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No-Contact Orders, Victim Safety, and Offender Recidivism in Cases of Misdemeanor Criminal Domestic Violence: A Randomized Experiment

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Abstract Using an experimental design, this research examined the impact of proactive enforcement of court-imposed no-contact orders (NCOs) on offender behavior and victim safety in cases of misdemeanor domestic violence. The major research goals and objectives were to assess whether proactive enforcement: (1) enhanced victim safety by reducing offender recidivism; (2) increased victim knowledge about no-contact orders; and (3) reduced contact between offenders and victims. A prospective design was used to randomly assign 466 cases of misdemeanor criminal domestic violence to either systematic, proactive enforcement or to routine, reactive enforcement of the court-ordered no-contact order conditions. Treatment effectiveness was assessed by analyses of official criminal records data and victim survey data. Study results suggest that the treatment had no impact on victim safety or offender recidivism. Notably, victims in the treatment group were more likely to be aware that the no-contact order was in place, had higher level of contact with law enforcement and victim advocates, and more often viewed the contact with their batterer as stalking or harassment. Overall, findings from

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this study suggest important directions for future research examining the effectiveness of interventions for intimate partner violence and abuse.

Keywords Domestic violence intervention · Experiment · No contact orders · Offender recidivism · Victim safety

Introduction

Domestic violence continues to challenge policy makers, social service providers, and criminal justice officials. Domestic violence advocates, criminal justice personnel, and academic research studies consistently underscore the serial nature of violence between intimate partners. Increasingly, domestic violence researchers are highlighting the importance of social and criminal justice interventions that preemptively reduce the likelihood of offender recidivism and promote victim well-being. One such policy is the use of protection or "no-contact" orders that prohibit offenders from having any contact with victims between a defendant's arraignment and sentencing. The purpose of a no-contact order (NCO) is to provide an immediate remedy to the continued threat of violence by prohibiting contact by a victim's abusive partner and serving as a symbolic threat of criminal justice system accountability. Although the use of NCOs between bond hearings and judicial proceedings is widespread, the level of compliance with these orders is not well understood. Similarly, empirical research does not offer insight as to whether proactive enforcement of NCOs reduces harm to victims, is ineffective, or is counterproductive. The goal of the current research was to evaluate the effectiveness of proactive enforcement of NCOs through the implementation of an experimental design.

Some criminal justice experts have suggested that enforcement of protection orders (POs) has the potential to offer a positive step toward increasing both victim safety and offender accountability. While a host of literature has evaluated civil protection and restraining orders in general, it is interesting that the social science literature is void of studies that specifically focus on criminal NCOs. Used to disrupt the cycle of violence between bond hearings and judicial proceedings, a NCO typically prohibits an offender from contacting a victim during the period between arraignment and case disposition. Although some researchers have suggested that POs are rarely treated seriously by the police or the courts (Rigakos 1995), there is some evidence to suggest that the police and the criminal justice system may be more responsive to victims' calls for help after they have obtained a PO (Chaudhuri and Daly 1992). This responsiveness includes a greater likelihood of arrest in cases where the police are subsequently called to a domestic violence incident and the increased completion of the prosecution process for those cases in which a PO was in place (Weisz, Tolman, and Bennett 1998).

The Effectiveness of Protection Orders

The effect of POs on offender recidivism and subsequent victim safety has been explored in a number of studies, with mixed results reported (Carlson et al. 1999; Holt, Kernic, Wolf, and Rivara 2003; McFarlane, Malecha, Gist, Watson, Batten, Hall, and Smith 2004). Generally speaking, efforts to assess the

effectiveness of POs typically rely on observational research designs and gauge efficacy in one of three ways: 1) victims' perceptions of POs and the PO process; 2) enforcement of POs by the police; and 3) the success of POs in preventing future abuse (Burgess-Proctor 2003). Grau, Fagan and Wexler (1984) conducted one of the first evaluations of the effectiveness of civil restraining orders. Their research found no significant differences in subsequent abuse or violence for those with POs compared to those without, although they found lower rates of abuse among women with an order when they had lower initial levels of violence severity and injury. Similarly, Klein's (1996) analysis of restraining order cases found that half of all batterers re-abused their victim within 2 years of the issuance of the order and that recidivism did not differ between those who maintained the order and those who dropped it. Klein suggested that the optimal use of POs might be in conjunction with vigorous prosecution and significant sanctioning of batterers. Based on the earlier research, Davis and Smith (1995) provided a bleak picture of the effectiveness of restraining and POs, especially for those women with lengthy prior histories of abuse at the hands of their intimate partners (Klein 1996), suggesting that these orders may not be effective in reducing revictimization.

Meloy and colleagues (1997) meta analysis of 11 studies on the effectiveness of POs suggests that the severity of violence experienced by the victim (Keilitz, Hannaford and Efkeman 1998) and the laxity of enforcement of the protective order are likely to reduce the deterrent effects of these orders. Yet, Klein and Orloff (1996; p. 215) have suggested "civil protective orders that are properly drafted and consistently enforced can offer effective protection for victims of domestic violence." Similarly, research by Weisz, Tolman, and Bennett (1998) suggests that the level of police intervention prior to the issuance of the order was associated with the level of subsequent intervention.

Some research has identified important deterrent effects of court and criminal justice-based POs (Sherman, Gottfredson, MacKenzie, Eck, Reuter, and Bushway 1998), suggesting that POs may reduce the risk of revictimization (Carlson et al. 1999; Holt et al. 2003; McFarlane et al. 2004) and improve victim well-being (Johnson, Luna, and Stein 2003; Keilitz, Davis, Efkeman, Flango, and Hannaford 1998). Holt and colleagues (2002, 2003) examined the effects of POs on offender recidivism based on data from two groups of women—one group had obtained a temporary or permanent civil protective order and the other group had contacted the police after being abused but had not obtained an order. Findings indicated that POs decreased the risk of contact by the abuser and resulted in fewer injuries and violent threats with weapons. They also found that the greatest effect of POs on reducing the risk of re-victimization was for those cases in which the order was kept in place. Similarly, Carlson et al. (1999) found a 66 % decline in the probability of abuse following the issuance of the protective order compared to women's experiences with violence prior to the issuance of the order. At the same time, however, their findings indicated no differences in recidivism outcomes for those women who had obtained a permanent order versus a temporary order. Consistent with research on the effects of mandatory arrest policies and the "stake in conformity" thesis, POs may be a particularly effective deterrent for some offenders–principally those who have had no prior arrests and have had

limited contact with the criminal justice system (Adhikari, Reinhard, and Johnson 1993).

Holt et al. (2003) argued that POs have become more effective since the shift from being a civil, victim-initiated action to a criminal justice system response that includes enforcement by the police. Carlson et al.'s (1999) study evaluated the effectiveness of POs by estimating the relative risk of re-abuse for women who procured these orders over a 2-years period. They found that 68 % of the women in the sample reported some form of physical violence in the 2-years period preceding the PO. During the 2-years period after the PO, only 23 % of the women reported subsequent re-abuse. Fischer (1992) suggested that it is important to conceptualize the effectiveness of POs in a broad fashion, pointing to the need to examine whether POs empower the victim in her decision-making and in actions intended to end a violent intimate relationship. Fischer and Rose noted that certain women feel empowered after obtaining a PO, saying that: "…it makes you feel as if you ate a can of spinach, like Popeye" and "…it's just like you're spreading your wings all over again" (p. 424). Mills (1998) suggested research needs to develop questionnaire measures that tap empowerment in terms of a woman's perceived ability to assert her own will. This is consistent with research suggesting that the perceived effectiveness of the order may be independent of whether there was a violation or breach of the order or subsequent violence. In other words, an assessment of the effectiveness of POs may reflect to a greater extent the impact of the order on victim well-being more generally, including the woman's self-esteem and efficacy (Johnson et al. 2003; Keilitz, Davis et al. 1998; Keilitz, Davis, Efkeman, Flango, and Hannaford 1998).

