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The Impact of Proactive Enforcement of No-Contact Orders on Victim Safety and Repeat Victimization

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ABSTRACT

Research Goals and Objectives:

Our NIJ-funded research and collaboration with the Lexington County Sheriff's Department 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) increased victim knowledge about no-contact orders; (2) reduced contact between offenders and victims; and (3) increased victim safety and promoted well-being.

We used a prospective experiment in which 466 cases of misdemeanor criminal domestic violence were randomly assigned to either systematic, proactive enforcement or to routine, reactive enforcement of the court-ordered no-contact conditions. Law enforcement contacts were directed at victims in the treatment group whose abusers had been arrested for domestic violence and released on bond with the restriction that the offenders have no contact with their victims.

Dedicated officer contacts were divided into two types. Those contacts prior to first appearance were designed to educate victims on NCOs, provide them information on CDV and NCOs, teach them how to document offender contact, and conduct offender surveillance. These contacts included an initial mail contact and in-person or phone contacts. The personal contacts were scheduled to occur 72 hours after the order's imposition, one week after the imposition, and one week prior to first appearance. The second set of attempted contacts was to occur after the offender's first appearance. The goal of these

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contacts was continued risk assessment, visits to check on victims, and offender surveillance. The schedule for these contacts would vary according to the offender's path through the criminal justice system. For pretrial intervention cases attempts were made to contact once every three months, for bench trial cases contacts were schedule within 5 days of the bench trial, and for jury trial cases contact was scheduled once every three months and 48 hours before the jury trial. These victim-directed contacts were designed to accomplish three objectives: (1) to ensure that victims understood the requirements of the no-contact orders; (2) to advise the victim on how to collect evidence and mobilize law enforcement in the event of a violation; and (3) to monitor compliance with the no-contact order.

We assessed the effectiveness of the proactive enforcement of no-contact orders using analyses of official criminal records data and victim survey data. With respect to the implementation of the treatment, there was in fact a measurable difference on this variable in the average treatment of the two groups, although the effects of the treatment on victim and offender outcomes were modest.

While research limitations, with respect to ensuring the implementation of the treatment condition and contacting victims for interviews, may have limited our ability to identify strong treatment effects, our research shows that "proactive enforcement" conceived as enhanced contact between law enforcement and victims is not an effective means of increasing victim safety or reducing offender recidivism. And most importantly, these law enforcement contacts with victims in the treatment group did not place women at additional safety risks. Additionally, since both groups (treatment and control)

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had NCOs issued at first appearance in a criminal domestic assault case, both groups had less than a 15 percent recidivism rate. Perhaps merely the presence of the no-contact order enhances victim safety. Moreover, we believe the design for the contacts and intention to implement the treatment constitutes a qualitatively new interaction between victims and law enforcement and represents a type of "proactive enforcement" that might reasonably been expected to create differences in the observed outcomes.

Research Design and Methodology:

Starting in the fall of 2005, we implemented a prospective field experiment in which 466 cases were randomly assigned to either the treatment or control condition; an additional 51 cases were enrolled as interim controls in the fall of 2006. Thus, a total of 517 cases were enrolled in the study from 2005 to 2007. Randomization was accomplished using a database program. Once a domestic violence offender was released on bond, paperwork documenting the bond restrictions would flow to the Lexington County Criminal Domestic Violence Court (CDVC). The Court Administrator provided photocopies of the bond restrictions to the dedicated officer, who entered the case information into the database. Entering this data initiated the randomization and proactive enforcement schedule for the cases in the treatment group. The dedicated officer then targeted cases in the treatment group for proactive, victim-directed contacts and maintained a log of those contact efforts using the database. The dedicated officer also provided a log of newly enrolled cases to the research team on a regular basis throughout the study, thus enabling the

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research team to list the cases in the research database and contact victims for interviews.

Efforts were made to contact and interview each of the 437 female victims enrolled in the study. While male victim cases were enrolled in the experiment and randomized to the treatment or control group and we have analyzed their offenders' criminal history data, no efforts were made to contact male victims for interviews. Interviews were targeted to occur at six weeks after the gateway incident and then again at six months after the gateway incident. Official criminal history records and other official administrative data allowed us to examine recidivism for the treatment and control cases.

Research Results and Conclusions:

Our analyses identified few differences between the treatment and control groups. When differences appeared, they were generally not statistically significant across the full range of our analyses. We conclude that proactive enforcement of no-contact orders as implemented in this experiment may yield some benefits in terms of reductions in arrests for subsequent domestic violence, but these reductions are modest and not statistically significant.

For example, the analyses consistently indicated higher rates of contact between sheriff's deputies, law enforcement victim advocates (LEVAs), and victims. Although the differences were not statistically significant across the full range of analyses, we believe this finding indicates some success in differentiating the experiences of the treatment and control cases. Although LEVA contacts were not part of the intervention, it is possible that heightened levels of contact

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between treatment victims and victim advocates may have been the result of their earlier contact with sheriff's deputies, resulting in victims' heightened awareness of the resources and services available to domestic violence victims.

The treatment and control group also exhibited differences with respect to three types of offender behavior. First, at the Time 1 interview, we found that the treatment group victims report significantly higher scores on the physical aggression variety scale (this is a count of the number of different types of physical aggression experienced since the gateway arrest) (see Table 32). We are cautious not to place a lot of weight on this finding given that this difference is based on very small numbers, with less than ten percent of treatment group victims and approximately three percent of control group victims reporting to have experienced any physical aggression at all; the effect was not statistically significant at the Time 2 interview (see Table 34).

We also found that victims in the treatment group reported significantly higher levels of stalking and threats by the offender since the gateway arrest compared with the levels of stalking and threats reported by victims in the control group in the individual Time 1 and Time 2 analyses (see Tables 32 and 34). But this effect was not statistically significant in the combined Time 1 - Time 2 analyses (see Table 36).

Additionally, our Time 1 interview data indicate that victims in the treatment group were more likely to report being separated or divorced from their batterer than control group victims (see Table 32). This finding may indicate that victims were attempting to end

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their violent relationships and begin transforming their lives. At the same time, victims in the treatment group noted higher levels of stalking by their batterer. And, at the Time 2 interview, victims in the treatment group reported experiencing higher levels of psychological aggression (see Table 34). This is consistent with the nature of domestic violence and the underlying dynamic of power and control in violent relationships. Violence often escalates when victims make attempts to exert power and reduce their batterer's control. However, the divorce difference was not statistically significant at the Time 2 interview and the psychological aggression difference was not statistically significant at the Time 1 interview. While the pattern of differences is consistent across the various analyses, only some of the differences are statistically significant.

Our findings suggest a number of directions for researchers examining the effectiveness of interventions in partner-perpetrated violence and abuse. This would include the implementation of a wider array of interventions that make use of both law enforcement and social service interventions. One strategy would be to implement risk assessment of battering relationships and make a clear distinction between Michael Johnson's common couple violence and intimate terrorism prior to the implementation of treatment. This would allow for the development of more meaningful differences in a range of safety outcomes. With respect to no-contact orders this would include an examination of the nature of contact between victims and their batterers. Contacts related to marital counseling, child care arrangements, and other family commitments are qualitatively different from aggressive behaviors that may be indicative of future violence.

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EXECUTIVE SUMMARY

Domestic violence continues to challenge policy makers, social service providers, and criminal justice officials. Domestic violence advocates, criminal justice personnel, and academic research consistently underscore the serial nature of violence between intimate partners. Increasingly, domestic violence researchers are highlighting the importance of social and criminal justice policies that preemptively reduce the likelihood of offender recidivism and promote victim well-being. Moreover, given the long-term and continuing nature of intimate partner violence, some research suggests that the most dangerous time for battered women may be after their initial contacts with the criminal justice system or subsequent to the arrest of their abuser (Sherman, Gottfredson, MacKenzie, Eck, Reuter, and Bushway, 1998).

One policy measure courts are increasingly relying on is the use of protection or "no-contact" orders that prohibit offenders from having any contact with their victims between the defendant's arraignment and sentencing. No-contact orders may offer swifter relief than criminal actions (Davis and Smith, 1995) by serving as an immediate remedy to the continued threat of violence, prohibiting contact by a woman's abusive partner, and serving as a symbolic threat of the criminal justice system. Although the use of no-contact orders to disrupt the cycle of violence between bond hearings and judicial proceedings is widespread, the level of compliance with these orders is not well understood; nor is it known whether proactive enforcement of no-contact orders reduces harm to victims, is ineffective, or is counterproductive. As a general rule, protection orders are more

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often reactively enforced only after victims notify the police that a violation has taken place. Some criminal justice experts have suggested that enforcement of protection orders has the potential to offer a positive, crucial step toward increasing both victim safety and offender accountability. No-contact orders are often used in jurisdictions as a condition of release when bond is approved. It is sometimes a tactic of pre-arraignment used with individuals who are arrested for criminal domestic violence. The purposes of no-contact orders are prevent individuals who have been arrested for domestic violence from making contact with the individual they are accused of assaulting. They are commonly used as a restriction, placed on defendants prior to case disposition.

This report discusses a randomized experiment in which active criminal domestic violence cases were randomly assigned to proactive enforcement of court-ordered no-contact orders. Our research represents a collaborative effort with officials in Lexington County, South Carolina. The goal of the project was to assess whether proactive enforcement of no-contact orders promoted victim safety and reduced offender recidivism. We used a prospective experimental design in which 466 cases were randomly assigned to the current or status-quo level of no-contact order enforcement (the control condition) or to systematic, proactive enforcement (the treatment condition). Data were collected from official records obtained from the Lexington County Sherriff’s Office, Lexington County Criminal Domestic Violence Court, the 11th Judicial Circuit's Office of Diversion Programs records, and two sets of interviews with victims to measure victimization experiences and order compliance levels.

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Between fall 2005 and summer 2007, 466 misdemeanor domestic violence cases processed by the Lexington County Criminal Domestic Violence Court were randomly assigned to either a treatment condition that included proactive contacts with domestic violence victims by a dedicated Sherriff's deputy, or the control condition (the previously implemented status quo, or reactive enforcement). The cases that were assigned to the treatment group received proactive monitoring and follow-up as well as information and education about the presence and nature of no-contact orders.

Prior to the defendants' first appearance, a dedicated officer attempted to make contact with victims in the treatment group via a letter and information pamphlet that was geared towards educating victims about no-contact orders, provide them information on the criminal domestic violence courts, teach them how to document offender contact, and conduct offender surveillance.

The initial schedule of proactive enforcement of the no-contact orders included four planned attempts. This included a letter that was sent to the victim by the research staff (but signed by the dedicated officer) shortly after receipt of the victim's contact information, a phone or in-person contact by the dedicated officer shortly after the order's imposition, an in-person or phone contact one week after the imposition, and a contact one week prior to the defendant's first appearance.

A second phase of enforcement was planned subsequent to the defendant's first court appearance. This contact was primarily focused on continued risk assessment, visits to check up on victims, and offender surveillance. For pre-trial intervention cases

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(diversion eligible cases), contacts were scheduled for every three months; for bench trial cases, contacts were scheduled for once every three months and 5 days prior to the bench trial; for jury trials, contact was scheduled for once every three months and 48 hours before the jury trial.

The random assignment and enforcement schedule was determined by the computer database and the dedicated officer was provided with the database for tracking contact attempts and recording monitoring events. To fully examine the effectiveness of no-contact orders requires a thorough understanding of the level of enforcement of the order that actually occurs.

Rigakos (1995) suggests that research on the effectiveness of no-contact and protection orders will depend to a large degree on whether these orders are actually enforced by the police (especially through arrest) and prosecuted by the courts. A key component to our research was to measure the actual level of enforcement that occurred in the treatment and control group. Our analyses of the dedicated officers' contact data indicate a 68% contact effort in the treatment group (contact effort was less than 8% in the control group). A closer examination indicated that there were differences between the two officers who implemented the proactive enforcement during the study period-- approximately 84% contact effort for one officer and approximately 50% enforcement effort for the other officer. This notwithstanding, both officers ended up with at least one successful contact in about 38% of the treatment group cases; in other words, while the two officers differed on counted contact efforts, they ended up with virtually identical successful contact rates with victims in

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the treatment group. Thus, most offenders in the treatment group experienced at least some effort to contact but only 4 out of 10 of these offenders were actually contacted successfully at least one time. Overall, proactive law enforcement contact with victims proved to be a difficult task.

To examine the effect of proactive enforcement on victim and offender outcomes, we analyzed the survey and official administrative data. The administrative data were analyzed to examine offender recidivism and no-contact order compliance levels. The interview data were used to assess the impact of proactive enforcement of no-contact orders on victim reports of offender compliance with no-contact orders, offender recidivism, and victim safety. Interviews were targeted to occur at six weeks after the arresting incident that brought the couple into the study (the gateway incident) and then again at six months after the gateway incident.

Our analyses of both the survey and official criminal records data suggest modest effects of proactive enforcement of no-contact orders for victim and offender outcomes. We therefore conclude that while proactive enforcement of no-contact orders in cases of misdemeanor domestic violence may yield some beneficial results in terms of changing the victim's perceptions of their offender's behavior, these orders are limited in their ability to change offender behaviors. Yet there may be wider social implications of maintaining and enforcing no-contact orders as part of a continued effort to address violence against women. The continued utilization and proactive enforcement of these types of orders may serve an important

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symbolic role in promoting a very public stand against domestic violence.

We used the administrative data to examine the rates at which offenders were rearrested for any offense between the gateway arrest date and the record search date. Our analyses of the offenders' criminal history records suggest modest (and non-significant) effects of proactive enforcement over the follow-up period. The comparison indicates that both the treatment and control groups were rearrested at about the same rate (treatment group = 38.8%; control group = 40.6%).

We also 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 the zero arrests (treatment group = 0.654; control group = 0.659). The crime-specific differences between recidivism rates for the treatment and control groups are very small and none of them were statistically significant. A difference between the treatment and control group 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 substantively interesting given that it is in the predicted and anticipated direction. Our overall conclusion from the official record analysis is that the treatment condition may yield some beneficial results in terms of reductions in arrests for subsequent domestic violence and other predatory offenses, but these reductions are relatively small and not statistically significant.

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Our analyses of the survey data point to several noteworthy differences between the treatment and control groups. First, we found that the dedicated officers were able to create a meaningful difference in the average experience of treatment cases compared to the average experience of control cases. Our Time 1 interview findings indicate that victims in the treatment group were more likely to report having been contacted by a Lexington County Sheriff's deputy than victims in the control group. We also found that the women in the treatment group were more likely to have experienced contacts by a law enforcement victim advocate (LEVA) from the Lexington County Sheriff's Department. Although these differences were not statistically significant after nonresponse weighting and adjustments for pretreatment group differences, the basic comparisons are illustrative of substantively important differences. Although the LEVAs were not part of the planned treatment condition, it is possible that contacts with the dedicated officer and knowledge about no-contact orders may have increased the likelihood that victims would seek out the assistance of victim advocates and be willing to accept outreach services by advocates. This finding in particular may have implications for future victim behavior in terms of whether a victim chooses to testify against her batterer and eventually leave the battering relationship. These are issues that future research will need to address.

The treatment group also differed from the control group with respect to two types of offender behavior. First, victims in the treatment group were more likely to report higher scores on the physical aggression variety scale (a count of the number of different

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types of physical aggression experienced since the gateway arrest). Yet this difference is based on very small numbers with less than 10% of treatment group victims and approximately 3% of control group victims reporting any instances of physical aggression. Moreover, the effect does not appear clearly in all of our analyses.

Second, our separate analyses of each of the two interview data sets showed that victims in the treatment group were more likely to perceive their batterer to have stalked and threatened them since the gateway arrest, compared with the levels of stalking and threats reported by victims in the control group. This finding, although counter to predictions for the effect of the treatment on offender behavior, is not all that surprising. It suggests that the perceptions and attitudes of victims toward their batterer and his behavior are likely to change with information on the cause, nature, and consequences of intimate partner violence. Moreover, the goal of the treatment (proactive enforcement) was to offer women information about no-contact orders and behavior that signals a breach of the order. An alternative explanation for the stalking effect might be that the receipt of information from the Lexington County Sheriff's Department and the presence of the officer at the victim's home might have aroused suspicion in the offender and led to changes in his behavior, including a reduction in threatening and stalking behaviors. While this finding is potentially quite interesting, we note that it does not reach statistical significance in our analysis of the combined Time 1 - Time 2 interview data.

Finally, our analyses of the Time 1 interview data point to treatment differences with respect to marital status, in that women in

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the treatment group were more likely to be separated or divorced from their batterer than those women in the control group. This finding may reflect victims' attempts to end their violent relationships. However, this effect was not statistically significant in the Time 2 interview data.

With respect to our analyses of the survey data, one of our principal concerns was whether interview response rates varied for treatment and control cases. Our examinations of the differences between the two groups indicate that victim interview response rates were slightly but not significantly higher for treatment cases than for victims whose offender was assigned to the control group.

Our findings offer a number of contributions to social and criminal justice policy. With respect to limitations, it is important to note that our intervention may be viewed as an effort by the dedicated officer to implement treatment via contact and communication with domestic violence victims. While we did have significant differences in contact attempts and criminal justice contacts between the treatment and control groups, overall contact was still disappointingly low. Yet our data indicate that the levels of contact between the dedicated officer and victims are substantially higher in the treatment group than in the control group. Thus, on average, the treatment and control groups had different contact experiences with the dedicated officer.

It is important to note that attempts to contact victims are impacted by the officer's ability to locate the victim and the victim's right to refuse to accept law enforcement's attempt to notify them of the presence and nature of no-contact orders. In short,

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victims have no legal duty to cooperate with or participate in conversations with law enforcement officers about no-contact orders. In fact, the orders are written to control offender behavior and are often put into place over the objections of victims.

Based on our own experiences in attempting to contact and interview victims for this study, we are not surprised that efforts to contact victims were often unsuccessful. Among victims with whom we were able to establish contact for Time 1 recruitment, 28 refused to participate in our study, 48 who scheduled a Time 1 interview failed to show up to complete the interview (in some instances, multiple appointments were missed), and an additional 10 cases were not recruited because they could not speak English. Ultimately, out of 227 established contacts, we interviewed only 141 female victims. In a total of 210 cases, no contact with the victim was made by the dedicated officer. Future researchers will need to take this difficulty into account in research projects that rely on victim-directed interventions and interview data from victims.

Our findings point to the importance of domestic violence interventions directed at educating women and offering victims information on the criminal justice system and social services. With respect to stalking in particular, our analyses indicate the importance of empowering women by ensuring they understand their rights as victims, which may in turn help to shape their decision-making in regard to leaving violent and abusive relationships. It is crucial to continue providing victims with information on the nature of intimate partner violence and the resources available to assist them in terminating violent relationships. Through the provision of

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information on the presence and nature of no-contact orders (via proactive enforcement by a dedicated domestic violence officer or a victim advocate), victims have access to a tool (and legal right) that may be used to prevent their batterer from continuing contact, thereby potentially reducing the risk of re-victimization. Future long-term follow-up of victims under proactive enforcement would allow for an examination of this hypothesis. As Meloy, Yim Cowett, Parker, Hofland, and Friedland's (1997) meta-analysis suggests, the effectiveness of protection orders is dependent on the severity of violence experienced by the victim (Keilitz, Nannaford and Efkeman, 1998). The laxity of enforcement of the protection order itself is likely to reduce the deterrent effects of these orders. Our findings therefore point to the importance of continued efforts to increase the enforcement of criminal justice interventions in general, and no-contact and protection orders in particular.

While our research shows that "proactive enforcement" conceived as enhanced contact between law enforcement and victims is not an effective means of increasing victim safety or reducing offender recidivism, this is an important finding in and of itself. This finding indicates what doesn't work in reducing domestic violence recidivism. Additionally, law enforcement contacts with victims in the treatment group did not place women at additional safety risks. Moreover, we believe the design for the contacts and intention to implement the treatment constitutes a qualitatively new interaction between victims and law enforcement and represents a type of "proactive enforcement" that might reasonably have been expected to create differences in the observed outcomes.

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INTRODUCTION

In 1965 if a woman called 911 for help because her husband punched her in the face and then left the home for the corner bar, the dispatcher would most likely attempt to talk the woman out of requesting a patrol car to respond to her residence for help (Parnas, 1967). The dispatcher instead would talk to the victim about the negative situation she might find herself in financially should her husband be arrested. The dispatcher may go so far as to suggest that she spend the night at her Mother's house (Parnas, 1967). During these times "domestic violence" wasn't even a term that was used, because violence against a spouse was not against the law. Even so, domestic violence was and still is a phenomenon found within all demographics in the population.

Battery by a spouse or lover is the single most common reason for women entering hospital emergency rooms, exceeding childbirth, automobile accidents, muggings, and all other medical emergencies (Mills, 1996). American women are certainly not alone in this regard. Levinson's cross-cultural study of family violence illustrated that domestic abuse occurred in over 84 percent of the 90 societies examined (Mills, 1996).

Every year in America, approximately 1.5 million women and 800,000 men are raped or physically assaulted—some repeatedly—by an intimate partner. According to the 2000 National Violence Against Women Survey, nearly one of four women will be raped or physically assaulted by a current or former spouse, cohabiting partner, or date during her lifetime (Tjaden & Thoennes, 1999). One study reported

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that 44 percent of women who were murdered by their intimate partners had sought medical care at an emergency room within two years prior to their death; 93 percent of these murdered women had previously visited an emergency room for an injury at least once (Crandall, Nathens, Kernic, Hold, & Rivara, 2004).

Due to the diligent work of the women's rights, civil rights and domestic violence movements from the 1960's to today, domestic violence has come to be recognized as a public health concern that continues to affect every social and economic group in the population. When domestic violence was first acknowledged, it was not considered to be a matter for the criminal justice system because it was only considered to be a 'domestic disturbance' (Parnas, 1967). During the 1980's, domestic violence was still thought of as "one of those issues that no one talks about." It was certainly not a common topic of conversation, and it if the topic did warrant a conversation it remained at a whisper.

As a society, we respond to domestic violence in a drastically different way today than the typical law enforcement response of the 1960's. However, because we haven't yet mastered how to re-program an individual so that they don't need to use coercion to obtain power and control over another person, we must continue to improve our response to domestic violence in the best way possible. Though domestic violence certainly occurred well before social movements broached the issue in 1960's, it is still in line for the ultimate cure.

In assessing the current response to domestic violence in comparison to that of forty years ago, one may make a giant leap and consider this an indicator that society has evolved; or not. The main

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reason that the criminal justice system responds differently to domestic violence now is because abuse of one's partner has since been criminalized in all states either by statute or sentencing enhancements. Today, domestic violence may be statutorily referred to as domestic violence, spousal abuse, intimate partner violence, wife assault, or criminal domestic violence. The actual term varies by state, as do all domestic violence related statutes.

As domestic violence became more widely criminalized, more resources became available for victims of this type of abuse. Along with the adoption of mandatory arrest laws and no-drop prosecution policies, the implementation of personal protection orders was one of the most important advances for the legal treatment of domestic violence cases.

The issue of domestic violence raises difficult problems for policy makers, social service providers, and criminal justice officials. As the criminal justice system continues to progress in terms of reform for victim safety, it is important to evaluate and measure the outcomes of these interventions. Armed with data on what works and what does not, we can work to improve victim safety and increase offender accountability.

Studies consistently demonstrate the serial nature of violence between intimate partners (Fleury et al., 2000; Tjaden & Thoennes 2000). 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 no-contact orders - an increasingly used tool to disrupt dangerous patterns of contact between offenders and victims. These

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are criminal orders that are primarily used as conditions of release for defendants. Used to reduce the cycle of violence between bond hearings and judicial proceedings, a no-contact order typically prohibits an offender from contacting a victim during the period between his arraignment and case disposition.

Given the long-term and continued nature of intimate partner violence, the most dangerous time for battered women may be after her initial contact with the criminal justice system or subsequent to the arrest of their abuser (Sherman, Gottfredson, MacKenzie, Eck, Reuter, & Bushway, 1998). Protective orders, therefore, may offer swifter relief than other criminal actions (Davis & Smith, 1995), by providing an immediate remedy to the continued threat of violence and serving as a deterrent for potential offenders. Although some researchers have suggested that protective orders 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 womens' calls for help after they have received a protective order (Chauduri & 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 there was a protective order in place (Weisz, Tolman, & Bennett, 1998).

Domestic violence is a complicated cycle that results in one person maintaining power and control over another individual. It involves a cyclical pattern where victims become accustomed to living like they are walking on eggshells one day, enduring a physical beating the next, and receiving flowers the next day. Clearly

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domestic violence involves unique dynamics that are different from all other forms of violence. This study focuses on criminal no-contact orders that are put in place by the court in cases of domestic violence. Before discussing the methodology of the current project we present the literature in terms of civil protection orders since the purpose for both types of orders is similar and the literature on criminal protection orders is sadly bleak.

Two Types of Protection Orders: Civil and Criminal

A protection order (PO) is a court-ordered injunction designed to restrain an individual's use of physical violence, threats, or intimidation against another person. The main purpose of a PO is to protect victims by prohibiting their abuser from: "committing acts of family violence, directly communicating with a member of the family or household in a threatening or harassing manner, and going to or near the residence or place of employment of a member of the family or household" (Carlson, Harris, & Holden, 1999, p. 206). Many states differ as to what types of relationships are included in POs. In some states, individuals who share an intimate relationship without residing together or members of same-sex relationships are excluded (DeJong & Burgess-Proctor, 2006). States also differ in their procedures of issuance and enforcement of protection orders.

Most protection orders issued in the United States are civil - not criminal - protection orders. Since only about 20% of women who experience intimate partner violence obtain a civil protective order (Holt, Kernic, Lumley, Wolf, & Rivara, 2002), it is important to keep

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in mind that those who seek such orders may be systematically different from the general population of domestic violence victims.

The criminal no-contact order is similar to a civil protection order – it prohibits the defendant from calling, writing, or having a third party contact the victim. The main difference between criminal protection orders and civil protection orders is that criminal protection orders are ordered by the court and are often a condition of a defendant's release. To obtain a civil, restraining, or temporary protection order it is the victim's responsibility to petition the court and request the protection order that she hopes will make her safer. Court-ordered, criminal protection orders are not victim initiated; instead, the order is initiated by the court. These orders 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. No-contact orders are usually used to disrupt the cycle of violence between bond hearings and judicial proceedings by prohibiting an offender from contacting a victim during that period. The use of no-contact orders also sends a message to the public that the criminal justice system doesn't take the crime of domestic violence lightly. Violations of no-contact orders may include fines and/or jail time.

Although the use of no-contact orders to disrupt the cycle of violence between the bond hearings and judicial proceedings is widespread, the level of compliance with these orders is not well understood, nor is it known whether proactive enforcement (or any kind of enforcement whatsoever) reduces harm to victims, is ineffective, or is counterproductive (Burgess-Proctor, 2003; Capshew & McNeece, 2000;

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Sherman et al., 1992). Nevertheless, proactive enforcement is often suggested as a way to produce higher levels of victim safety and offender accountability (Violence Against Women Act of 2000, P.L. 106-386; Klein & Orloff, 1996; Violence Against Women Act of 1994, P.L. 103-322). To date, an experimental design has not been utilized to examine the effectiveness of no-contact order enforcement on subsequent violence.

While criminal and civil protection orders are similar in theory, the major difference between the two types often creates a practical issue for law enforcement to recognize when putting these policies into practice. Since civil protection orders are requested by victims, these cases typically involve situations where victims do not want further contact with defendants, as evidenced by the fact that victims are taking steps to have the behavior stopped. However, there are many cases where a victim (or a third party) calls 911 because she wants the immediate abuse to stop; yet, she does not realize that her call to the police will result in her partner's arrest if she has very clear physical injuries.

In cases involving criminal no-contact orders, victims' wishes are not considered because in a majority of the cases the no-contact order is automatically ordered by the court, at the recommendation of the prosecutor, as a condition of the defendant's release. The practical issue here is that among these cases there are many victims who do not want a no-contact order in place. In some circumstances, victims may not want the order in place because they fear more violence and blame. Other victims may not want to be apart from their batterer, or might feel like they are not able to survive without the

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offender, despite the abuse, because of emotional and/or economic dependency.

Relevant Historical Information for the Current Study

The location for the current research is Lexington County, South Carolina. Lexington County consists of a mixture of rural and urban areas and there are approximately 235,000 residents. The average income per capita is approximately $25,000. The Criminal Domestic Violence Court is operated by the Lexington County Sheriff's Department, which is a unique aspect of this court's structure. The domestic violence prosecutor derives prosecutorial authority from the State of S.C. Eleventh Circuit Solictor but is employed by the Sheriff's Office. The Sheriff has been in his elected position since 1972 and obtained his Ed.D. from the University of South Carolina. Sheriff Metts has a long history of research collaborations with the Department of Criminology and Criminal Justice at USC. In addition, many of his employees have graduated from USC's Master's Program in Criminal Justice. Given that Sheriff Metts is supportive of innovative interventions for public safety purposes and has had a long-standing relationship with the Department of Criminology and Criminal Justice at USC, a natural collaborative partnership was formed for the project: 'The Lexington County Domestic Violence Court: A Partnership and Evaluation,' funded by the National Institute of Justice, U.S. Department of Justice. The Sheriff's Department implemented a Criminal Domestic Violence Court (CDVC) in 1999 with a grant from the Office on Violence Against Women. Between January 2001 and January 2003, researchers from the Department of Criminology and

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Criminal Justice at the University of South Carolina conducted a process and impact evaluation of the Lexington County CDVC (Gover, MacDonald, & Alpert 2003). The results from the outcome evaluation indicated that processing domestic violence offenders in the specialized CDVC significantly reduced re-arrests by 40% compared to a historical control group of offenders processed in one of eight magistrate level courts throughout the county (traditional court). Offenders were followed for eighteen months for recidivism. While recidivism in Lexington County was declining, the average number of arrests for first time offenders was increasing. Therefore, when looking at the big picture, it appears that the judicial response to domestic violence changed in Lexington County after the establishment of the CDVC, since recidivism was declining and first-time offenders were not recidivating, yet the criminal justice system was intervening.

In addition to these positive outcome evaluation findings, three forms of data were collected for a process evaluation of the CDVC (Gover, Brank, & MacDonald, 2007). These included courtroom observations, in-depth interviews with key courtroom players (judge, prosecutor, court administrator, victim advocate, etc), and brief interviews with 50 victims and 50 defendants as they were leaving court. Every outcome measure assessed from these sources of data was positive. Of particular interest are the brief surveys that were conducted with victims and defendants. After signing a consent form, a respondent would either have the interviewer read the questions to them or complete the survey on their own. There were 10 questions, with only 2 being open-ended. Most questions measured procedural

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justice concepts. Nearly all individuals approached for the interview participated.

The major theme that evolved from these surveys was that the majority of both defendants and victims felt that their experience in the CDVC was fair, that they were treated with respect and dignity, and that they had the opportunity to tell their side of the story (i.e., have voice in the process). Even if victims and defendants did not agree with the outcome of their case they still felt treated fairly.

Despite these positive quantitative findings, there are two ways that the research team and the prosecutor became concerned that no-contact orders were often violated. First, the researchers routinely noticed victims and defendants leaving court together after completing an interview. Additionally, the prosecutor would briefly talk with victims before court and ask, "So what's the situation with him now… are you together or broken up or did you get things worked out…." In some cases the research team wondered if victims' thought they were supposed to say they worked things out. In any event, after the prosecutor talked with each victim, defendants were called into court and were instructed to remain facing forward, so as not to intimidate victims - who would be seated at the back of the courtroom. The prosecutor would first let the judge know which defendants were in violation of their release by having contact with the victim, and appropriate sanctions (fine or jail time) were then imposed.

Interviews with court personnel during the process evaluation identified an important gap in the Lexington County's response to criminal domestic violence cases. Specifically, victim safety was

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identified as a serious problem in Lexington County during the period between an offender's initial appearance in bond court and trial (Gover et al., 2007). At arraignment during bond court, the prosecutor estimated that approximately 80% of defendants are placed on a no-contact order as a condition of their release. Offenders violate their no-contact orders if they attempt to contact victims by telephone or in person or in any way. During the prior CDVC evaluation (Gover et al., 2003), court observations, interviews with court personnel, and interviews with victims and defendants indicated that a significant proportion of defendants were violating their NCOs—with some offenders even arriving at court with their victims. The current study was designed to address this breakdown in the system. Based on the previous successful research collaboration between USC researchers and the LCSD/CDVC, we were able to come to a working agreement to start a second research project, which is the topic of this report. For the current project, we measured various outcomes related to the enforcement of no-contact orders. This research addressed gaps in the current knowledge base by providing important findings on the prevalence of violation of NCOs and the impact of a proactive enforcement intervention on offender and victim outcomes.

