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PREVENTING REPEAT INCIDENTS OF FAMILY VIOLENCE:

A Reanalysis of Data from Three Field Tests

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Abstract

Preventing repeat victimization is one area of criminology that has shown particular promise in recent years. Based on the premise that persons victimized once are at higher risk than others of being victimized again, the British have developed successhl programs that focus crime prevention efforts on victims. This has been argued to be a sensible use of crime prevention resources and, indeed, a useful strategy for bringing down overall crime rates.

Of all crimes, domestic violence has the highest repeat rate, especially in the first few weeks after an incident is reported to the police. In the early 1980s, officials in New York City developed the first programs to try to reduce repeat incidents of family abuse. On three occasions thereafter between 1987 and 1997, three separate field tests aimed at public housing residents who reported family violence to the police were conducted in order to evaluate the basic approaches used in these and other prevention programs. The field-test interventions consisted of (a) a follow-up home visit to households reporting a domestic incident by a police officer and social worker and (b) a public education program using community meetings, posters, and flyers to educate participants about family violence. The three field tests had inconsistent results. One analysis suggested a beneficial effect of the program while another suggested that the program had iatrogenic effects. Since the composition of the samples vaned across studies (two used family violence incidents and the third elder abuse incidents), one possibility was that the prevention program had different effects with different populations.

The present paper combines the three databases to conduct a series of reanalyses to try to resolve earlier inconsistencies. The results of the reanalysis indicate that the interventions brought about an increase in reporting of new abusive incidents both to authorities and to research interviewers. We cannot say whether the higher reporting rates among people who received the interventions were due to increased incidents of abuse or greater sensitivity to abuse. The findings in the reanalysis were consistent across measures and across the three studies, indicating that increased reporting of abuse is not idiosyncratic to one of the samples, but holds across the three different types of samples used in these studies.

The results suggest an urgent need for more research on interventions designed to reduce repeat incidents of domestic violence, especially in light of current trends toward stronger and more coordinated intervention in family violence matters. In the meantime, our results suggest the need for monitoring and strong supervision of programs that intervene in households whose residents have recently reported domestic violence.

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Introduction

Background

One of the most promising areas of research in modem criminology is work on repeat victimization. For the past 25 years, victimization surveys have noted that a small percentage of the population experiences a relatively large proportion of all crime, and that one of the strongest predictors of victimization that researchers have isolated is being a victim on an earlier occasion (see, for example, Hindelang et al. 1978; Sorenson, Siegel, Golding and Stein, 1991). Reports by the Canada Solicitor General (1988), the National Board for Crime Prevention (1994), and others have shown that sexual assault survivors stand a 35 times greater chance of revictimization than non-victims (see also Messman-Moore and Long, 2000; Gold, Sinclair, and Balge, 1999; Muehlenhard, Highby, and Lee, 1998; Collins, 1998), robbery victims a nine times greater chance, and residential burglary victims a four times greater risk (see also Budd, 1999; Bowers, Hirschfield, and Johnson, 1998; Robinson, 1998). Even many commercial thefts target the same premises victimized within the past 30 days (Whitehead and Gray, 1998).

Criminologists who study repeat victimization have wondered whether victimization changes the probability of subsequent victimization (event dependency), or whether it operates as a marker of pre-existing risk (risk heterogeneity) (Sparks, 1981). In other words, is there some condition created by victimization that makes people more vulnerable to subsequent crime (for example, offenders revisiting an easy or rewarding target)? Or are certain individuals more vulnerable targets or more likely to be selected for victimization and for revictimization? Surprisingly, evidence points to both risk heterogeneity and event dependency as co-explanators of victimization (Ellingworth, Osbom, Trickett, and Pease, 1995; Lauritsen and Quinet, 1995).