Limitations of the Prior Research on Protection Orders

Fagan (1996) has suggested that the failure to find strong support for the positive effects of criminal justice-oriented domestic violence interventions, including PO effectiveness, may be due to a number of analytic and measurement issues. Most studies have been primarily qualitative or have used non-experimental designs relying on purposive sampling. Selection bias, in the form of sample attrition and the exclusion of some cases, also plagues much of the research. In viewing POs as a promising solution in combating domestic violence, Sherman et al. (1998) suggested that an important area for future research is the use of randomized trials to provide a strong test of the effectiveness of orders of protection. Another important limitation of the research on POs concerns the samples that are used, especially when focusing on which types of women are more or less likely to obtain an order of protection. Usually samples include women who are already in the process or are about to begin the process of obtaining a PO. Therefore, most of the samples used in prior research are contaminated by self-selection bias. Only a few studies actually include a control group, which is a crucial factor; the absence of a control group presents difficulties when generalizing study findings. Low response rates may also limit the credibility of earlier findings (Holt et al. 2003). As with any study, researchers encounter the problem of underreporting and have to determine what type of data are best to use (i.e., official statistics versus self-report data). In the case of POs, and especially the violation of

them, it is beneficial to combine and compare these sources, since many times women are hesitant to report re-abuse to the police after a PO has taken effect. Given the limitations of previous research, Capshew and McNeece (2000) argued that it might be too soon to draw any firm conclusions on the effectiveness of POs as intimate partner violence interventions.

Summary

In addition to issuing a PO, the system must take action against further domestic violence by initiating stronger prosecution and tougher sentencing guidelines against first time and repeat domestic violence offenders. The effectiveness of POs does depend, in part, on mandated sanctions for violations and on police response to violations (Holt et al. 2003). DeJong and Burgess-Proctor (2006) also stressed that the successful use of POs hinges on enforcement by police officers, and that both petitioners and respondents must take them seriously. Additionally, research shows that the criminal justice system should take into account demographic factors of victims and offenders in its quest for effective domestic violence interventions (Carlson et al. 1999). Finally, to fully examine the effectiveness any type of PO requires a thorough understanding of the level of enforcement that occurs. Rigakos (1997) suggests that research on the effectiveness of POs will depend to a large degree on whether these orders are enforced by the police (especially through arrest) and prosecuted by the courts.

To increase the effectiveness of POs, Keilitz (1994) suggested that the criminal justice system needs to take steps to increase their power to provide safety to victims while enforcing orders. Gondolf, McWilliams, Hart, and Stuehling (1994) have argued that the systems in place for the enforcement of POs are lacking and (Grau et al. 1984) have suggested the need for comprehensive legislation that coordinates civil and criminal remedies. Finn (1991) suggests that formal policies regarding the violation of POs be developed and enforced that encourage respect for the court's order thereby increasing compliance. The issuance and enforcement of POs, therefore, need elaborate safety plans that include systematic investigation and stringent law enforcement of breaches of POs that simultaneously link victims to appropriate health and social services (Finn 1991; Keilitz 1994) and provide police officers with up-to-date electronic information on POs to alleviate confusion (Rigakos 1997).

Methods

Research Design

This research was conducted in Lexington County, South Carolina, in cooperation with the Lexington County Criminal Domestic Violence Court and involved cases of domestic violence with NCOs (see Brame, Kaukinen, Gover, and Lattimore 2009; see also Gover et al. 2003). Using a prospective, experimental design, the goal of the current study was to estimate the effect of proactive law enforcement contact with domestic violence victims on their safety and well-being. The NCO restricted the offender from having any kind of contact with the victim between the offender's release on bond and case

disposition. The victim-directed intervention consisted of proactive contacts by law enforcement that were designed to accomplish three objectives: (1) to ensure that victims understood the purpose and offender requirements of no-contact orders; (2) to advise victims on how to collect evidence and mobilize law enforcement in the event that a violation occurred; and (3) to monitor no-contact order compliance. These contacts were scheduled to occur before first appearance. Ideally, the contacts would have been in-person but they could also have been by telephone. For offenders whose cases continued after first appearance, additional contacts were scheduled. An important feature of the proactive scheduled contacts is that they are relatively easy and inexpensive to implement provided the victim can be located. Moreover, these contacts were in addition to any contacts that would have occurred with law enforcement, prosecutors, and other court personnel in the absence of the intervention.

The unit of analysis in this study is each of the 517 domestic violence cases enrolled in the study. A "case" includes information about the offender, the victim, the incident, which led to its inclusion in the study, and post-enrollment information about contacts and recidivism derived from administrative records and interviews with victims. Cases in the control group received routine, reactive status quo enforcement of the court-ordered NCO conditions. Under the status quo control condition, a variety of different types of enforcement were possible. For example, other officers in the department as well as law enforcement victim advocates and the prosecutor could interact with either the victim or the offender. These contacts depended on the individual circumstances of each case and occurred for both the treatment and control groups. In order to implement the treatment, the Lexington County Sheriff's Department (LCSD) assigned a deputy sheriff (referred to hereafter as the "Dedicated Officer" or "DO") to receive paperwork identifying cases where an offender was released from bond court with a no-contact order as a condition of their release. The Court Administrator provided photocopies of the bond restrictions to the DO, who entered the case information into a database.

The DO began enrolling cases in November 2005 and enrollment progressed continuously until September 2006 when the DO was reassigned. In late December 2006, a new DO began work on the project, resuming the prior DO's responsibilities of case enrollment and proactive contacts until July 2007 when the study ended. During the period between the departure of the first DO and the arrival of the second DO, the court administrator provided information about new cases on several of the dockets directly to us. Because these cases did not receive the treatment, we wanted to explore the idea of studying them as de facto control cases. These cases are referred to hereafter as "interim controls." Letters on LCSD letterhead with a brochure introducing the DO as a LCSD point of contact and an explanation of the NCO over the DO's signature were mailed to each victim in the treatment group.

The database program randomly assigned each newly enrolled case to a treatment group or control group and printed out a proactive contact schedule for treatment cases. Starting in the fall of 2005, 466 cases were randomly assigned to either the treatment (N=237) or control condition (N=229) and were enrolled in the study. The analysis also included 51 interim control group cases. Random assignment was used to ensure that treatment and control group cases would be comparable to each other prior to treatment delivery. The DO targeted cases in the treatment group for proactive, victim-directed contacts and maintained a log of those contact efforts using a database. Victims of offenders who were randomly assigned to the treatment group

received the proactive victim intervention (contact). The DO also provided a log of newly enrolled cases to the research team on a regular basis throughout the study, thus enabling the research team to contact victims for interviews.

Victim Interviews

The incident that led to each case's inclusion in the study is hereafter referred to as the "gateway" arrest or incident. Interviews were targeted to occur at 6 weeks after the gateway incident (Time 1) and then again at 6 months after the gateway incident (Time 2). Interviews with victims commenced in January 2006. Efforts were made to contact each of the 437 female victims enrolled in the study (victims of the defendants who were randomly assigned to the treatment and control groups). Our standardized interview solicitation process involved an initial effort to contact the victim with a letter introducing the "Women's Health and Well-Being Study." For safety reasons, the letters did not mention domestic violence and only referred to the study as research related to "Women's Health and Well-Being." Victims were told that they would receive $50 compensation for their time and effort if they agreed to participate in the study.[1] The vast majority of the interviews were face-to-face meetings conducted in public areas at a local hospital (with 11 initial interviews and 10 follow-up interviews were conducted via telephone).

Official Record Searches

At the conclusion of study enrollment during summer 2007, we began the process of organizing the DO case enrollment database and seeking access to case booking information for the cases enrolled in the study. At this stage, we sought specific information from the booking sheets for each gateway incident. After obtaining this information, we obtained arrest records (both before and after the gateway incident), and court dispositions for the cases enrolled in the study.

The DO acquired official Lexington County Detention Center booking sheets for each offender in the study. An initial effort was undertaken during the fall of 2007 to acquire all criminal history information for all offenders, and all of this information was entered into the analysis databases. Cases with missing or discrepant information (i.e., information on the booking sheet did not correspond with information in the criminal history) were flagged for discussion with the DO. In March 2008, a final criminal history check was conducted on each randomized case (all on the same day) and in early April 2008 a final criminal history check was conducted on the interim control cases (all on the same day). The criminal history checks involved national searches, which queried the National Criminal Information Center (NCIC) database. All criminal history information was entered into our analysis databases.