Current Study

The current study focused on domestic violence victims whose alleged batterers were free on bond with a NCO as a condition of their release. NCO restrictions typically remain in place while defendants await further judicial proceedings. The treatment condition involved the following intervention: (1) a special domestic violence

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investigator assigned by the jurisdiction's sheriff proactively "checked in" with the "treatment" group of victims to verify that they understood the NCO and to monitor compliance; (2) the investigator provided advice on mobilizing law enforcement and collecting evidence to help sanction the offender if the order was violated. Additionally, the officer reminded victims of NCO requirements for suspects, inquired about violations and the victim's safety, and encouraged the victim to call for assistance if a violation occurred.

Meanwhile, a control group of victims experienced the "status quo" response to NCO cases. Under the status quo control condition, a variety of different types of enforcement are possible. Even though the proactive contacts by the designated officer did not occur in this group, other officers in the department as well as law enforcement victim advocates and the prosecutor might interact with either the victim or the offender. These types of contacts depended on the individual circumstances of each case and could be expected to occur in both the treatment and control groups. Proactive contact efforts for the purposes described above only occurred systematically in the treatment group.

The project involved a prospective, randomized experimental study in which we randomly assigned 466 NCO cases either to the current level of NCO enforcement (the control condition; N = 229) or to proactive enforcement (the treatment condition; N = 237). An additional 51 interim control cases were enrolled in the study during a coverage gap between the two officers that implemented the treatment condition over the course of the study. Data were collected from official LCSD, CDVC, and diversion records. Efforts were made to

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interview victims to measure background characteristics, life experiences, circumstances surrounding the "gateway incident" which resulted in the case being enrolled in the study, and subsequent victimization experiences and no-contact order compliance levels. These interviews were targeted to occur six weeks and six months after the gateway arrest.

Subsequent chapters use both the interview and official record data to assess the impact of proactive enforcement of NCOs on both offender and victim outcomes. The offender outcomes for the study include official records and victim reports of offender compliance with the NCO and offender recidivism. The victim outcomes include perceptions of the effectiveness of the NCO, victim safety, and victim physical and mental well-being.

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REVIEW OF THE RECENT CIVIL PROTECTION ORDER LITERATURE

Although the focus of this project is not on civil protection orders, we hope it will be beneficial to the reader of this report to have a clear understanding of the similarities and differences between criminal and civil orders. There is actually quite a bit of overlap in their procedural use so a review of the civil protection literature is necessary. Additionally, it is a good idea to include relevant information regarding civil protection orders for several other reasons. First, since we have found several jurisdictions that do use criminal no-contact orders, we should be aware of the state of the literature on civil orders so that we can compare outcomes of criminal orders to civil orders. This makes sense because the orders are actually doing the same thing, in theory. Two main inquires here have to do with whether victims are safer when they are not the ones requesting the orders, and to see what happens in cases when victims do not initiate no-contact orders.

DeJong and Burgess-Proctor (2006) conducted a comprehensive review of protective order statutes throughout all of the states including the District of Columbia. This research was complicated since each state uses its own terms for domestic violence and battering. The authors identified “victim-friendly” states based on three specific parts of the statutes: the type of relationship that was included in the statute domestic violence, the feasibility of the administrative process (i.e., required fees), and the punishment for protection order violations. All states received a score reflecting how progressive their protection order statutes were. DeJong and

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Burgess-Proctor (2006) found that the top-scoring progressive state was Missouri, followed second by Massachusetts. Florida, Indiana, Kansas, Michigan, New Hampshire, Washington, and Wyoming were tied for third place in terms of scores.

In addition to the problem of statutes differing across states, there are also differences in procedures for and enforcement of protection orders within states. Interestingly, Logan, Shannon, and Walker (2005) looked at differences in protection order processes and effectiveness between urban and rural counties in Kentucky and found that victims were treated differently depending on whether they resided in a rural or urban county.

As mentioned previously, most types of protection orders are civil remedies, yet violation of the order's conditions may merit a transition to the criminal domain. The resulting civil contempt, misdemeanor, or felony offense charges have penalties ranging from verbal reprimand or monetary fines to incarceration (Holt et al., 2003). For several reasons, the juncture between civil and criminal justice systems makes protection orders a versatile resource for women who experience domestic violence. According to DeJong and Burgess-Proctor (2006), they provide a lifetime alternative when there is no criminal case in which to pursue prosecution. Thus, protection orders provide state protection for women who otherwise might not be able to secure protection on their own. Also, the criminal sanctions that accompany violation of a PO have a potentially deterrent effect on batterers, forcing them to consider the consequences of violating the protection order. Finally, the enactment of protection order legislation in small communities sends the message that violence

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against women will no longer be tolerated and will bring about public intervention (DeJong & Burgess-Proctor, 2006; McFarlane, Malecha, Gist, Watson, Batten, Hall, & Smith, 2004).

The implementation of personal protection orders was one of the most important advances in the legal treatment of domestic violence cases since the movement began. Sometimes referred to as "no-contact orders" or "restraining orders," these orders prohibit an individual accused of domestic abuse from contacting the alleged victim. However, this can sometimes be confusing for victims. Often times the meaning of 'no-contact' is not sufficiently explained to victims, and they are not told what constitutes a violation or what to do if a violation occurs. Worse yet, many victims leave court feeling as if the order has been imposed on them and that they are somehow in legal trouble (Gover et al., 2007). In some states, no-contact orders are reserved for criminal cases only. According to Holt, Kernic, Wolf, and Rivara (2003), a shift in policy and practice occurred recently so that the issuance of criminal protective orders is now seen as an appropriate criminal justice system response to domestic violence instead of, or in addition to, civil action brought by the victim. Nonetheless, civil protection orders are obtained by only approximately 20% of the 2 million U.S. women who are physically abused, raped, or stalked by partners annually (Tjaden & Thoennes, 2000).

Overall, the fact that PO's are an increasingly popular form of criminal justice system response allows the problem of domestic violence to remove its private façade and come to light as the important social problem that it truly is. So we must ask ourselves:

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Do protection orders actually help reduce further violence? Do they make women feel safer? What drives some women to obtain protection orders? The review of the literature below will attempt to answer these questions.

Protection Order Evaluations

The effect of civil protective orders on offender recidivism and subsequent victim safety has been explored in a number of studies, with mixed results. Findings on the effectiveness of protection orders tend to lack consistency throughout recent literature (Carlson et al., 1999; Holt et al., 2003; McFarlane et al., 2004). Generally speaking, efforts to assess how well POs work typically gauge efficacy in one of three ways: victims' perceptions of protection orders and the PO process, enforcement of POs by the police, and the success of POs in preventing future re-abuse (Burgess-Proctor, 2003). As previously mentioned, we do not have research to guide our exploration into the relationship between criminal no-contact order enforcement and victim safety. Hence, we provide a review of civil protection order studies that examine desired outcomes; the reader may or may not draw inferences from this literature in terms of criminal no-contact orders.

Grau, Fagan and Wexler (1984) conducted one of the first evaluations of the effectiveness of restraining orders. Their research found no significant differences in subsequent abuse or violence for those with protective orders compared with those without, although they found lower rates of abuse among women with an order when they had lower initial levels in the severity of violence and

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injury. While this research is often cited in the literature on the effects of protective orders on offender recidivism, it is important to note that the analysis is limited since it relied on data from a family violence demonstration program in which the women without protective orders received some form of intervention. Similarly, Klein's (1996) analysis of restraining order cases in Quincy, Massachusetts in 1990 found that half of all batterers re-abused their victim within two years of the issuance of the order and that the rate of recidivism did not differ between those who maintained the order and those who dropped it. Klein suggests that the optimal use of protective orders may 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 protective orders, especially for those women with lengthy prior histories of abuse at the hands of their intimate partner (Klein, 1996), suggesting that these orders may not be effective in reducing revictimization. However, before we accept such a conclusion, it is important to remember that much of the past research on the effectiveness of protective orders (restraining orders) on offender recidivism is based on small, purposive samples that examined short follow-up periods, failed to include controls for potentially important confounders (Holt, Kernic, Wolf, and Rivara, 2003), and focused on the effectiveness of victim-initiated civil protective/restraining orders. Low response rates may also limit the credibility of earlier findings (Holt et al., 2003). Given the limitations of previous research, Capshew and McNeece (2000) argued that it may be too soon to draw any

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firm conclusions on the effectiveness of protective orders as intimate partner violence interventions.

Meloy, Cowett, Parker, Hofland, and Friedland's (1997) meta-analysis of eleven previous studies on the effectiveness of protective orders suggests that the severity of violence experienced by the victim (Keilitz, Nannaford 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: 215) have argued that "civil protective orders that are properly drafted and consistently enforced can offer effective protection for victims of domestic violence" and 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 protective orders (Sherman, Gottfredson, MacKenzie, Eck, Reuter, and Bushway, 1998), suggesting that protective orders may reduce the risk of re-victimization (Carlson, Harris, and Holden, 1999; Holt, Kernic, Wolf, and Rivara, 2003; McFarlane et al., 2004)and improve victim well-being (Johnson, Luna and Stein, 2003; Keilitz, Nannaford and Efkeman, 1998; Keilitz, Davis, Efkeman, Flango and Hannaford, 1998). Using police-reported violence and interviews with women measuring psychological and physical violence on the Revised Conflict Tactics Scale (CTS2), research by Holt and colleagues (2002; 2003) examined the effects of protective orders on offender recidivism. Their analyses were 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

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but had not obtained an order. Findings indicated that protective orders 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 protective orders on reducing the risk of re-victimization was for those cases in which the order was kept in place. Their findings suggest that permanent orders reduce the risk of re-victimization by 80%. Similarly, in a two-year follow-up of court and police records of women who had received a protective order, Carlson et al. (1999) found a decline in the probability of abuse (66%) 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, their findings indicated no differences in recidivism outcomes for those women who had received a permanent as compared to a temporary order. Their research also indicated that low income and African American women reported higher rates of re-abuse. Consistent with research on the effects of mandatory arrest policies and the "stake in conformity" thesis, protective orders 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, & Johnson, 1993).

Holt et al. (2003) found that women who obtained protection orders were significantly more likely to be working full-time, pregnant, and depressed at the time of the incident, and were less likely to have an alcohol or drug problem. Women who obtained protection orders were also less likely to be living with the perpetrator at the time of the index incident. Wolf, Holt, Kernic, and Rivara's (2000) study compared domestic violence victims who

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obtained protection orders to those who did not, in order to determine characteristics that might alert clinicians to a woman's readiness to obtain such an order. This study produced results similar to that of Holt et al.'s (2003) research. Their sample consisted of 448 adult women who were abused or threatened by male intimate partners in an incident reported to the police and who obtained domestic violence related civil protection orders. They found that women who obtained protection orders were more likely to be employed full-time and to have health insurance. Women who obtained protection orders were also more likely to be married or previously married to the abuser, and were less likely to be currently involved or living with the perpetrator. In addition to this, Wolf et al. (2000) found that women seeking protection orders were more likely to be older, pregnant, have experienced threats of violence against themselves or their families, have had family members or friends abused on the index date, have severe depressive symptoms, and have been forced to have sex in the prior year, than were women who did not seek protection orders (Wolf et al., 2000). Zoellner et al. (2000) also noted that women who were employed and could be financially stable on their own were more likely to follow through with the complete process of obtaining a protection order. This research also produced interesting results, namely that assault characteristics such as severity of abuse, seeking medical attention, and history of weapon involvement were unrelated to victims' completion of the protection order process (Zoellner et al., 2000).

In outlining a change in the enforcement and provision of protective orders, Holt et al. (2003) have argued that protective

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orders 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. The next several examples will give more in-depth details of the study. In Carlson et al.'s (1999) study, the researchers evaluated the effectiveness of protection orders in a sample of 210 couples in Texas. This study used both court records and police reports, yet they excluded several cases where the order was filed by a same-sex couple, a man against a woman, or a family member against another family member (i.e., not spouse against spouse). They looked at the effectiveness of protective orders by estimating the relative risk of re-abuse for women who procured these orders over a two year period. Carlson et al. (1999) found that 68% of the women in the sample reported some form of physical violence in the two year period preceding the PO. During the two year period after the PO, only 23% of the women reported being subjected to re-abuse. Even though findings showed that the PO affected whether a perpetrator chose to be violent or not, the results also showed that for men who continued to be violent, the rate of violence did not change (Carlson et al., 1999). This study also looked at differences in re-abuse based on socio-economic status (SES). Carlson et al. (1999) found that very low SES women experienced a 53% decline in re-abuse, while low and medium SES women experienced a 71% decrease. This finding suggests that future research should tease out the relationship between SES and the efficacy of POs.

Holt et al. (2003) found that in addition to obtaining a protection order, maintaining the order reduces subsequent violence.

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Their qualitative study of almost 400 women included a control group, an important factor that many similar studies often omit. They obtained extensive information through interviews and determined the occurrence of all post-index-incident domestic violence among participants, regardless of whether it was reported to the police. The non-PO group (control group) consisted of a random sample of women who had police contact for domestic violence during the study period but did not obtain a PO following the reported incident. The PO group included women who obtained protection orders during the study period, whether or not the precipitating incident was reported to the police. Women who obtained and maintained POs were significantly less likely to be contacted by the perpetrator, threatened by him, or sustain psychological or physical abuse from him between the index incident and the first follow-up interview (Holt et al., 2003). This effect was strengthened during the time between the first and second interview. During this period, women with POs were significantly less likely than those without POs to be contacted by the perpetrator, threatened by him with a weapon, psychologically abused, sexually abused, physically abused, injured from abuse, or to receive medical care after abuse (Holt et al., 2003). Overall findings showed a 70% decrease in physical abuse among women who obtained and maintained their PO's throughout their follow-up period.

McFarlane et al.'s (2004) study presents slightly different perspective regarding the maintenance of POs. This study looked at differences in the effectiveness of POs based on whether or not the women who applied for these orders were granted them by the courts. The 149 women who took part in this study reported significantly lower

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levels of abuse, including worksite harassment, up to 18 months following their application for a PO. Whether the women were granted or were not granted the PO made no significant difference in terms of the amount of violence they reported at the time of application or during the follow up periods. McFarlane et al. (2004) show that regardless of the outcome from the criminal justice system, applying for an order of protection reduces subsequent abuse in domestic violence relationships. They explain that some women use protection orders as a way to regain some control in the relationship and to notify the abuser that the law knows about his behavior. The women in this study viewed the legal system as a force larger than themselves, with power over the abuser that they themselves had lost as a consequence of the abuse (McFarlane et al., 2004).

Burgess-Proctor's (2003) research used a previously collected sample of 277 women who received either a temporary or a permanent protection order. She determined efficacy of protection orders by looking at whether abusers violated their partners' protection orders; she also addressed efficacy differences based on victims' demographic factors. This data came from victim self-reports, not official court records. In this study, Burgess-Proctor found that unemployed victims had a higher percentage of protection order violations than employed women. Women who resided with their partners at the time of the protection order had a lower percentage of protection order violations (Burgess-Proctor, 2003). One result that Burgess-Proctor (2003) addressed specifically was that black victims had a lower percentage of protection order violations. She explained this finding by suggesting that "Black women in this sample may have been less

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inclined to report a violation of their protection orders, or they simply have experienced fewer violations as the data suggest. It is clear that court personnel and advocates working with victims must acknowledge that race is an important factor in battered women's experiences with their protection orders" (Burgess-Proctor, 2003, p. 50).

Fischer (1992) suggests that it is important to think about the effectiveness of protective orders in a broad fashion. He points to the need to examine whether protective orders empower the victim in her decision-making and in actions intended to end a violent intimate relationship. Mills (1998) suggests that 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, reflecting 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 et al., 1998; Keilitz, Davis, Efkeman, Flango, & Hannaford, 1998).

Another way to "think outside the box" in considering ways to measure PO effectiveness is evaluating women's perceptions of, response to, and adherence to the conditions of the order. These factors have a direct effect on the data collected and the conclusions drawn from research. While many researchers have examined the impact of domestic violence interventions on offender recidivism, Capshew and McNeece (2000) also note that the effectiveness of protective orders may be operationalized in several ways.

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Mills (1996) points out that even civil protection orders, which enjoin a batterer from further violence or threats, have limited value as a means to curtail violence in many women's lives. However, in most states, civil protection orders can be used either in conjunction with criminal proceedings or on their own in civil court; thus protection orders potentially give women the means to fashion remedies according to their needs (Mills, 1996). When the protection order is a civil rather than a criminal proceeding, a woman may be more willing to take action on the belief that her abusive mate will be less likely to retaliate than with criminal proceedings (Carlson et al., 1999).

So, what separates the women who obtain protection orders from the ones that do not? Studies addressing this issue are discussed in the section below. As we discuss in the following section, the usefulness of conclusions from these studies has been affected by evaluation design problems, including small, purposive samples; selection bias, due to exclusion of cases and sample attrition; short follow-up periods; and lack of statistical controls for potentially important confounders. Additionally, much of this research has focused on the effectiveness of victim-initiated civil protective orders, with relatively little work on the effectiveness of orders initiated by the courts. As victims who initiate a civil protective order are more often the victims of severe, repeated abuse, the findings from these studies are not easily generalized to the wider population of domestic violence victims. Finally, one of the most significant limitations of previous work may be that the efficacy of protective orders has been examined without careful consideration of the effects of variation in the enforcement of these orders.

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Characteristics of Women Who Seek Protection Orders

Common sense tells us that there must be individual reasons behind each woman's choice whether or not to obtain a protection order, because each case is so unique. Yet the dynamics of domestic violence suggest that victims do share certain characteristics, and the behavior among victims is often similar. Researchers attempt to identify certain characteristics that serve as predictors or risk factors to aid in identifying which women are more or less likely to begin, complete, and follow through on the protection order process.

In addition to physical and situational factors, research has explored emotional factors that affect women's decisions regarding obtaining a protection order. Fischer and Rose's (1995) study used qualitative data from women who were in court to obtain a protection order against their abuser, in order to address this question. One of the main themes found in this study was that nearly all women acknowledged that they had had "enough," stating that they were tired of the abuse and that was why they were obtaining the protection order (Fischer & Rose, 1995). Other reasons for obtaining a protection order included using it as a last resort and stating that they were ready to make a change in their lives. Other women also said that friends and family had a significant role in encouraging them to obtain these orders. The distinguishing factor about these women was the confidence in their ability to provide for themselves and their family, despite a nagging belief that the abuser would violate the order and intrude in their lives (Fischer & Rose, 1995). These women had to overcome the fear of leaving the relationship permanently, because they knew that obtaining the order would signal the

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potentially undesirable end of their relationship (Fischer & Rose, 1995). The women also had to admit to themselves that they could not stop the abuse alone. "They chose the law as an attempt to enhance the resources they needed, the most pressing of which was to communicate the basic message that he is not allowed to abuse her" (Fischer & Rose, 1995, p. 421).

Fischer and Rose (1995) also explored the barriers that women faced in the process of obtaining a protection order. They found that the most salient barriers were the emotional costs of being forced to call the police to have the abuser arrested, fear of retaliation for seeking legal protection, fear of ending the relationship permanently, and perceptions of the intimidating environment of the courthouse. They also noted that some women needed to renew their trust in a reinvigorated legal system, despite prior negative experiences (Fisher & Rose, 1995).

Another study that focused on women who obtained and maintained protection orders found that less than half of the women who initiated the restraining order process actually obtained final orders of protection (Zoellner et al., 2000). Addressing these results, the researchers stressed that "if such orders are conceptualized as a primary intervention aimed at facilitating a battered woman's ability to end partner violence, the low number of women who persist is of utmost concern" (Zoellner et al., 2000, p. 1092). This study also mentioned that the process of obtaining such an order is rather difficult and time-consuming. Regarding emotional and psychological factors pertaining to obtaining protection orders, perception of threat and attachment to the offender were two aspects that

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significantly influenced a woman's likelihood of obtaining a protection order (Zoellner et al., 2000). Attachment to the batterer referred to women who reported loving their partner or believing the partner was able to change; these women were less likely to obtain an order. Perception of threat referred to women whose partner threatened to kill them; these women were more likely to complete the protection order process. On the other hand, those women whose abusive partners made threats to their children were less likely to complete the PO process. It was assumed that fear for the children influenced this decision (Zoellner et al., 2000).

The reasons presented above for obtaining or not obtaining a protection order apply to women in rural as well as urban communities. Logan et al.'s (2005) study of urban and rural counties found that in both regions the top reasons women didn't obtain an order of protection were: the victim's desire to reconcile with the offender; the victim being coerced, intimidated, or pressured by the abuser to drop the protection order; and the victim being dependent on the abuser. Logan et al. (2005) also found that the biggest barrier for women to file a protective order was the lack of resources necessary to leave the abuser, while the second biggest barrier was general fear. This study also pointed out that there is a need for women to know and understand the resources and rights that are available to them. Some women mentioned stories reported by the media in which a victim ended up dead even though she had a protective order, therefore it is important to inform women that, in many cases, violence does not decrease when protective orders are obtained (Logan et al., 2005).

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Other Issues Regarding Obtaining Protection Orders

In addition to dealing with their own fears, women who obtain protection orders have to deal with systemic barriers placed in front of them by the criminal justice system. Yearwood (2005) addresses this issue by looking at the concordance between the plaintiff's request and what type of relief the court actually granted. Yearwood's (2005) research included a North Carolina sample of 240 court cases which pertained to obtaining ex-parte and domestic violence protection orders. The difference between an ex-parte protection order and a regular domestic violence protection order is the length of the order. An ex-parte order, sometimes referred to as an emergency order, is valid for 72 hours, with a full hearing to be scheduled during this time. This type of order can be renewed periodically until a permanent one is in effect. Understandably, this can become rather inconvenient for victims. A regular domestic violence order of protection is issued after a hearing has taken place and is renewable annually.

Regarding ex-parte requests, Yearwood (2005) found little concordance between what the plaintiff requested and what the court granted. The top four court ordered services or relief requests which were awarded included: the plaintiff claiming possession of the residence, the defendant having no contact with the plaintiff, the plaintiff being given possession of a vehicle, and the defendant being evicted from the shared place of residency. The greatest discordance occurred between the percent of plaintiffs requesting that the defendant not interfere with the minor children and the courts granting this request (Yearwood, 2005). This finding was especially

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disturbing because the court was not only interfering with the safety of the direct victim, but also with the safety of minor children. This type of discordance was also found when examining the regular protection order cases.

Yearwood (2005) notably addressed how North Carolina judges and magistrates are unwilling, during domestic violence protection order proceedings, to address financial support, such as child support and/or temporary support for the victim, nor do they regularly address issues involving minor children, such as child custody, visitation and the stipulation that the defendant not interfere with the children. This is a frightening finding that deserves further exploration.

Limitations of Prior Research

As noted by Fagan (1996) and Capshew and McNeece (2000), there has been limited research on the effects of criminal justice interventions in general that focus on reducing the incidence and prevalence of intimate partner violence. Fagan (1996) suggested that the failure to find support for the positive effects of criminal justice-oriented domestic violence interventions may be due to a number of analytic and measurement issues. Most studies have been primarily qualitative or have used non-experimental and quasi-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 protective orders as a promising solution in combating domestic violence, Sherman et al., (1998) suggested that an important area for future research is the use

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of randomized trials to provide a strong test of the effectiveness of orders of protection.

Research on efficacy of protective orders is limited in a number of ways. The three main limitations include measures used, samples used, and the generalizability of the findings. Measures used to appropriately evaluate the effectiveness of these orders often include women's perceptions of, response to, and adherence to the conditions of the protective orders. Future research should focus on using more concrete measures of effectiveness, rather than relying on the victim's perceptions. Although perceptions are important, they are hard to quantify and it is difficult to formulate concrete policy implications based on them. Measures used to address effectiveness of protection orders should include amount of re-abuse and violations of the stipulations of the protection order.

Another important limitation of the research on protection orders concerns the samples that are used, especially when focusing on what types of women are more or less likely to obtain an order of protection. Usually samples include women that are already in the process or are about to begin the process of obtaining a protection order. Most of the samples used in this research are guilty of 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 generalizations are attempted. Also, external validity becomes a problem due to the fact that protection order statutes vary among the states. If a study is conducted in one state, it may be problematic to use the results for policy in another state with different statutes.

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As with any research that includes obtaining victimization data, researchers encounter the problem of underreporting and having to decide what type of source is best to use, i.e., official statistics versus self-report data. In the case of protection orders, and especially the violation of them, it is extremely beneficial to combine and compare these sources, since many times women are hesitant to report re-abuse to the police after a protection order has taken effect. Researchers may even take this opportunity to compare the actual amount of re-victimization that takes place as reported by victim surveys and the amount that is reported to the police after a protection has been obtained.

In trying to determine what characteristics make a difference among those that do and do not obtain protection orders, it is of utmost importance to include all possible factors in research. Wolf et al. (2000) found that men whose partners obtained protection orders against them were older than the men against whom orders were not obtained. Compared to their female partners, perpetrators against whom protection orders were obtained were less educated and often from a different racial or ethnic group. But this was the only finding regarding perpetrator characteristics that was presented in all of the research reviewed. This area of research clearly deserves a more in-depth examination that would allow advocates and other key players in the criminal justice and social services systems to not only take victim risk characteristics into account, but also the characteristics of the abuser, as this may influence victim safety.

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Section Summary

Recent research on protective orders does show that they can be effective tools in reducing subsequent domestic violence (Murphy, Musser, and Maton, 1998). Fischer and Rose (1995) note that certain women feel empowered after obtaining a protection order, 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). But, in addition to issuing a protective order, 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 protection orders 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 stress that the successful use of protection orders hinges on enforcement by police officers, and that both petitioners and respondents must take them seriously. Research also shows that the criminal justice system should take into account demographic factors of victims and offenders in its quest for effective domestic violence intervention (Carlson et al., 1999).

Although there are individual differences that determine whether or not a woman will file and maintain a protection order, it is import to minimize the institutional barriers that some women face when attempting to obtain a protection order. Individual states should work toward making these processes more universal and victim-friendly. Although the modern protection order system is much more responsive to the needs and desires of individual women, in many states it still relies far more heavily on batterer treatment programs than on victim

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support to prevent future violence (Goodman & Epstein, 2005). States also need to include all types of relationships in their protection order statutes, including same-sex couples, never married couples, and couples that do not live together.

While obtaining a protection order is no guarantee that further abuse will be prevented for any individual, health and criminal justice providers should provide information about the availability of protection orders to all individuals affected by intimate partner violence (Holt et al., 2003). There is no perfect guarantee that the violence will stop unless the perpetrator is incarcerated indefinitely. Because this is an unrealistic expectation for every case, we should focus on making the lives of these women "safer," since there is a high probability that they will never be completely "safe." Based on Carlson et al.'s (1999) findings, we not only need to focus on the amount of re-abuse, but also on the rate of this abuse. Rather than condemning women's decisions to drop protection orders or return to the abuser as a step backward or a willingness to be abused, police and court officers should conceptualize this event as a necessary step in the process of leaving (Fisher & Rose, 1995).

Awareness of victims' rights and the availability of legal options like protection orders should be heightened not only among victims and potential victims, but also among workers in the criminal justice and social services systems. In order to make a real and lasting difference in their clients' lives, these key players need to be aware of the complex dynamics of domestic violence and how to properly deal with victims at the key point when they make the decision to put an end to their abuse.

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Finally, to fully examine the effectiveness any type of protection order requires a thorough understanding of the level of enforcement that occurs. Rigakos (1997) suggests that research on the effectiveness of protective orders will depend to a large degree on whether these orders are enforced by the police (especially through arrest) and prosecuted by the courts. Since one of the first evaluations of protective orders pointed to their failure in protecting those victims at risk for the most severe physical violence (Grau et al., 1984), a number of researchers in recent years have attempted to more thoroughly examine their effectiveness.

Researchers have attempted to identify the factors that are associated with a woman's decision to seek a civil protective order. In examining the effectiveness of protective orders on subsequent victim safety it is therefore important that these factors be identified and treated as confounding variables in any empirical evaluation. For example, as with research on the factors that influence women's decisions to call the police and invoke a criminal justice response (Kaukinen, 2004), the women who seek orders of protection suffer the highest rates of severe injury prior to the issuance of the order. The effectiveness of these orders may therefore reflect the characteristics of the batterers for whom the orders were issued against. Other factors that have been identified as associated with the decision to seek an order of protection include financial independence, symptoms of depression (Wolf et al., 2000), and prior police intervention (Weisz, Tolman, and Bennett, 1998).

As noted earlier, much of the previous research has focused on victim-initiated protective orders, reflecting the preponderance of

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victim-initiated protective orders over those that are court ordered. Recently, Holt and colleagues (2003) have noted a shift in policy and practice so that issuance of protective orders is now seen as an appropriate criminal justice system response to intimate partner violence instead of or in addition to civil action brought by the victim. Our research focuses on court orders of protection.

The U.S. Department of Justice (1996) identified a number of historical characteristics of protective orders that may have limited their effectiveness in ensuring victim safety and reducing offender recidivism. These limitations include the requirement that victims be separated from their abuser and initiate divorce proceedings, the need to enforce orders across state boundaries, and the difficulty, expense and time required for victims to obtain an order. While the federal Violence Against Women Act of 1994 has helped to eliminate a number of these obstacles to obtaining and maintaining the conditions and prohibitions of a protective order, serious limitations still persist. These include the qualifying criteria required for obtaining an order (Gist, McFarlane, Malecha, Fredland, Schultz, & Willson, 2001), a lack of enforcement on the part of the police even though such orders allow for expansion of police powers to arrest and increased ability to monitor repeat offenders (Finn & Colson, 1998, Rigakos, 1995), and a reluctance by prosecutors to prosecute those who breech a protective order (Kinports & Fischer, 1993).

To increase the effectiveness of protective orders Keilitz (1994) suggests that the criminal justice system needs to take affirmative steps to increase their power to provide safety to the victims while enforcing the order. Gondolf, McWilliams, Hart, & Stuehling (1994)

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have argued that the systems in place for the enforcement of protective orders 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 protective orders be developed and enforced that encourage respect for the court's order thereby increasing compliance. The issuance and enforcement of protective orders, therefore, need elaborate safety plans that include systematic investigation and stringent law enforcement of breaches of protective orders that simultaneously link victims to appropriate health and social services (Finn, 1991; Keilitz, 1991). To accomplish these steps may require changes in legislation to rectify problems with enforcement, to provide police officers with up-to-date electronic information on protective orders to alleviate confusion (Rigakos, 1997), to expand community policing, and to compile location histories that record all responses to a given residence or victim (U.S. Department of Justice, 1996).

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METHODOLOGY

The overall goal of this study was to estimate the effect of proactive law enforcement contact with victims on their safety and well-being. Law enforcement contacts were directed at victims whose abusers had been arrested for committing domestic violence and were placed on a criminal no-contact order by the court as a condition of their release. The no-contact order restricted the offender from having any kind of contact with the victim until the time of case disposition. Victims did have the option of petitioning the court to request that the no-contact order be dropped for various reasons, but this typically occurred in relatively few cases. Therefore, the treatment examined in this study was victim-focused. The victim-directed intervention consisted of 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. Contacts were scheduled to occur before first appearance; for offenders whose cases continued after first appearance, additional contacts were scheduled.

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. Victims of offenders who were randomly assigned to the treatment group received the proactive victim intervention (contact). The DO contacts did not

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preclude or substitute for any "business as usual" contact between the victims or offenders and other deputy sheriff's or law enforcement personnel. In fact, some officers are quite diligent at monitoring cases in which they have been involved. It is important to note that the DO had other law enforcement responsibilities outside of those related to this project and the Sheriff's Department donated the officer's time to serve as the DO for this study. The focus of this study is to examine the impact of the proactive enforcement condition on victim safety and well-being.

To ensure that cases which received proactive DO contacts were comparable to cases without the contacts, we worked with the LCSD to implement a randomized experiment. Once the offender was released on bond, paperwork documenting the bond restrictions would flow from the bond court to the Lexington County Criminal Domestic Violence Court (CDVC). The CDVC provided photocopies of the bond restrictions to the DO, who entered the case information into a database program.

The database program randomly assigned each newly enrolled case to a treatment group or control group and - for treatment cases - printed out a proactive contact schedule. The DO then targeted cases in the treatment group for proactive victim-directed contacts. All contacts and contact efforts were documented by the DO in a contact log. The DO also provided a log of newly enrolled cases to the research team so that cases could be listed in the research database and victims could be contacted to solicit their participation in a sequence of interviews.

The DO began enrolling cases in November 2005, and enrollment progressed continuously until September 2006. At that time, the DO

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notified us that he would be leaving his position at the LCSD. In late December 2006, a new DO began work on the project and she resumed 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 as "interim controls." Also, beginning in late June 2006, letters with a brochure introducing the DO as a LCSD point of contact and an explanation of the no-contact order on LCSD letterhead under the DO's signature were mailed to each victim in the treatment group.

