (9%). This difference does not seem meaningful given the overall low numbers of men from this one zip code area ($n = 64$).

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APPENDIX

The community-level treatments were implemented under the direction of a manager (a professional community trainer paid by the victim advocate organization) who would either offer training to established community organizations and other social service providers, or meet with informal community groups (such as church groups) and train volunteers to perform outreach to others in the community. Although volunteers were included in the intervention, treatment fidelity was maintained by the paid manager who attended each event and kept track of the activities in a log. Volunteers organized various community events such as training workshops, rallies, marches, and informational presentations (lasting 1-1.5 hours in length) made to various community groups including local employers, child care centers, faith-based groups, social service organizations, counseling centers, and neighborhood action programs. Informational meetings and trainings presented information about the prevalence of domestic violence in a particular area; why domestic violence is a public concern; what services are available in a community for batterers and victims; how awareness plus community action can produce change; how collective and individual action can serve as powerful forces in reducing domestic violence; examples of successful community efforts against domestic violence; the theoretical position that many men are socialized to be violent; specific steps to deal with batterers; and a final call to action.

Events were scheduled in an area when an interested group contacted the domestic violence advocate agency and requested an event or when advocates contacted respected groups in the community and asked for cooperation in scheduling an event. It is unknown whether any particular batterer included in the study attended or was directly exposed to the community-level intervention.