[1] Although there were 80 female offenders processed by the court during the study period, only cases involving male offenders and female victims were targeted for interviews. There were several reasons for this. First and foremost, the research was supported by monies provided under the Violence Against Women Act (VAWA). Since the interviews were the most expensive part of the project, we elected to devote our resources to securing interviews with female victims in male-offender/female-victim cases. Second, the vast majority of the cases enrolled in our study (86 %) were male-offender/female-victim cases. Third, the development of a separate set of interviews and protocols for female-offender/male-victim cases would have significantly increased project complexity and logistical difficulty.

Findings

Overview

We begin our description of the results by presenting information on the comparability of the treatment and control groups on pretreatment characteristics. Next, we discuss the implementation of the treatment condition for cases in the treatment group. We then present information on recidivism and victim safety outcomes from the official record data. Finally, we assess recidivism and victim safety outcomes using data from the victim interviews. This discussion focuses on response rates along with whether the treatment influenced the victim's knowledge of the NCO, contact with the offender, and victim safety and well-being.

Treatment Implementation

Table 1 presents a summary of the contact levels for the treatment and control groups. Overall, we see that the DO expended virtually all proactive contact effort to victims in the treatment group and that very little proactive contact effort occurred with victims in the control group. We reiterate that the DO contact occurred independently of other contact the victim may have had with the LCSD, including contacts with other officers and the Law Enforcement Victim Advocates (LEVAs). Also, the information in Table 1 does not account for contact with the victims via the letter and brochure that were sent out to all victims in the treatment group after June 2006. Therefore, Table 1 provides a lower-bound estimate of the amount of contact actually created by the intervention.

The information in Table 1 suggests that establishing law enforcement contact with a population of domestic violence victims is difficult. As noted above, the DO's goal was to proactively contact each victim in the treatment group, but this was not possible in all cases. Although proactive victim contact was less than 100 %, it is likely that the conditions encountered by the DOs involved in this study in terms of their ability and availability to contact victims are typical of the conditions that officers involved in victim-directed activities would encounter in other jurisdictions.

Overall, contact efforts were made in about two-thirds of the treatment cases while contact was actually achieved in 37.1 % of the cases. Contact prior to first appearance was achieved in 25.9 % of the cases.[2] Most contact occurred either by telephone (45.2 % of the treatment cases) or in a courtroom setting (40.5 %). In addition, the vast

[2] The two DOs in our study reported different levels of effort to contact victims in the treatment group. One DO reported effort to contact in over 80 % of the treatment group cases while the other reported effort in slightly over 50 % of the cases. Despite this difference, the two DOs achieved remarkably similar successful contact rates of about 38 %. The DO with less contact effort said that some of the cases actually reached her after the case had already been disposed. We do have case disposition dates for the cases that have been disposed and dates that cases were enrolled in the study (although we do not have the dates the Lexington County Domestic Violence Court Administrator forwarded information to the DO). Since this problem occurred in both the treatment and control groups, we may be able to identify the affected cases in both groups and estimate the treatment effect without those cases. It may also be useful to identify individuals who clearly received the treatment and match them with people in the control group with similar characteristics to estimate the treatment effect as well. The treatment group described in this report is broadly construed to include everyone who was randomly assigned to the treatment condition. Thus, our analysis is best viewed as an "intent to treat" estimate of the treatment effect (Horvitz-Lennon, O'Malley, Frank, and Normand, 2005). Overall, these issues add up to the general problem of treatment noncompliance - a problem that exists in many field experiments in both the medical and social sciences.

Table 1 Contact Experiences with LCSD Dedicated Officer

Contact Description	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Any Effort At Contact (d)	237	229	51	0.6751	0.0306	0.0196	14.5057
Any Contact Success (d)	237	229	51	0.3713	0.0175	0.0000	9.5932
Time Between Gateway Arrest and First Contact Effort (c)	158	6	1	28.2089	51.0000	67.0000	−0.7501
Time Between Gateway Arrest and First Successful Contact (c)	63	3	0	21.4921	20.3333		0.1215
Any Phone Contact Effort (d)	237	229	51	0.4515	0.0262	0.0196	10.7085
Any Personal Contact Effort (d)	237	229	51	0.0169	0.0000	0.0000	N/A
Any Court Contact Effort (d)	237	229	51	0.4051	0.0044	0.0000	10.6513
Any Voicemail Contact Effort (d)	237	229	51	0.1392	0.0000	0.0000	N/A
Any Victim Initiated Contact (d)	237	229	51	0.1097	0.0044	0.0000	4.8658
Officer Contacting Victim Effort (d)	237	229	51	0.6582	0.0306	0.0196	14.2035
Officer Contacting Offender Effort (d)	237	229	51	0.0759	0.0000	0.0000	N/A
Any Effort to Contact Prior to First Appearance (d)	236	229	51	0.4873	0.0218	0.0000	11.4677
Any Success at Contacting Prior to First Appearance (d)	236	229	51	0.2585	0.0131	0.0000	7.6783

Note: a (d) indicates that this variable is dichotomous and that the mean in the table is interpreted as the proportion of individuals in that group with that characteristic; a (c) indicates that this variable is continuous or a count and that the mean should be interpreted as the sum of the scores divided by the number of people in the group. The Z-test for dichotomous variables is a test for the difference between two proportions while the Z-test for continuous/counted variables tests for the difference between two means. The Z-tests in this table only apply to comparisons between the randomized treatment and control groups. The interim controls are not included in these tests. One case in the treatment group has missing data on the date of a contact

majority of contact that occurred was initiated by the DO, with relatively little contact initiated by victims. Also, there was very little effort to contact victims in the control group (3.1 % of the cases). This intervention is best viewed as the intention and effort by the DO to contact and communicate with domestic violence victims whose offenders were under NCOs as a condition of their release. Sometimes, however, the effort to contact was successful and sometimes it was not. It is important to note that victims were under no legal obligation to cooperate with or even be available to law enforcement for the purpose of this intervention. Based on our experiences interviewing victims for this study, we are not surprised that efforts to contact victims were often unsuccessful. In our view, this difficulty should be taken into consideration whenever efforts to implement victim-directed domestic violence interventions are undertaken. Even so, it is clear that the threshold levels of contact between the DO and victims were substantially higher in the treatment group than in the control group.

Official Record Outcomes

We now turn to a comparison of officially recorded criminal activities that occurred after the gateway arrest for offenders in both groups. These comparisons are based on criminal histories obtained through NCIC record searches conducted at the LCSD and are based on the assumption that the random assignment to treatment and control groups is sufficient to ensure comparability.[3]

We first check to determine whether the length of follow-up is comparable for the two groups. Since individual cases were enrolled in our study between fall 2005 and summer 2007 and we searched all criminal histories at the same time, there is considerable variation in the length of the follow-up period for cases in our sample. A key issue, then, is whether the length of the follow-up period was approximately the same for the two groups. The first line of Table 2 shows that the average follow-up period was about 1.5 years for both groups (553 days and 541 days for the treatment and control groups, respectively) and also that the variation in follow-up time that was used to assess offender recidivism was comparable for offenders in both groups.

The next comparison in Table 2 examines the proportion of offenders who were rearrested for any offense between the gateway arrest date and the record search date. The comparison indicates that both groups were rearrested at about the same rate (treatment group =38.8 %; control group =40.6 %). We then examined the number of post-gateway arrests for both groups. The first comparison excludes offenders who had zero arrests (treatment group =1.685; control group =1.624) during the follow-up period and the second comparison includes offenders who had zero arrests (treatment group =0.654; control group = 0.659). Neither comparison suggests any important difference in the rearrest frequencies for offenders in the treatment and control groups.