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 our study. At this stage, we sought specific information about the incident that led to each case's inclusion in the study (hereafter referred to as the "gateway" arrest or incident). After obtaining this information, our goal was to secure follow-up arrest records (both before and after the gateway incident), and court dispositions for the cases in our study. We contacted the LCSD, the CDVC, and the Office of Diversion Programs (ODP) for assistance in this endeavor. The Sheriff's Department and the Office of Diversion Programs worked very closely with us to help provide the information needed for the study. Although the CDVC donated the administrator's time through its distribution of case information to the DO, the Court was unwilling to

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meet with us or work with us for purposes of obtaining detailed information on court dispositions for the cases in our study. Thus, the information about court dispositions for the cases in our study comes entirely from information that is publicly available on the South Carolina Judicial Department's website.

The second DO continued efforts on the project in the fall of 2007 and January of 2008 by acquiring official Lexington County Detention Center booking sheets for each of the offenders in our study. An initial effort was also made during this period to acquire all of the criminal history information for all of the offenders in our study. During the fall of 2007 and first two months of 2008, all of this information was entered into our 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 of the randomized cases (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). All of this information was entered into our analysis databases from March-May 2008.

Some of the offenders in our study were also eligible for a pretrial diversion program for first-time offenders. One of the outcomes we wanted to consider was whether offenders in the treatment group made better progress through the diversion program than offenders in the control group. Since the Sheriff's Department does not maintain this information, we approached the Office of Diversion Programs (ODP) Director for the 11th Judicial Circuit to see whether

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they would be willing to work with us to obtain this information. The director agreed to assist us in this effort. The procurement of the ODP data proved to be more difficult than we had initially expected. To obtain data from the LCSD, we did not have to provide them with any data because all of our data originated with them. But the ODP databases did not permit that office to provide us with a "dump" of all people who had participated in pretrial diversion programs. Instead, they required that we provide them with identifying information (specifically, the name and state identification numbers) for all of the individuals in our study so the PTI status of each individual could be determined. By providing them with this information, we would be transferring identifiable information to individuals not directly involved in the study which would be a violation of our protocols that protect human beings as research subjects (which had previously been approved by the Institutional Review Board) and our Privacy Certificate (28 Code of Federal Regulations Part 22). To address this issue, we amended our protocol and the privacy certificate so that we could procure the ODP data and maintain compliance. We obtained the data in May 2008 and entered the cases into our analysis databases at that time.

Finally, we sought information on case dispositions for the gateway incident for all cases in our study. Because we were unable to obtain this information directly from the CDVC, we were confined to case searches on the South Carolina Judicial Department website. These searches were conducted from April 2008 to June 2008. Some of the cases in the study were still not disposed of at that time. It is also possible that some of them were actually disposed of at that time, but

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this information had not yet been updated on the website. Fortunately, the vast majority of the cases in our study appeared to have been disposed as of July 1, 2008.

Over the course of the study, 466 cases were randomly assigned to either the treatment or control condition and an additional 51 cases were enrolled as interim controls during the time period when a DO was not in place to implement the treatment condition beginning fall of 2006. Thus, a total of 517 cases were enrolled in the study. A small number of cases (9) actually were enrolled in the study two times. Five of these cases were enrolled as a treatment cases on two occasions, one case was enrolled as a randomized control case on two occasions, and two cases were enrolled as a control case and an interim control case. A final dual-entry case was enrolled as a treatment case before the departure of the first DO and then as an interim control case. In each of these cases, we treated the first entry into the study as the gateway incident and any subsequent domestic violence activity was regarded as recidivism. In addition, there were five dual arrest cases (both parties arrested) included in the study. Thus, a total of ten cases are dual arrest cases and, by coincidence, they are evenly split with five in the randomized treatment group and five in the randomized control group.

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 or control group). Although there were 80 female offenders, only female victim cases were enrolled in the experiment. Therefore, no efforts were made to contact male victims for interviews. Interviews

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were targeted to occur at six weeks after the gateway incident (Time 1) and then again at six months after the gateway incident (Time 2). The vast majority of our interviews were face-to-face meetings at a local hospital. In some instances when it was not possible for the victim to participate in a face-to-face interview, we attempted to conduct interviews over the telephone (e.g., she missed multiple appointments to conduct the interview at the hospital, lived in another city, or told us that there was no other way she could participate). We developed a telephone interviewing protocol for determining whether it was safe to conduct the interview and received approval for this protocol in the spring of 2006 (please see Appendix Protocol Manual for details). By the end of the study, we conducted 11 Time 1 telephone interviews and 10 Time 2 telephone interviews.

Our standard interview protocol involved an initial effort to contact the victim with a letter introducing the University of South Carolina "Women's Health and Well-Being Study." These letters included a $1 bill and invited the victims to contact the study office for information about how to participate in the health and well-being study. Generic letters notifying potential study participants about the study were also provided to victims who sought help at the only domestic violence women's shelter in the county. The letters did not mention domestic violence and only referred to the study as research related to "Women's Health and Well-Being." Once phone contact was established with victims (initiated either by the victim or by ourselves), we described the study in vague terms that made no mention of domestic violence. We were concerned that description of the study's true focus on domestic violence would increase risks to the

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victims. Victims were told that they would receive $50 compensation for their time and effort if they agreed to participate in the study.

Victim and interviewer safety was the primary concern in the scheduling and location of interviews. If we were not able to contact victims at the telephone number provided for them, we never tried to contact them at the number provided by the offender. We also decided not to approach victims at the court because the offenders were always present at court proceedings and we did not want to create confusion about whether we worked for the LCSD or the court. Finally, we conducted all of our interviews at a local hospital in public but in quiet areas where privacy could be assured but help could be summoned quickly if needed.

Once our interviewers had their initial meeting with the victim a determination was made about whether it was safe to do the interview (according to our training and protocols). An example of a situation that might be deemed as unsafe included one where the interviewer determined that there was a possibility that the victim's batterer had brought her to the interview. If the interviewer believed it was unsafe to do the interview, then we conducted a diversionary informed consent process and a diversionary interview which did not contain any references to domestic violence. The purpose of the diversionary interview was to avoid putting victims in situations where they may have to lie to their batterer about the types of questions asked during the interview. We felt that this procedure would minimize risks to the victim and our staff if interference were to occur. We conducted eleven diversionary initial interviews over the course of the study. In two cases, we were able to schedule another initial

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interview where the real informed consent form and survey were administered. We treated the remaining nine cases with a diversionary initial interview as non-responses for purposes of this report.

If the interviewer believed it was safe to proceed with the initial interview, then the informed consent form was given to the victim and read to her. The informed consent form revealed the domestic violence focus of the study and asked women whether they wished to participate. The informed consent form also encouraged victims to destroy the form or to put it where the offender would not be able to find it. At the point of being told about the true focus of the study, some victims seemed surprised, but most did not and none refused to participate in the initial interview. Victims were told they would be paid regardless of whether they agreed to participate and that they would be free to terminate the interview early and not answer any questions that made them uncomfortable. Victims were also told that we would be contacting them for a subsequent interview and asked whether they would be willing to participate in that interview. Throughout the course of the project only one victim indicated that she did not want to participate in the second interview. Victims were also told they would be paid $50 to participate in the second interview and that we would pay them an additional $5 if they proactively called our office to schedule that interview. All payments were made in cash or cash equivalents.

Even after the first interview was completed, calls to the research office did not mention domestic violence; the study was always referred to as the "Women's Health and Well-Being Study." Protocols for scheduling and conducting the follow-up interviews were

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the same as those for the initial interview. In two cases diversionary follow-up interviews were conducted out of safety concerns. Questions on both interviews asked about a wide range of demographic characteristics, life experiences, questions about the gateway arrest, the offender, both past and subsequent abuse, and experience and contacts with law enforcement and court officials. A constant concern over the course of the study revolved around the issue of whether victims would convey information to us that needed to be reported to authorities. However, the need to report any information provided to us by the victims never arose.

It is necessary to emphasize that the interview consent process did not seek the victim's consent to be in the experiment. Rather, we sought victims' consent to participate in the interviews. In addition, we obtained approval from the Institutional Review Board for a waiver of informed consent for the information we collected about the offenders in our study.

Although the methods involved in our study are based on a strong research design and extensive data collection effort, we think it is important to note some limitations:

1. The target population for our study includes all cases where the offender is arrested by the LCSD and where the case falls under the original jurisdiction of the Lexington County CDVC. Prior to January 1, 2006 the CDVC had jurisdiction over individuals charged with a first or second offense of criminal domestic violence (CDV1 or CDV2). After January 1, 2006 the CDVC only had original jurisdiction of CDV1 cases. Thus, our study will tend to focus overwhelmingly on cases that

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are charged with a CDV1 and are processed in the CDVC, which does not represent the entire population of domestic violence cases committed in Lexington County.

2. While we believe that most of the cases that should have entered our study actually were enrolled, we have no way to verify this. Case enrollment depended on the efforts of the CDVC administrator to photocopy the relevant paperwork and provide it to the DO. During the summer of 2006, we convened a meeting between the DO, the CDVC prosecutor, and the CDVC administrator to check on whether all cases that should be enrolled were actually being enrolled. We singled out a specific docket for one first appearance session in late spring 2006. This session included 21 cases on the docket. Out of these 21 cases, 3 had been charged with a CDV2 which removed them from eligibility for our study. Out of the remaining 18 cases, 2 had been in jail prior to first appearance which meant that they had not been free in the community on bond restrictions and 2 had been free in the community on bond but did not have no-contact order restrictions as a condition of their release. Among the remaining 14 cases, 12 had been enrolled in the study and 2 mistakenly had not.

Over the remainder of the study we checked in periodically with the CDVC administrator to try to minimize the number of mistaken omissions but ultimately she was very busy with her many administrative responsibilities. It was suggested to us that there was a need to "tread lightly" with respect to the administrator's time because all of her effort was donated to the project and could be withheld if our demands became too great.

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All indications are that the CDVC administrator worked in good faith to relay the appropriate information to the DO and we do not have any reason to believe that large numbers of cases were inadvertently omitted or that there was any effort to divert cases away from the study. The most likely scenario is that the vast majority of cases that were eligible for study inclusion were enrolled and that there are no important systematic differences between those who were enrolled and those who were not enrolled but should have been.

3. The DO made efforts to proactively contact victims in the treatment group in order to convey information about no-contact orders and monitor compliance. It is important to note that victims may experience contact with the LCSD in a variety of ways including the CDVC prosecutor, law enforcement victim advocates (LEVAs), and officers involved in the gateway incident. Our official record information includes DO contacts but does not include any of these other contacts. We do ask victims in the interviews about their contacts with a LCSD deputy sheriff(s) and LEVAs but this information is only available for the interviewed victims. So, it is important to recognize that some aspects of "the treatment" could have been delivered to victims by other people in addition to the DO. But, those efforts should be occurring approximately equally between the two randomized groups unless victims in the treatment group become more proactive in seeking contact with other LCSD or court officials.

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4. The DO's goal was to proactively contact each victim in the treatment group following a schedule printed out by the database program. Although the LCSD generously donated the DO time for this project, the DO had other responsibilities in addition to proactively contacting victims for our study. Depending on a range of factors from the quality of victim contact information to the status of the case when they received the paperwork from the CDVC administrator to the on-duty time available to make contacts, the DO may or may not have actually contacted victims in the treatment group. We do have measures of how often contact was attempted in each case, and how often successful contacts were actually made. All indications are that most contact efforts were normally limited to the letter and brochure (after June 2006) and one or two phone calls by the DO to the victim. Another issue is the timeliness with which the necessary information was received by the DO from the CDVC administrator. According to one DO this information did not arrive in some cases until after the case had already been disposed. While the lack of complete contact can be viewed as a limitation of our study (and it is), it can also be viewed as an issue that is likely to arise if law enforcement agencies try to implement proactive contact policies with domestic violence victims. Of course, an important counterweight to this point is that contact efforts were made and contact was achieved in enough cases to ensure that the average experience of the treatment group was, in fact, meaningfully different than the average experience of the control group.

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5. Normally, the LCSD and court officials try to maintain no-contact orders throughout the period between the gateway arrest and case disposition. The bond restrictions typically expire when the case is officially disposed - either by a verdict, dismissal, or decision not to prosecute. Often these orders are maintained even against the victim's wishes. But sometimes, offenders are successful in getting no-contact orders lifted early. Unfortunately, this information is systematically housed only in the CDVC and is not available on the SCJD public website. Because the CDVC would not work with us to obtain information on the disposal of the cases in our study, we are not currently able to access this information. We remain open to the possibility of acquiring it and if that becomes possible we will seek that information. But a current limitation of our study is that we will not be able to distinguish between cases that had the no contact orders lifted early and those that did not.

Overall, our study has both important strengths and limitations. But, we believe the information provided by this analysis will be relevant and useful for policy discussions about proactive contact with victims, victim follow-up, and no-contact order enforcement for cases awaiting disposition. We also hope it will be useful in helping other researchers designing field studies with other agencies to learn and benefit from our experiences.

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RESULTS

Pretreatment Equivalence

Because the treatment condition was randomly assigned to offenders, some of the difficulties of establishing equivalent comparison groups do not create major problems for this study. Nevertheless, randomized assignment to groups only ensures equivalence in expectation (that is to say, as the sample size grows infinitely large). It is possible in any given sample that random assignment to treatment and control groups will not lead to entirely comparable groups. If this happens, we know why the groups are not equivalent (it occurred by chance) but the problem of nonequivalence itself would remain.[1] Thus, we devoted considerable effort to verifying that the offender treatment and control groups were, in fact, equivalent to each other on a large number of "pretreatment" characteristics.

Table 1 relies on data from the enrollment database, the Lexington County Detention Center (LCDC) jail booking sheets, and the National Crime Information Center (NCIC) arrest histories to list a wide range of characteristics on which offenders in the treatment and control groups can be compared to each other. All of the comparisons in Table 1 are based on information about each individual that are measured prior to the process of random assignment to groups. Variables with a (d) are dichotomous and the mean is actually the proportion of individuals in the group with that characteristic. Variables with a (c) are continuous or counted variables and the mean for these variables is the sum of the scores divided by the number of people in the group. The Z-tests in this table are differences in

[1] We acknowledge Dr. Amelia Haviland of the RAND Corporation for this insight.

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proportions or differences in average characteristics between victims whose offender was randomized to the treatment and control groups. The interim control group is not included in these tests.

Our first impression of this table is that the interim control group looks somewhat different from the randomized groups in several respects. For example, there appear to be more males, whites, Hispanics, and fewer blacks in the interim control group compared to the randomized groups. There also appears to be a greater prevalence of prior military service in the interim control group than in the randomized groups. Additionally, the interim controls appear to be somewhat more likely to have been born in South Carolina and to have been booked on a non-domestic violence charge at the gateway arrest than domestic violence offenders in the randomized groups. Finally, the interim controls appear to have been less likely to have a prior arrest record in South Carolina or Lexington County or to have experienced prior arrests for various violent, property, and drug offenses compared to offenders in the randomized treatment and control groups.

Overall, our impression is that the interim controls appear to differ from the randomized groups in some important ways. Since the interim controls were all enrolled in the fall of 2006, it is possible that there are some seasonality issues that affect the comparability of the interim controls. Perhaps domestic violence cases appearing in October, November, and December differ in important ways from cases that appear at other times of the year. Because of these differences, we chose not to combine the interim and randomized control groups.

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Thus, all of the tests reported in Table 1 focus on differences between cases in the randomized treatment and control groups.

The large majority of the cases in the randomized trial were male offender, female victim cases. In addition, a little over two-thirds of the sample was white while only about one-fourth of the sample was black or another race. About three-fourths of the offenders reported to LCDC officials that they had at least one child and less than 10% reported any prior history of military service. As expected, virtually all of the offenders in the study were booked on a formal domestic violence charge while only about 13% were booked on at least one non-domestic violence charge. On average, the offenders in our study were about 35 years old at the time of the gateway arrest. Among these offenders, the average age at the time of the first arrest in the NCIC arrest history was 24 years. Also, according to the NCIC arrest histories, a little over three-fourths of the offenders in this study had been arrested at least one time prior to the gateway arrest. Among those who had been arrested at least once in the past, a little over 4.5 years had elapsed on average since the date of the last arrest. Over 70% of the offenders in this study had been arrested at least once before the gateway arrest in South Carolina and over 45% of the offenders had been arrested at least once before the gateway arrest by the LCSD. Based on these findings, it is fair to say that our sample of offenders is not - at least on average - a group of novice or first-time offenders. In fact, first-time arrestees appear to be the exception rather than the rule among the offenders in our study.

After reviewing the list of charges associated with each arrest on the offender's arrest history, we created a series of variables to

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indicate the types of crimes that offenders had been charged with at arrests occurring before the gateway arrest. The most common prior arrest charges appearing in our sample included traffic-related offenses, drug offenses, assaults, alcohol violations, driving while impaired, disorderly conduct, fraud, theft, and domestic violence. Although the rates of involvement in other offenses were lower, some of the offenders in our sample had been charged in the past with a wide range of serious crimes including homicide, kidnapping, child abuse, various sex offenses, robbery, burglary, and motor vehicle theft. We also combined charges for assault, child abuse, domestic violence, harassment, homicide, kidnapping, robbery, and sex offenses to create an overall measure indicating whether each offender had been arrested and charged with a violent offense. Similarly, for property offending, we combined burglary, theft, fraud, motor vehicle theft, vandalism, and other property crimes into a single measure that represents an overall measure indicating whether each offender had been arrested and charged with a property offense.

Based on the results in Table 1, there appears to be a good deal of similarity between demographic and case characteristics among those in the randomized treatment and control groups. According to a search of the NCIC criminal history database, in terms of demographic characteristics, the existence of a prior arrest record, the number of prior arrests, and arrests prevalence rates for most offenses (including overall violent and overall property offending), and the length of time between the gateway arrest and the date, the groups are quite comparable to each other.

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This similarity notwithstanding, there are a few noteworthy differences between the groups. First, even though offenders in both groups were comparable in terms of whether they reported having any children, the mean number of children appears to be somewhat higher for offenders in the control group (1.8899) than for offenders in the treatment group (1.6624). Additionally, within the treatment group, 57.8% of the offenders reported having been born in South Carolina while 64.8% of the offenders in the control group reported being born in South Carolina. Offenders in the control group appear to have been somewhat more likely to have been arrested for kidnapping (3.1%) than offenders in the treatment group (0.8%). This difference should be interpreted with caution since the prevalence rates for both groups are so close to zero. Traffic offenses also appear to have been more prevalent for offenders in the control group (41.9%) than for offenders in the treatment group (34.6%). Finally, offenders in the treatment group were somewhat less likely to have been arrested for domestic violence in the past (19.4%) than offenders in the control group (26.2%). Since our study is about domestic violence, this difference in the prevalence of prior arrests for domestic violence is particularly noteworthy and suggests that offenders in the control group are more prone to engage in domestic violence that leads to an arrest compared to offenders in the treatment group.

Overall, the comparability of the groups appears to be very good but our efforts to document whether any differences exist have resulted in some evidence that the groups do differ in a few respects. While we do not think these between-group differences are sufficiently large to fundamentally alter our analysis approach, we nevertheless we

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think it is important to supplement our group comparisons with adjustments for the differences we have noted.

Contacts by Designated Officer

The primary emphasis of the intervention was to establish contact between the DO and the victim for purposes of notifying the victim about the existence of the no-contact order, explaining the requirements of the no-contact order, instructing the victim what to do if a violation occurs, and monitoring compliance with and enforcing the no-contact order. Through June 2006, contacts with victims were exclusively based on proactive efforts by the officer to make voice contact with the victim and to have a discussion. After June 2006, these efforts were supplemented with a letter from the DO (on LCSD letterhead) to the victim at the address on file and a brochure (please see Appendix C for copies of the letter and the brochure).

Once the DO received notification from the CDVC that an offender had been released on bond with no-contact restrictions, the DO would enter information about that case into the project enrollment database. The enrollment database program would randomly assign each case to the treatment or control group (thus, the DO had no control over the randomization process). Among cases randomly assigned to the treatment group, the DO was charged with the responsibility of making proactive, direct contact with the victims in the treatment group. It was left to the DO's discretion to determine what type of contact with the victim would be most appropriate and effective (in-person, telephone, at court, etc.). Based on guidance from the CDVC prosecutor, the DO was asked to try to make contact both before the

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defendant's first appearance in court and after the defendant's first appearance in court (among cases where the no-contact restrictions remained active after first appearance); ideally, these contacts would have occurred on a schedule printed out by the computer database program used to enroll the cases.

The DO was also asked to enter information into the enrollment database program to document contacts with both treatment and control cases (which was linked to the enrollment database). Thus, for each contact, we can identify cases receiving contact efforts, cases where the contact effort was successful, and the type of contact (i.e., personal contact, contact at court, or telephone contact). We do not have any way of knowing systematically whether victims actually received and read the letters and brochures (after we began this procedure in June 2006). However, we are confident that letters were sent to all randomized treatment cases because those letters were mailed directly from our research office at the university.

The overall goal of the experiment was to ensure that the level of contact between the LCSD and victims was elevated during the period the no-contact order was in place and to ensure that victims in the treatment group had a meaningfully higher probability of being contacted and provided information about the no-contact orders, understanding those orders, and could assist the DO in detecting noncompliance. Since victims ultimately had the choice to cooperate with law enforcement in detecting violations of the no-contact order, the intervention could not be expected to directly increase the likelihood of detecting violations. However, if victims in the treatment and control groups were equally willing to cooperate and

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attempts by offenders to violate no-contact orders were equivalent between the two groups, the provision of information to victims about exactly what the order required and efforts by the police to monitor noncompliance in the treatment group could be expected to produce higher rates of contempt for bond violations for offenders in the treatment group. A prerequisite to the existence of such an effect would be a meaningful difference in the actual contact experiences for the two groups.

The contact database was maintained by the DO throughout the study. Each record in the database represents a documented attempt to contact a victim. To analyze this database, we aggregated the data to the date level. So, the database contained a separate record for each unique date that contact was attempted for each individual. Thus, on some dates there could be multiple efforts at contact (by both the victim and the DO). We then aggregated this database up to the level of each victim to construct Table 2.

Table 2 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). The information in Table 2 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.

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The information in Table 2 also indicates that establishing law enforcement contact with this population (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. In addition to proactively contacting victims for our study, the DO had a variety of responsibilities related to domestic violence cases under LCSD jurisdiction. DO efforts were contingent on the quality of victim contact information, the status of the case at the time or receiving paperwork from the CDVC administrator, and the on-duty time available to make contacts. Depending on these factors, the DO may or may not have actually contacted or attempted to contact victims in the treatment group. But, we think the conditions encountered by the DOs involved in this study are probably quite 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 and contact prior to first appearance was achieved in 25.9% of the cases.[2] Most contact occurred either by telephone (45.2% of the

[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 don't have the dates the CDVC administrator forwarded information to the DO) and 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 for us 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. But the treatment group described in this report is

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treatment cases) or in a courtroom setting (40.5%). In addition, the vast majority of contact that occurred was initiated by the DO. In fact, relatively little contact was initiated by the victim. Also, there was very little effort to contact victims in the control group (3.1% of the cases).

This intervention is best viewed as intention and effort by the DO to contact and communicate with domestic violence victims whose offenders were under no-contact orders as a condition of their release. Sometimes, the effort to contact was successful and sometimes it was not. But, it is worth reminding readers that victims were under no legal obligation to cooperate with or even make themselves available to law enforcement for the purpose of this intervention. Based on our own experiences in interviewing victims for this study, we are not surprised that efforts to contact victims were often unsuccessful. In our view, this difficulty will need to be taken into consideration whenever efforts to plan 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. Thus, on average, the treatment and control groups had different contact experiences with the DO.

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broadly construed to include everyone who was randomly assigned to treatment. Thus, 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.

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Gateway Arrest Dispositions

Since no-contact orders typically remain active until a disposition is reached in the case, one of the issues we wanted to explore was whether case disposition patterns would vary in important ways between offenders in the treatment and control groups. To examine this issue, we compared the treatment and control groups on several different disposition-related outcomes. Table 3 presents the results of these comparisons.

The first comparison reported in Table 3 examines the length of time offenders spent in jail prior to being released on bond. This is really a pretreatment characteristic since cases cannot be assigned to the treatment or control groups until after they are released on bond. Thus, it would be unusual if the groups experienced different lengths of time in jail. In fact, for both groups, the average amount of time spent in jail was similar (1.63 days for offenders in the treatment group and 1.68 days for offenders in the control group); 495 of the 517 offenders in our study spent seven days or less in jail prior to being released on bond. Overall, then, it appears quite typical for offenders in our study to be released on bond fairly quickly and this tendency was evident for offenders in both the treatment and control groups.

Next, we examined the mean number of days between the gateway arrest and the offenders' first appearance date. The first appearance date is a critical time because that date represents the offender's opportunity to enter a guilty or no-contest plea (both of which result in a conviction) or request a bench trial or jury trial. Under some circumstances, at first appearance, the prosecutor will offer the

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offender an opportunity to enter a Pre-Trial Intervention (PTI) program.

The PTI program is only available to a comparatively small group of offenders who meet the eligibility requirements for the program. Eligibility requirements include never previously being convicted of a felony, no prior CDV convictions, consent of the prosecutor, payment of fees, and are not currently on probation. Approximately 10% of the cases that are processed in the CDVC are referred to PTI. If an offender successfully completes PTI program requirements (i.e., session attendance for 26 weeks, satisfactory program participation, payment of program fees, no new criminal activities, passing drug tests, and keeping program administrators updated with a current address and telephone number), then the prosecutor drops the charges against the offender. Offenders also have the option to have their record expunged at the successful conclusion of their PTI program by paying appropriate fees and filing appropriate paperwork with the court and solicitor's offices. Overall, 85 of the 517 offenders (16.4%) in our study had the opportunity to participate in one of the PTI programs.

Oversight of this programming and the custody of records documenting the progress being made or not made by each offender in the treatment program are managed by ODP. Thus, the first appearance date is a key point in the processing of CDV1 cases in Lexington County. The analysis of time to first appearance indicates that on average, about 30 days passed between the gateway arrest date and the first appearance date for offenders in both the treatment and control groups.

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Most of the cases in our study were disposed before July 1, 2008 but some were not. Table 3 indicates that 81.4% of the treatment group cases had a disposition (193 cases out of 237 cases) by the end of June while 79.5% of the control group cases had been disposed (182 cases out of 229 cases) by the end of June. Therefore, the rate at which cases were disposed appears to be about the same for both groups. Among the cases that were disposed, we calculated the number of days between the gateway arrest date and the final disposition date. Overall, this average length of time to disposition was just over 180 days and the difference between the groups (186.5 days for offenders in the treatment group and 176.5 days for offenders in the control group) was small.

In terms of the actual disposition of the cases, there does not appear to be substantive or important differences between the groups. In both the treatment and control groups, 59% of the cases were disposed with a conviction while less than 10% were disposed with an acquittal. For both groups, the prosecutor declined to prosecute (nolle pros) about 15% of the cases.

Table 3 compares the dispositions of these cases between the treatment and control groups. The comparisons reveal no important differences in the disposition of these cases. For both groups, approximately 55% of the offenders who had the opportunity to participate actually completed their treatment program successfully. Another similarity between offenders in the two groups is that approximately 20% of the offenders who participated were terminated from the program before completing. Overall, about 5% of the offenders who were given the opportunity to participate were not able

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to enroll successfully while the remaining 20% of the cases were still pending at the time the official records were searched in June 2008.

In sum, the analyses of processing time, disposition of the case, and pretrial diversion programming presented in Table 3 provide us with no indication that the offenders in the treatment group were processed or had their cases disposed any differently than the offenders in the treatment group. In fact, all of the comparisons suggest that the processing patterns for the two groups were quite similar to each other.

Official Record Outcome Analysis

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. Our first set of comparisons is based on the assumption that the random assignment to treatment and control groups is sufficient to ensure comparability. An important issue that arises in any recidivism study is the length of the follow-up period. Since individual cases entered 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 in our sample. A key issue, then, is whether the length of the follow-up period was about the same for the two groups. Based on the first line of Table 4, the answer to this question appears to be "yes." In other words, the length in the variation in follow-up time that was used to assess offender

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recidivism was comparable for offenders in both the treatment and control groups.

The next comparison in Table 4 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 the 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 (i.e., crimes where there is a logical victim) against the same victim. We were only able to identify crime victims for LCSD arrests.[3] For each of these outcomes the rearrest rate for offenders in the treatment group is slightly lower than the re-arrest rate for offenders in the control group. But the differences are not large and they are not statistically significant at a two-tailed $p < .05$ significance level.

[3] 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.

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Most of the crime-specific differences between recidivism rates for the treatment and control groups presented in Table 4 are very small. None of them are statistically significant (two-tailed $p < .05$ significance level). Still, two of the differences in particular seem 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, 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 no-contact order violations that resulted in an arrest.[4] In fact, the treatment group does have a higher rearrest rate for contempt but the difference is not large and it is not statistically significant.

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 with substantive interest. 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. While this pattern of results is certainly consistent with a beneficial treatment effect, the differences are relatively small and, as discussed previously, there is ambiguity about the comparability of the groups.

[4] Some no-contact order 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.

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To address this ambiguity, we estimated a set of logistic regression equations reported in Table 5. These regression equations were estimated among the randomized treatment and control cases and were designed to adjust for the differences between the treatment and control groups noted in Table 1. In each of these regressions, we included an indicator variable for the randomized treatment condition, a count of the number of children reported by the offender at the jail booking, an indicator variable for whether the offender was born in South Carolina, an indicator variable for whether the offender had ever been previously arrested (prior to the gateway arrest) for domestic violence and an indicator variable for whether the offender had ever been previously arrested (prior to the gateway arrest) for a traffic offense. Some of these models could not be estimated because of very low recidivism rates for some offense categories. But the models could be estimated for contempt, domestic violence, and any subsequent violent and property offending. Although the direction of the effect continues to support the idea that treatment effects are beneficial none of the treatment effect estimates are statistically significant at the two-tailed $p < .05$ significance level.

Our overall conclusion from the official record analysis is that the treatment condition may yield some beneficial results in terms of reductions in arrests for subsequent domestic violence and other predatory offenses but these reductions are relatively small and not statistically significant.

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Overview of Victim Interview Analysis

To complement the official record analysis, we attempted to conduct two interviews with all female victims of the offenders who were enrolled in our study. In other words, victims of all cases being prosecuted in the CDVC whose case was under a no-contact order were targeted for two interviews, regardless of their offender's group membership. The initial or Time 1 interview was targeted for six weeks after the gateway arrest while the follow-up or Time 2 interview was targeted for six months after the gateway arrest. Beginning with cases enrolled in the spring of 2007, Time 2 interviews were conducted with victims whose offenders were in both the treatment and control groups on an accelerated basis. The goal of the victim interviews was to measure victim demographic characteristics, living circumstances, life experiences, interactions with the offender, interactions with the LCSD and the CDVC, an understanding of the no-contact order, safety, and well-being.

One of our principal concerns was whether interview response rates varied for treatment and control cases. Table 6 presents an overview of our interview response rates for the Time 1 interview, the Time 2 interview, and the Time 1 and Time 2 interviews combined. Three individuals completed a diversionary Time 1 interview followed by a normal Time 2 interview. These individuals are counted as responders for the Time 2 interview but are treated as missing for the Time 1 interview. So, while there are 100 completed Time 2 interviews there are only 97 combined sets of Time 1-Time 2 interviews.[5]

[5] The two co-principal investigators on this project received funds from another source to conduct additional interviews of these victims in the

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Among victims with whom we were able to establish contact for Time 1 recruitment, 28 actually refused to participate in our study. An additional 10 cases were not recruited because they could not speak English. In 48 cases, we successfully scheduled a Time 1 interview but the victim did not show up to complete the interview (in some instances, multiple appointments were missed). We consider these victims to be "passive refusers." Combined with the 141 female victims we were able to interview, this leaves a total of 210 cases where no contact with the victim was made. For Time 2 recruitment, no effort was made to contact victims unless they had successfully completed a Time 1 interview. There were no overt refusals to participate during Time 2 interview recruitment. One victim did refuse to participate in a Time 2 interview but she expressed her refusal at the conclusion of the Time 1 interview.

The results in Table 6 indicate that victim interview response rates were slightly higher for treatment cases than for victims whose offender was in the control group. But the differences between the groups were relatively small and no differences were statistically significant. Response rates for the interim control group were substantially lower than those for the other two groups, and we therefore do not include this group in our interview outcome analyses.

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summer of 2007 while the field phase of the study was wrapping up. The goal of this effort was to create a database of pilot interviews which could be used as the basis for requesting future funding to continue following the victims in our study prospectively. Some victims who completed a Time 1 interview and did not complete a Time 2 interview did receive a pilot interview. But we note that this only occurred when the length of time between the Time 1 interview and the pilot interview would have been too short for meaningful follow-up between the two interviews. For example, in most of these instances a Time 1 interview was followed by a pilot interview within two-three weeks. We considered using these pilot interviews as Time 2 interviews but rejected this possibility because of the short time period and differences in the format and objectives of the interviews.

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The lower quality of victim contact information received during the interim period had an adverse effect on our ability to contact these victims of offenders who were arrested for CDV during this time period. Even with the randomized groups, our response rate was lower than we would have liked. Nevertheless, given our avoidance of the court as a forum to contact victims combined with the difficulties of locating individuals who primarily use unlisted cell phones to communicate and who are in comparatively less stable living situations because they move around quite often for safety reasons, we think we are fortunate to have achieved the response rates we actually attained.

The interviews were extensive and covered a wide range of issues with each victim - more issues than we could examine within the framework of a single report. Still, we have conducted a large number of analyses with the victim interview data which we will briefly outline here. First, we examine the differences between the treatment and control groups on pretreatment characteristics for the cases that were actually interviewed. Although treatment group victims and control group victims were quite comparable to each other as a whole, we need to determine whether this relatively high degree of comparability applies to the subset of individuals that were interviewed, out of the total population of all victims that could have been interviewed.

Second, we conduct analyses where victims in the treatment and control groups are compared on outcomes measured by the interviews. These analyses parallel those of the official record study insofar as we conduct direct group comparisons and we then estimate regression

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models where variables whose levels appear to differ between the groups are held constant.

Third, we conduct a detailed set of analyses where we examine differences between victims who were interviewed and victims who were not on characteristics that are observed for everyone. A study of these differences allows us to determine how victims that were not able to be contacted (the missing cases) might be different in important ways from victims that we were able to successfully interview. Based on these analyses, we construct weights that can be applied to the observed cases. The goal of the weighting exercise is to estimate treatment effects that generalize to the entire population of interest - not just the individuals who were interviewed.

Last, we conduct an analysis with the weighted sample of victims who completed interviews where we compare the treatment and control groups on pretreatment characteristics and interview outcomes. The weighted interview outcome analysis involves both direct comparisons between the treatment and control groups and weighted regressions which include adjustments for differences between the groups on pretreatment characteristics.

Pretreatment Equivalence of Interviewed Cases

The randomized treatment and control cases appear to be quite similar to each other on many pretreatment characteristics. Moreover, the randomized treatment and control cases appear to have similar response rates. It does not follow, however, that the interviewed treatment and control groups are similar to each other on pretreatment characteristics. The analysis reported in this section attempts to

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establish the equivalence or comparability of the treatment and control victims who were actually interviewed.

Table 7 presents a comparison of the treatment and control victims who completed a Time 1 interview on a wide range of pretreatment characteristics measured in the official records. Although there are many similarities between the groups, two differences are noteworthy. First, control group victims were more likely to have been born in South Carolina (71.0%) than treatment victims (52.6%) and treatment group victims were more likely to have been arrested for an assault (36.8%) compared to control victims (17.7%) Table 8 presents additional comparisons based on pretreatment characteristics measured in the Time 1 victim interviews. Again, while there are many similarities there also appear to be two substantively important differences. First, the control group had a higher prevalence of black victims (25.8%) than the treatment group (13.2%). Additionally, the treatment group victims were more likely to report that the offender was an ex-boyfriend at the time of the gateway incident (10.7%) than the control group victims (3.2%).

Tables 9 and 10 present a similar analysis for the Time 2 interview data. The official record analysis presented in Table 9 indicates substantively important differences between treatment and control groups on the following characteristics: (1) offender born in South Carolina (79.6% in control group vs. 58.5% in the treatment group); (2) any prior assault charges (35.9% in the treatment group vs. 20.5% in the control group); (3) contempt of court (9.1% in the control group vs. 1.9% in the treatment group); (4) any prior charges for traffic offenses (47.7% in the control group vs. 26.4% in the

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treatment group); and (5) number of days between the gateway arrest and the date of the Time 1 interview (mean of 53.98 days for the treatment group vs. 44.37 days for the control group). None of the comparisons reported in Table 10 point to large differences between the groups.

We next compared treatment and control group victims who completed both a Time 1 and a Time 2 interview on pretreatment characteristics measured in the official records and on the Time 1 interview. The results are presented in Tables 11 and 12.[6] With the exception of three victims (one in the randomized treatment group, one in the randomized control group, and one in the interim control group), who completed a Time 2 interview but not a Time 1 interview, this sample provides the same comparisons as the tests for equivalence among victims who completed a Time 2 interview. Thus, the differences identified for Time 2 interviewees are identical to those identified for those who completed both Time 1 and Time 2 interviews.

Overall, the comparisons presented in this section indicate that the randomized treatment and control victims who were interviewed are quite comparable to each other. But there are some differences between the groups that will require adjustment when we examine the effects of treatment on interview outcomes.

[6] Table 12 duplicates Table 10 because the analysis is based on information provided in the Time 1 interview. So, the only individuals who can contribute to the analysis in Table 10 are individuals who completed both the Time 1 and Time 2 interviews.

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Unweighted Interview Outcomes

We turn now to a comparison of the randomized treatment and control group outcomes, as reported by victims. The comparisons are organized as follows. First, we present direct comparisons of outcomes measured at the Time 1 interview. Then, we present regression-adjusted estimates of the effects of treatment on the Time 1 interview outcomes. The regression models include estimates for the effects of treatment and other characteristics that distinguished between treatment and control cases among those who completed a Time 1 interview. We then conduct a similar analysis for victims who completed a Time 2 interview. Finally, similar models are estimated for outcomes combined across both interviews.

Although there are many potential outcomes, we focused on several that relate most directly to the targets of the intervention: contact with the offender, knowledge about the no-contact order, contact with the LCSD, and safety. At the Time 1 interview, the questions focused on the time period between the gateway arrest and the 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 contacted by a LEVA

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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 kinds of abuse. These questions were combined into logical groupings of psychological aggression, physical aggression, sexual coercion, injury, and stalking/threatening behavior. 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 added up the number of different kind of behaviors each victim experienced within each group. 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.

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.

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)

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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.

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.

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.

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.

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Table 13 presents the direct treatment-control comparisons of outcomes measured at the Time 1 interview. Unlike our official record analysis, some of the differences in this table are statistically significant (two-tailed, $p < .05$ significance level). Victims in the treatment group were more likely to report being divorced at the Time 1 interview (44.7%) than control group victims (26.2%). In addition, victims in the treatment group were significantly more likely to report having been contacted by a LCSD deputy (52.6%) than victims in the control group (35.5%). Treatment group victims also reported significantly higher scores on the physical aggression variety scale (count of the number of different types of physical aggression experienced since the gateway arrest) but this difference is based on very small numbers since 9.3% of treatment group victims and 3.2% of control group victims reported experiencing any physical aggression at all. Victims in the treatment group also 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.

Because the groups do exhibit some important differences, we conducted a second analysis of the Time 1 interview data where we regressed the outcomes on the treatment indicator variable and the four major pretreatment differences identified in Tables 7 and 8 (born in South Carolina, prior assault charges, victim's race is black, and an indicator for whether the offender was the victim's ex-boyfriend at the time of the gateway incident). The results of these regressions are presented in Table 14. Because of sparse data for some

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combinations of these characteristics, some of the models would not converge with all five predictor variables. When this occurred, the variable causing the problem was removed and the model was re-estimated. The results of these regressions indicate that all but one of the treatment effects are statistically non-significant at the two-tailed $p < .05$ significance level after adjusting for the influence of the treatment-control group differences. The one exception occurs with the estimated effect of treatment on the physical aggression variety scale. But, as noted above, this result is based on a scale where there is very little variation (since the overwhelming majority of victims in both groups reported no physical aggression between the gateway arrest and the Time 1 interview).

Next, we turn to the Time 2 interview results. Table 15 presents direct comparisons of the outcomes between the two groups and reveals that the only statistically significant differences involve reports of stalking or threatening behavior by the offender. In both comparisons, victims in the treatment group report higher levels of stalking/threatening behavior than victims in the control group. As noted previously, there were several important pretreatment differences between the treatment and control group victims who completed a Time 2 interview. These differences included: (1) whether the offender was born in South Carolina; whether the offender had prior arrests for (2) assault charges, (3) contempt charges, or (4) traffic charges; and (5) the length of time between the gateway arrest and the Time 1 interview. Table 16 presents regression-adjusted treatment effect estimates where predictor variables measuring these differences were included in the models. After adjusting for these

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differences, none of the treatment effect estimates are statistically significant.

Our final unweighted interview comparison examines treatment and control group differences on outcomes combining information from the Time 1 and Time 2 interviews. According to Table 17, the only statistically significant difference in outcomes between the two groups is the percentage of victims who report having been contacted by a LEVA. Treatment group victims reported contact with a LEVA in 73.1% of the cases compared to a lower rate of 51.1% in the control group. Differences in contact between a LCSD deputy sheriff and the victim were sizable (55.8% in the treatment group compared to 39.5% in the control group) but not statistically significant. The differences between the groups in rates of subsequent abuse and victimization experienced by victims in the treatment and control groups were all substantially smaller.

Table 18 presents the regression-adjusted estimates of treatment effects. These regressions actually adjust for the same pretreatment differences noted for the Time 2 interviews. Except for the apparent effect of the treatment on LEVA contact, none of the regression-adjusted treatment effects were statistically significant.

Summary of the Unweighted Interview Analysis

Overall, our conclusion from the unweighted interview data is that there are some potentially interesting outcome differences in the direct comparisons of the treatment and control groups. Interesting differences in the Time 1 interview data include contacts with the LCSD, psychological abuse, physical abuse, and stalking. In the Time

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2 interview data, the direct comparisons suggested important differences between victim experiences in the treatment and control groups in stalking victimization. In the combined analysis, the only statistically significant direct comparison involved LEVA contact. However, since there are also some pretreatment differences between the groups these direct outcome comparisons are somewhat ambiguous. To address this ambiguity, we estimated regression models to adjust the treatment effect estimates for the effects of these pre-existing differences.

In the Time 1 interview data, only the effect of treatment on the physical abuse variety scale remained statistically significant. But this finding is of questionable significance since less than 10% of both groups experienced any physical aggression during the time period between the gateway arrest and the Time 1 interview. Turning to the Time 2 interview data, the only statistically significant difference found between victim experiences in the treatment and control groups was for stalking behaviors. But after adjusting for pretreatment differences between the groups, there were no statistically significant treatment effect estimates for any of the Time 2 outcomes.

For the analysis of the combined interviews, both the direct comparisons and the regression-adjusted treatment effect estimates indicated that victims in the treatment group were more likely to have experienced contact with a LEVA. But none of the remaining differences between victim experiences in the treatment and control groups were statistically significant.

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Differences between Interviewed and Non-Interviewed Cases

Our assessment of the victim interview data thus far depends on the assumption that the interviewed and non-interviewed cases do not differ from each other in important ways that are relevant to our research question. Because this is a strong (and probably unrealistic) assumption to make, we now turn to a survey of potential differences between the observed and missing cases. Because the missing cases do not contribute any information to the victim interview data, our assessment focuses on differences between the observed and missing cases on various items from the administrative offender data which are observed for all cases. This assessment is an important foundation upon which to estimate sampling weights and is used to make adjustments for nonresponse. We also note that the problems we face with nonresponse in this study are commonly encountered in domestic violence research that involves interviews with victims. We hope the methods used here will also be useful for other researchers facing similar problems.

Table 19 presents a comparison of the missing and observed Time 1 interview cases on pretreatment offender characteristics in the administrative data. The analysis suggests some differences in racial composition between the two groups. Specifically, the missing cases tend to have more offenders in the "other" race category and more offenders of Hispanic ethnicity, compared to the race of the offenders of victims who were interviewed. Additionally, the offenders in the observed cases (victims' offenders who were interviewed) tend to have more years of education and spent a little more time in jail on average prior to release compared to the offenders of victims who were

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not interviewed. The comparisons also indicate a higher rate of prior contempt charges among observed cases (offenders victims who were interviewed) while there is a higher rate of prior traffic offense charges for the missing cases (offenders of victims who were not interviewed). There is an apparent difference in the prevalence of prior kidnapping charges between the two groups (offenders of victims who were interviewed compared to those who were not) but the rates for both groups were very low.

In Table 20, we consider differences between the observed and missing cases on offender case dispositions and outcomes found in the official record data. This analysis indicates that guilty verdicts are more prevalent among the observed cases (offenders of victims who were interviewed) while not guilty verdicts are more prevalent among the missing cases (offenders of the victims who were not interviewed). In terms of outcomes in the official record data, a noteworthy difference is the higher rate of property offenses among missing cases (offenders of victims who were not interviewed).

We now turn to a comparison of the Time 2 interview participants and missing Time 2 cases. Table 21 reveals race and ethnicity differences similar to those noted at the Time 1 interviews (race and ethnicity of interviewed victims' offenders compared to the race and ethnicity of victims' offenders who were not interviewed). The offenders in the observed cases also tended to have higher education levels (also noted at the Time 1 interview). Offenders in observed cases (offenders of victims who were interviewed) were significantly more likely to have been born in South Carolina and to have been previously arrested by the LCSD compared to offenders of victims who

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were not interviewed. Additionally, the offenders in the observed cases (cases involving an interview) tended to be followed up about 45 days longer on average than the offenders in the missing cases (cases not involving an interview). Finally, the offenders in the missing cases (offenders of victims who were not interviewed) were more likely to have pre-gateway arrests for theft than offenders in the observed cases (offenders of victims who were interviewed). Table 22 compares the observed (interviewed) and missing (non-interviewed) cases on the administrative data outcomes. This analysis suggests a higher rate of guilty verdicts in the observed cases (cases involving an interview) and a slightly higher rate of post-gateway contempt charges for failure to appear or failure to pay fines in the observed group (cases involving an interview). We note, however, that both groups had very low rates of these types of contempt charges after the gateway arrest.

Tables 23 and 24 indicate that our comparisons of cases where the victim was interviewed at both Time 1 and Time 2 versus cases where the victim had a missing interview at either Time 1 or Time 2 yield the same substantive results as the Time 2 only comparison. Since the vast majority of individuals who completed a Time 2 survey also completed a Time 1 survey this is not particularly surprising.

Overall, the results presented in Tables 19-24 suggest that the observed and missing cases are, in fact, comparable in many respects. In other words, domestic violence cases where we were able to interview victims were similar to domestic violence cases where we were not able to interview victims. But there are some important differences between the groups in terms of race and ethnicity, offenders' years of education, whether the offender was born in South

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Carolina, case dispositions, and arrests for certain types of offending activity. Our next set of analyses will make an effort to adjust for these differences.

Nonresponse Weights

Based on the results presented in the last section, we estimated nonresponse weights to be used in our interview analyses. The procedure we used is discussed by Ridgeway and his colleagues (2006:16-19) and implemented in the R Package 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 - which is observed for all cases. We use the variables listed in Tables 25-27 to estimate these models.[7]

Based on the statistical model, each case is assigned an estimated probability of nonresponse which we use to create the first-round weights. For each non-respondent the first-round weight is equal to 1. For each respondent the first-round weight is equal to the estimated odds of non-response (i.e., the estimated probability of

[7] The statistical model used to estimate the probabilities of nonresponse is included in the TWANG package and is called generalized booted 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 which we implemented with the following parameters: stop.methods[c("es.stat.mean")], n.trees = 3000, interaction.depth = 4, shrinkage = 0.01. We assessed comparability between the first-round weighted responders and nonresponders 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.

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nonresponse divided by 1 minus the estimated probability of nonresponse). The second step of the analysis is to compare the weighted responders to the nonresponders on all of the characteristics discussed in Tables 25-27 to determine the extent to which the model helps us improve the comparability of the responders and nonresponders.

While the first-round weights are useful for comparing the responders to the non-responders, the goal of our analysis is to estimate the treatment effect for the population as a whole - both the responders and the nonresponders. To accomplish this objective, Ridgeway and colleagues (2006:16) show that a sampling weight - which is the inverse of the probability of being observed - is the sum of two numbers: 1 and the odds of nonresponse. Since the odds of nonresponse are identical to the first-round weight, the final nonresponse sampling weight is equal to 1 + the first-round weight. Thus, the third step is to construct the final nonresponse weight by adding 1 to each of the first-round weights.

Tables 25-27 present the results of the first two steps and we then use the final nonresponse weights obtained from the third step to estimate treatment effects. In each of these tables, we present the unweighted mean/proportion for the observed cases, the first-round weighted mean/proportion for the observed cases and the mean/proportion for the missing cases. Standardized difference statistics are calculated before and after weighting to identify sources of potential imbalance (lack of comparability) between the

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responders and nonresponders.[8] All standardized statistics with an absolute value exceeding 0.15 are shaded to indicate potential imbalances.

Table 25 presents our assessment of the Time 1 interview first round weights. Prior to the construction of the first-round weights there are twelve imbalances (defined as absolute standardized difference exceeding 0.15). After applying the first-round weights, this number was cut to 7. If we use a 0.2 cutoff, the number of pre-weighted imbalances is 5 and the number of post-weighted imbalances is 3. Considering the problem of Time 2 nonresponse (Table 26), we see 17 pre-weighted imbalances (using a 0.15 cutoff) and 12 post-weighted imbalances. If we adopt a 0.2 cutoff, there are 12 pre-weighted imbalances and 6 post-weighted imbalances. For the problem of combined Time 1-Time 2 nonresponse (Table 27) and using a 0.15 cutoff for the standardized difference statistic, there are 16 pre-weighted imbalances and 11 post-weighted imbalances. With a 0.2 cutoff, there are 10 pre-weighted imbalances and 5 post-weighted imbalances. In general, then, it appears that the weighting is helpful at reducing the number of imbalances resulting from nonresponse bias. In the next section we return to the issue of pretreatment equivalence after weighting for nonresponse. Then, we address the issue of estimating treatment effects on the outcomes after weighting for nonresponse.

[8] The standardized difference statistic is a fraction. The numerator is the difference between the two means being compared. The denominator is the standard deviation of the variable among the nonresponders. Generally standardized difference statistics exceeding +/-0.2 are considered evidence of imbalance or lack of comparability (Rosenbaum and Rubin 1985:36).

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Pretreatment Equivalence After Weighting for Nonresponse

In this section, we return to the issue of whether the randomized treatment and control group victims who were interviewed are comparable to each other on pretreatment characteristics after weighting for nonresponse (cases that did not involve an interview with a victim but should have). Table 28 presents a comparison of the treatment and control group cases among victims who completed a Time 1 interview with Time 1 interview nonresponse weights. Shaded lines in the table indicate potential differences between the groups based on chi-square statistics exceeding 2.25 or T-statistics exceeding 1.5. Some differences are apparent between the groups. Both victims and offenders in the control group were more likely to be black and control group offenders were more likely to have been born in South Carolina and arrested for contempt before the gateway incident. Offenders in the treatment group were more likely to have been arrested before the gateway incident for assault and driving while impaired while victims in the treatment group were more likely to report that the offender was their ex-boyfriend at the time of the incident. Finally, it appears that victims whose offenders were in the treatment group experienced a little over six days longer (on average) length of time between the gateway arrest and the Time 1 interview date than the victims whose offender was in the control group.

Table 29 presents treatment-control group comparisons for victims who completed a Time 2 interview after weighting for Time 2 nonresponse. According to this analysis, control group offenders were more likely to have been born in South Carolina and to have been arrested for alcohol and traffic offenses prior to the gateway arrest.

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Treatment group offenders, on the other hand, waited about nine days longer (on average) from the gateway arrest to the date of the first interview compared to offenders in the control group.

For the combined Time 1-Time 2 analysis, we compare treatment and control group cases among victims who completed both a Time 1 and a Time 2 interview after weighting for nonresponse. This analysis is presented in Table 30 and indicates that control group offenders were more likely to have experienced pre-gateway arrests for traffic offenses. Victim of offenders in the treatment group also experienced a longer waiting time between the gateway arrest date and the Time 1 interview.

Overall, after weighting for nonresponse, the treatment and control groups look very similar to each other on most pretreatment characteristics. Nevertheless, there are a few potentially important differences that require adjustmentin our weighted interview analyses.

Treatment Effects After Weighting for Nonresponse

In this section, we discuss our weighted interview outcome analyses. Beginning with the Time 1 interview outcomes, the analysis presented in Table 31 indicates that treatment group victims were more likely to report being divorced 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

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group also reported a higher average variety score for physical aggression but as in the unweighted analyses, very few victims reported experiencing any physical aggression by the Time 1 interview. We, therefore, put less emphasis on that result.

To adjust for potentially important pretreatment differences between the treatment and control groups, we estimated a series of regressions (reported in Table 32). 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 victim's ex-boyfriend at the time of the gateway arrest and the waiting time to the first interview. After adjusting for the effects of these potential confounding variables, only the stalking/threat measures and physical aggression variety score effects were statistically significant. Because of the small number of victims who reported any physical aggression at all, our conclusion from the Time 1 interview is that there appears to be a link between the intervention and stalking or threatening behaviors.

The Time 2 interview outcomes are summarized in Table 33 and the results indicate greater levels of stalking victimization (both prevalence and variety). Table 34 reports regression analyses after adjusting for the following potential confounders: offender is a South Carolina native, whether the offender had been arrested for alcohol or traffic offenses prior to the gateway arrest, and the waiting time 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

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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.

Table 35 presents direct comparisons of treatment and control group outcomes after combining information from the Time 1 and Time 2 interviews and weighting for nonresponse. In this analysis only the effect of treatment on victim reports of contact with a LEVA are statistically significant. After adjusting for potential confounders (Table 36), the treatment's effect on LEVA contacts is still statistically significant but none of the other effects are statistically significant.

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DISCUSSION AND CONCLUSIONS

This research project offered the opportunity to examine the impact of proactive enforcement of no-contact orders on offender behavior and victim safety and well-being in cases of misdemeanor domestic violence. Although no-contact orders are widely 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 victim's lack of knowledge of the presence and nature of such order, law enforcement's lax attitude towards the enforcement of such orders, prosecutors who are reluctant to prosecute offenders who breech such an order, 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 no-contact orders. Using an experimental design, our research focused on the effect of proactive enforcement of court-ordered no-contact orders, as opposed to civil orders that have been the focus in much of the previous research. The primary emphasis of the intervention was to establish contact between the Lexington County Sheriff's Department's Dedicated Officer and the victim for purposes of notifying the victim about the existence of the no-contact order, explaining the requirements of the no-contact order, instructing the victim what to do if a violation occurred, and monitoring compliance with and enforcing the no-contact order. We assessed the effectiveness of this proactive enforcement of no-contact orders via

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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 a modest 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 that placed the offenders in both groups in our study. Our overall conclusion is that the treatment condition may yield some beneficial results in terms of reductions in arrests for subsequent domestic violence and other predatory offenses but these reductions are relatively small and not statistically significant.

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 were apparent with respect to contacts with the Lexington County Sherriff's Department, two types of offender behavior, and the victim and offender's relationship status; however, 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 no-contact orders to those in the treatment group. We found other treatment differences for contacts by law enforcement victim advocates from the Lexington County Sheriff's Department. In our combined analysis of the Time 1 and Time 2 survey data, we found treatment victims were significantly

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more likely to report contacts with victim advocates. These contacts between treatment victims and victim advocates may be the result of their earlier contact with the dedicated officer and victims' heightened awareness of the resources and services available to domestic violence victims.

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 physical aggression 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 less than ten percent of treatment group victims and approximately three percent of control group victims having reported experiencing any physical aggression at all. Also, while this finding was evident for the Time 1 and Time 2 surveys individually, it was not statistically significant in the combined analysis of both surveys.

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 strongly in evidence in the individual Time 1 and Time 2 surveys;

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however, it is not statistically significant in the combined analysis of both interview databases. This finding is discussed below.

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

It is clear from our research that proactive enforcement as currently operationalized as enhanced contact between law enforcement and victims is not an effective means of increasing victim safety or reducing offender recidivism. While research limitations in implementing the treatment and contacting victims for interviews limits our ability to fully examine the effect of intensive enforcement, this is also an incredibly important finding. Our research findings helps to further elaborate and tells us about 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 battering. First, our research demonstrates the difficulty of implementing even a simple domestic violence intervention. The design and plan for the implementation of the treatment protocol was developed in consultation with the Sheriff's Office and the prosecutor from the domestic violence court. This two-stage intervention was believed to address the times when victims were most at risk for

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subsequent victimization and witness tampering. Yet, our report notes the difficulty of ensuring adequate implementation of even 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 dedicated officer. Future research will need to address these logistical concerns. 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. 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 women's decision-making, offering women information on the legal system and their rights, and provide them with options in dealing with a violent relationship.

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

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dedicated officer at the Lexington County Sheriff's Department, 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 in 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 dedicated officer were also geared toward providing women with information on domestic violence and no-contact orders, thus educating and empowering women to enforce such orders. Notably, victims in the treatment group were not only more likely to be aware 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 of the Lexington County victims would allow for an examination of whether changes in perceptions of batterer behavior might have translated to an increased probability that some of the women have ended their violent relationships and transformed their lives.

An alternative explanation for the stalking effect might be that the receipt of information from the Lexington County Sheriff's Department and the presence of the officer at the victim's home might have aroused suspicion in the offender and lead to changes in his behavior, including threatening and stalking behaviors.

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Non-response Problems

During our project it became apparent that conducting research on domestic violence victims has changed over the last 20 years. Today there exist a number of challenges in contacting victims and conducting interviews that did not exist in the past (or were not part of human subjects purview), such as the ease at which contacts with respondents occurred during the 1980's research on the efficacy of mandatory arrest. We experienced great difficulty in attempting to contact victims on the phone and via regular postal mail. First and foremost it is important to note that this population is a highly transient one. We received our contact information from victim information sheets 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 our research subjects 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. We did not contact the victims in court, which might have been an ideal location to initiate research contact.

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Research Limitations

Despite being based on an experimental design and examination of a variety of victim and offender outcomes, this study was not without its limitations. One specific limitation relates to the caseload. Prior to the dedicated officer and research staff entering the field, South Carolina's criminal domestic violence law changed and the change was implemented in January 2006. The consequence of the change in the law for our study site was that the Lexington County Criminal Domestic Violence Court now only saw first time misdemeanor domestic violence cases. Repeat offenders were now sent up to general sessions court. The prosecutor has 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 the dedicated officer part way through the study period and the ability of the officers to make contact with victims in the treatment group. During the transition to a new dedicated officer some of the cases that came into the court were automatically placed in the control condition. Additionally, as we note, the attempts and successful contacts with victims in the treatment group were disappointingly low. Yet, it is important to note that this change in staffing and successful contact with this population of victims reflects the reality of working with an agency and the realistic conditions under which an intervention would be implemented. We also note that the Lexington County Sheriff's Department implemented this treatment using their existing resources. The availability of grants to fund interventions of this nature could potentially lead to higher levels of treatment compliance. Thus, our

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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.

Third, both the dedicated officers and our research staff had difficulty in attempting to contact the women 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 as we had hoped. 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 no-contact orders 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 it was implemented within this project, our research does call for serious consideration and evaluation of what might actually constitute

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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 providers. Given the clear distinctions between Michael Johnson's common couple violence and intimate 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 other less intensive treatments. With respect to no-contact orders this would include an examination of the nature of contact between victims and their batterers. Contacts related to marital counseling, child care arrangements, and other family commitments are qualitatively different than stalking and violence threatening behaviors that maybe indicative of future violence.

Social and Criminal Justice Policy Implications

Overall our findings suggest very moderate effects of the treatment for women's well-being and batterer recidivism. While our stalking effects point to the potential benefits of empowering victims, the remaining findings suggest that relatively simple and inexpensive domestic violence interventions have modest effects. At the same time, proactive enforcement of no-contact orders does not appear to place victims at risk for increased or continued violence and abuse. It is therefore possible that there are other important reasons for enforcing these types of criminal justice orders. These orders are increasingly seen as an appropriate criminal justice

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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 serve to send the message that continued contacts with victims (in which orders are in place) is 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. Given that the enforcement of no-contact orders does not place women at greater risk, there may therefore be a larger benefit to continuing to be "hard on partner violence." Most importantly, it is important that the criminal justice system continue to work towards involving victims in the criminal justice process and protect their rights within that system. An early evaluation of the Lexington County Criminal Domestic Violence Court indicated that many victims were unaware of the presence of no-contact orders. It is therefore crucial that if these orders are going to continue to be part of the prosecution of domestic violence in South Carolina victims continue to be made aware of their presence and provided with information on how to enforce these orders to protect themselves and their families.

With respect to our stalking effects, there are a number of social and criminal justice implications. First, we point to the continued need for domestic violence interventions that educate women and offer victims information on the criminal justice system and social services empower women in their decision-making. We believe it

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is important for domestic violence interventions to continue to provide women with information on the nature of intimate partner violence and the resources available to leave and end violent relationships. Through the provision of information on the presence and nature of no-contact orders (via proactive enforcement by a dedicated domestic violence officer or a victim advocate), victims have access to a tool that may be used to prevent their batterer from continuing contact, thereby potentially reducing the risk of re-victimization. Second, we suggest the continued need for criminal justice officials and legislators to examine the prevalence and nature of stalking of victims by current and previously intimate partners and develop early interventions and enforce orders that prohibit these behaviors by defendants facing domestic violence charges. Our findings suggest both a high prevalence of stalking behaviors by male partners and significant differences between the treatment and control group with respect to perceiving contact behaviors as stalking and potentially threatening. Tjaden and Thoennes (2000) research, a collaborative partnership with the Colorado Springs police department, found that although stalking is highly prevalent among misdemeanor and felony domestic violence cases, stalking rarely leads to independent charges for these offenses. More importantly stalking allegations were also more prevalent in reports involving victims and suspects who were former versus current intimates. Stalking and threatening behaviors in women's transitions out of violent relationships has implications for their health and well-being, physical safety, and decisions to testify against their batterer. Attending to these non-violent stalking and threatening behaviors through the use of criminal

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justice interventions might have implications for reducing the future risk of violent and potentially lethal behaviors by 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 the 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 for a variety of reasons and potentially because of the contacts between the dedicated officers and the women in the treatment group. Future research should explore the importance of advocates for victims experiencing domestic violence and look at how the presence and nature of contacts between victims and advocates shapes victim decision-making. Experimental and matched sample designs would be able to explore the impact of contacts with advocacy 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 problem of domestic violence through a multi-disciplinary collaboration of criminal justice and social service providers focusing on pro-arrest policies and procedures, case

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investigation and prosecution, and 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 indentify 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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Appendix B: Deputy Sheriff Letter and Victim Brochure

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Sheriff
James R. Metts, Ed. D.

LEXINGTON COUNTY SHERIFF'S DEPARTMENT

Dear :

You are listed on an incident report as the victim in a Criminal Domestic Violence case. The defendant in your case has been released on bond with a stipulation that he have no contact with you. Please find enclosed a brochure explaining no contact orders. If you have any problems or believe that the defendant has violated his bond, feel free to contact me at (803)785-2490 or contact the Sheriff's Department at (803)785-8230 to file an incident report.

Sincerely,

Irick A. Geary Jr.
Criminal Domestic Violence Unit

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What is a bench warrant?

A bench warrant is an order issued by a judge to have someone arrested and brought before the court.

What is contempt of court? What type of punishment does this result in?

Contempt of court is a finding by a judge that someone has violated an order of the court or engaged in other misconduct involving the court. **Punishment can be jail time or a fine.**

What type of evidence is needed to pursue a bench warrant?

Evidence needed to pursue a bench warrant varies by case. Possible types of evidence include handwritten notes left by the defendant, copies of e-mails, telephone records, recordings of telephone calls or voice mail messages, caller ID records, and witness statements.

If you feel you are in danger, call 911 immediately!