We also examined new arrests occurring in South Carolina, new arrests processed by the LCSD, and arrests processed by the LCSD involving predatory crimes against the

[3] We conducted a number of supplementary analyses to adjust for pretreatment differences between the treatment and control groups (analyses not presented but available upon request). These analyses substantively resulted in the same conclusions presented here.

Table 2 Official Record Outcomes

Official Record Outcome	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Number of Days Between Gateway Arrest and Record Search Date (c)	237	229	51	552.9283	540.7817	530.7451	0.7409
Any Subsequent Arrest Record (d)	237	229	51	0.3882	0.4061	0.4902	−0.3954
Number of Subsequent Arrests (Excluding Zeros) (c)	92	93	25	1.6848	1.6237	1.6400	0.4291
Number of Subsequent Arrests (Including Zeros) (c)	237	229	51	0.6540	0.6594	0.8039	−0.0573
Any Subsequent Arrests in South Carolina (d)	237	229	51	0.3671	0.3930	0.4706	−0.5764
Any Subsequent Arrests by Lexington County Sheriff's Department (d)	237	229	51	0.2278	0.2751	0.3137	−1.1762
Any Subsequent Arrests in LCSD For Crimes Against the Same Victim (d)	235	228	49	0.0723	0.1140	0.1633	−1.5453
Any Subsequent Charges for Alcohol Violations (d)	237	229	51	0.0506	0.0480	0.0392	0.1294
Any Subsequent Charges for Assaults (d)	237	229	51	0.0127	0.0306	0.0588	−1.3338
Any Subsequent Charges for Burglary (d)	237	229	51	0.0042	0.0218	0.0196	−1.6862
Any Subsequent Charges for Child Abuse (d)	237	229	51	0.0084	0.0087	0.0196	−0.0345
Any Subsequent Charges for FTA/FTP/Attorney Contempt (d)	237	229	51	0.0127	0.0087	0.0392	0.4111
Any Subsequent Charges for All Other Contempt (d)	237	229	51	0.1097	0.0786	0.1765	1.1479
Any Subsequent Charges for Disorder Offenses (d)	237	229	51	0.0211	0.0349	0.0588	−0.9068
Any Subsequent Charges for Drug Offenses (d)	237	229	51	0.0844	0.0655	0.1176	0.7733
Any Subsequent Charges for Domestic Violence (d)	237	229	51	0.0970	0.1397	0.1961	−1.4280
Any Subsequent Charges for Driving While Impaired (d)	237	229	51	0.0380	0.0218	0.0392	1.0204
Any Subsequent Charges for Fraud (d)	237	229	51	0.0295	0.0437	0.0588	−0.8135
Any Subsequent Charges for Harassment (d)	237	229	51	0.0169	0.0000	0.0392	N/A
Any Subsequent Charges for Homicide (d)	237	229	51	0.0000	0.0000	0.0000	N/A
Any Subsequent Charges for Kidnapping (d)	237	229	51	0.0084	0.0131	0.0000	−0.4883
Any Subsequent Charges for Motor Vehicle Theft (d)	237	229	51	0.0084	0.0044	0.0196	0.5495

Table 2 (continued)

Official Record Outcome	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Any Subsequent Charges for Robbery (d)	237	229	51	0.0084	0.0000	0.0000	N/A
Any Subsequent Charges for Sex Offenses (d)	237	229	51	0.0042	0.0044	0.0000	−0.0243
Any Subsequent Charges for Theft (d)	237	229	51	0.0211	0.0393	0.0000	−1.1509
Any Subsequent Charges for Traffic Offenses (d)	237	229	51	0.0886	0.1004	0.0392	−0.4366
Any Subsequent Charges for Vandalism (d)	237	229	51	0.0169	0.0087	0.0196	0.7796
Any Subsequent Charges for Weapons Violations (d)	237	229	51	0.0084	0.0044	0.0000	0.5495
Any Subsequent Charges for Probation/Parole Violations (d)	237	229	51	0.0253	0.0306	0.0392	−0.3441
Any Subsequent Charges for Other Property Offenses (d)	237	229	51	0.0042	0.0131	0.0000	−1.0390
Any Subsequent Charges for Other Offenses (d)	237	229	51	0.0338	0.0393	0.0588	−0.3192
Any Subsequent Charges for Offenses With Missing Charges (d)	237	229	51	0.0042	0.0000	0.0000	N/A
Any Subsequent Charges for Violent Offenses (d)	237	229	51	0.1181	0.1703	0.2941	−1.6044
Any Subsequent Charges for Property Offenses (d)	237	229	51	0.0675	0.1135	0.1176	−1.7345

Note: a (d) indicates that this variable is dichotomous and that the mean in the table is interpreted as the proportion of individuals in that group with that characteristic; a (c) indicates that this variable is continuous or a count and that the mean should be interpreted as the sum of the scores divided by the number of people in the group. The Z-test for dichotomous variables is a test for the difference between two proportions while the Z-test for continuous/counted variables tests for the difference between two means. The Ztests in this table only apply to comparisons between the randomized treatment and control groups. The interim controls are not included in these tests. None of the reported comparisons are statistically significant (two-tailed $p<.05$ significance level)

Table 3 Weighted Time 1 Interview Outcomes

Time 1 Victim Interview Items	Treatment Group Mean	Control Group Mean	Test Statistic
Categorical Variables			Chi-Square
Victim Living Situation = Currently Married	0.2062	0.2949	1.3121
Victim Living Situation = Currently Cohabiting	0.1651	0.1489	0.0558
Victim Living Situation = Divorced or Separated	0.4319	0.2403	5.1712
Victim Living Situation = Widowed	0.0000	0.0165	N/A
Victim Living Situation = Single, Never Married	0.1967	0.2994	1.7386
Offender and Victim Living Together at Time of Interview	0.2072	0.2002	0.0091
Offender and Victim Have Lived Together Since the Incident	0.3748	0.3367	0.1930
Victim Contacted Offender Since Incident	0.6646	0.6108	0.2331
Offender Contacted Victim Since Incident	0.7738	0.7516	0.0877
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	0.7194	0.5385	4.2020
Victim Reports Contact by Sheriff's Deputy	0.5123	0.3493	3.3571
Victim Reports Knowledge of No-Contact Order	0.8870	0.8494	0.3754
Victim Reports Concerns About Safety	0.4831	0.4359	0.2818
Victim Reports Carrying Weapon For Self-Defense	0.1348	0.1308	0.0048
Trend in Abuse = Got Worse After Gateway Arrest	0.0243	0.0000	N/A
Trend in Abuse = No New Abuse After Gateway Arrest	0.7550	0.8550	1.6807
Trend in Abuse = Stayed About the Same After Gateway Arrest	0.0647	0.0749	0.0449
Trend in Abuse = Less After Gateway Arrest	0.1560	0.0702	1.8401
Victim Reports Any Psychological Aggression	0.4665	0.3715	1.1521
Victim Reports Any Physical Aggression	0.1114	0.0287	2.8541
Victim Reports Any Sexual Coercion	0.0576	0.0451	0.1114
Victim Reports Any Injury	0.0779	0.0135	2.5988
Victim Reports Any Stalking/Threats	0.5661	0.3698	4.7798
Numerical Variables			T-Test
Psychological Aggression - Variety Scale	1.6574	1.0975	1.4300
Physical Aggression - Variety Scale	0.3645	0.0466	2.1000
Sexual Coercion - Variety Scale	0.0872	0.0451	0.8600
Injury - Variety Scale	0.2801	0.0809	1.2700
Stalking/Threats - Variety Scale	1.6539	0.7965	2.9200

Note: Rows with chi-square or *t*-test statistics significant at two-tailed p<.05 level are shaded

same victim. We were only able to identify crime victims for LCSD arrests.[4] For each of these outcomes, the rearrest rate for offenders in the treatment group is slightly lower than the rearrest rate for offenders in the control group. But the differences are not large and are not statistically significant at a two-tailed $p<.05$ significance level.