LEXINGTON COUNTY
SHERRIF'S DEPARTMENT

521 Gibson Road
Lexington, SC 29072
Phone: 803-785-8230

LEXINGTON COUNTY SHERRIF'S DEPARTMENT

What you need to know about bonds and bond restrictions

LEXINGTON COUNTY
SHERIFF'S
DEPARTMENT

803-785-8230

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Appendix C: Tables 1-36

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Table 1. Pretreatment Characteristics in Official Records Files Related to the Offenders

Background Characteristic	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Male Offender, Female Victim (d)	237	229	51	0.8397	0.8253	0.9608	0.4144
Offender Race = White (d)	237	227	51	0.7173	0.6828	0.8039	0.8105
Offender Race = Black (d)	237	227	51	0.2152	0.2643	0.1176	-1.2400
Offender Race = Other (d)	237	227	51	0.0675	0.0529	0.0784	0.6623
Offender Ethnicity = Hispanic (d)	231	221	49	0.0563	0.0724	0.1429	-0.6992
Offender Married (d)	232	224	50	0.3621	0.4063	0.3600	-0.9699
Number of Children Reported by Offender at Booking (c)	237	227	51	1.6624	1.8899	1.8824	-1.5829
Offender Reports Any Children at Booking (d)	237	227	51	0.7426	0.7665	0.7843	-0.5979
Offender Reports Any Past Military Service (d)	236	227	50	0.0890	0.0749	0.1800	0.5523
Number of Charges Booked at Gateway Arrest (c)	237	227	51	1.1941	1.3833	1.2549	-1.0263
Offender's Age at Time of Booking (c)	237	227	51	34.8397	34.9868	34.5490	-0.1470
Years of Education Reported by Offender at Booking (c)	231	221	49	11.9957	11.8597	11.2857	0.6692
Days in Jail Between Gateway Arrest and Bond Release (c)	237	227	51	1.6329	1.6784	1.5882	-0.1549
Offender Reports Being Born in South Carolina (d)	237	227	51	0.5781	0.6476	0.7255	-1.5362
Offender Booked on Domestic Violence Charge (d)	237	227	51	0.9916	0.9956	1.0000	-0.5419
Offender Booked on Non-Domestic Violence Charge (d)	237	227	51	0.1266	0.1322	0.1961	-0.1789
Age at First Arrest (Including Current Arrest if No Prior Arrests) (c)	237	227	51	24.3882	24.3744	24.9020	0.0162
Any Prior Arrest Record (Before Gateway Arrest) (d)	237	229	51	0.7595	0.7991	0.6863	-1.0308
Number of Prior Arrests (Not Including Zeros) (c)	180	183	35	5.6889	5.1530	5.3429	0.9794
Number of Prior Arrests (Including Zeros) (c)	237	229	51	4.3209	4.1179	3.6667	0.4285
Number of Days Since Last Arrest (Among Those With Priors) (c)	180	183	35	1780.8333	1751.8142	2120.6571	0.1417
Any Prior Arrest Record in South Carolina (d)	237	229	51	0.7173	0.7424	0.5490	-0.6089
Any Prior Arrest Record by Lexington County Sheriff's Department (d)	237	229	51	0.4599	0.4541	0.3725	0.1249
Both Parties Arrested at Gateway Arrest (Dual Arrest) (d)	237	229	51	0.0211	0.0218	0.0000	-0.0549
Number of Days Between Gateway Arrest and Record Search Date (c)	237	229	51	552.9283	540.7817	530.7451	0.7409
Any Prior Charges for Alcohol Violations (d)	237	229	51	0.2405	0.2402	0.1569	0.0084
Any Prior Charges for Assaults (d)	237	229	51	0.2700	0.2751	0.1961	-0.1228
Any Prior Charges for Burglary (d)	237	229	51	0.1181	0.0830	0.1373	1.2605
Any Prior Charges for Child Abuse (d)	237	229	51	0.0295	0.0218	0.0000	0.5248
Any Prior Charges for FTA/FTP/Attorney Contempt (d)	237	229	51	0.0084	0.0175	0.0196	-0.8643
Any Prior Charges for All Other Contempt (d)	237	229	51	0.0253	0.0393	0.0196	-0.8551
Any Prior Charges for Disorder Offenses (d)	237	229	51	0.2278	0.2533	0.2549	-0.6422
Any Prior Charges for Drug Offenses (d)	237	229	51	0.3460	0.3537	0.2941	-0.1747
Any Prior Charges for Domestic Violence (d)	237	229	51	0.1941	0.2620	0.1961	-1.7484
Any Prior Charges for Driving While Impaired (d)	237	229	51	0.2954	0.2620	0.2157	0.8025
Any Prior Charges for Fraud (d)	237	229	51	0.1941	0.1616	0.1961	0.9173
Any Prior Charges for Harassment (d)	237	229	51	0.0506	0.0611	0.0392	-0.4938
Any Prior Charges for Homicide (d)	237	229	51	0.0169	0.0131	0.0196	0.3351
Any Prior Charges for Kidnapping (d)	237	229	51	0.0084	0.0306	0.0000	-1.7353
Any Prior Charges for Motor Vehicle Theft (d)	237	229	51	0.0506	0.0655	0.0784	-0.6868
Any Prior Charges for Robbery (d)	237	229	51	0.0549	0.0655	0.0000	-0.4836
Any Prior Charges for Sex Offenses (d)	237	229	51	0.0506	0.0306	0.0588	1.0950
Any Prior Charges for Theft (d)	237	229	51	0.1814	0.1965	0.2157	-0.4156
Any Prior Charges for Traffic Offenses (d)	237	229	51	0.3460	0.4192	0.2745	-1.6264
Any Prior Charges for Vandalism (d)	237	229	51	0.0928	0.0742	0.0784	0.7245
Any Prior Charges for Weapons Violations (d)	237	229	51	0.1181	0.1092	0.1176	0.3050
Any Prior Charges for Probation/Parole Violations (d)	237	229	51	0.0338	0.0306	0.0196	0.1949
Any Prior Charges for Other Property Offenses (d)	237	229	51	0.1308	0.0917	0.1373	1.3401
Any Prior Charges for Other Offenses (d)	237	229	51	0.2025	0.1747	0.2549	0.7682
Any Prior Charges for Offenses With Missing Charges (d)	237	229	51	0.0169	0.0175	0.0196	-0.0490
Any Prior Charges for Violent Offenses (d)	237	229	51	0.4388	0.4760	0.3137	-0.8051
Any Prior Charges for Property Offenses (d)	237	229	51	0.4346	0.3930	0.3333	0.9111

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. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important pretreatment difference.

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Table 2. 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)	237	229	51	0.4873	0.0218	0.0000	11.4677
Any Success at Contacting Prior to First Appearance (d)	237	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.

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Table 3. Case Dispositions (for DV Charge Leading to Gateway Arrest)

Case Disposition Measure	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Days in Jail Between Gateway Arrest and Bond Release (c)	237	229	51	1.6329	1.6784	1.5882	-0.1549
Number of Days Between Gateway Arrest and First Appearance (c)	237	229	51	30.0000	29.8035	34.3725	0.1420
Days Between Gateway Arrest and Final Case Disposition (c)	193	182	44	186.5233	176.5275	212.4091	0.5651
Case Disposition = Guilty (d)	237	229	51	0.5907	0.5939	0.6667	-0.0696
Case Disposition = Not Guilty (d)	237	229	51	0.0675	0.0393	0.0784	1.3511
Case Disposition = Nolle Prosequi (d)	237	229	51	0.1561	0.1528	0.1176	0.0979
Case Disposition = Death (d)	237	229	51	0.0000	0.0087	0.0000	-1.4418
Case Disposition = Case Pending (d)	237	229	51	0.1857	0.2052	0.1373	-0.5332
Pre-Trial Intervention Program Status = Program Complete (d)	36	44	5	0.5556	0.5455	0.4000	0.0903
Pre-Trial Intervention Program Status = Terminated (d)	36	44	5	0.1944	0.2045	0.2000	-0.1124
Pre-Trial Intervention Program Status = Rejected (d)	36	44	5	0.0278	0.0682	0.0000	-0.8249
Pre-Trial Intervention Program Status = Pending (d)	36	44	5	0.2222	0.1818	0.4000	0.4495

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. None of the reported comparisons are statistically significant (two-tailed $p < .05$ significance level).

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Table 4. 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
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 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. None of the reported comparisons are statistically significant (two-tailed $p < .05$ significance level).

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Table 5. Logistic Regression Recidivism Models Adjusting For Pretreatment Imbalances (Official Record Outcomes)

Official Record Outcome	Treatment Coefficient	Odds Multiplier for Treatment	Treatment Coefficient Standard Error	Wald Chi-Square
Any Subsequent Arrest Record	0.0275	1.0279	0.1968	0.0195
Any Subsequent Arrests in South Carolina	-0.0131	0.9870	0.1981	0.0044
Any Subsequent Arrests by Lexington County Sheriff's Department	-0.1470	0.8633	0.2213	0.4410
Any Subsequent Arrests in LCSD For Crimes Against the Same Victim	-0.4498	0.6378	0.3333	1.8218
Any Subsequent Charges for Alcohol Violations	0.0847	1.0884	0.4343	0.0381
Any Subsequent Charges for Assaults	-0.7495	0.4726	0.7079	1.1211
Any Subsequent Charges for Burglary	-1.5176	0.2192	1.1087	1.8735
Any Subsequent Charges for Child Abuse	0.0087	1.0087	1.0223	0.0001
Any Subsequent Charges for FTA/FTP/Attorney Contempt	N/A			
Any Subsequent Charges for All Other Contempt	0.3799	1.4621	0.3253	1.3636
Any Subsequent Charges for Disorder Offenses	-0.2617	0.7697	0.5945	0.1939
Any Subsequent Charges for Drug Offenses	0.3811	1.4639	0.3627	1.1042
Any Subsequent Charges for Domestic Violence	-0.3416	0.7106	0.2956	1.3359
Any Subsequent Charges for Driving While Impaired	0.3770	1.4579	0.5793	0.4235
Any Subsequent Charges for Fraud	-0.4283	0.6516	0.5077	0.7119
Any Subsequent Charges for Harassment	N/A			
Any Subsequent Charges for Homicide	N/A			
Any Subsequent Charges for Kidnapping	-0.4026	0.6686	0.9337	0.1859
Any Subsequent Charges for Motor Vehicle Theft	N/A			
Any Subsequent Charges for Robbery	N/A			
Any Subsequent Charges for Sex Offenses	N/A			
Any Subsequent Charges for Theft	-0.3275	0.7207	0.5860	0.3123
Any Subsequent Charges for Traffic Offenses	-0.0163	0.9838	0.3292	0.0025
Any Subsequent Charges for Vandalism	N/A			
Any Subsequent Charges for Weapons Violations	N/A			
Any Subsequent Charges for Probation/Parole Violations	-0.0040	0.9960	0.5773	0.0000
Any Subsequent Charges for Other Property Offenses	N/A			
Any Subsequent Charges for Other Offenses	-0.1254	0.8821	0.5025	0.0623
Any Subsequent Charges for Offenses With Missing Charges	N/A			
Any Subsequent Charges for Violent Offenses	-0.3593	0.6982	0.2723	1.7407
Any Subsequent Charges for Property Offenses	-0.4750	0.6219	0.3405	1.9460

Note: The logistic regressions reported in this table include the following predictor variables: (1) indicator variable for treatment (coded 1 if case is randomized to treatment and 0 if the case is randomized to control); (2) a count of the number of children reported by the offender at the jail booking; (3) an indicator variable coded 1 if the offender is a South Carolina native and 0 otherwise; (4) an indicator variable coded 1 if the offender has any prior arrests for domestic violence and 0 otherwise; and (5) an indicator variable coded 1 if the offender has any prior arrests for traffic offenses and 0 otherwise. In some models, convergence could not be attained due to quasi-complete separation. When this occurred, the predictor variable causing the problem was dropped. Wald chi-square tests in this table only apply to the logistic regression coefficient for the treatment indicator variable in these models. The interim controls are not included in these tests. None of the reported comparisons are statistically significant (two-tailed $p < .05$ significance level).

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Table 6. Interview Response Rates

Interview Responses	Treatment Group	Control Group	Interim Control Group	Total
Total Number of Cases	237	229	51	517
Number of Male Offender, Female Victim Cases	199	189	49	437
Number of Victims Completing Time 1 Interview	76	62	3	141
Number of Victims Completing Time 2 Interview	53	44	3	100
Number of Victims Completing Both Interviews	52	43	2	97
Percentage of Victims Completing Time 1 Interview	38.19%	32.80%	6.12%	32.27%
Percentage of Victims Completing Time 2 Interview	26.63%	23.28%	6.12%	22.88%
Percentage of Victims Completing Both Interviews	26.13%	22.75%	4.08%	22.20%

Note: to compare response rates between the treatment and control groups, we calculated chi-square tests of independence. Each of these chi-square tests has one degree of freedom. The test statistic for Time 1 interview response rates was 1.2274, for Time 2 interview response rates the test statistic was 0.5811 and for the combined Time 1-Time 2 response rates the test statistic was 0.5987. None of these tests is statistically significant.

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Table 7. Official Record Background Characteristics For Cases Where a Time 1 Interview Was Completed

Background Characteristic	Treatment Group N	Control Group N	Treatment Group Mean	Control Group Mean	Z-Test
Male Offender, Female Victim (d)	76	62	1.0000	1.0000	N/A
Offender Race = White (d)	76	62	0.7368	0.6613	0.9661
Offender Race = Black (d)	76	62	0.2105	0.3226	-1.4911
Offender Race = Other (d)	76	62	0.0526	0.0161	1.1414
Offender Ethnicity = Hispanic (d)	75	61	0.0267	0.0164	0.4057
Offender Married (d)	75	61	0.4400	0.4262	0.1612
Number of Children Reported by Offender at Booking (c)	76	62	1.6447	2.0484	-1.4287
Offender Reports Any Children at Booking (d)	76	62	0.7368	0.7903	-0.7326
Offender Reports Any Past Military Service (d)	75	62	0.0800	0.1613	-1.4746
Number of Charges Booked at Gateway Arrest (c)	76	62	1.2500	1.8871	-0.9734
Offender's Age at Time of Booking (c)	76	62	34.8816	35.4516	-0.3065
Years of Education Reported by Offender at Booking (c)	75	61	12.0400	12.1639	-0.3442
Days in Jail Between Gateway Arrest and Bond Release (c)	76	62	2.2237	1.9677	0.3534
Offender Reports Being Born in South Carolina (d)	76	62	0.5263	0.7097	-2.1954
Offender Booked on Domestic Violence Charge (d)	76	62	0.9868	1.0000	N/A
Offender Booked on Non-Domestic Violence Charge (d)	76	62	0.1579	0.1290	0.4791
Age at First Arrest (Including Current Arrest if No Prior Arrests) (c)	76	62	23.8421	24.6452	-0.5092
Any Prior Arrest Record (Before Gateway Arrest) (d)	76	62	0.7895	0.8065	-0.2467
Number of Prior Arrests (Not Including Zeros) (c)	60	50	6.0833	5.3400	0.7511
Number of Prior Arrests (Including Zeros) (c)	76	62	4.8026	4.3065	0.5629
Number of Days Since Last Arrest (Among Those With Priors) (c)	60	50	1586.8000	1910.9800	0.9692
Any Prior Arrest Record in South Carolina (d)	76	62	0.7237	0.7258	-0.0278
Any Prior Arrest Record by Lexington County Sheriff's Department (d)	76	62	0.5263	0.4355	1.0620
Both Parties Arrested at Gateway Arrest (Dual Arrest) (d)	76	62	0.0132	0.0000	N/A
Number of Days Between Gateway Arrest and Record Search Date (c)	76	62	569.7895	534.5806	1.1916
Any Prior Charges for Alcohol Violations (d)	76	62	0.2368	0.2419	-0.0698
Any Prior Charges for Assaults (d)	76	62	0.3684	0.1774	2.4787
Any Prior Charges for Burglary (d)	76	62	0.1447	0.0645	1.5060
Any Prior Charges for Child Abuse (d)	76	62	0.0526	0.0000	N/A
Any Prior Charges for FTA/FTP/Attorney Contempt (d)	76	62	0.0132	0.0323	-0.7653
Any Prior Charges for All Other Contempt (d)	76	62	0.0395	0.0806	-1.0295
Any Prior Charges for Disorder Offenses (d)	76	62	0.3158	0.2258	1.1771
Any Prior Charges for Drug Offenses (d)	76	62	0.4211	0.4194	0.0201
Any Prior Charges for Domestic Violence (d)	76	62	0.2500	0.2581	-0.1083
Any Prior Charges for Driving While Impaired (d)	76	62	0.3289	0.2258	1.3385
Any Prior Charges for Fraud (d)	76	62	0.1974	0.1290	1.0715
Any Prior Charges for Harassment (d)	76	62	0.0789	0.0806	-0.0366
Any Prior Charges for Homicide (d)	76	62	0.0132	0.0000	N/A
Any Prior Charges for Kidnapping (d)	76	62	0.0000	0.0161	N/A
Any Prior Charges for Motor Vehicle Theft (d)	76	62	0.0789	0.0645	0.3253
Any Prior Charges for Robbery (d)	76	62	0.0658	0.0323	0.8929
Any Prior Charges for Sex Offenses (d)	76	62	0.0395	0.0323	0.2256
Any Prior Charges for Theft (d)	76	62	0.1579	0.1774	-0.3061
Any Prior Charges for Traffic Offenses (d)	76	62	0.3158	0.4194	-1.2590
Any Prior Charges for Vandalism (d)	76	62	0.1316	0.0806	0.9562
Any Prior Charges for Weapons Violations (d)	76	62	0.1184	0.1935	-1.2222
Any Prior Charges for Probation/Parole Violations (d)	76	62	0.0395	0.0484	-0.2554
Any Prior Charges for Other Property Offenses (d)	76	62	0.1711	0.1129	0.9652
Any Prior Charges for Other Offenses (d)	76	62	0.2368	0.1613	1.0978
Any Prior Charges for Offenses With Missing Charges (d)	76	62	0.0000	0.0323	N/A
Any Prior Charges for Violent Offenses (d)	76	62	0.5132	0.4032	1.2881
Any Prior Charges for Property Offenses (d)	76	62	0.5132	0.4032	1.2881
Number of Days Between Gateway Arrest and Time 1 Interview (c)	76	62	54.5000	49.2097	1.2132

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. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important pretreatment difference.

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Table 8. Background Characteristics From Time 1 Interview

Time 1 Victim Interview Items	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Victim Age (c)	76	62	3	34.5263	32.9677	36.3333	0.8213
Victim is a High School Graduate (d)	74	61	3	0.7432	0.7705	1.0000	-0.3666
Victim Race = White (d)	76	62	3	0.8289	0.7258	0.6667	1.4612
Victim Race = Black (d)	76	62	3	0.1316	0.2581	0.0000	-1.8901
Victim Race = Other Race (d)	76	62	3	0.0395	0.0806	0.3333	-1.0295
Victim Race = Missing (d)	76	62	3	0.0000	0.0000	0.0000	N/A
Victim Ethnicity = Hispanic (d)	75	62	3	0.0800	0.0000	0.0000	N/A
Victim Reports Living With Offender at Time of Gateway Incident (d)	75	61	3	0.8000	0.7869	0.3333	0.1881
Offender-Victim Relationship = Offender is Husband (d)	75	62	3	0.4133	0.4677	0.3333	-0.6389
Offender-Victim Relationship = Offender is Ex-Husband (d)	75	62	3	0.0133	0.0323	0.3333	-0.7534
Offender-Victim Relationship = Offender is Boyfriend (d)	75	62	3	0.3600	0.3710	0.0000	-0.1327
Offender-Victim Relationship = Offender is Ex-Boyfriend (d)	75	62	3	0.1067	0.0323	0.3333	1.6665
Offender-Victim Relationship = Other (d)	75	62	3	0.1067	0.0968	0.0000	0.1903

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. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important pretreatment difference.

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Table 9. Official Record Background Characteristics For Cases Where a Time 2 Interview Was Completed

Official Record Background Characteristics	Treatment Group N	Control Group N	Treatment Group Mean	Control Group Mean	Z-Test
Male Offender, Female Victim (d)	53	44	1.0000	1.0000	N/A
Offender Race = White (d)	53	44	0.7547	0.6818	0.7975
Offender Race = Black (d)	53	44	0.2264	0.2955	-0.7739
Offender Race = Other (d)	53	44	0.0189	0.0227	-0.1332
Offender Ethnicity = Hispanic (d)	52	43	0.0192	0.0233	-0.1360
Offender Married (d)	52	43	0.4231	0.3953	0.2735
Number of Children Reported by Offender at Booking (c)	53	44	1.7170	1.9773	-0.7822
Offender Reports Any Children at Booking (d)	53	44	0.7547	0.7955	-0.4770
Offender Reports Any Past Military Service (d)	52	44	0.0769	0.1136	-0.6149
Number of Charges Booked at Gateway Arrest (c)	53	44	1.3019	2.1136	-0.8850
Offender's Age at Time of Booking (c)	53	44	36.3396	35.1136	0.5319
Years of Education Reported by Offender at Booking (c)	52	43	12.2500	12.2093	0.0879
Days in Jail Between Gateway Arrest and Bond Release (c)	53	44	2.4340	1.8864	0.6124
Offender Reports Being Born in South Carolina (d)	53	44	0.5849	0.7955	-2.2139
Offender Booked on Domestic Violence Charge (d)	53	44	0.9811	1.0000	N/A
Offender Booked on Non-Domestic Violence Charge (d)	53	44	0.1887	0.1364	0.6912
Age at First Arrest (Including Current Arrest if No Prior Arrests) (c	53	44	25.1132	24.0227	0.5221
Any Prior Arrest Record (Before Gateway Arrest) (d)	53	44	0.7925	0.8409	-0.6112
Number of Prior Arrests (Not Including Zeros) (c)	42	37	5.8571	5.4865	0.3212
Number of Prior Arrests (Including Zeros) (c)	53	44	4.6415	4.6136	0.0267
Number of Days Since Last Arrest (Among Those With Priors) (c)	42	37	1448.3333	1744.7027	0.8013
Any Prior Arrest Record in South Carolina (d)	53	44	0.7736	0.7955	-0.2604
Any Prior Arrest Record by Lexington County Sheriff's Department (d)	53	44	0.5849	0.5682	0.1660
Both Parties Arrested at Gateway Arrest (Dual Arrest) (d)	53	44	0.0000	0.0000	N/A
Number of Days Between Gateway Arrest and Record Search Date (c)	53	44	594.1509	562.2045	0.9617
Any Prior Charges for Alcohol Violations (d)	53	44	0.2075	0.3409	-1.4763
Any Prior Charges for Assaults (d)	53	44	0.3585	0.2045	1.6658
Any Prior Charges for Burglary (d)	53	44	0.1321	0.0682	1.0303
Any Prior Charges for Child Abuse (d)	53	44	0.0189	0.0000	N/A
Any Prior Charges for FTA/FTP/Attorney Contempt (d)	53	44	0.0189	0.0227	-0.1332
Any Prior Charges for All Other Contempt (d)	53	44	0.0189	0.0909	-1.5975
Any Prior Charges for Disorder Offenses (d)	53	44	0.3396	0.2500	0.9599
Any Prior Charges for Drug Offenses (d)	53	44	0.3962	0.4318	-0.3545
Any Prior Charges for Domestic Violence (d)	53	44	0.2642	0.2500	0.1586
Any Prior Charges for Driving While Impaired (d)	53	44	0.3019	0.2727	0.3155
Any Prior Charges for Fraud (d)	53	44	0.2264	0.1136	1.4545
Any Prior Charges for Harassment (d)	53	44	0.0943	0.0682	0.4662
Any Prior Charges for Homicide (d)	53	44	0.0189	0.0000	N/A
Any Prior Charges for Kidnapping (d)	53	44	0.0000	0.0227	N/A
Any Prior Charges for Motor Vehicle Theft (d)	53	44	0.0755	0.0682	0.1381
Any Prior Charges for Robbery (d)	53	44	0.0566	0.0455	0.2472
Any Prior Charges for Sex Offenses (d)	53	44	0.0377	0.0227	0.4251
Any Prior Charges for Theft (d)	53	44	0.1132	0.1591	-0.6604
Any Prior Charges for Traffic Offenses (d)	53	44	0.2642	0.4773	-2.1759
Any Prior Charges for Vandalism (d)	53	44	0.1509	0.0682	1.2798
Any Prior Charges for Weapons Violations (d)	53	44	0.1132	0.2045	-1.2387
Any Prior Charges for Probation/Parole Violations (d)	53	44	0.0377	0.0455	-0.1903
Any Prior Charges for Other Property Offenses (d)	53	44	0.1887	0.1136	1.0177
Any Prior Charges for Other Offenses (d)	53	44	0.2264	0.1364	1.1358
Any Prior Charges for Offenses With Missing Charges (d)	53	44	0.0000	0.0227	N/A
Any Prior Charges for Violent Offenses (d)	53	44	0.5094	0.4091	0.9866
Any Prior Charges for Property Offenses (d)	53	44	0.5094	0.3864	1.2121
Number of Days Between Gateway Arrest and Time 1 Interview (c)	52	43	53.9808	44.3721	2.2139
Number of Days Between Gateway Arrest and Time 2 Interview (c)	53	44	177.0566	171.2955	0.5273
Number of Days Between Time 1 and Time 2 Interviews (c)	53	44	121.7547	125.8409	-0.3724

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. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important pretreatment difference.

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Table 10. Background Characteristics From Time 1 Interview For Victims Who Completed a Time 2 Interview

Time 1 Victim Interview Items	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Victim Age (c)	52	43	2	35.4615	32.9070	36.0000	1.1038
Victim is a High School Graduate (d)	52	42	2	0.7885	0.7619	1.0000	0.3073
Victim Race = White (d)	52	43	2	0.7885	0.7674	1.0000	0.2458
Victim Race = Black (d)	52	43	2	0.1538	0.2093	0.0000	-0.7019
Victim Race = Other Race (d)	52	43	2	0.0577	0.0930	0.0000	-0.6561
Victim Race = Missing (d)	52	43	2	0.0000	0.0000	0.0000	N/A
Victim Ethnicity = Hispanic (d)	51	43	2	0.0588	0.0000	0.0000	N/A
Victim Reports Living With Offender at Time of Gateway Incident (d)	51	42	2	0.7255	0.7619	0.5000	-0.3994
Offender-Victim Relationship = Offender is Husband (d)	51	43	2	0.4118	0.4419	0.5000	-0.2940
Offender-Victim Relationship = Offender is Ex-Husband (d)	51	43	2	0.0196	0.0465	0.0000	-0.7393
Offender-Victim Relationship = Offender is Boyfriend (d)	51	43	2	0.3137	0.3256	0.0000	-0.1228
Offender-Victim Relationship = Offender is Ex-Boyfriend (d)	51	43	2	0.1373	0.0465	0.5000	1.4896
Offender-Victim Relationship = Other (d)	51	43	2	0.1176	0.1395	0.0000	-0.3168

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. None of the reported Z-tests exceed 1.5.

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Table 11. Official Record Background Characteristics For Cases Where Both Time 1 and Time 2 Interviews Were Completed

Background Characteristic	Treatment Group N	Control Group N	Treatment Group Mean	Control Group Mean	Z-Test
Male Offender, Female Victim (d)	52	43	1.0000	1.0000	N/A
Offender Race = White (d)	52	43	0.7500	0.6744	0.8130
Offender Race = Black (d)	52	43	0.2308	0.3023	-0.7884
Offender Race = Other (d)	52	43	0.0192	0.0233	-0.1360
Offender Ethnicity = Hispanic (d)	51	42	0.0196	0.0238	-0.1390
Offender Married (d)	51	42	0.4314	0.3810	0.4922
Number of Children Reported by Offender at Booking (c)	52	43	1.6538	1.9767	-0.9673
Offender Reports Any Children at Booking (d)	52	43	0.7500	0.7907	-0.4681
Offender Reports Any Past Military Service (d)	51	43	0.0588	0.1163	-0.9945
Number of Charges Booked at Gateway Arrest (c)	52	43	1.2885	2.1395	-0.9071
Offender's Age at Time of Booking (c)	52	43	35.9615	35.1860	0.3340
Years of Education Reported by Offender at Booking (c)	51	42	12.2157	12.2143	0.0030
Days in Jail Between Gateway Arrest and Bond Release (c)	52	43	2.4615	1.8837	0.6332
Offender Reports Being Born in South Carolina (d)	52	43	0.5962	0.7907	-2.0305
Offender Booked on Domestic Violence Charge (d)	52	43	0.9808	1.0000	N/A
Offender Booked on Non-Domestic Violence Charge (d)	52	43	0.1731	0.1395	0.4463
Age at First Arrest (Including Current Arrest if No Prior Arrests) (c	52	43	24.6538	24.1163	0.2586
Any Prior Arrest Record (Before Gateway Arrest) (d)	52	43	0.7885	0.8372	-0.6035
Number of Prior Arrests (Not Including Zeros) (c)	41	36	5.9756	5.5833	0.3341
Number of Prior Arrests (Including Zeros) (c)	52	43	4.7115	4.6744	0.0350
Number of Days Since Last Arrest (Among Those With Priors) (c)	41	36	1428.1463	1722.0556	0.7769
Any Prior Arrest Record in South Carolina (d)	52	43	0.7692	0.7907	-0.2510
Any Prior Arrest Record by Lexington County Sheriff's Department (d)	52	43	0.5769	0.5581	0.1840
Both Parties Arrested at Gateway Arrest (Dual Arrest) (d)	52	43	0.0000	0.0000	N/A
Number of Days Between Gateway Arrest and Record Search Date (c)	52	43	597.4231	557.0000	1.2122
Any Prior Charges for Alcohol Violations (d)	52	43	0.2115	0.3256	-1.2565
Any Prior Charges for Assaults (d)	52	43	0.3654	0.2093	1.6609
Any Prior Charges for Burglary (d)	52	43	0.1346	0.0698	1.0251
Any Prior Charges for Child Abuse (d)	52	43	0.0192	0.0000	N/A
Any Prior Charges for FTA/FTP/Attorney Contempt (d)	52	43	0.0192	0.0233	-0.1360
Any Prior Charges for All Other Contempt (d)	52	43	0.0192	0.0930	-1.6033
Any Prior Charges for Disorder Offenses (d)	52	43	0.3462	0.2558	0.9517
Any Prior Charges for Drug Offenses (d)	52	43	0.4038	0.4186	-0.1456
Any Prior Charges for Domestic Violence (d)	52	43	0.2692	0.2558	0.1478
Any Prior Charges for Driving While Impaired (d)	52	43	0.3077	0.2791	0.3046
Any Prior Charges for Fraud (d)	52	43	0.2308	0.1163	1.4491
Any Prior Charges for Harassment (d)	52	43	0.0769	0.0698	0.1329
Any Prior Charges for Homicide (d)	52	43	0.0192	0.0000	N/A
Any Prior Charges for Kidnapping (d)	52	43	0.0000	0.0233	N/A
Any Prior Charges for Motor Vehicle Theft (d)	52	43	0.0769	0.0698	0.1329
Any Prior Charges for Robbery (d)	52	43	0.0577	0.0465	0.2429
Any Prior Charges for Sex Offenses (d)	52	43	0.0385	0.0233	0.4218
Any Prior Charges for Theft (d)	52	43	0.1154	0.1628	-0.6692
Any Prior Charges for Traffic Offenses (d)	52	43	0.2692	0.4651	-1.9824
Any Prior Charges for Vandalism (d)	52	43	0.1538	0.0698	1.2748
Any Prior Charges for Weapons Violations (d)	52	43	0.1154	0.2093	-1.2496
Any Prior Charges for Probation/Parole Violations (d)	52	43	0.0385	0.0465	-0.1945
Any Prior Charges for Other Property Offenses (d)	52	43	0.1923	0.1163	1.0115
Any Prior Charges for Other Offenses (d)	52	43	0.2308	0.1395	1.1295
Any Prior Charges for Offenses With Missing Charges (d)	52	43	0.0000	0.0233	N/A
Any Prior Charges for Violent Offenses (d)	52	43	0.5000	0.4186	0.7919
Any Prior Charges for Property Offenses (d)	52	43	0.5192	0.3953	1.2053
Number of Days Between Gateway Arrest and Time 1 Interview (c)	52	43	53.9808	44.3721	2.2139
Number of Days Between Gateway Arrest and Time 2 Interview (c)	52	43	177.1154	171.1860	0.5312
Number of Days Between Time 1 and Time 2 Interviews (c)	52	43	123.1346	126.8140	-0.3322

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. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important pretreatment difference.

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Table 12. Background Characteristics From Time 1 Interview For Victims Who Completed Both Time 1 and Time 2 Interviews

Time 1 Victim Interview Items	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Victim Age (c)	52	43	2	35.4615	32.9070	36.0000	1.1038
Victim is a High School Graduate (d)	52	42	2	0.7885	0.7619	1.0000	0.3073
Victim Race = White (d)	52	43	2	0.7885	0.7674	1.0000	0.2458
Victim Race = Black (d)	52	43	2	0.1538	0.2093	0.0000	-0.7019
Victim Race = Other Race (d)	52	43	2	0.0577	0.0930	0.0000	-0.6561
Victim Race = Missing (d)	52	43	2	0.0000	0.0000	0.0000	N/A
Victim Ethnicity = Hispanic (d)	51	43	2	0.0588	0.0000	0.0000	N/A
Victim Reports Living With Offender at Time of Gateway Incident (d)	51	42	2	0.7255	0.7619	0.5000	-0.3994
Offender-Victim Relationship = Offender is Husband (d)	51	43	2	0.4118	0.4419	0.5000	-0.2940
Offender-Victim Relationship = Offender is Ex-Husband (d)	51	43	2	0.0196	0.0465	0.0000	-0.7393
Offender-Victim Relationship = Offender is Boyfriend (d)	51	43	2	0.3137	0.3256	0.0000	-0.1228
Offender-Victim Relationship = Offender is Ex-Boyfriend (d)	51	43	2	0.1373	0.0465	0.5000	1.4896
Offender-Victim Relationship = Other (d)	51	43	2	0.1176	0.1395	0.0000	-0.3168

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. None of the reported Z-tests exceed 1.5.