Most of the differences between crime-specific recidivism rates for the treatment and control groups presented in Table 2 are small. None are statistically significant (two-tailed $p<.05$ significance level). Still, two of the differences are noteworthy. First, there are two types of contempt charges: (1) FTA/FTP/Attorney Contempt; and (2) All Other Contempt. The first type of contempt includes failure to appear, failure to pay fines, and contempt by an attorney while the second captures all other types of contempt. We would expect that the treatment group might have higher rates of all other contempt because that category would include NCO violations that resulted in an arrest.[5] In fact, the treatment group does have a higher re-arrest rate for contempt but the difference is not large nor is it statistically significant.

[4] Two cases in the treatment group, one case in the control group, and two cases in the interim control group were rearrested by the LCSD for a crime where the victim could have been the gateway incident victim. In these instances, however, we could not determine the victim's identity so we don't know whether the victim was the gateway victim.

[5] Some NCO violations are detected at the court and do not result in an arrest because the prosecutor sometimes uses these violations as leverage to solicit a guilty plea. These types of violations are not systematically recorded in either the arrest records or the court records because they are handled informally.

A second difference worth noting is the lower rate of rearrest for domestic violence in the treatment group (9.7 %) than the control group (14.0 %). While this difference is not statistically significant, it strikes us as interesting. We also note that the rate of subsequent charges for violent and property offenses is lower for offenders in the treatment group than for offenders in the control group although, once again, neither difference is statistically significant at the two-tailed $p<.05$ significance level.

Victim Interview Outcomes

Victim Knowledge, Safety, and Contact with Offender

Although we collected information on many potential outcomes in the interviews, we focus here on several that relate most directly to the targets of the intervention: contact with the offender, knowledge about the NCO, contact with the LCSD, and safety. At the Time 1 interview, the questions focused on the time period between the gateway arrest and the first interview date. At the Time 2 interview, the questions focused on the period of time that had passed since the first interview. These outcomes include the victim's relationship with the offender at the time of the interview, whether the victim was worried about her safety, whether the victim carried a weapon to defend herself against the abuser, whether the victim had contacted the offender, whether the offender and victim were living together at the time of the interview, whether the offender and victim had lived together at all during the reference period, whether the victim had been contacted by a LCSD deputy during the reference period, whether the victim had been

Table 4 Weighted Time 2 Interview Outcomes

Time 2 Victim Interview Items	Treatment Group Mean	Control Group Mean	Test Statistic
Categorical Variables			Chi-Square
Victim Living Situation = Currently Married	0.2064	0.2510	0.2713
Victim Living Situation = Currently Cohabiting	0.0900	0.1233	0.2642
Victim Living Situation = Divorced or Separated	0.4773	0.3409	1.7505
Victim Living Situation = Widowed	0.0157	0.0269	0.1428
Victim Living Situation = Single, Never Married	0.1905	0.2579	0.5853
Offender and Victim Living Together at Time of Interview	0.2713	0.4002	1.7489
Offender and Victim Have Lived Together Since the First Interview	0.3489	0.4247	0.5632
Victim Contacted Offender Since First Intrview	0.5834	0.6128	0.0827
Offender Contacted Victim Since First Interview	0.7601	0.7424	0.0386
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	0.2228	0.1595	0.5732
Victim Reports Contact by Sheriff's Deputy	0.1537	0.0859	0.9932
Knowledge of No-Contact Order Status = Don't Know Status	0.1263	0.1888	0.6838
Knowledge of No-Contact Order Status = Yes, Still in Place	0.5512	0.3749	2.7852
Victim Reports Concerns About Safety	0.4463	0.2829	2.5708
Victim Reports Carrying Weapon For Self-Defense	0.2215	0.1462	0.8300
Trend in Abuse = Got Worse After Gateway Arrest	0.0251	0.0271	0.0030
Trend in Abuse = No New Abuse After Gateway Arrest	0.6508	0.7481	0.7529
Trend in Abuse = Stayed About the Same After Gateway Arrest	0.1438	0.0332	1.9930
Trend in Abuse = Less After Gateway Arrest	0.1804	0.1916	0.0140
Victim Reports Any Psychological Aggression	0.6018	0.5449	0.3064
Victim Reports Any Physical Aggression	0.1046	0.1105	0.0089
Victim Reports Any Sexual Coercion	0.1127	0.0975	0.0550
Victim Reports Any Injury	0.1046	0.0669	0.4240
Victim Reports Any Stalking/Threats	0.6305	0.3239	8.3145
Numerical Variables			T-Test
Psychological Aggression - Variety Scale	3.1880	2.3507	1.2500
Physical Aggression - Variety Scale	0.4204	0.4579	-0.1100
Sexual Coercion - Variety Scale	0.1913	0.1716	0.1500
Injury - Variety Scale	0.5493	0.2809	0.7700
Stalking/Threats - Variety Scale	2.0700	0.8888	2.8200

Note: Rows with chi-square or t-test statistics significant at two-tailed p<.05 level are shaded

contacted by a LEVA during the reference period, and the trend in the abuse by the offender (no abuse, stayed about the same, reduction in abuse, increase in abuse).

In addition to being asked about the trend in the abuse, victims were asked a number of questions about various types of abuse. These questions were combined into logical groupings of psychological aggression,[6] physical aggression,[7] sexual coercion,[8] injury,[9] and stalking/threatening behavior.[10] For each group of items, we distinguished between those who experienced any of the behaviors within that grouping and those who experienced none of the behaviors. Next, we created "variety scales" where we computed a cumulative number of different types of behaviors each victim experienced. So, as an example, if a victim experienced three different types of psychological aggression, she received a score of 3 on the psychological aggression variety scale.

Because of the differences between offender characteristics among the interviewed and non-interviewed victims, we estimated non-response weights to be used in our interview analyses. One set of weights was created for the Time 1 interview data while a second set of weights was used for the Time 2 data. The procedure we used is discussed by Ridgeway and his colleagues (2006) and implemented in R with a procedure called TWANG (Toolkit for the Weighting and Analysis of Nonequivalent Groups). The first step in developing the weights is to estimate a statistical model to predict which individuals are observed and which individuals are missing based on information available in the administrative data that are observed for all cases.[11]

[6] The psychological aggression group included questions about the offender engaging in the following behaviors toward the victim: (1) insults/swears; (2) shouts; (3) stomps out of the room; (4) threatens to hit; (5) threatens to throw things; (6) destroys property; (7) threatens to hurt others; (8) calls names like "fat" and "ugly"; (9) accusations of laziness; (10) accusation of being a lousy lover; (11) preventing access to family money; (12) preventing from seeing family or friends; (13) preventing from working; (14) insisting on knowing whereabouts; and (15) knowing who calls on the phone.

[7] The physical aggression group includes questions about the offender engaging in overt physical attacks on the victim including: (1) kicking; (2) biting or punching; (3) slapping; (4) beating up; (5) hitting with an object; (6) choking; (7) slamming into a wall; (8) grabbing; (9) throwing something; (10) using knife or gun; (11) pushing or shoving; (12) twisting arm or hair; (13) burning or scalding.

[8] The sexual coercion group includes the following questions about actions by the offender against the victim: (1) insisting on anal sex but no force; (2) insisting on unprotected sex but no force; (3) using threats to coerce sex; (4) using threats to coerce anal sex; (5) forcing victim to have sex; and (6) forcing victim to have anal sex.

[9] The injury group asks victims about actual injuries inflicted on them by the offender during the reference period. This group is comprised of the following items: (1) cut or bleeding; (2) aches or pains; (3) felt pain the next day; (4) sprains or bruises; (5) scratched; (6) private parts bleeding; (7) broken bones or teeth; (8) head injury or concussion; (9) knocked unconscious; (10) hair pulled out; (11) eye or ear injury; (12) internal injuries; (13) received medical treatment for injuries; (14) offered medical treatment but declined; (15) saw medical doctor afterward; (16) needed to see a doctor but didn't; and (17) received medical care at the hospital.

[10] The final group of questions asks about stalking or threatening behaviors by the offender directed at the victim during the reference period including: (1) following; (2) spying; (3) standing outside home; (4) going to parents' house; (5) leaving items to find; (6) unsolicited telephone calls; (7) vandalizing victim's property; (8) showing up where he doesn't belong; (9) electronic communication; (10) giving messages through others; and (11) threats to deter the victim from going to court.

[11] The statistical model used to estimate the probabilities of non-response is included in the TWANG package and is called generalized boosted regression. Boosting methods are relatively new to the field of criminology and criminal justice but are being used with increasing frequency to estimate propensity scores which can then be used to estimate treatment effects in observational studies. The model in TWANG is estimated by calling the ps function. We assessed comparability between the first-round weighted responders and non-responders using TWANG's bal.table function. Multi-category variables (race, court disposition, pre-trial diversion program disposition) are treated as factor (categorical) variables in R for purposes of these analyses.

Table 3 presents the estimated differences between the treatment and control groups on victim knowledge, safety, and contact outcomes after weighting for non-response for the Time 1 interview data. Table 4 presents the non-response weighted comparisons for the Time 2 interview data.

Beginning with the Time 1 interview outcomes, the findings presented in Table 3 indicate that treatment group victims were more likely to report being divorced or separated than control group victims. Additionally, victims in the treatment group indicated that they were more likely to have been contacted by a LEVA (contacts by a Sheriff's Deputy were not significant). Victims in the treatment group also reported a greater likelihood of having experienced at least one stalking/threatening behavior and a higher average number of different types of stalking and threatening behaviors. Victims in the treatment group also reported a higher average variety score for physical aggression but as in the un-weighted analyses, very few victims reported experiencing any physical aggression at the Time 1 interview. The non-response weighted Time 2 interview comparisons summarized in Table 4 indicate greater levels of stalking victimization (both prevalence and variety) for victims in the treatment group.

To adjust for these potentially important pretreatment differences between the treatment and control groups, we estimated a series of weighted regressions (reported in Table 5) to determine the treatment effects for the Time 1 interview. In each of these regressions we included control variables for whether the offender was Black, whether the offender was a South Carolina native, whether the offender had any prior arrests for assault or contempt, whether the victim was Black, whether the offender was the

Table 5 Time 1 Interview Regression Models Adjusting For Pretreatment Imbalances and Nonresponse Weights

Time 1 Interview Outcomes	Treatment Coefficient	Odds Multiplier for Treatment	Treatment Coefficient Standard Error	Test Statistic
Categorical Variables				Chi-Square
Victim Living Situation = Currently Married	-0.6478	0.8421	0.5038	1.6535
Victim Living Situation = Currently Cohabiting	0.1970	0.6596	0.5137	0.1470
Victim Living Situation = Divorced or Separated	0.8813	1.8289	0.4321	4.1602
Victim Living Situation = Widowed	N/A	N/A	N/A	N/A
Victim Living Situation = Single, Never Married	-0.7225	0.5369	0.4896	2.1778
Offender and Victim Living Together at Time of Interview	0.2100	1.2337	0.4686	0.2009
Offender and Victim Have Lived Together Since the Incident	0.2133	1.2378	0.4112	0.2690
Victim Contacted Offender Since Incident	0.3806	1.4632	0.4112	0.8564
Offender Contacted Victim Since Incident	-0.0418	0.9591	0.4692	0.0079
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	0.7566	2.1310	0.4268	3.1432
Victim Reports Contact by Sheriff's Deputy	0.4274	1.5333	0.4304	0.9859
Victim Reports Knowledge of No-Contact Order	0.2676	1.3068	0.5314	0.2536
Victim Reports Concerns About Safety	-0.0160	0.9841	0.3936	0.0016
Victim Reports Carrying Weapon For Self-Defense	-0.3015	0.7397	0.6333	0.2266
Trend in Abuse = Got Worse After Gateway Arrest	N/A			
Trend in Abuse = No New Abuse After Gateway Arrest	-0.6182	0.5389	0.4742	1.6992
Trend in Abuse = Stayed About the Same After Gateway Arrest	-0.2551	0.7748	0.6778	0.1417
Trend in Abuse = Less After Gateway Arrest	0.7787	2.1786	0.5845	1.7750
Victim Reports Any Psychological Aggression	0.3166	1.3725	0.4057	0.6089
Victim Reports Any Physical Aggression	1.5682	4.7980	0.8692	3.2551
Victim Reports Any Sexual Coercion	0.4685	1.5976	0.8376	0.3128
Victim Reports Any Injury	1.8433	6.3174	1.1318	2.6525
Victim Reports Any Stalking/Threats	0.8195	2.2694	0.4044	4.1063
Numerical Variables				T-Test
Psychological Aggression - Variety Scale	0.5051		0.4348	1.1617
Physical Aggression - Variety Scale	0.3774		0.1576	2.3947
Sexual Coercion - Variety Scale	0.0822		0.0601	1.3677
Injury - Variety Scale	0.2422		0.1753	1.3816
Stalking/Threats - Variety Scale	0.7201		0.3173	2.2695

Note: The regressions reported in this table include the following predictor variables: treatment/control condition; offender is black; offender is a South Carolina native; any prior arrests for assault; any prior arrests for driving while impaired; victim is black; offender is victim's ex-boyfriend; and waiting time between gateway arrest and first interview. Chi-square and T-tests in this table only apply to the regression coefficient for the treatment indicator variable in these models. The interim controls are not included in these tests. Some models would not converge with all control variables; in these models the problematic variable (s) were dropped and re-estimated. Shaded lines indicate that the treatment coefficient is statistically significant (two-tailed $p<.05$ significance level)

Table 6 Time 2 Interview Regression Models Adjusting For Pretreatment Imbalances and Nonresponse Weights

Time 2 Interview Outcomes	Treatment Coefficient	Odds Multiplier for Treatment	Treatment Coefficient Standard Error	Test Statistic
Categorical Variables				Chi-Square
Victim Living Situation = Currently Married	-0.1719	0.8421	0.5676	0.0917
Victim Living Situation = Currently Cohabiting	-0.4161	0.6596	0.7423	0.3141
Victim Living Situation = Divorced or Separated	0.6037	1.8289	0.5072	1.4167
Victim Living Situation = Widowed	N/A	N/A	N/A	N/A
Victim Living Situation = Single, Never Married	-0.6220	0.5369	0.5355	1.3491
Offender and Victim Living Together at Time of Interview	-0.6432	0.5256	0.5060	1.6156
Offender and Victim Have Lived Together Since the First Interview	-0.2499	0.7789	0.4813	0.2695
Victim Contacted Offender Since First Interview	0.1125	1.1191	0.0492	0.8244
Offender Contacted Victim Since First Interview	0.4950	1.6405	0.5682	0.7591
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	0.6833	1.9804	0.6517	1.0993
Victim Reports Contact by Sheriff's Deputy	1.1205	3.0664	0.8054	1.9355
Victim Reports That No-Contact Order Still in Place	0.8043	2.2351	0.4960	2.6296
Victim Reports No Knowledge of No-Contact Order Status	-0.8500	0.4274	0.6871	1.5305
Victim Reports Concerns About Safety	0.7364	2.0884	0.5128	2.0619
Victim Reports Carrying Weapon For Self-Defense	0.6554	1.9259	0.6350	1.0651
Trend in Abuse = Got Worse After Gateway Arrest	N/A			
Trend in Abuse = No New Abuse After Gateway Arrest	-0.6416	0.5265	0.6075	1.1155
Trend in Abuse = Stayed About the Same After Gateway Arrest	1.7650	5.8416	1.0525	2.8122
Trend in Abuse = Less After Gateway Arrest	0.1912	1.2107	0.7400	0.0668
Victim Reports Any Psychological Aggression	0.5554	1.7426	0.5025	1.2218
Victim Reports Any Physical Aggression	0.1223	1.1301	0.6732	0.0330
Victim Reports Any Sexual Coercion	1.0481	2.8522	0.7618	1.8928
Victim Reports Any Injury	0.6880	1.9897	0.7767	0.7848
Victim Reports Any Stalking/Threats	1.6277	5.0921	0.5430	8.9861
Numerical Variables				T-Test
Psychological Aggression - Variety Scale	1.6464		0.7701	2.1379
Physical Aggression - Variety Scale	0.2236		0.3677	0.6081
Sexual Coercion - Variety Scale	0.1827		0.1466	1.2462
Injury - Variety Scale	0.5304		0.4348	1.2199
Stalking/Threats - Variety Scale	1.5474		0.4917	3.1470