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Table 13. Time 1 Interview Outcomes

Time 1 Victim Interview Items	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Victim Living Situation = Currently Married (d)	76	61	3	0.1974	0.3115	0.0000	-1.5367
Victim Living Situation = Currently Cohabiting (d)	76	61	3	0.1579	0.1311	0.0000	0.4407
Victim Living Situation = Divorced or Separated (d)	76	61	3	0.4474	0.2623	0.6667	2.2363
Victim Living Situation = Widowed (d)	76	61	3	0.0000	0.0164	0.0000	N/A
Victim Living Situation = Single, Never Married (d)	76	61	3	0.1974	0.2787	0.3333	-1.1180
Offender and Victim Living Together at Time of Interview (d)	75	62	3	0.1867	0.2097	0.0000	-0.3370
Offender and Victim Have Lived Together Since the Incident (d)	75	61	3	0.3467	0.3443	0.0000	0.0293
Victim Contacted Offender Since Incident (d)	76	62	3	0.6447	0.5968	0.0000	0.5784
Offender Contacted Victim Since Incident (d)	74	62	3	0.7703	0.7419	0.6667	0.3839
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA) (d)	75	60	3	0.7200	0.5667	0.6667	1.8583
Victim Reports Contact by Sheriff's Deputy (d)	76	62	3	0.5263	0.3548	0.5000	2.0144
Victim Reports Knowledge of No-Contact Order (d)	73	62	3	0.8904	0.8387	1.0000	0.8806
Victim Reports Concerns About Safety (d)	76	62	3	0.4868	0.4355	0.6667	0.6018
Victim Reports Carrying Weapon For Self-Defense (d)	76	62	3	0.1579	0.1290	0.6667	0.4791
Trend in Abuse = Got Worse After Gateway Arrest (d)	67	58	2	0.0299	0.0000	0.0000	1.3265
Trend in Abuse = No New Abuse After Gateway Arrest (d)	67	58	2	0.7761	0.8621	1.0000	-1.2368
Trend in Abuse = Stayed About the Same After Gateway Arrest (d)	67	58	2	0.0597	0.0690	0.0000	-0.2110
Trend in Abuse = Less After Gateway Arrest (d)	67	58	2	0.1343	0.0690	0.0000	1.1939
Psychological Aggression - Variety Scale (c)	76	62	3	1.5658	1.0484	0.0000	1.4975
Physical Aggression - Variety Scale (c)	75	62	3	0.3067	0.0484	0.0000	2.0621
Sexual Coercion - Variety Scale (c)	76	62	3	0.1053	0.0484	0.0000	0.9734
Injury - Variety Scale (c)	76	62	3	0.2500	0.0968	0.0000	0.9790
Stalking/Threats - Variety Scale (c)	75	61	3	1.6933	0.9016	1.0000	2.6745
Victim Reports Any Psychological Aggression (d)	76	62	3	0.4605	0.3710	0.0000	1.0602
Victim Reports Any Physical Aggression (d)	75	62	3	0.0933	0.0323	0.0000	1.4362
Victim Reports Any Sexual Coercion (d)	76	62	3	0.0658	0.0484	0.0000	0.4351
Victim Reports Any Injury (d)	76	62	3	0.0658	0.0161	0.0000	1.4230
Victim Reports Any Stalking/Threats (d)	75	61	3	0.5867	0.4098	0.3333	2.0514

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. Shaded lines indicate that the Z-test is statistically significant (two-tailed $p < .05$ significance level).

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Table 14. Time 1 Interview Regression Models Adjusting For Pretreatment Imbalances

Time 1 Interview Outcomes	Treatment Coefficient	Odds Multiplier for Treatment	Treatment Coefficient Standard Error	Test Statistic
Categorical Variables				**Chi-Square**
Offender and Victim Living Together at Time of Interview	0.0394	1.0402	0.4565	0.0074
Offender and Victim Have Lived Together Since the Incident	0.0716	1.0742	0.3840	0.0348
Victim Contacted Offender Since Incident	0.2345	1.2643	0.3799	0.3810
Offender Contacted Victim Since Incident	-0.0884	0.9154	0.4300	0.0423
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	0.6498	1.9152	0.3918	2.7502
Victim Reports Contact by Sheriff's Deputy	0.5103	1.6658	0.3786	1.8169
Victim Reports Knowledge of No-Contact Order	0.2506	1.2848	0.5609	0.1996
Victim Reports Concerns About Safety	-0.0388	0.9619	0.3733	0.0108
Victim Reports Carrying Weapon For Self-Defense	-0.1448	0.8652	0.5364	0.0729
Trend in Abuse = Got Worse After Gateway Arrest	N/A			
Trend in Abuse = No New Abuse After Gateway Arrest	-0.5406	0.5824	0.5059	1.1420
Trend in Abuse = Stayed About the Same After Gateway Arrest	-0.2531	0.7764	0.7805	0.1051
Trend in Abuse = Less After Gateway Arrest	0.6235	1.8654	0.6613	0.8887
Victim Reports Any Psychological Aggression	0.3035	1.3546	0.3746	0.6562
Victim Reports Any Physical Aggression	1.3601	3.8966	0.8594	2.5047
Victim Reports Any Sexual Coercion	0.5321	1.7025	0.8755	0.3693
Victim Reports Any Injury	1.6003	4.9545	1.1406	1.9686
Victim Reports Any Stalking/Threats	0.6698	1.9538	0.3729	3.2260
Numerical Variables				**T-Test**
Psychological Aggression - Variety Scale	0.5072		0.3697	1.3700
Physical Aggression - Variety Scale	0.3134		0.1456	2.1500
Sexual Coercion - Variety Scale	0.0820		0.0617	1.3300
Injury - Variety Scale	0.1926		0.1742	1.1100
Stalking/Threats - Variety Scale	0.6175		0.3214	1.9200

Note: The regressions reported in this table include the following predictor variables: (1) indicator variable for treatment (coded 1 if case is randomized to treatment and 0 if the case is randomized to control); (2) an indicator variable coded 1 if the offender is a South Carolina native and 0 otherwise; (3) an indicator variable coded 1 if the offender has any prior arrests for assault and 0 otherwise; (4) an indicator variable coded 1 if the victim is black and 0 otherwise; and (5) an indicator variable coded 1 if the offender was the victim's ex-boyfriend at the time of the gateway incident and 0 otherwise. Wald chi-square tests in this table only apply to the logistic regression coefficient for the treatment indicator variable in these models. In some models with categorical outcomes, convergence could not be attained due to quasi-complete separation. When this occurred, the problematic variable was dropped. The interim controls are not included in these tests. Shaded lines indicate that the treatment coefficient is statistically significant (two-tailed $p < .05$ significance level).

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Table 15. Time 2 Interview Outcomes

Time 2 Victim Interview Items	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Victim Living Situation = Currently Married (d)	53	44	3	0.2075	0.2727	0.3333	-0.7514
Victim Living Situation = Currently Cohabiting (d)	53	44	3	0.0943	0.1136	0.3333	-0.3112
Victim Living Situation = Divorced or Separated (d)	53	44	3	0.4717	0.3409	0.0000	1.3027
Victim Living Situation = Widowed (d)	53	44	3	0.0189	0.0227	0.0000	-0.1332
Victim Living Situation = Single, Never Married (d)	53	44	3	0.1887	0.2500	0.3333	-0.7300
Offender and Victim Living Together at Time of Interview (d)	53	44	3	0.2830	0.4091	0.3333	-1.3047
Offender and Victim Have Lived Together Since the First Interview (d)	53	44	3	0.3585	0.4318	0.6667	-0.7365
Victim Contacted Offender Since First Interview (d)	53	44	3	0.5849	0.6136	0.6667	-0.2873
Offender Contacted Victim Since First Interview (d)	53	44	3	0.7547	0.7500	0.6667	0.0536
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA) (d)	50	43	3	0.2200	0.1628	0.0000	0.6962
Victim Reports Contact by Sheriff's Deputy (d)	53	44	3	0.1509	0.0909	0.0000	0.8940
Knowledge of No-Contact Order Status = Don't Know Status (d)	52	43	3	0.1346	0.1860	0.0000	-0.6843
Knowledge of No-Contact Order Status = Yes, Still in Place (d)	52	43	3	0.5385	0.3721	0.3333	1.6187
Victim Reports Concerns About Safety (d)	53	44	3	0.4528	0.2727	0.3333	1.8279
Victim Reports Carrying Weapon For Self-Defense (d)	53	44	3	0.2264	0.1364	0.3333	1.1358
Trend in Abuse = Got Worse After Gateway Arrest (d)	41	32	2	0.0244	0.0313	0.5000	-0.1782
Trend in Abuse = No New Abuse After Gateway Arrest (d)	41	32	2	0.6585	0.7500	0.0000	-0.8450
Trend in Abuse = Stayed About the Same After Gateway Arrest (d)	41	32	2	0.1463	0.0313	0.0000	1.6571
Trend in Abuse = Less After Gateway Arrest (d)	41	32	2	0.1707	0.1875	0.5000	-0.1858
Psychological Aggression - Variety Scale (c)	53	44	3	3.0755	2.3864	2.3333	1.0132
Physical Aggression - Variety Scale (c)	53	44	3	0.4340	0.4545	1.6667	-0.0622
Sexual Coercion - Variety Scale (c)	53	44	3	0.1887	0.1591	0.3333	0.2387
Injury - Variety Scale (c)	53	44	3	0.5660	0.2727	0.0000	0.8580
Stalking/Threats - Variety Scale (c)	53	43	3	2.0377	0.8837	0.3333	2.7964
Victim Reports Any Psychological Aggression (d)	53	44	3	0.5849	0.5455	0.3333	0.3904
Victim Reports Any Physical Aggression (d)	53	44	3	0.1132	0.1136	0.3333	-0.0066
Victim Reports Any Sexual Coercion (d)	53	44	3	0.1132	0.0909	0.3333	0.3596
Victim Reports Any Injury (d)	53	44	3	0.1132	0.0682	0.0000	0.7609
Victim Reports Any Stalking/Threats (d)	53	43	3	0.6226	0.3256	0.3333	2.8954

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. Shaded lines indicate that the Z-test is statistically significant (two-tailed $p < .05$ significance level).

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Table 16. Time 2 Interview Regression Models Adjusting For Pretreatment Imbalances

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	0.0131	1.0132	0.1757	0.0056
Victim Living Situation = Currently Cohabiting	-0.2109	0.8099	0.4311	0.2395
Victim Living Situation = Divorced or Separated	-0.0913	0.9127	0.2080	0.1925
Victim Living Situation = Widowed	N/A	N/A	N/A	N/A
Victim Living Situation = Single, Never Married	0.1229	1.1308	0.1844	0.4444
Offender and Victim Living Together at Time of Interview	-0.4409	0.6435	0.3657	1.4537
Offender and Victim Have Lived Together Since the First Interview	0.0395	1.0403	0.1720	0.0526
Victim Contacted Offender Since First Interview	-0.0276	0.9728	0.1717	0.0257
Offender Contacted Victim Since First Interview	-0.0616	0.9403	0.1761	0.1223
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	-0.0439	0.9570	0.2481	0.0313
Victim Reports Contact by Sheriff's Deputy	0.1296	1.1384	0.2804	0.2135
Victim Reports That No-Contact Order Still in Place	0.1450	1.1560	0.1764	0.6759
Victim Reports No Knowledge of No-Contact Order Status	-0.6684	0.5125	0.6752	0.9798
Victim Reports Concerns About Safety	-0.0294	0.9710	0.2277	0.0167
Victim Reports Carrying Weapon For Self-Defense	0.2845	1.3291	0.1886	2.2771
Trend in Abuse = Got Worse After Gateway Arrest	N/A			
Trend in Abuse = No New Abuse After Gateway Arrest	-0.6538	0.5201	0.4966	1.7331
Trend in Abuse = Stayed About the Same After Gateway Arrest	0.1066	1.1125	0.3337	0.1020
Trend in Abuse = Less After Gateway Arrest	0.5891	1.8024	0.3856	2.3345
Victim Reports Any Psychological Aggression	-0.2269	0.7970	0.2578	0.7745
Victim Reports Any Physical Aggression	0.0564	1.0580	0.5706	0.0098
Victim Reports Any Sexual Coercion	0.3322	1.3940	0.4040	0.6764
Victim Reports Any Injury	0.2575	1.2937	0.4375	0.3466
Victim Reports Any Stalking/Threats	0.2432	1.2753	0.1927	1.5923
Numerical Variables				**T-Test**
Psychological Aggression - Variety Scale	-0.0017		0.2838	-0.0060
Physical Aggression - Variety Scale	0.0566		0.1326	0.4267
Sexual Coercion - Variety Scale	0.0349		0.0481	0.7258
Injury - Variety Scale	0.1227		0.1394	0.8802
Stalking/Threats - Variety Scale	0.1224		0.1759	0.6960

Note: The regressions reported in this table include the following predictor variables: (1) indicator variable for treatment (coded 1 if case is randomized to treatment and 0 if the case is randomized to control); (2) an indicator variable coded 1 if the offender is a South Carolina native and 0 otherwise; (3) an indicator variable coded 1 if the offender has any prior arrests for assault and 0 otherwise; (4) an indicator variable coded 1 offender has any prior arrests for contempt and 0 otherwise; (5) an indicator variable coded 1 if the offender prior arrests for traffic offenses and 0 otherwise; and (6) the length of time (in days) between the gateway arrest and the first interview. Wald chi-square tests in this table only apply to the logistic regression coefficient for the treatment indicator variable in these models. In some models with categorical outcomes, convergence could not be attained due to quasi-complete separation. When this occurred, the problematic variable was dropped. The interim controls are not included in these tests.

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Table 17. Combined Time 1 - Time 2 Interview Outcomes

Combined Victim Interview Items	Treatment Group N	Control Group N	Interim Control Group N	Treatment Group Mean	Control Group Mean	Interim Control Group Mean	Z-Test
Offender and Victim Living Together at Time of Either Interview	52	43	2	0.3077	0.3953	0.0000	-0.8932
Offender and Victim Have Lived Together Since Incident	52	43	2	0.4423	0.4651	0.5000	-0.2223
Victim Contacted Offender	52	43	2	0.7885	0.6744	0.5000	1.2565
Offender Contacted Victim	52	43	2	0.8269	0.8837	1.0000	-0.7774
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	52	43	2	0.7308	0.5116	1.0000	2.2040
Victim Reports Contact by Sheriff's Deputy	52	43	2	0.5577	0.3953	0.5000	1.5760
Victim Reports Concerns About Safety	52	43	2	0.6154	0.5349	0.5000	0.7910
Victim Reports Carrying Weapon For Self-Defense	52	43	2	0.3077	0.1860	0.5000	1.3582
Victim Reports Any Psychological Aggression	52	43	2	0.6923	0.5814	0.0000	1.1225
Victim Reports Any Physical Aggression	52	43	2	0.1346	0.1628	0.0000	-0.3856
Victim Reports Any Sexual Coercion	52	43	2	0.1731	0.1163	0.0000	0.7774
Victim Reports Any Injury	52	43	2	0.1154	0.0698	0.0000	0.7557
Victim Reports Any Stalking/Threats	52	43	2	0.7500	0.6279	1.0000	1.2862

Note: all of the comparisons in this table involve dichotomous variables. 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. Shaded lines indicate that the Z-test is statistically significant (two-tailed $p < .05$ significance level).

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Table 18. Combined Interview Regression Models Adjusting For Pretreatment Imbalances

Combined Time 1 - Time 2 Interview Outcomes	Treatment Coefficient	Odds Multiplier for Treatment	Treatment Coefficient Standard Error	Chi-Square Test
Offender and Victim Living Together at Time of Either Interview	-0.2495	0.7792	0.5014	0.2475
Offender and Victim Have Lived Together Since Incident	0.0005	1.0005	0.4741	0.0000
Victim Contacted Offender	0.9839	2.6749	0.5682	2.9987
Offender Contacted Victim	-0.4248	0.6539	0.7469	0.3236
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	0.9857	2.6797	0.4929	3.9993
Victim Reports Contact by Sheriff's Deputy	0.5196	1.6814	0.4783	1.1802
Victim Reports Concerns About Safety	0.0687	1.0711	0.5054	0.0185
Victim Reports Carrying Weapon For Self-Defense	0.8113	2.2508	0.5558	2.1304
Victim Reports Any Psychological Aggression	0.3454	1.4126	0.4974	0.4822
Victim Reports Any Physical Aggression	-0.0284	0.9720	0.6392	0.0020
Victim Reports Any Sexual Coercion	0.5330	1.7040	0.7116	0.5610
Victim Reports Any Injury	0.6053	1.8318	0.8103	0.5579
Victim Reports Any Stalking/Threats	0.4229	1.5264	0.5422	0.6084

Note: The logistic regressions reported in this table include the following predictor variables: (1) indicator variable for treatment (coded 1 if case is randomized to treatment and 0 if the case is randomized to control); (2) an indicator variable coded 1 if the offender is a South Carolina native and 0 otherwise; (3) an indicator variable coded 1 if the offender has any prior arrests for assault and 0 otherwise; (4) an indicator variable coded 1 offender has any prior arrests for contempt and 0 otherwise; (5) an indicator variable coded 1 if the offender prior arrests for traffic offenses and 0 otherwise; and (6) the length of time (in days) between the gateway arrest and the first interview. Wald chi-square tests in this table only apply to the logistic regression coefficient for the treatment indicator variable in these models. In some models with categorical outcomes, convergence could not be attained due to quasi-complete separation. When this occurred, the problematic variable was dropped. The interim controls are not included in these tests. Shading indicates that the treatment effect is statistically significant.

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Table 19. Pretreatment Characteristics and Missing Time 1 Interview Data

Background Characteristic	Observed N	Missing N	Observed Mean	Missing Mean	Z-Test
Randomly Assigned to Treatment Condition (d)	138	250	0.5507	0.4920	-1.1079
Male Offender, Female Victim (d)	138	250	1.0000	1.0000	N/A
Offender Race = White (d)	138	248	0.7029	0.6976	-0.1092
Offender Race = Black (d)	138	248	0.2609	0.2177	-0.9604
Offender Race = Other (d)	138	248	0.0362	0.0847	1.8200
Offender Ethnicity = Hispanic (d)	136	241	0.0221	0.0954	2.7000
Offender Married (d)	136	244	0.4338	0.3770	-1.0842
Number of Children Reported by Offender at Booking (c)	138	248	1.8261	1.6734	-0.9044
Offender Reports Any Children at Booking (d)	138	248	0.7609	0.7258	-0.7511
Offender Reports Any Past Military Service (d)	137	248	0.1168	0.0847	-1.0235
Number of Charges Booked at Gateway Arrest (c)	138	248	1.5362	1.2177	-1.0709
Offender's Age at Time of Booking (c)	138	248	35.1377	34.6653	-0.4161
Years of Education Reported by Offender at Booking (c)	136	241	12.0956	11.7054	-1.6988
Days in Jail Between Gateway Arrest and Bond Release (c)	138	249	2.1087	1.4458	-1.6813
Offender Reports Being Born in South Carolina (d)	138	248	0.6087	0.5847	-0.4604
Offender Booked on Domestic Violence Charge (d)	138	248	0.9928	0.9919	-0.0877
Offender Booked on Non-Domestic Violence Charge (d)	138	248	0.1449	0.1411	-0.1023
Age at First Arrest (Including Current Arrest if No Prior Arrests) (c)	138	248	24.2029	23.2863	-0.9666
Any Prior Arrest Record (Before Gateway Arrest) (d)	138	250	0.7971	0.8160	0.4536
Number of Prior Arrests (Not Including Zeros) (c)	110	204	5.7455	5.6716	-0.1182
Number of Prior Arrests (Including Zeros) (c)	138	250	4.5797	4.6280	0.0866
Number of Days Since Last Arrest (Among Those With Priors) (c)	110	204	1734.1545	1738.1422	-0.0183
Any Prior Arrest Record in South Carolina (d)	138	250	0.7246	0.7760	1.1304
Any Prior Arrest Record by Lexington County Sheriff's Department (d)	138	250	0.4855	0.4720	-0.2550
Both Parties Arrested at Gateway Arrest (Dual Arrest) (d)	138	250	0.0072	0.0160	0.7319
Number of Days Between Gateway Arrest and Record Search Date (c)	138	250	553.9710	540.3040	-0.7299
Any Prior Charges for Alcohol Violations (d)	138	250	0.2391	0.2680	0.6224
Any Prior Charges for Assaults (d)	138	250	0.2826	0.3160	0.6844
Any Prior Charges for Burglary (d)	138	250	0.7087	0.1280	0.5579
Any Prior Charges for Child Abuse (d)	138	250	0.0290	0.0160	-0.8617
Any Prior Charges for FTA/FTP/Attorney Contempt (d)	138	250	0.0217	0.0120	-0.7443
Any Prior Charges for All Other Contempt (d)	138	250	0.0580	0.0200	-1.9897
Any Prior Charges for Disorder Offenses (d)	138	250	0.2754	0.2560	-0.4148
Any Prior Charges for Drug Offenses (d)	138	250	0.4203	0.3680	-1.0125
Any Prior Charges for Domestic Violence (d)	138	250	0.2536	0.2320	-0.4776
Any Prior Charges for Driving While Impaired (d)	138	250	0.2826	0.3120	0.6039
Any Prior Charges for Fraud (d)	138	250	0.1667	0.1680	0.0337
Any Prior Charges for Harassment (d)	138	250	0.0797	0.0520	-1.0847
Any Prior Charges for Homicide (d)	138	250	0.0072	0.0240	1.1869
Any Prior Charges for Kidnapping (d)	138	250	0.0072	0.0320	1.5507
Any Prior Charges for Motor Vehicle Theft (d)	138	250	0.0725	0.0680	-0.1654
Any Prior Charges for Robbery (d)	138	250	0.0507	0.0760	0.9532
Any Prior Charges for Sex Offenses (d)	138	250	0.0362	0.0560	0.8638
Any Prior Charges for Theft (d)	138	250	0.1667	0.2120	1.0771
Any Prior Charges for Traffic Offenses (d)	138	250	0.3623	0.4600	1.8631
Any Prior Charges for Vandalism (d)	138	250	0.1087	0.0800	-0.9445
Any Prior Charges for Weapons Violations (d)	138	250	0.1522	0.1240	-0.7798
Any Prior Charges for Probation/Parole Violations (d)	138	250	0.0435	0.0360	-0.3658
Any Prior Charges for Other Property Offenses (d)	138	250	0.1449	0.1080	-1.0672
Any Prior Charges for Other Offenses (d)	138	250	0.2029	0.1960	-0.1631
Any Prior Charges for Offenses With Missing Charges (d)	138	250	0.0145	0.0200	0.3902
Any Prior Charges for Violent Offenses (d)	138	250	0.4638	0.5000	0.6835
Any Prior Charges for Property Offenses (d)	138	250	0.4638	0.3960	-1.2947

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 interim controls are not included in these tests. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important pretreatment difference between observed and missing Time 1 interview cases.

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Table 20. Case Dispositions, Official Record Outcomes, and Missing Time 1 Interview Data

Case Dispositions and Official Record Outcomes	Observed N	Missing N	Observed Mean	Missing Mean	Z-Test
Number of Days Between Gateway Arrest and First Appearance (c)	138	250	28.9783	30.5200	0.9839
Number of Days Between Gateway Arrest and Case Disposition (c)	117	207	165.0000	181.3720	0.8322
Case Disposition = Guilty (d)	138	250	0.7174	0.6040	-2.2335
Case Disposition = Not Guilty (d)	138	250	0.0217	0.0680	1.9729
Case Disposition = Nolle Prosequi (d)	138	250	0.1087	0.1480	1.0879
Case Disposition = Death (d)	138	250	0.0000	0.0080	N/A
Case Disposition = Case Pending (d)	138	250	0.1522	0.1720	0.5037
Pre-Trial Intervention Program Status = Program Complete (d)	19	33	0.5789	0.5152	-0.4444
Pre-Trial Intervention Program Status = Terminated (d)	19	33	0.2105	0.1818	-0.2529
Pre-Trial Intervention Program Status = Rejected (d)	19	33	0.0526	0.0606	0.1188
Pre-Trial Intervention Program Status = Pending (d)	19	33	0.1579	0.2424	0.7187
Any Subsequent Arrest Record (d)	138	250	0.3913	0.4200	0.5502
Number of Subsequent Arrests (Excluding Zeros) (c)	54	105	1.6481	1.6476	-0.0033
Number of Subsequent Arrests (Including Zeros) (c)	138	250	0.6449	0.6920	0.4408
Any Subsequent Arrests in South Carolina (d)	138	250	0.3623	0.4080	0.8824
Any Subsequent Arrests by Lexington County Sheriff's Department (d)	138	250	0.2536	0.2760	0.4764
Any Subsequent Arrests in LCSD For Crimes Against the Same Victim (d)	137	248	0.1022	0.1008	-0.0431
Any Subsequent Charges for Alcohol Violations (d)	138	250	0.0362	0.0440	0.3684
Any Subsequent Charges for Assaults (d)	138	250	0.0145	0.0200	0.3902
Any Subsequent Charges for Burglary (d)	138	250	0.0145	0.0160	0.1152
Any Subsequent Charges for Child Abuse (d)	138	250	0.0072	0.0080	0.0811
Any Subsequent Charges for FTA/FTP/Attorney Contempt (d)	138	250	0.0145	0.0040	-1.1296
Any Subsequent Charges for All Other Contempt (d)	138	250	0.0797	0.1120	1.0126
Any Subsequent Charges for Disorder Offenses (d)	138	250	0.0362	0.0280	-0.4484
Any Subsequent Charges for Drug Offenses (d)	138	250	0.0652	0.0920	0.9181
Any Subsequent Charges for Domestic Violence (d)	138	250	0.1087	0.1320	0.6674
Any Subsequent Charges for Driving While Impaired (d)	138	250	0.0145	0.0280	0.8462
Any Subsequent Charges for Fraud (d)	138	250	0.0217	0.0360	0.7768
Any Subsequent Charges for Harassment (d)	138	250	0.0072	0.0120	0.4438
Any Subsequent Charges for Homicide (d)	138	250	0.0000	0.0000	N/A
Any Subsequent Charges for Kidnapping (d)	138	250	0.0145	0.0120	-0.2084
Any Subsequent Charges for Motor Vehicle Theft (d)	138	250	0.0072	0.0080	0.0811
Any Subsequent Charges for Robbery (d)	138	250	0.0072	0.0040	-0.4275
Any Subsequent Charges for Sex Offenses (d)	138	250	0.0072	0.0040	-0.4275
Any Subsequent Charges for Theft (d)	138	250	0.0145	0.0400	1.3893
Any Subsequent Charges for Traffic Offenses (d)	138	250	0.0942	0.1080	0.4279
Any Subsequent Charges for Vandalism (d)	138	250	0.0072	0.0160	0.7319
Any Subsequent Charges for Weapons Violations (d)	138	250	0.0000	0.0120	N/A
Any Subsequent Charges for Probation/Parole Violations (d)	138	250	0.0435	0.0200	-1.3339
Any Subsequent Charges for Other Property Offenses (d)	138	250	0.0072	0.0080	0.0811
Any Subsequent Charges for Other Offenses (d)	138	250	0.0217	0.0320	0.5830
Any Subsequent Charges for Offenses With Missing Charges (d)	138	250	0.0072	0.0000	N/A
Any Subsequent Charges for Violent Offenses (d)	138	250	0.1377	0.1480	0.2769
Any Subsequent Charges for Property Offenses (d)	138	250	0.0507	0.1080	1.9101

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 interim controls are not included in these tests. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important difference between observed and missing Time 1 interview cases.

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Table 21. Pretreatment Characteristics and Missing Time 2 Interview Data

Pretreatment Characteristic	Observed N	Missing N	Observed Mean	Missing Mean	Z-Test
Randomly Assigned to Treatment Condition (d)	97	291	0.5464	0.5017	-0.7623
Male Offender, Female Victim (d)	97	291	1.0000	1.0000	N/A
Offender Race = White (d)	97	289	0.7216	0.6920	-0.5503
Offender Race = Black (d)	97	289	0.2577	0.2249	-0.6614
Offender Race = Other (d)	97	289	0.0206	0.0830	2.1226
Offender Ethnicity = Hispanic (d)	95	282	0.0211	0.0851	2.1309
Offender Married (d)	95	285	0.4105	0.3930	-0.3026
Number of Children Reported by Offender at Booking (c)	97	289	1.8351	1.6920	-0.7617
Offender Reports Any Children at Booking (d)	97	289	0.7732	0.7266	-0.9026
Offender Reports Any Past Military Service (d)	96	289	0.0938	0.0969	0.0903
Number of Charges Booked at Gateway Arrest (c)	97	289	1.6701	1.2180	-1.0812
Offender's Age at Time of Booking (c)	97	289	35.7835	34.5156	-0.9747
Years of Education Reported by Offender at Booking (c)	95	282	12.2316	11.7163	-1.9625
Days in Jail Between Gateway Arrest and Bond Release (c)	97	290	2.1856	1.5138	-1.3965
Offender Reports Being Born in South Carolina (d)	97	289	0.6804	0.5640	-2.0193
Offender Booked on Domestic Violence Charge (d)	97	289	0.9897	0.9931	0.3289
Offender Booked on Non-Domestic Violence Charge (d)	97	289	0.1649	0.1349	-0.7314
Age at First Arrest (Including Current Arrest if No Prior Arrests) (c)	97	289	24.6186	23.2768	-1.1645
Any Prior Arrest Record (Before Gateway Arrest) (d)	97	291	0.8144	0.8076	-0.1492
Number of Prior Arrests (Not Including Zeros) (c)	79	235	5.6835	5.7021	0.0275
Number of Prior Arrests (Including Zeros) (c)	97	291	4.6289	4.6048	-0.0395
Number of Days Since Last Arrest (Among Those With Priors) (c)	79	235	1587.1392	1787.0383	-0.8792
Any Prior Arrest Record in South Carolina (d)	97	291	0.7835	0.7491	-0.6841
Any Prior Arrest Record by Lexington County Sheriff's Department (d)	97	291	0.5773	0.4433	-2.2887
Both Parties Arrested at Gateway Arrest (Dual Arrest) (d)	97	291	0.0000	0.0172	N/A
Number of Days Between Gateway Arrest and Record Search Date (c)	97	291	579.6592	533.6667	-2.3285
Any Prior Charges for Alcohol Violations (d)	97	291	0.2680	0.2543	-0.2681
Any Prior Charges for Assaults (d)	97	291	0.2887	0.3093	0.3823
Any Prior Charges for Burglary (d)	97	291	0.1031	0.1271	0.6288
Any Prior Charges for Child Abuse (d)	97	291	0.0103	0.0241	0.8250
Any Prior Charges for FTA/FTP/Attorney Contempt (d)	97	291	0.0206	0.0137	-0.4751
Any Prior Charges for All Other Contempt (d)	97	291	0.0515	0.0275	-1.1402
Any Prior Charges for Disorder Offenses (d)	97	291	0.2990	0.2509	-0.9322
Any Prior Charges for Drug Offenses (d)	97	291	0.4124	0.3780	-0.6019
Any Prior Charges for Domestic Violence (d)	97	291	0.2577	0.2337	-0.4806
Any Prior Charges for Driving While Impaired (d)	97	291	0.2887	0.3058	0.3193
Any Prior Charges for Fraud (d)	97	291	0.1753	0.1649	-0.2355
Any Prior Charges for Harassment (d)	97	291	0.0825	0.0550	-0.9734
Any Prior Charges for Homicide (d)	97	291	0.0103	0.0206	0.6606
Any Prior Charges for Kidnapping (d)	97	291	0.0103	0.0275	0.9736
Any Prior Charges for Motor Vehicle Theft (d)	97	291	0.0722	0.0687	-0.1152
Any Prior Charges for Robbery (d)	97	291	0.0515	0.0722	0.7033
Any Prior Charges for Sex Offenses (d)	97	291	0.0309	0.0550	0.9507
Any Prior Charges for Theft (d)	97	291	0.1340	0.2165	1.7725
Any Prior Charges for Traffic Offenses (d)	97	291	0.3608	0.4467	1.4822
Any Prior Charges for Vandalism (d)	97	291	0.1134	0.0825	-0.9208
Any Prior Charges for Weapons Violations (d)	97	291	0.1546	0.1271	-0.6883
Any Prior Charges for Probation/Parole Violations (d)	97	291	0.0412	0.0378	-0.1520
Any Prior Charges for Other Property Offenses (d)	97	291	0.1546	0.1100	-1.1678
Any Prior Charges for Other Offenses (d)	97	291	0.1856	0.2027	0.3675
Any Prior Charges for Offenses With Missing Charges (d)	97	291	0.0103	0.0206	0.6606
Any Prior Charges for Violent Offenses (d)	97	291	0.4639	0.4948	0.5278
Any Prior Charges for Property Offenses (d)	97	291	0.4536	0.4089	-0.7720

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 interim controls are not included in these tests. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important pretreatment difference between observed and missing Time 2 interview cases.