Note: The regressions reported in this table include the following predictor variables: treatment/control condition; offender is a South Carolina native; any prior arrests for alcohol offenses; any prior arrests for traffic offenses; offender is victim's ex-boyfriend; and waiting time between gateway arrest and Time 1 Interview. Chi-square and T-tests in this table only apply to the regression coefficient for the treatment indicator variable in these models. The interim controls are not included in these tests. Some of the models would not converge with all predictor variables. In these models, the problematic variable (s) were dropped and re-estimated. Shaded lines indicate that the treatment coefficient is statistically significant (two-tailed $p<.05$ significance level)

victim's ex-boyfriend at the time of the gateway arrest, and the time that lapsed between the gateway arrest and the first interview. After adjusting for the effects of these potential confounding variables, the apparent difference between treatment and control cases in LEVA contacts became insignificant. However, the other significant differences identified in Table 3 persist. Our conclusion from the Time 1 interview is that there appears to be a link between the intervention and increases in stalking or threatening behaviors.

Table 6 reports a series of weighted regression analyses adjusting for the following potential confounders: the offender is a South Carolina native, whether the offender had been arrested for alcohol or traffic offenses prior to the gateway arrest, and the time that lapsed between the gateway arrest and the Time 1 interview. In these regressions, the effect of the treatment condition on stalking continues to be evident and the effect of treatment on the psychological aggression variety scale is also significant. In both instances, victims in the treatment group report significantly worse outcomes than victims in the control group.

Discussion and Conclusions

Summary

Using an experimental design in which we randomly assigned cases to an intervention designed by the research team and representatives of the LCSD, our research project

offered the opportunity to examine the impact of proactive enforcement of NCOs on victim safety and well-being and offender behavior in cases of misdemeanor domestic violence. Although NCOs are commonly used, research to date is inconclusive about whether these orders have the intended effect of protecting victims, promoting victim well-being, and reducing offender recidivism. The major elements that have limited their effectiveness include victims' lack of knowledge of the presence and nature of such orders, law enforcement's lax attitude towards the enforcement of such orders, prosecutors who are reluctant to prosecute offenders who breech such orders, and judges who are reluctant to (and often don't) issue bench warrants to permit police to enforce the orders. In previously conducted domestic violence research, design limitations and questions about the extent to which orders are enforced have limited our ability to ascertain the impact of NCOs. The primary emphasis of the intervention was to establish contact between the LCSD's DO and the victim for purposes of notifying the victim about the existence of the NCO, explaining the requirements of the NCO, providing instructions to victims about what to do if a violation occurred, and monitoring compliance with and enforcing the NCO. We assessed the effectiveness of this proactive enforcement of NCOs via analyses of official criminal records data and two sets of victim interview data.

Our overall findings indicated few differences between the treatment and control groups, and therefore no clear effect of the treatment on offender and victim outcomes. Our analyses included an examination of officially recorded criminal activities that occurred after the gateway arrest for offenders in both treatment and control groups. With respect to our analyses of the interview data, we see few differences between the treatment and control groups on most of the outcomes studied. Some difference was apparent with respect to contacts with the LCSD, two types of offender behavior, and the victim and offender's relationship status; although, these differences did not consistently reach statistical significance across the full range of our analyses. Nevertheless, the patterns are of some interest so we briefly discuss them here.

First, at the Time 1 interview, victims in the treatment group were significantly more likely to report having been contacted by a LCSD deputy than victims in the control group, suggesting successful implementation of the proactive enforcement of NCOs to victims in the treatment group. We found other treatment differences for contacts by law enforcement victim advocates from the LCSD. These contacts between treatment victims and victim advocates may be the result of their contact with the DO and victims' heightened awareness of the resources and services available to domestic violence victims. These differences point to the potential of enhanced contacts between law enforcement and victims to educate and empower victims of intimate partner violence.

The treatment and control group also differed with respect to two types of offender behavior. Counter to predictions and the goal of proactive enforcement, the treatment group victims reported significantly higher scores on the physical aggression variety scale (this is a count of the number of different types of physically aggressive behaviors experienced since the gateway arrest). We are cautious to place significant weight on this finding given that this difference is based on very small numbers, with only about 10 % of treatment group victims and approximately 3 % of control group victims having reported experiencing any physical aggression at all.

The second type of treatment difference with respect to offender behavior (and victim perceptions of their batterer's behavior) was found for stalking and threatening

behaviors. Our survey data suggest that victims in the treatment group reported significantly higher levels of stalking and threats by the offender since the gateway arrest (differences in both prevalence and variety of stalking and threats) compared with the levels of stalking and threats reported by victims in the control group. We also note that this finding is evident in both the individual Time 1 and Time 2 surveys.

Finally, our Time 1 data indicate that victims in the treatment group were more likely to report being separated or divorced from their offender than control group victims. This finding may indicate that victims were attempting to end their relationships with their violent intimate partners. However, this finding does not materialize in the Time 2 interview data.

It is clear from our research that proactive enforcement as operationalized in this field experiment via enhanced contact between law enforcement and victims is not an effective means of increasing victim safety or reducing offender recidivism. Though there were barriers to implementing the treatment and contacting victims for interviews which limit our ability to fully examine the effect of proactive enforcement, the fact that these limitations arose is itself an important finding. We suspect officers attempting to implement treatment conditions like those evaluated in our study will encounter similar difficulties in contacting and communicating with domestic violence victims. Researchers and policy makers should proceed cautiously when considering interventions that depend heavily on victim accessibility.

Our research findings help to further elaborate and indicate what doesn't work in enhancing the safety of domestic violence victims. Our findings also address previous concerns raised by other researchers, advocates, and social service providers about what constitutes an effective domestic violence intervention and the relative risks posed by victim-initiated versus court-ordered law enforcement remedies for intimate partner violence.

First, our research demonstrates the difficulty of implementing a simple domestic violence intervention. The design and plan for the implementation of the treatment protocol was developed in consultation with the LCSD and the prosecutor from the Lexington County Domestic Violence Court. This two-stage intervention, which included DO contacts with victims about 6 weeks and 6 months following the gateway incident, was believed to address the times when victims were most at risk for subsequent victimization and witness tampering. Yet, our study notes the difficulty of ensuring adequate implementation of even such a simple treatment regime. The implementation of the treatment was limited by the difficulty in contacting victims and other law enforcement duties faced by the DO. Future research will need to address these logistical concerns and issues.

Second, and more importantly, our research findings indicate that court-imposed NCOs, and the enforcement of those orders, do not appear to jeopardize women's safety or aggravate recidivism.[12] Third, our findings may be viewed as part of the growing literature that refutes claims that all law enforcement interventions, especially court-imposed remedies for dealing with male perpetrated domestic violence, either disempower women or put them at greater risk. Our intervention was designed to specifically enhance female victim's decision-making, offering women information

[12] As suggested previously, however, our analyses suggested that women in the treatment group perceived they were being stalked at higher rates than women in the control group. We discuss this issue further below.

about the legal system and their rights, and provide them with options for dealing with violent relationships.

The Stalking Effect

As we note, our research findings from both the Time 1 and Time 2 survey data indicate that women in the treatment group reported a greater likelihood of having experienced at least one stalking/threatening behavior and a higher average number of different types of stalking and threatening behaviors. While the goal of treatment implementation was to reduce offender recidivism, we do not find this result all that surprising. The nature of the victim-component of the treatment (proactive enforcement of no-contact orders) was such that victims were provided with a letter from the DO at the LCSD, together with a brochure outlining the presence and describing the nature of the no-contact order in place. Victims in the treatment group were provided with examples and illustrations of breeches of the orders and were instructed as to their rights with respect to the order. The brochure also offered victims a number of suggestions as to how they might document these breeches, including the use of caller id and photographing their incoming calls. Initial contacts to the victim by the DO were also geared toward providing women with information on domestic violence and NCOs, thus educating and empowering women to enforce such orders. Notably, victims in the treatment group were not only more likely to be aware that such an order was in place, but were also more likely to have had contacts with law enforcement and victim advocates.

The treatment in this case may have served to change women's perceptions of their batterer's behavior. Long-term follow-up with the Lexington County victims from this study would allow for an examination of whether changes in perceptions of batterer behavior might have translated to an increased probability that some women ended their violent relationships and began transforming their lives. An alternative explanation for the stalking effect might be that the receipt of information from the LCSD and the presence of the officer at the victim's home might have aroused suspicion with the offender and led to changes in his behavior, including threatening and stalking behaviors. Nevertheless, with the information currently available, it is not possible to say which of these explanations is more plausible.

Non-Response Problems

During our project, it became apparent that conducting interview-based research with domestic violence victims poses a number of challenges. In fact, we experienced considerable difficulty in attempting to contact victims on the phone and via regular postal mail. We speculate that this population is a highly transient one. For example, we received our contact information from victim information sheets provided by the LCSD and by the time we attempted contact many phone numbers had been disconnected. Additionally, we used an online tracking service to attempt to locate our respondent population. Many of the women in our study have had multiple residential locations. Transportation problems also presented a problem for victims to reach the interview location. This resulted in many missed interviews and multiple attempts to reschedule. Additionally, with the advent of cell phones, making contacts with research subjects has become and will continue to be increasingly difficult.

The use of cell phones presented problems for our research staff in contacting victims since these numbers are not listed publically. Finally, our efforts at establishing contact with the victim population were hindered by important safety and human subject concerns. Specifically, we did not attempt to contact the victims in court because of safety concerns, although the court might have been an ideal venue in which to initiate research contact.

Research Limitations

Despite the use of an experimental design and examination of a variety of victim and offender outcomes, this study was not without its limitations. One specific limitation related to caseload. Prior to the DO and research staff entering the field, South Carolina's criminal domestic violence law changed in Jan uary 2006. The consequence of the change in the law was that the Lexington County Criminal Domestic Violence Court was now limited to only processing first-time misdemeanor domestic violence cases. Per the new law, repeat offenders were processed in general sessions court. The Domestic Violence Court prosecutor estimated that this reduced the caseload in Lexington County by roughly one-half.

Second, the implementation of the treatment condition was impacted by a change in DO personnel part way through the study period and the ability of the officers to make contact with victims in the treatment group. During the transition period to a new DO, some of the cases that came into the court were automatically placed in the control condition. Additionally, as we note, the attempts and successful DO contacts with victims in the treatment group were disappointingly low. Yet, it is important to note that this change in staffing and level of successful contact with this population of victims reflects the reality of working with an agency and the real-life conditions under which an intervention would be implemented. We also note that the LCSD implemented the intervention in this study using their existing resources. Although the availability of grants to fund interventions of this nature could potentially lead to higher levels of treatment compliance, it is not clear whether these would persist once the grant funds ended. Therefore, we reiterate that our analysis is best viewed as an "intent to treat" estimate of the treatment effect (Horvitz-Lennon et al. 2005). Overall, these sorts of problems add up to the general problem of treatment noncompliance - a problem that exists in many field experiments in both the medical and social sciences. Going forward, we will be closely examining how to address treatment noncompliance problems in our data to develop alternative estimates of our treatment effects. Yet it is important to note that these issues that limited the implementation of treatment speak to the external validity of our study findings in that incomplete implementation would be likely under any real-world implementation.

Third, both DOs and our research staff had difficulty contacting the victims in our study for an interview. With the growth of cell phone use, this will continue to be a problem for researchers. The implication for our research was that we were not able to conduct as many interviews that we originally anticipated.

Fourth, while our analyses were able to detect and identify substantive differences in recidivism between the treatment and control group, for many offenders in our study the observation of their criminal behavior was relatively brief. Future long-term follow-

up of offenders and victims under proactive enforcement of NCOs would allow for an examination of a host of outcomes that may be the consequences of this type of criminal justice intervention.

Given these limitations, our research offers a number of suggestions for future academic work. Although our research indicates modest effects of NCO intensive enforcement, given how the intervention was implemented within this project, our research does call for serious consideration and evaluation of what might actually constitute effective "enforcement" of court-imposed NCOs. Specifically, domestic violence interventions need to continue to draw on a wide range of law enforcement, social service, healthcare, and mental health service providers. Given the clear distinctions between (Johnson et al. 2003) common couple violence and intimate partner terrorism, it is important that domestic violence interventions address these elements. Whereas, some battering relationships are at high risk for subsequent violence and potentially lethal violence requiring intensive interventions, other batterers may be amenable to less intensive treatments. With respect to NCO policy, the types of contact between victims and their batterers may need to be distinguished. Contacts related to marital counseling, child care arrangements, and other family commitments are qualitatively different than stalking, violence, and threatening behaviors that maybe indicative of future violence.

Social and Criminal Justice Policy Implications

Overall, our findings suggest no effect of the treatment for women's well-being and batterer recidivism. Nevertheless, there may be other important reasons for enforcing these types of criminal justice orders. For example, NCO's are increasingly seen as an appropriate criminal justice response to domestic violence, either in lieu of or in addition to a civil action brought by the victim. Used to disrupt the cycle of violence between bond hearings and judicial proceedings, a NCO typically prohibits an offender from contacting a victim during the period between his arraignment and sentencing. Failure to enforce such orders may also potentially send the message that continued contacts with victims (in which orders are in place) are acceptable. This is consistent with anecdotal evidence that suggests some batterers in the past have waived restraining orders in the faces of their victims asking them what the paper might do to protect them.

Of course, it is important that the criminal justice system continue to work towards involving victims in the criminal justice process and protecting their rights within the system . An early evaluation of the Lexington County Criminal Domestic Violence Court indicated that many victims were unaware of the presence of NCOs (Gover et al. 2007). If these orders are going to continue to be part of the prosecution process of domestic violence in Lexington County, it is crucial that victims be made aware of their presence and provided with information on how to enforce these orders to protect themselves and their families.

We discovered some evidence that victims in the treatment group perceived higher rates of stalking behavior than control group victims. With the current evidence, it is not possible to determine whether this is a perceptual difference or a real behavioral difference on the part of offenders. There is clearly a continued need for criminal justice officials and legislators to examine the prevalence and nature of stalking of victims by current and previous intimate partners, to develop timely interventions, and to evaluate efforts to enforce orders that prohibit these behaviors by defendants facing domestic violence charges. Such investigations should be a priority because research has indicated

that stalking and threatening behaviors toward victimized women may affect transitions out of violent relationships, have implications for their health and physical safety, and decisions to testify against batterers.

Directions for Future Research

Our findings point to a number of directions for future research on the efficacy of criminal justice interventions geared toward reducing offender recidivism and ensuring victim safety. Given significant differences between the treatment and control group with respect to contacts with law enforcement victim advocates, we suggest this is a fruitful area of future research. Although the advocates were not part of the treatment condition, it appears that they were more likely to have contacts with victims in the treatment group and potentially because of the contacts between the DOs and the women in the treatment group. Future research should explore the role of advocates for victims experiencing domestic violence and look at how the presence and nature of contacts between victims and advocates shape victim decision-making. Experimental and matched sample designs would be able to examine the impact of contacts with advocates for a host of victim outcomes. We also suggest that criminal justice agencies and criminal justice focused interventions collaborate with social service providers. An example would be Colorado's Domestic Violence Enhanced Response Team (DVERT) programs that work in partnership with community agencies to address domestic violence. DVERT's goal is to provide a systematic community response to the problem of domestic violence through a multidisciplinary collaboration of criminal justice and social service providers focusing on pro-arrest policies and procedures, case investigation and prosecution, and the implementation of innovative forms of outreach, advocacy, and services to victims. Evaluations of similar programs using experimental and quasi-experimental designs will be an important component of future domestic violence research as scholars attempt to identify and broaden this list of "what works" with respect to attending to the tremendous social burden of domestic and family violence.

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