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Table 22. Case Dispositions, Official Record Outcomes, and Missing Time 2 Interview Data

Case Dispositions and Official Record Outcomes	Observed N	Missing N	Observed Mean	Missing Mean	Z-Test
Number of Days Between Gateway Arrest and First Appearance (c)	97	291	29.1340	30.2509	0.6283
Number of Days Between Gateway Arrest and Case Disposition (c)	83	241	162.3253	179.9834	0.8283
Case Disposition = Guilty (d)	97	291	0.7320	0.6151	-2.0817
Case Disposition = Not Guilty (d)	97	291	0.0309	0.0584	1.0605
Case Disposition = Nolle Prosequi (d)	97	291	0.0928	0.1478	1.3766
Case Disposition = Death (d)	97	291	0.0000	0.0069	N/A
Case Disposition = Case Pending (d)	97	291	0.1443	0.1718	0.6318
Pre-Trial Intervention Program Status = Program Complete (d)	17	35	0.6471	0.4857	-1.0948
Pre-Trial Intervention Program Status = Terminated (d)	17	35	0.1176	0.2286	0.9521
Pre-Trial Intervention Program Status = Rejected (d)	17	35	0.0588	0.0571	-0.0244
Pre-Trial Intervention Program Status = Pending (d)	17	35	0.1765	0.2286	0.4315
Any Subsequent Arrest Record (d)	97	291	0.4124	0.4089	-0.0596
Number of Subsequent Arrests (Excluding Zeros) (c)	40	119	1.6750	1.6387	-0.2095
Number of Subsequent Arrests (Including Zeros) (c)	97	291	0.6907	0.6701	-0.1721
Any Subsequent Arrests in South Carolina (d)	97	291	0.4021	0.3883	-0.2402
Any Subsequent Arrests by Lexington County Sheriff's Department (d)	97	291	0.2990	0.2577	-0.7941
Any Subsequent Arrests in LCSD For Crimes Against the Same Victim (d)	96	289	0.1042	0.1003	-0.1075
Any Subsequent Charges for Alcohol Violations (d)	97	291	0.0412	0.0412	0.0000
Any Subsequent Charges for Assaults (d)	97	291	0.0103	0.0206	0.6606
Any Subsequent Charges for Burglary (d)	97	291	0.0206	0.0137	-0.4751
Any Subsequent Charges for Child Abuse (d)	97	291	0.0000	0.0103	1.0039
Any Subsequent Charges for FTA/FTP/Attorney Contempt (d)	97	291	0.0206	0.0034	-1.6731
Any Subsequent Charges for All Other Contempt (d)	97	291	0.1031	0.0997	-0.0975
Any Subsequent Charges for Disorder Offenses (d)	97	291	0.0515	0.0241	-1.3544
Any Subsequent Charges for Drug Offenses (d)	97	291	0.0722	0.0859	0.4262
Any Subsequent Charges for Domestic Violence (d)	97	291	0.1134	0.1271	0.3561
Any Subsequent Charges for Driving While Impaired (d)	97	291	0.0206	0.0241	0.1947
Any Subsequent Charges for Fraud (d)	97	291	0.0309	0.0309	0.0000
Any Subsequent Charges for Harassment (d)	97	291	0.0103	0.0103	0.0000
Any Subsequent Charges for Homicide (d)	97	291	0.0000	0.0000	N/A
Any Subsequent Charges for Kidnapping (d)	97	291	0.0103	0.0137	0.2599
Any Subsequent Charges for Motor Vehicle Theft (d)	97	291	0.0103	0.0069	-0.3346
Any Subsequent Charges for Robbery (d)	97	291	0.0000	0.0069	N/A
Any Subsequent Charges for Sex Offenses (d)	97	291	0.0103	0.0034	-0.8186
Any Subsequent Charges for Theft (d)	97	291	0.0206	0.0344	0.6772
Any Subsequent Charges for Traffic Offenses (d)	97	291	0.1031	0.1031	0.0000
Any Subsequent Charges for Vandalism (d)	97	291	0.0103	0.0137	0.2599
Any Subsequent Charges for Weapons Violations (d)	97	291	0.0000	0.0103	N/A
Any Subsequent Charges for Probation/Parole Violations (d)	97	291	0.0206	0.0309	0.5298
Any Subsequent Charges for Other Property Offenses (d)	97	291	0.0103	0.0069	-0.3346
Any Subsequent Charges for Other Offenses (d)	97	291	0.0206	0.0309	0.5298
Any Subsequent Charges for Offenses With Missing Charges (d)	97	291	0.0103	0.0000	N/A
Any Subsequent Charges for Violent Offenses (d)	97	291	0.1237	0.1512	0.6672
Any Subsequent Charges for Property Offenses (d)	97	291	0.0722	0.0928	0.6220

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 interim controls are not included in these tests. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important difference between observed and missing Time 2 interview cases.

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Table 23. Pretreatment Characteristics and Missing Time 1 or Time 2 Interview Data

Pretreatment Characteristic	Observed N	Missing N	Observed Mean	Missing Mean	Z-Test
Randomly Assigned to Treatment Condition (d)	95	293	0.5474	0.5017	-0.7738
Male Offender, Female Victim (d)	95	293	1.0000	1.0000	N/A
Offender Race = White (d)	95	291	0.7158	0.6942	-0.3993
Offender Race = Black (d)	95	291	0.2632	0.2234	-0.7964
Offender Race = Other (d)	95	291	0.0211	0.0825	2.0739
Offender Ethnicity = Hispanic (d)	93	284	0.0215	0.0845	2.0811
Offender Married (d)	93	287	0.4086	0.3937	-0.2547
Number of Children Reported by Offender at Booking (c)	95	291	1.8000	1.7045	-0.5085
Offender Reports Any Children at Booking (d)	95	291	0.7684	0.7285	-0.7682
Offender Reports Any Past Military Service (d)	94	291	0.0851	0.0997	0.4161
Number of Charges Booked at Gateway Arrest (c)	95	291	0.0851	0.0997	0.4161
Offender's Age at Time of Booking (c)	95	291	35.6105	34.5808	-0.7895
Years of Education Reported by Offender at Booking (c)	93	284	12.2151	11.7254	-1.8420
Days in Jail Between Gateway Arrest and Bond Release (c)	95	292	2.2000	1.5137	-1.4015
Offender Reports Being Born in South Carolina (d)	95	291	0.6842	0.5636	-2.0783
Offender Booked on Domestic Violence Charge (d)	95	291	0.9895	0.9931	0.3521
Offender Booked on Non-Domestic Violence Charge (d)	95	291	0.1579	0.1375	-0.4948
Age at First Arrest (Including Current Arrest if No Prior Arrests) (c)	95	291	24.4105	23.3540	-0.9230
Any Prior Arrest Record (Before Gateway Arrest) (d)	95	293	0.8105	0.8089	-0.0356
Number of Prior Arrests (Not Including Zeros) (c)	77	237	5.7922	5.6667	-0.1836
Number of Prior Arrests (Including Zeros) (c)	95	293	4.6947	4.5836	-0.1807
Number of Days Since Last Arrest (Among Those With Priors) (c)	77	237	1565.5584	1792.3629	-0.9853
Any Prior Arrest Record in South Carolina (d)	95	293	0.7789	0.7509	-0.5554
Any Prior Arrest Record by Lexington County Sheriff's Department (d)	95	293	0.5684	0.4471	-2.0574
Both Parties Arrested at Gateway Arrest (Dual Arrest) (d)	95	293	0.0000	0.0171	N/A
Number of Days Between Gateway Arrest and Record Search Date (c)	95	293	579.1263	534.1536	-2.2661
Any Prior Charges for Alcohol Violations (d)	95	293	0.2632	0.2560	-0.1391
Any Prior Charges for Assaults (d)	95	293	0.2947	0.3072	0.2289
Any Prior Charges for Burglary (d)	95	293	0.1053	0.1263	0.5456
Any Prior Charges for Child Abuse (d)	95	293	0.0105	0.0239	0.7966
Any Prior Charges for FTA/FTP/Attorney Contempt (d)	95	293	0.0211	0.0137	-0.5080
Any Prior Charges for All Other Contempt (d)	95	293	0.0526	0.0273	-1.1921
Any Prior Charges for Disorder Offenses (d)	95	293	0.3053	0.2491	-1.0797
Any Prior Charges for Drug Offenses (d)	95	293	0.4105	0.3788	-0.5511
Any Prior Charges for Domestic Violence (d)	95	293	0.2632	0.2321	-0.6166
Any Prior Charges for Driving While Impaired (d)	95	293	0.2947	0.3038	0.1664
Any Prior Charges for Fraud (d)	95	293	0.1789	0.1638	-0.3430
Any Prior Charges for Harassment (d)	95	293	0.0737	0.0580	-0.5507
Any Prior Charges for Homicide (d)	95	293	0.0105	0.0205	0.6333
Any Prior Charges for Kidnapping (d)	95	293	0.0105	0.0273	0.9441
Any Prior Charges for Motor Vehicle Theft (d)	95	293	0.0737	0.0683	-0.1806
Any Prior Charges for Robbery (d)	95	293	0.0526	0.0717	0.6450
Any Prior Charges for Sex Offenses (d)	95	293	0.0316	0.0546	0.9038
Any Prior Charges for Theft (d)	95	293	0.1368	0.2150	1.6684
Any Prior Charges for Traffic Offenses (d)	95	293	0.3579	0.4471	1.5283
Any Prior Charges for Vandalism (d)	95	293	0.1158	0.0819	-1.0016
Any Prior Charges for Weapons Violations (d)	95	293	0.1579	0.1263	-0.7860
Any Prior Charges for Probation/Parole Violations (d)	95	293	0.0421	0.0375	-0.2005
Any Prior Charges for Other Property Offenses (d)	95	293	0.1579	0.1092	-1.2637
Any Prior Charges for Other Offenses (d)	95	293	0.1895	0.2014	0.2525
Any Prior Charges for Offenses With Missing Charges (d)	95	293	0.0105	0.0205	0.6333
Any Prior Charges for Violent Offenses (d)	95	293	0.4632	0.4949	0.5376
Any Prior Charges for Property Offenses (d)	95	293	0.4632	0.4061	-0.9784

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 interim controls are not included in these tests. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important pretreatment difference between observed and missing interview cases at either Time 1 or Time 2.

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Table 24. Court Dispositions, Official Record Outcomes, and Missing Time 1 or Time 2 Interview Data

Case Dispositions and Official Record Outcomes	Observed N	Missing N	Observed Mean	Missing Mean	Z-Test
Number of Days Between Gateway Arrest and First Appearance (c)	95	293	28.7789	30.3584	0.8844
Number of Days Between Gateway Arrest and Case Disposition (c)	82	242	163.7317	179.4339	0.7322
Case Disposition = Guilty (d)	95	293	0.7368	0.6143	-2.1675
Case Disposition = Not Guilty (d)	95	293	0.0316	0.0580	1.0129
Case Disposition = Nolle Prosequi (d)	95	293	0.0947	0.1468	1.2934
Case Disposition = Death (d)	95	293	0.0000	0.0068	N/A
Case Disposition = Case Pending (d)	95	293	0.1368	0.1741	0.8494
Pre-Trial Intervention Program Status = Program Complete (d)	16	36	0.6250	0.5000	-0.8345
Pre-Trial Intervention Program Status = Terminated (d)	16	36	0.1250	0.2222	0.8210
Pre-Trial Intervention Program Status = Rejected (d)	16	36	0.0625	0.0556	-0.0991
Pre-Trial Intervention Program Status = Pending (d)	16	36	0.1875	0.2222	0.2830
Any Subsequent Arrest Record (d)	95	293	0.4105	0.4096	-0.0167
Number of Subsequent Arrests (Excluding Zeros) (c)	39	120	1.6667	1.6417	-0.1418
Number of Subsequent Arrests (Including Zeros) (c)	95	293	0.6842	0.6724	-0.0983
Any Subsequent Arrests in South Carolina (d)	95	293	0.4000	0.3891	-0.1895
Any Subsequent Arrests by Lexington County Sheriff's Department (d)	95	293	0.2947	0.2594	-0.6760
Any Subsequent Arrests in LCSD For Crimes Against the Same Victim (d)	94	291	0.0957	0.1031	0.2053
Any Subsequent Charges for Alcohol Violations (d)	95	293	0.0421	0.0410	-0.0490
Any Subsequent Charges for Assaults (d)	95	293	0.0105	0.0205	0.6333
Any Subsequent Charges for Burglary (d)	95	293	0.0211	0.0137	-0.5080
Any Subsequent Charges for Child Abuse (d)	95	293	0.0000	0.0102	N/A
Any Subsequent Charges for FTA/FTP/Attorney Contempt (d)	95	293	0.0211	0.0034	-1.7057
Any Subsequent Charges for All Other Contempt (d)	95	293	0.0947	0.1024	0.2156
Any Subsequent Charges for Disorder Offenses (d)	95	293	0.0526	0.0239	-1.4061
Any Subsequent Charges for Drug Offenses (d)	95	293	0.0737	0.0853	0.3584
Any Subsequent Charges for Domestic Violence (d)	95	293	0.1053	0.1297	0.6284
Any Subsequent Charges for Driving While Impaired (d)	95	293	0.0211	0.0239	0.1597
Any Subsequent Charges for Fraud (d)	95	293	0.0316	0.0307	-0.0422
Any Subsequent Charges for Harassment (d)	95	293	0.0000	0.0137	N/A
Any Subsequent Charges for Homicide (d)	95	293	0.0000	0.0000	N/A
Any Subsequent Charges for Kidnapping (d)	95	293	0.0105	0.0137	0.2347
Any Subsequent Charges for Motor Vehicle Theft (d)	95	293	0.0105	0.0068	-0.3578
Any Subsequent Charges for Robbery (d)	95	293	0.0000	0.0068	N/A
Any Subsequent Charges for Sex Offenses (d)	95	293	0.0105	0.0034	-0.8414
Any Subsequent Charges for Theft (d)	95	293	0.0211	0.0341	0.6398
Any Subsequent Charges for Traffic Offenses (d)	95	293	0.1053	0.1024	-0.0801
Any Subsequent Charges for Vandalism (d)	95	293	0.0105	0.0137	0.2347
Any Subsequent Charges for Weapons Violations (d)	95	293	0.0000	0.0102	N/A
Any Subsequent Charges for Probation/Parole Violations (d)	95	293	0.0211	0.0307	0.4932
Any Subsequent Charges for Other Property Offenses (d)	95	293	0.0105	0.0068	-0.3578
Any Subsequent Charges for Other Offenses (d)	95	293	0.0211	0.0307	0.4932
Any Subsequent Charges for Offenses With Missing Charges (d)	95	293	0.0105	0.0000	N/A
Any Subsequent Charges for Violent Offenses (d)	95	293	0.1158	0.1536	0.9109
Any Subsequent Charges for Property Offenses (d)	95	293	0.0737	0.0922	0.5531

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 interim controls are not included in these tests. Shaded lines indicate that the absolute value of the Z-test exceeds 1.5 and may be suggestive of an important difference between observed and missing interview cases at either Time 1 or Time 2.

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Table 25. Comparability of Observed and Missing Time 1 Interview Cases

Pretreatment Characteristics	Unweighted Observed Mean	Weighted Observed Mean	Missing Mean	Std. Difference (Before Weighting)	Std. Difference (After Weighting)
Randomly Assigned to Treatment Condition	0.551	0.548	0.492	-0.117	-0.113
Offender Race = White	0.703	0.709	0.692	-0.024	-0.037
Offender Race = Black	0.261	0.229	0.216	-0.109	-0.033
Offender Race = Other	0.036	0.062	0.084	0.172	0.081
Offender Race = Missing	0.000	0.000	0.008	0.090	0.090
Offender Ethnicity = Hispanic	0.022	0.053	0.095	0.249	0.145
Offender Ethnicity = Missing	0.014	0.012	0.036	0.115	0.131
Offender Married	0.434	0.369	0.377	-0.117	0.016
Offender Marital Status = Missing	0.014	0.009	0.024	0.062	0.101
Number of Children Reported by Offender at Booking	1.826	1.804	1.673	-0.102	-0.087
Number of Children = Missing	0.000	0.000	0.008	0.090	0.090
Offender Reports Any Past Military Service	0.117	0.099	0.085	-0.115	0.099
Offender Past Military Service = Missing	0.007	0.008	0.008	0.008	0.004
Number of Charges Booked at Gateway Arrest	1.536	1.436	1.218	-0.527	-0.361
Number of Charges = Missing	0.000	0.000	0.008	0.090	0.090
Offender's Age at Time of Booking	35.138	34.474	34.665	-0.045	0.018
Offender's Age at Time of Booking = Missing	0.000	0.000	0.008	0.090	0.090
Years of Education Reported by Offender at Booking	12.096	11.851	11.705	-0.174	-0.065
Years of Education = Missing	0.014	0.019	0.036	0.115	0.094
Days in Jail Between Gateway Arrest and Bond Release	2.109	1.655	1.446	-0.277	-0.087
Days in Jail = Missing	0.000	0.000	0.004	0.063	0.063
Offender Reports Being Born in South Carolina	0.609	0.579	0.585	-0.049	0.012
Location of Birth = Missing	0.000	0.000	0.008	0.090	0.090
Offender Booked on Non-Domestic Violence Charge	0.145	0.139	0.141	-0.011	0.005
Offender Booking Charges = Missing	0.000	0.000	0.008	0.090	0.090
Age at First Arrest (Including Current Arrest if No Prior Arrests)	24.203	23.714	23.286	-0.110	-0.051
Age at First Arrest = Missing	0.000	0.000	0.008	0.090	0.090
Number of Prior Arrests (Including Zeros)	4.580	4.476	4.628	0.009	0.028
Any Prior Arrest Record in South Carolina	0.725	0.707	0.776	0.123	0.166
Any Prior Arrest Record by Lexington County Sheriff's Department	0.486	0.479	0.472	-0.027	-0.015
Any Prior Charges for Alcohol Violations	0.239	0.264	0.268	0.065	0.009
Any Prior Charges for Assaults	0.283	0.278	0.316	0.072	0.082
Any Prior Charges for Burglary	0.109	0.116	0.128	0.058	0.035
Any Prior Charges for Child Abuse	0.029	0.029	0.016	-0.103	-0.106
Any Prior Charges for FTA/FTP/Attorney Contempt	0.022	0.021	0.012	-0.089	-0.081
Any Prior Charges for All Other Contempt	0.058	0.061	0.020	-0.271	-0.289
Any Prior Charges for Disorder Offenses	0.275	0.261	0.256	-0.044	-0.011
Any Prior Charges for Drug Offenses	0.420	0.400	0.368	-0.108	-0.065
Any Prior Charges for Domestic Violence	0.254	0.218	0.232	-0.051	0.034
Any Prior Charges for Driving While Impaired	0.283	0.311	0.312	0.063	0.002
Any Prior Charges for Fraud	0.167	0.106	0.168	0.004	0.165
Any Prior Charges for Harassment	0.080	0.061	0.052	-0.125	-0.041
Any Prior Charges for Homicide	0.007	0.006	0.024	0.109	0.120
Any Prior Charges for Kidnapping	0.007	0.005	0.032	0.140	0.151
Any Prior Charges for Motor Vehicle Theft	0.073	0.063	0.068	-0.018	0.019
Any Prior Charges for Robbery	0.051	0.068	0.076	0.095	0.029
Any Prior Charges for Sex Offenses	0.036	0.035	0.056	0.086	0.090
Any Prior Charges for Theft	0.167	0.211	0.212	0.111	0.002
Any Prior Charges for Traffic Offenses	0.362	0.386	0.460	0.196	0.148
Any Prior Charges for Vandalism	0.109	0.092	0.080	-0.106	-0.044
Any Prior Charges for Weapons Violations	0.152	0.153	0.124	-0.085	-0.088
Any Prior Charges for Probation/Parole Violations	0.044	0.035	0.036	-0.040	0.005
Any Prior Charges for Other Property Offenses	0.145	0.109	0.108	-0.119	-0.003
Any Prior Charges for Other Offenses	0.203	0.194	0.196	-0.017	0.005
Any Prior Charges for Offenses With Missing Charges	0.015	0.015	0.020	0.039	0.033
Any Prior Charges for Violent Offenses	0.464	0.459	0.500	0.072	0.082
Any Prior Charges for Property Offenses	0.464	0.430	0.396	-0.138	-0.069

(Continued)

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Table 25. Comparability of Observed and Missing Time 1 Interview Cases (Continued)

Pretreatment Characteristics	Unweighted Observed Mean	Weighted Observed Mean	Missing Mean	Std. Difference (Before Weighting)	Std. Difference (After Weighting)
Number of Subsequent Arrests (Including Zeros)	0.645	0.668	0.692	0.046	0.023
Any Subsequent Arrests in South Carolina	0.362	0.393	0.408	0.093	0.030
Any Subsequent Arrests by Lexington County Sheriff's Department	0.254	0.301	0.276	0.050	-0.055
Any Subsequent Charges for Alcohol Violations	0.036	0.030	0.044	0.038	0.066
Any Subsequent Charges for Assaults	0.014	0.008	0.020	0.039	0.087
Any Subsequent Charges for Burglary	0.014	0.021	0.016	0.012	-0.038
Any Subsequent Charges for Child Abuse	0.007	0.004	0.008	0.008	0.043
Any Subsequent Charges for FTA/FTP/Attorney Contempt	0.014	0.015	0.004	-0.166	-0.179
Any Subsequent Charges for All Other Contempt	0.080	0.085	0.112	0.102	0.087
Any Subsequent Charges for Disorder Offenses	0.036	0.045	0.028	-0.050	-0.100
Any Subsequent Charges for Drug Offenses	0.065	0.075	0.092	0.092	0.058
Any Subsequent Charges for Domestic Violence	0.109	0.117	0.132	0.069	0.045
Any Subsequent Charges for Driving While Impaired	0.014	0.020	0.028	0.082	0.051
Any Subsequent Charges for Fraud	0.022	0.034	0.036	0.076	0.009
Any Subsequent Charges for Harassment	0.007	0.003	0.012	0.044	0.082
Any Subsequent Charges for Kidnapping	0.014	0.014	0.012	-0.023	-0.015
Any Subsequent Charges for Motor Vehicle Theft	0.007	0.010	0.008	0.008	-0.022
Any Subsequent Charges for Robbery	0.007	0.007	0.004	-0.051	-0.049
Any Subsequent Charges for Sex Offenses	0.007	0.004	0.004	-0.051	-0.007
Any Subsequent Charges for Theft	0.014	0.013	0.040	0.130	0.137
Any Subsequent Charges for Traffic Offenses	0.094	0.106	0.108	0.044	0.007
Any Subsequent Charges for Vandalism	0.007	0.010	0.016	0.070	0.048
Any Subsequent Charges for Weapons Violations	0.000	0.000	0.012	0.110	0.110
Any Subsequent Charges for Probation/Parole Violations	0.043	0.034	0.020	-0.167	-0.097
Any Subsequent Charges for Other Property Offenses	0.007	0.004	0.008	0.008	0.049
Any Subsequent Charges for Other Offenses	0.022	0.012	0.032	0.058	0.113
Any Subsequent Charges for Violent Offenses	0.138	0.137	0.148	0.029	0.032
Any Subsequent Charges for Property Offenses	0.051	0.069	0.108	0.184	0.126
Any Subsequent Arrests in LCSD For Crimes Against the Same Victim	0.102	0.115	0.101	-0.005	-0.046
Same Victim Status = Missing	0.007	0.004	0.008	0.008	0.050
Number of Days Between Gateway Arrest and First Appearance	28.978	30.150	30.520	0.097	0.023
Days Between Gateway Arrest and Final Case Disposition	165.000	176.315	181.372	0.096	0.030
Days Between Gateway Arrest and Disposition = Missing	0.152	0.184	0.172	0.053	-0.032
Case Disposition = Guilty	0.717	0.665	0.604	-0.232	-0.124
Case Disposition = Not Guilty	0.022	0.016	0.068	0.184	0.205
Case Disposition = Noll Pros	0.109	0.135	0.148	0.111	0.037
Case Disposition = Death	0.000	0.000	0.008	0.090	0.090
Case Disposition = Case Pending	0.152	0.184	0.172	0.053	-0.032
Pre-Trial Intervention Program Status = Program Complete	0.080	0.077	0.068	-0.047	-0.036
Pre-Trial Intervention Program Status = Terminated	0.029	0.028	0.024	-0.033	-0.029
Pre-Trial Intervention Program Status = Rejected	0.007	0.004	0.008	0.008	0.041
Pre-Trial Intervention Program Status = Pending	0.022	0.036	0.032	0.058	-0.020
Pre-Trial Intervention = Not Applicable	0.862	0.855	0.868	0.017	0.039
Both Parties Arrested at Gateway Arrest (Dual Arrest)	0.007	0.006	0.016	0.070	0.081
Number of Days Between Gateway Arrest and Record Search Date	553.971	537.820	540.304	-0.076	0.014
Number of Days Since Last Arrest (Among Those With Priors)	1734.155	1747.008	1738.142	-0.002	0.004
Number of Days Since Last Arrest = Missing	0.203	0.219	0.184	-0.049	-0.090

Note: Rows with standardized statistics with absolute values exceeding 0.15 (either before or after weighting) are shaded.

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Table 26. Comparability of Observed and Missing Time 2 Interview Cases

Pretreatment Characteristics	Unweighted Observed Mean	Weighted Observed Mean	Missing Mean	Std. Difference (Before Weighting)	Std. Difference (After Weighting)
Randomly Assigned to Treatment Condition	0.546	0.541	0.502	-0.089	-0.079
Offender Race = White	0.722	0.719	0.687	-0.074	-0.069
Offender Race = Black	0.258	0.219	0.223	-0.083	0.011
Offender Race = Other	0.021	0.062	0.082	0.225	0.074
Offender Race = Missing	0.000	0.000	0.007	0.083	0.083
Offender Ethnicity = Hispanic	0.021	0.063	0.085	0.229	0.078
Offender Ethnicity = Missing	0.021	0.015	0.031	0.060	0.092
Offender Married	0.411	0.377	0.393	-0.036	0.032
Offender Marital Status = Missing	0.021	0.015	0.021	0.000	0.038
Number of Children Reported by Offender at Booking	1.835	1.763	1.692	-0.094	-0.047
Number of Children = Missing	0.000	0.000	0.007	0.083	0.083
Offender Reports Any Past Military Service	0.094	0.103	0.097	0.011	-0.020
Offender Past Military Service = Missing	0.010	0.009	0.007	-0.042	-0.025
Number of Charges Booked at Gateway Arrest	1.670	1.466	1.218	-0.721	-0.395
Number of Charges = Missing	0.000	0.000	0.007	0.083	0.083
Offender's Age at Time of Booking	35.784	34.739	34.516	-0.122	-0.021
Offender's Age at Time of Booking = Missing	0.000	0.000	0.007	0.083	0.083
Years of Education Reported by Offender at Booking	12.232	11.971	11.642	-0.238	-0.117
Years of Education = Missing	0.021	0.033	0.031	0.060	-0.014
Days in Jail Between Gateway Arrest and Bond Release	2.186	1.788	1.514	-0.255	-0.104
Days in Jail = Missing	0.000	0.000	0.003	0.059	0.059
Offender Reports Being Born in South Carolina	0.680	0.665	0.564	-0.234	-0.204
Location of Birth = Missing	0.000	0.000	0.007	0.083	0.083
Offender Booked on Non-Domestic Violence Charge	0.165	0.134	0.135	-0.088	0.004
Offender Booking Charges = Missing	0.000	0.000	0.007	0.083	0.083
Age at First Arrest (Including Current Arrest if No Prior Arrests)	24.619	23.994	23.277	-0.167	-0.089
Age at First Arrest = Missing	0.000	0.000	0.007	0.083	0.083
Number of Prior Arrests (Including Zeros)	4.629	4.171	4.605	-0.004	0.080
Any Prior Arrest Record in South Carolina	0.784	0.708	0.749	-0.079	0.094
Any Prior Arrest Record by Lexington County Sheriff's Department	0.577	0.504	0.443	-0.269	-0.122
Any Prior Charges for Alcohol Violations	0.268	0.280	0.254	-0.032	-0.059
Any Prior Charges for Assaults	0.289	0.245	0.309	0.045	0.138
Any Prior Charges for Burglary	0.103	0.106	0.127	0.072	0.063
Any Prior Charges for Child Abuse	0.010	0.006	0.024	0.090	0.120
Any Prior Charges for FTA/FTP/Attorney Contempt	0.021	0.009	0.014	-0.059	0.044
Any Prior Charges for All Other Contempt	0.052	0.055	0.027	-0.147	-0.167
Any Prior Charges for Disorder Offenses	0.299	0.257	0.251	-0.111	-0.014
Any Prior Charges for Drug Offenses	0.412	0.371	0.378	-0.071	0.015
Any Prior Charges for Domestic Violence	0.258	0.217	0.234	-0.057	0.039
Any Prior Charges for Driving While Impaired	0.289	0.283	0.306	0.037	0.049
Any Prior Charges for Fraud	0.175	0.111	0.165	-0.028	0.146
Any Prior Charges for Harassment	0.082	0.053	0.055	-0.120	0.008
Any Prior Charges for Homicide	0.010	0.007	0.021	0.072	0.096
Any Prior Charges for Kidnapping	0.010	0.006	0.027	0.105	0.131
Any Prior Charges for Motor Vehicle Theft	0.072	0.070	0.069	-0.014	-0.004
Any Prior Charges for Robbery	0.052	0.066	0.072	0.080	0.023
Any Prior Charges for Sex Offenses	0.031	0.021	0.055	0.105	0.149
Any Prior Charges for Theft	0.134	0.164	0.216	0.200	0.128
Any Prior Charges for Traffic Offenses	0.361	0.359	0.447	0.173	0.176
Any Prior Charges for Vandalism	0.113	0.080	0.082	-0.112	0.011
Any Prior Charges for Weapons Violations	0.155	0.140	0.127	-0.082	-0.040
Any Prior Charges for Probation/Parole Violations	0.041	0.029	0.038	-0.018	0.048
Any Prior Charges for Other Property Offenses	0.155	0.118	0.110	-0.143	-0.024
Any Prior Charges for Other Offenses	0.186	0.157	0.203	0.043	0.114
Any Prior Charges for Offenses With Missing Charges	0.010	0.014	0.021	0.072	0.048
Any Prior Charges for Violent Offenses	0.464	0.390	0.495	0.062	0.208
Any Prior Charges for Property Offenses	0.454	0.406	0.409	-0.091	0.007

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Table 26. Comparability of Observed and Missing Time 2 Interview Cases (Continued)

Pretreatment Characteristics	Unweighted Observed Mean	Weighted Observed Mean	Missing Mean	Std. Difference (Before Weighting)	Std. Difference (After Weighting)
Number of Subsequent Arrests (Including Zeros)	0.691	0.641	0.670	-0.020	0.028
Any Subsequent Arrests in South Carolina	0.402	0.365	0.388	-0.028	0.048
Any Subsequent Arrests by Lexington County Sheriff's Department	0.299	0.279	0.258	-0.094	-0.048
Any Subsequent Charges for Alcohol Violations	0.041	0.035	0.041	0.000	0.032
Any Subsequent Charges for Assaults	0.010	0.007	0.021	0.072	0.098
Any Subsequent Charges for Burglary	0.021	0.029	0.014	-0.059	-0.131
Any Subsequent Charges for Child Abuse	0.000	0.000	0.010	0.102	0.102
Any Subsequent Charges for FTA/FTP/Attorney Contempt	0.021	0.014	0.003	-0.293	-0.183
Any Subsequent Charges for All Other Contempt	0.103	0.082	0.100	-0.011	0.060
Any Subsequent Charges for Disorder Offenses	0.052	0.056	0.024	-0.179	-0.208
Any Subsequent Charges for Drug Offenses	0.072	0.089	0.086	0.049	-0.013
Any Subsequent Charges for Domestic Violence	0.113	0.110	0.127	0.041	0.051
Any Subsequent Charges for Driving While Impaired	0.021	0.026	0.024	0.022	-0.010
Any Subsequent Charges for Fraud	0.031	0.033	0.031	0.000	-0.012
Any Subsequent Charges for Harassment	0.010	0.003	0.010	0.000	0.074
Any Subsequent Charges for Kidnapping	0.010	0.012	0.014	0.029	0.015
Any Subsequent Charges for Motor Vehicle Theft	0.010	0.008	0.007	-0.042	-0.014
Any Subsequent Charges for Robbery	0.000	0.000	0.007	0.083	0.083
Any Subsequent Charges for Sex Offenses	0.010	0.012	0.003	-0.117	-0.147
Any Subsequent Charges for Theft	0.021	0.018	0.034	0.075	0.088
Any Subsequent Charges for Traffic Offenses	0.103	0.088	0.103	0.000	0.049
Any Subsequent Charges for Vandalism	0.010	0.008	0.014	0.029	0.049
Any Subsequent Charges for Weapons Violations	0.000	0.000	0.010	0.102	0.102
Any Subsequent Charges for Probation/Parole Violations	0.021	0.012	0.031	0.059	0.109
Any Subsequent Charges for Other Property Offenses	0.010	0.002	0.007	-0.042	0.053
Any Subsequent Charges for Other Offenses	0.021	0.012	0.031	0.059	0.108
Any Subsequent Charges for Violent Offenses	0.124	0.117	0.151	0.077	0.096
Any Subsequent Charges for Property Offenses	0.072	0.073	0.093	0.071	0.069
Any Subsequent Arrests in LCSD For Crimes Against the Same Victim	0.104	0.099	0.100	-0.013	0.006
Same Victim Status = Missing	0.010	0.007	0.007	-0.042	0.002
Number of Days Between Gateway Arrest and First Appearance	29.134	30.009	30.251	0.073	0.016
Days Between Gateway Arrest and Final Case Disposition	162.325	162.908	179.983	0.103	0.099
Days Between Gateway Arrest and Disposition = Missing	0.144	0.165	0.172	0.073	0.017
Case Disposition = Guilty	0.732	0.683	0.615	-0.240	-0.140
Case Disposition = Not Guilty	0.031	0.029	0.058	0.117	0.128
Case Disposition = Noll Pros	0.093	0.123	0.148	0.155	0.070
Case Disposition = Death	0.000	0.000	0.007	0.083	0.083
Case Disposition = Case Pending	0.144	0.165	0.172	0.073	0.017
Pre-Trial Intervention Program Status = Program Complete	0.113	0.132	0.058	-0.234	-0.312
Pre-Trial Intervention Program Status = Terminated	0.021	0.020	0.027	0.042	0.048
Pre-Trial Intervention Program Status = Rejected	0.010	0.006	0.007	-0.042	0.016
Pre-Trial Intervention Program Status = Pending	0.031	0.042	0.027	-0.021	-0.089
Pre-Trial Intervention = Not Applicable	0.825	0.801	0.880	0.169	0.242
Both Parties Arrested at Gateway Arrest (Dual Arrest)	0.000	0.000	0.017	0.132	0.132
Number of Days Between Gateway Arrest and Record Search Date	579.660	563.835	533.667	-0.254	-0.166
Number of Days Since Last Arrest (Among Those With Priors)	1587.139	1839.677	1787.038	-0.099	0.026
Number of Days Since Last Arrest = Missing	0.186	0.258	0.192	0.017	-0.166

Note: Rows with standardized statistics with absolute values exceeding 0.15 (either before or after weighting) are shaded.

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Table 27. Comparability of Observed and Missing Interview (Both Time 1 and Time 2) Cases

Pretreatment Characteristics	Unweighted Observed Mean	Weighted Observed Mean	Missing Mean	Std. Difference (Before Weighting)	Std. Difference (After Weighting)
Randomly Assigned to Treatment Condition	0.547	0.528	0.502	-0.091	-0.053
Offender Race = White	0.716	0.718	0.689	-0.057	-0.061
Offender Race = Black	0.263	0.231	0.222	-0.099	-0.023
Offender Race = Other	0.021	0.051	0.082	0.222	0.113
Offender Race = Missing	0.000	0.000	0.007	0.083	0.083
Offender Ethnicity = Hispanic	0.022	0.052	0.085	0.226	0.117
Offender Ethnicity = Missing	0.021	0.016	0.031	0.056	0.085
Offender Married	0.409	0.364	0.394	-0.030	0.061
Offender Marital Status = Missing	0.021	0.014	0.020	-0.004	0.048
Number of Children Reported by Offender at Booking	1.800	1.755	1.704	-0.062	-0.033
Number of Children = Missing	0.000	0.000	0.007	0.083	0.083
Offender Reports Any Past Military Service	0.085	0.097	0.100	0.048	0.008
Offender Past Military Service = Missing	0.011	0.010	0.007	-0.045	-0.040
Number of Charges Booked at Gateway Arrest	1.674	1.502	1.220	-0.724	-0.450
Number of Charges = Missing	0.000	0.000	0.007	0.083	0.083
Offender's Age at Time of Booking	35.611	34.790	34.581	-0.099	-0.020
Offender's Age at Time of Booking = Missing	0.000	0.000	0.007	0.083	0.083
Years of Education Reported by Offender at Booking	12.215	11.997	11.725	-0.226	-0.125
Years of Education = Missing	0.021	0.037	0.031	0.056	-0.035
Days in Jail Between Gateway Arrest and Bond Release	2.200	1.749	1.514	-0.262	-0.090
Days in Jail = Missing	0.000	0.000	0.003	0.059	0.059
Offender Reports Being Born in South Carolina	0.684	0.656	0.564	-0.243	-0.186
Location of Birth = Missing	0.000	0.000	0.007	0.083	0.083
Offender Booked on Non-Domestic Violence Charge	0.158	0.145	0.137	-0.059	-0.021
Offender Booking Charges = Missing	0.000	0.000	0.007	0.083	0.083
Age at First Arrest (Including Current Arrest if No Prior Arrests)	24.411	23.656	23.354	-0.130	-0.037
Age at First Arrest = Missing	0.000	0.000	0.007	0.083	0.083
Number of Prior Arrests (Including Zeros)	4.695	4.240	4.584	-0.021	0.064
Any Prior Arrest Record in South Carolina	0.779	0.731	0.751	-0.065	0.046
Any Prior Arrest Record by Lexington County Sheriff's Department	0.568	0.500	0.447	-0.244	-0.106
Any Prior Charges for Alcohol Violations	0.263	0.280	0.256	-0.016	-0.054
Any Prior Charges for Assaults	0.295	0.263	0.307	0.027	0.096
Any Prior Charges for Burglary	0.105	0.108	0.126	0.063	0.056
Any Prior Charges for Child Abuse	0.011	0.009	0.024	0.087	0.100
Any Prior Charges for FTA/FTP/Attorney Contempt	0.021	0.008	0.014	-0.064	0.053
Any Prior Charges for All Other Contempt	0.053	0.060	0.027	-0.155	-0.200
Any Prior Charges for Disorder Offenses	0.305	0.252	0.249	-0.130	-0.006
Any Prior Charges for Drug Offenses	0.411	0.370	0.379	-0.065	0.018
Any Prior Charges for Domestic Violence	0.263	0.207	0.232	-0.073	0.059
Any Prior Charges for Driving While Impaired	0.295	0.298	0.304	0.020	0.013
Any Prior Charges for Fraud	0.179	0.096	0.164	-0.041	0.184
Any Prior Charges for Harassment	0.074	0.050	0.058	-0.067	0.034
Any Prior Charges for Homicide	0.011	0.008	0.020	0.070	0.086
Any Prior Charges for Kidnapping	0.011	0.005	0.027	0.103	0.136
Any Prior Charges for Motor Vehicle Theft	0.074	0.070	0.068	-0.021	-0.007
Any Prior Charges for Robbery	0.053	0.062	0.072	0.074	0.036
Any Prior Charges for Sex Offenses	0.032	0.023	0.055	0.101	0.139
Any Prior Charges for Theft	0.137	0.184	0.215	0.190	0.076
Any Prior Charges for Traffic Offenses	0.358	0.370	0.447	0.179	0.156
Any Prior Charges for Vandalism	0.116	0.091	0.082	-0.123	-0.031
Any Prior Charges for Weapons Violations	0.158	0.154	0.126	-0.095	-0.084
Any Prior Charges for Probation/Parole Violations	0.042	0.033	0.038	-0.024	0.025
Any Prior Charges for Other Property Offenses	0.158	0.111	0.109	-0.156	-0.007
Any Prior Charges for Other Offenses	0.189	0.160	0.201	0.030	0.102
Any Prior Charges for Offenses With Missing Charges	0.011	0.018	0.020	0.070	0.015
Any Prior Charges for Violent Offenses	0.463	0.399	0.495	0.063	0.191
Any Prior Charges for Property Offenses	0.463	0.424	0.406	-0.116	-0.036

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Table 27. Comparability of Observed and Missing Interview (Both Time 1 and Time 2) Cases (Continued)

Pretreatment Characteristics	Unweighted Observed Mean	Weighted Observed Mean	Missing Mean	Std. Difference (Before Weighting)	Std. Difference (After Weighting)
Number of Subsequent Arrests (Including Zeros)	0.684	0.656	0.672	-0.012	0.016
Any Subsequent Arrests in South Carolina	0.400	0.370	0.389	-0.022	0.040
Any Subsequent Arrests by Lexington County Sheriff's Department	0.295	0.283	0.259	-0.081	-0.055
Any Subsequent Charges for Alcohol Violations	0.042	0.039	0.041	-0.006	0.010
Any Subsequent Charges for Assaults	0.011	0.006	0.020	0.070	0.102
Any Subsequent Charges for Burglary	0.021	0.025	0.014	-0.064	-0.097
Any Subsequent Charges for Child Abuse	0.000	0.000	0.010	0.102	0.102
Any Subsequent Charges for FTA/FTP/Attorney Contempt	0.021	0.013	0.003	-0.302	-0.172
Any Subsequent Charges for All Other Contempt	0.095	0.071	0.102	0.025	0.102
Any Subsequent Charges for Disorder Offenses	0.053	0.056	0.024	-0.188	-0.211
Any Subsequent Charges for Drug Offenses	0.074	0.078	0.085	0.042	0.027
Any Subsequent Charges for Domestic Violence	0.105	0.112	0.130	0.073	0.053
Any Subsequent Charges for Driving While Impaired	0.021	0.030	0.024	0.019	-0.038
Any Subsequent Charges for Fraud	0.032	0.044	0.031	-0.005	-0.079
Any Subsequent Charges for Harassment	0.000	0.000	0.014	0.117	0.117
Any Subsequent Charges for Kidnapping	0.011	0.012	0.014	0.027	0.015
Any Subsequent Charges for Motor Vehicle Theft	0.011	0.008	0.007	-0.045	-0.019
Any Subsequent Charges for Robbery	0.000	0.000	0.007	0.083	0.083
Any Subsequent Charges for Sex Offenses	0.011	0.012	0.003	-0.122	-0.145
Any Subsequent Charges for Theft	0.021	0.017	0.034	0.072	0.096
Any Subsequent Charges for Traffic Offenses	0.105	0.100	0.102	-0.009	0.007
Any Subsequent Charges for Vandalism	0.011	0.008	0.014	0.027	0.045
Any Subsequent Charges for Weapons Violations	0.000	0.000	0.010	0.102	0.102
Any Subsequent Charges for Probation/Parole Violations	0.021	0.013	0.031	0.056	0.100
Any Subsequent Charges for Other Property Offenses	0.011	0.006	0.007	-0.045	0.004
Any Subsequent Charges for Other Offenses	0.021	0.012	0.031	0.056	0.109
Any Subsequent Charges for Violent Offenses	0.116	0.118	0.154	0.105	0.099
Any Subsequent Charges for Property Offenses	0.074	0.084	0.092	0.064	0.027
Any Subsequent Arrests in LCSD For Crimes Against the Same Victim	0.096	0.101	0.103	0.024	0.008
Same Victim Status = Missing	0.011	0.006	0.007	-0.045	0.010
Number of Days Between Gateway Arrest and First Appearance	28.779	30.331	30.358	0.103	0.002
Days Between Gateway Arrest and Final Case Disposition	163.732	155.744	179.434	0.091	0.138
Days Between Gateway Arrest and Disposition = Missing	0.137	0.151	0.174	0.098	0.062
Case Disposition = Guilty	0.737	0.718	0.614	-0.252	-0.212
Case Disposition = Not Guilty	0.032	0.031	0.058	0.113	0.117
Case Disposition = Noll Pros	0.095	0.101	0.147	0.147	0.129
Case Disposition = Death	0.000	0.000	0.007	0.083	0.083
Case Disposition = Case Pending	0.137	0.151	0.174	0.098	0.062
Pre-Trial Intervention Program Status = Program Complete	0.105	0.109	0.061	-0.183	-0.197
Pre-Trial Intervention Program Status = Terminated	0.021	0.028	0.027	0.038	-0.002
Pre-Trial Intervention Program Status = Rejected	0.011	0.007	0.007	-0.045	-0.007
Pre-Trial Intervention Program Status = Pending	0.032	0.046	0.027	-0.026	-0.117
Pre-Trial Intervention = Not Applicable	0.832	0.810	0.877	0.139	0.205
Both Parties Arrested at Gateway Arrest (Dual Arrest)	0.000	0.000	0.017	0.132	0.132
Number of Days Between Gateway Arrest and Record Search Date	579.126	559.493	534.154	-0.248	-0.140
Number of Days Since Last Arrest (Among Those With Priors)	1565.558	1973.859	1792.363	-0.113	0.091
Number of Days Since Last Arrest = Missing	0.189	0.231	0.191	0.004	-0.101

Note: Rows with standardized statistics with absolute values exceeding 0.15 (either before or after weighting) are shaded.

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Table 28. Pretreatment Equivalence of Weighted Treatment and Control Cases (Weighting for Time 1 Interview Nonresponse)

Background Characteristic	Treatment Group Mean	Control Group Mean	Test Statistic
Categorical Variables			**Chi-Square**
Offender Race = White	0.7354	0.6696	0.6452
Offender Race = Black	0.1921	0.3114	2.5219
Offender Race = Other	0.0725	0.0190	1.4392
Offender Ethnicity = Hispanic	0.0510	0.0193	0.6386
Offender Married	0.4284	0.3713	0.4366
Offender Reports Any Children at Booking	0.7580	0.7731	0.0395
Offender Reports Any Past Military Service	0.0787	0.1436	1.4420
Offender Reports Being Born in South Carolina	0.5076	0.7000	4.7004
Offender Booked on Non-Domestic Violence Charge	0.1628	0.1171	0.5420
Any Prior Arrest Record (Before Gateway Arrest)	0.7801	0.8007	0.0763
Any Prior Arrest Record in South Carolina	0.7049	0.7295	0.0911
Any Prior Arrest Record by Lexington County Sheriff's Department	0.5142	0.4438	0.6205
Both Parties Arrested at Gateway Arrest (Dual Arrest)	0.0119	0.0000	N/A
Any Prior Charges for Alcohol Violations	0.2544	0.2469	0.0092
Any Prior Charges for Assaults	0.3575	0.1860	4.3884
Any Prior Charges for Burglary	0.1420	0.0762	1.2384
Any Prior Charges for Child Abuse	0.0530	0.0000	N/A
Any Prior Charges for FTA/FTP/Attorney Contempt	0.0080	0.0375	1.5653
Any Prior Charges for All Other Contempt	0.0338	0.0902	1.8559
Any Prior Charges for Disorder Offenses	0.3066	0.2219	1.1657
Any Prior Charges for Drug Offenses	0.4070	0.4144	0.0073
Any Prior Charges for Domestic Violence	0.2339	0.2394	0.0057
Any Prior Charges for Driving While Impaired	0.3528	0.2275	2.3195
Any Prior Charges for Fraud	0.1672	0.1017	1.3901
Any Prior Charges for Harassment	0.0680	0.0742	0.0218
Any Prior Charges for Homicide	0.0117	0.0000	N/A
Any Prior Charges for Kidnapping	0.0000	0.0141	N/A
Any Prior Charges for Motor Vehicle Theft	0.0738	0.0610	0.0866
Any Prior Charges for Robbery	0.0770	0.0373	0.7573
Any Prior Charges for Sex Offenses	0.0348	0.0370	0.0046
Any Prior Charges for Theft	0.1856	0.1908	0.0050
Any Prior Charges for Traffic Offenses	0.3290	0.4285	1.2997
Any Prior Charges for Vandalism	0.1247	0.0714	1.0902
Any Prior Charges for Weapons Violations	0.125	0.1864	0.8843
Any Prior Charges for Probation/Parole Violations	0.0326	0.0478	0.2131
Any Prior Charges for Other Property Offenses	0.1536	0.0959	1.0773
Any Prior Charges for Other Offenses	0.2328	0.1568	1.1614
Any Prior Charges for Offenses With Missing Charges	0.0000	0.0331	N/A
Any Prior Charges for Violent Offenses	0.5144	0.3970	1.7360
Any Prior Charges for Property Offenses	0.4976	0.3861	1.5810
Victim is a High School Graduate	0.7455	0.7746	0.1431
Victim Race = White	0.8274	0.7316	1.6277
Victim Race = Black	0.1185	0.2483	3.7936
Victim Race = Other Race	0.0569	0.1044	0.6688
Victim Race = Missing	0.0000	0.0000	N/A
Victim Ethnicity = Hispanic	0.0873	0.0000	N/A
Victim Reports Living With Offender at Time of Gateway Incident	0.7994	0.7923	0.0097
Offender-Victim Relationship = Offender is Husband	0.4201	0.4280	0.0079
Offender-Victim Relationship = Offender is Ex-Husband	0.0138	0.0310	0.4399
Offender-Victim Relationship = Offender is Boyfriend	0.3617	0.4330	0.6447
Offender-Victim Relationship = Offender is Ex-Boyfriend	0.1053	0.0267	3.1377
Offender-Victim Relationship = Other	0.0990	0.0813	0.1381
Numerical Variables			**T-Test**
Number of Days Between Gateway Arrest and Time 1 Interview	54.9048	48.2992	1.6400
Victim Age	34.0824	32.5004	0.8100
Number of Days Between Gateway Arrest and Record Search Date	564.7888	523.5532	1.3400
Number of Charges Booked at Gateway Arrest	1.2358	1.7960	-0.9800
Offender's Age at Time of Booking	34.6882	34.9788	-0.1600
Years of Education Reported by Offender at Booking	11.9579	12.0031	-0.1300
Days in Jail Between Gateway Arrest and Bond Release	1.9698	1.7951	0.3000
Number of Children Reported by Offender at Booking	1.7019	1.9538	-0.9300
Age at First Arrest (Including Current Arrest if No Prior Arrests)	23.6502	24.3568	-0.4500
Number of Prior Arrests (Excluding Zeros)	6.0127	5.4130	0.5800
Number of Prior Arrests (Including Zeros)	4.6904	4.3342	0.3900
Number of Days Since Last Arrest (Among Those With Priors)	1633.7840	1866.8646	-0.6500

Note: Rows with chi-square statistics exceeding 2.25 or t-test statistics exceeding 1.5 are shaded to indicate possible pretreatment imbalances.

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Table 29. Pretreatment Equivalence of Weighted Treatment and Control Cases (Weighting for Time 2 Interview Nonresponse)

Background Characteristic	Treatment Group Mean	Control Group Mean	Test Statistic
Categorical Variables			**Chi-Square**
Offender Race = White	0.7453	0.6920	0.3208
Offender Race = Black	0.2206	0.2823	0.4815
Offender Race = Other	0.0341	0.0257	0.0409
Offender Ethnicity = Hispanic	0.0347	0.0263	0.0386
Offender Married	0.4305	0.3695	0.3532
Offender Reports Any Children at Booking	0.7676	0.7853	0.0415
Offender Reports Any Past Military Service	0.0823	0.1117	0.2211
Offender Reports Being Born in South Carolina	0.5867	0.7850	3.9325
Offender Booked on Non-Domestic Violence Charge	0.1837	0.1264	0.5932
Any Prior Arrest Record (Before Gateway Arrest)	0.7686	0.8324	0.5349
Any Prior Arrest Record in South Carolina	0.7461	0.7900	0.2331
Any Prior Arrest Record by Lexington County Sheriff's Department	0.5483	0.5746	0.0640
Both Parties Arrested at Gateway Arrest (Dual Arrest)	0.0000	0.0000	N/A
Any Prior Charges for Alcohol Violations	0.2022	0.3532	2.6328
Any Prior Charges for Assaults	0.3407	0.2041	2.1302
Any Prior Charges for Burglary	0.1282	0.0746	0.6718
Any Prior Charges for Child Abuse	0.0169	0.0000	N/A
Any Prior Charges for FTA/FTP/Attorney Contempt	0.0156	0.0205	0.0380
Any Prior Charges for All Other Contempt	0.0196	0.0914	1.9672
Any Prior Charges for Disorder Offenses	0.3187	0.2538	0.4748
Any Prior Charges for Drug Offenses	0.3753	0.4356	0.3513
Any Prior Charges for Domestic Violence	0.2553	0.2399	0.0300
Any Prior Charges for Driving While Impaired	0.2962	0.2769	0.0421
Any Prior Charges for Fraud	0.2090	0.1019	2.0802
Any Prior Charges for Harassment	0.0835	0.0663	0.7746
Any Prior Charges for Homicide	0.0175	0.0000	N/A
Any Prior Charges for Kidnapping	0.0000	0.0205	N/A
Any Prior Charges for Motor Vehicle Theft	0.0703	0.0732	0.0028
Any Prior Charges for Robbery	0.0567	0.0530	0.0057
Any Prior Charges for Sex Offenses	0.0354	0.0205	0.1980
Any Prior Charges for Theft	0.1170	0.1694	0.4951
Any Prior Charges for Traffic Offenses	0.2574	0.4839	5.0608
Any Prior Charges for Vandalism	0.1410	0.0630	1.5337
Any Prior Charges for Weapons Violations	0.1074	0.2040	1.6844
Any Prior Charges for Probation/Parole Violations	0.0310	0.0471	0.1777
Any Prior Charges for Other Property Offenses	0.1769	0.1090	0.8772
Any Prior Charges for Other Offenses	0.2092	0.1425	0.6950
Any Prior Charges for Offenses With Missing Charges	0.0000	0.0244	N/A
Any Prior Charges for Violent Offenses	0.4892	0.3961	0.8102
Any Prior Charges for Property Offenses	0.4933	0.3815	1.1628
Victim is a High School Graduate	0.7936	0.7592	0.1535
Victim Race = White	0.7783	0.7757	0.0008
Victim Race = Black	0.1501	0.1950	0.3346
Victim Race = Other Race	0.0723	0.1132	0.3546
Victim Race = Missing	0.0000	0.0000	N/A
Victim Ethnicity = Hispanic	0.0691	0.0000	N/A
Victim Reports Living With Offender at Time of Gateway Incident	0.7302	0.7696	0.1859
Offender-Victim Relationship = Offender is Husband	0.4121	0.4180	0.0032
Offender-Victim Relationship = Offender is Ex-Husband	0.0190	0.0527	0.7078
Offender-Victim Relationship = Offender is Boyfriend	0.3230	0.3594	0.1270
Offender-Victim Relationship = Offender is Ex-Boyfriend	0.1277	0.0391	2.3146
Offender-Victim Relationship = Other	0.1182	0.1308	0.0334
Numerical Variables			**T-Test**
Number of Days Between Gateway Arrest and Time 1 Interview	53.0367	43.8932	2.2800
Number of Days Between Gateway Arrest and Time 2 Interview	177.6924	169.3781	0.7400
Number of Days Between Time 1 and Time 2 Interviews	123.5404	124.3410	-0.0700
Victim Age	35.1721	32.5997	1.1000
Number of Days Between Gateway Arrest and Record Search Date	591.4722	557.4100	1.0100
Number of Days Since Last Arrest (Among Those With Priors)	1546.2689	1747.4872	-0.5000
Number of Prior Arrests (Not Including Zeros)	5.7229	5.6109	0.0900
Number of Prior Arrests (Including Zeros)	4.3989	4.6705	-0.2500
Age at First Arrest (Including Current Arrest if No Prior Arrests)	25.0543	23.7767	0.6400
Days in Jail Between Gateway Arrest and Bond Release	2.2969	1.8489	0.5400
Years of Education Reported by Offender at Booking	12.1939	12.1446	0.1100
Offender's Age at Time of Booking	36.0649	34.9122	0.5000
Number of Charges Booked at Gateway Arrest	1.2841	2.0285	-0.8900
Number of Children Reported by Offender at Booking	1.7360	1.9170	-0.5600

Note: Rows with chi-square statistics exceeding 2.25 or t-test statistics exceeding 1.5 are shaded to indicate possible pretreatment imbalances.

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Table 30. Pretreatment Equivalence of Weighted Treatment and Control Cases (Weighting for Time 1 or Time 2 Interview Nonresponse)

Background Characteristic	Treatment Group Mean	Control Group Mean	Test Statistic
Categorical Variables			**Chi-Square**
Offender Race = White	0.7440	0.6854	0.3458
Offender Race = Black	0.2093	0.2876	0.7433
Offender Race = Other	0.0468	0.0270	0.1557
Offender Ethnicity = Hispanic	0.0474	0.0277	0.1496
Offender Married	0.4424	0.3152	1.4498
Offender Reports Any Children at Booking	0.7880	0.7717	0.0334
Offender Reports Any Past Military Service	0.0760	0.1098	0.2683
Offender Reports Being Born in South Carolina	0.5979	0.7504	2.0081
Offender Booked on Non-Domestic Violence Charge	0.1818	0.1144	0.7625
Any Prior Arrest Record (Before Gateway Arrest)	0.7508	0.8303	0.7217
Any Prior Arrest Record in South Carolina	0.7211	0.7887	0.4785
Any Prior Arrest Record by Lexington County Sheriff's Department	0.5032	0.5621	0.2870
Both Parties Arrested at Gateway Arrest (Dual Arrest)	0.0000	0.0000	N/A
Any Prior Charges for Alcohol Violations	0.2099	0.3447	1.8898
Any Prior Charges for Assaults	0.3301	0.2155	1.3653
Any Prior Charges for Burglary	0.1268	0.0831	0.4015
Any Prior Charges for Child Abuse	0.0176	0.0000	N/A
Any Prior Charges for FTA/FTP/Attorney Contempt	0.0113	0.0162	0.0636
Any Prior Charges for All Other Contempt	0.0210	0.0982	1.9794
Any Prior Charges for Disorder Offenses	0.2908	0.2582	0.1156
Any Prior Charges for Drug Offenses	0.3481	0.4348	0.6795
Any Prior Charges for Domestic Violence	0.2430	0.2198	0.0709
Any Prior Charges for Driving While Impaired	0.3012	0.2910	0.0103
Any Prior Charges for Fraud	0.1750	0.0839	1.9901
Any Prior Charges for Harassment	0.0603	0.0611	0.0004
Any Prior Charges for Homicide	0.0172	0.0000	N/A
Any Prior Charges for Kidnapping	0.0000	0.0162	N/A
Any Prior Charges for Motor Vehicle Theft	0.0682	0.0756	0.0179
Any Prior Charges for Robbery	0.0576	0.0584	0.0003
Any Prior Charges for Sex Offenses	0.0343	0.0182	0.2628
Any Prior Charges for Theft	0.1430	0.1860	0.2280
Any Prior Charges for Traffic Offenses	0.2593	0.4862	4.5906
Any Prior Charges for Vandalism	0.1294	0.0699	0.8550
Any Prior Charges for Weapons Violations	0.1105	0.2084	1.5560
Any Prior Charges for Probation/Parole Violations	0.0252	0.0506	0.4708
Any Prior Charges for Other Property Offenses	0.1591	0.1009	0.7135
Any Prior Charges for Other Offenses	0.1993	0.1431	0.4810
Any Prior Charges for Offenses With Missing Charges	0.0000	0.0320	N/A
Any Prior Charges for Violent Offenses	0.4622	0.3879	0.4749
Any Prior Charges for Property Offenses	0.4808	0.3956	0.6062
Victim is a High School Graduate	0.8027	0.7539	0.2980
Victim Race = White	0.7809	0.7714	0.0106
Victim Race = Black	0.1410	0.1897	0.4056
Victim Race = Other Race	0.0834	0.1253	0.2744
Victim Race = Missing	0.0000	0.0000	N/A
Victim Ethnicity = Hispanic	0.0736	0.0000	N/A
Victim Reports Living With Offender at Time of Gateway Incident	0.7314	0.7747	0.2152
Offender-Victim Relationship = Offender is Husband	0.4299	0.3868	2.0688
Offender-Victim Relationship = Offender is Ex-Husband	0.0195	0.0634	0.9290
Offender-Victim Relationship = Offender is Boyfriend	0.3077	0.3904	0.5957
Offender-Victim Relationship = Offender is Ex-Boyfriend	0.1206	0.0409	1.8001
Offender-Victim Relationship = Other	0.1222	0.1184	0.0030
Numerical Variables			**T-Test**
Victim Age	34.9952	32.3597	1.0900
Number of Days Between Time 1 and Time 2 Interviews	126.3546	122.9767	0.2800
Number of Days Between Gateway Arrest and Time 2 Interview	178.9613	166.3906	1.0100
Number of Days Between Gateway Arrest and Time 1 Interview	52.6068	43.4139	2.4300
Number of Days Between Gateway Arrest and Record Search Date	590.9394	541.9726	1.3800
Age at First Arrest (Including Current Arrest if No Prior Arrests)	24.3749	23.5506	0.4300
Number of Prior Arrests (Including Zeros)	4.1800	4.7484	-0.5200
Number of Days Since Last Arrest (Among Those With Priors)	1779.7194	1792.8728	0.0200
Days in Jail Between Gateway Arrest and Bond Release	2.1006	1.7770	0.4400
Years of Education Reported by Offender at Booking	12.0956	12.0945	0.0000
Offender's Age at Time of Booking	35.4474	34.8194	0.2700
Number of Charges Booked at Gateway Arrest	1.2699	1.9365	-0.8900
Number of Children Reported by Offender at Booking	1.7225	1.8360	-0.3600

Note: Rows with chi-square statistics exceeding 2.25 or t-test statistics exceeding 1.5 are shaded to indicate possible pretreatment imbalances.

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Table 31. 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.

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Table 32. 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).

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Table 33. 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.

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Table 34. 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).

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Table 35. Weighted Interview Outcomes (Time 1 and Time 2 Combined)

Combined Victim Interview Items	Treatment Group Mean	Control Group Mean	Chi-Square Test
Offender and Victim Living Together at Time of Either Interview	0.2999	0.3539	0.2793
Offender and Victim Have Lived Together Since Incident	0.4387	0.4281	0.0096
Victim Contacted Offender	0.8092	0.6600	2.4480
Offender Contacted Victim	0.8210	0.8676	0.3171
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	0.7160	0.4895	4.2473
Victim Reports Contact by Sheriff's Deputy	0.5465	0.3981	1.7822
Victim Reports Concerns About Safety	0.6080	0.5563	0.2275
Victim Reports Carrying Weapon For Self-Defense	0.3939	0.2101	0.7459
Victim Reports Any Psychological Aggression	0.7112	0.5645	1.9808
Victim Reports Any Physical Aggression	0.1568	0.1552	0.0004
Victim Reports Any Sexual Coercion	0.1655	0.1199	0.3597
Victim Reports Any Injury	0.1050	0.0705	0.3293
Victim Reports Any Stalking/Threats	0.7695	0.5791	3.4940

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

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Table 36. Combined Interview Regression Models Adjusting For Pretreatment Imbalances and Nonresponse Weights

Combined Time 1 - Time 2 Interview Outcomes	Treatment Coefficient	Odds Multiplier for Treatment	Treatment Coefficient Standard Error	Chi-Square Test
Offender and Victim Living Together at Time of Either Interview	-0.4788	0.6195	0.5059	0.8957
Offender and Victim Have Lived Together Since Incident	0.0880	1.0920	0.4699	0.0351
Victim Contacted Offender	0.9700	2.6379	0.5494	3.1176
Offender Contacted Victim	0.0763	1.0793	0.6749	0.0128
Victim Reports Contact by Law Enforcement Victim Advocate (LEVA)	1.0147	2.7585	0.4838	4.3980
Victim Reports Contact by Sheriff's Deputy	0.7246	2.0639	0.4770	2.3076
Victim Reports Concerns About Safety	0.4713	1.6021	0.4830	0.9523
Victim Reports Carrying Weapon For Self-Defense	0.5559	1.7435	0.5699	0.9516
Victim Reports Any Psychological Aggression	0.7121	2.0383	0.4937	2.0802
Victim Reports Any Physical Aggression	0.3116	1.3656	0.6206	0.2520
Victim Reports Any Sexual Coercion	1.0393	2.8272	0.6859	2.2959
Victim Reports Any Injury	0.8334	2.3011	0.7868	1.1221
Victim Reports Any Stalking/Threats	1.0366	2.8196	0.5344	3.7622

Note: The regressions reported in this table include the following predictor variables: treatment/control condition; any prior arrests for traffic offenses; and waiting time between gateway arrest and Time 1 Interview. Chi-square tests in this table only apply to the logistic regression coefficient for the treatment indicator variable in these models. The interim controls are not included in these tests. None of the reported treatment coefficients is statistically significant at two-tailed $p < .05$ significance level.