The risk of revictimization is greatest in the period soon after the previous victimization for crimes as diverse as school crime, residential burglary, bias crime, domestic violence, auto crimes, neighbor disputes, and retail crimes (Farrell and Pease, 1993). In domestic violence cases, for example, the risk of revictimization is highest within the first 11 days and declines thereafter (Lloyd, Farrell, and Pease, 1994). In residential burglary, 40% of repeat burglaries occur within one month of the previous burglary (Anderson, Chenery, and Pease, 1995); after about six months the likelihood of repeat burglary returns to the average levels for a given area (Polvi, Looman, Humphnes, and Pease, 1991). These studies support the notion of event dependency in repeat victimization: That is, there is something about being victimized a first time that increases the risk of another victimization. For example, burglars visiting a house for the first time may note additional items worth coming back for.

Work on repeat victimization provides the rationale for the new American emphasis on proactive policing. "Hotspot" policing concentrates police attention on particular

locations that request disproportionate numbers of calls for service (Sherman and Buerger, 1989; Townsley, Homel, and Chaseling, 2000; Johnson, Bowers, and Hirschfield, 1997). Police resources are devoted to identify and solve problems at hot spot locations to prevent further crimes from occumng. Problem oriented policing (Goldstein, 1990) puts police officers in the role of problem solvers. A major responsibility of law enforcement officers becomes developing strategies to prevent repeat incidents from occurring at residences or business that have reported a crime. Even more recently, COMPSTAT, with its emphasis on crime mapping, has forced district commanders to discern and prevent recurring patterns of criminal activity (Bratton and Knobler, 1998).

Yet, ironically, the British have been much better at capitalizing on the practical implications of repeat victimization than the Americans. British criminologists and law enforcement administrators have realized that, if being victimized once is a good predictor of who will be victimized in the future, then it makes sense to concentrate crime prevention efforts on persons who report victimization to the authorities (Farrell and Pease, 1993). This is seen as an efficient use of police resources: "By pointing to the most probable times and places of future offenses, repeat victimization also helps identify the times and places where offenders may be found and apprehended. There is potential for the development of a symbiotic relationship between crime prevention and offender detection...." (National Board for Crime Prevention, 1994, page 2). The British also recognize that it is clear that any program incorporating the problem-solving approach to policing should pay special attention to repeat victims, who contribute disproportionately to an area crime statistics, especially in high-crime areas (Trickett, Osborne, Seymour, and Pease, 1992).

Neatly enough, people are likely to be especially receptive to crime prevention opportunities immediately following victimization. There is a "window of opportunity" during the first weeks after a crime during which victims feel vulnerable and are willing to consider seriously behavioral and lifestyle changes (Davis and Smith, 1994a; Anderson, Chenery, and Pease, 1995). Anderson, et al. (1995: p.3) recently argued that "Crime prevention and victim support are necessary for the *same* people (recent victims) at the *same* time (promptly after their victimization). *Reaction* to the last offence, if it has a preventive element, is *proaction* to the next."

A recent study found that all police forces surveyed had a repeat victimization strategy (Farrell, Edmunds, and Hobbs, 2000). Even the British insurance industry has been advised to recognize the significance of repeat victimization. Litton (2000) suggests that the industry is in a unique position to motivate its clients and to develop crime prevention programs for them.

In their work on repeat victimization, the British have pursued a model of interaction bitween research and practice (Anderson, Chenery, and Pease, 1995; Farrell and Pease 1993; see Farrell, 1995 for a recent summary). In crimes ranging from burglary to

domestic violence to racial violence, researchers have forged alliances with law enforcement authorities to define problems and assess the results of interventions (for reviews, see Pease, 1998; Pease and Laycock, 1996; Farrell, 1995).

This model integrating practice and research was used to develop a crime prevention program for victims of residential burglary on a housing estate. The strategies included replacement of coin operated gas and electricity meters (a frequent target), security upgrades, property marking, victim support and information, and "cocoon" neighborhood watch (involving the victim's six nearest neighbors). The evaluation showed a substantial reduction in burglary for the entire estate over the next three years, compared to itself and also compared to the remainder of the police subdivision (Pease, 1992). Similar programs for residential burglary victims in other British locales showed a decrease in repeat victimization without any evidence of displacement (Farrell, Chenery, and Pease, 1998; Tilley and Webb, 1994). Other programs have targeted commercial burglaries with encouraging results (Taylor, 1999).

In a West Yorkshire project, victims of burglary or automobile theft received an increasing level of police response based on the number of victimizations suffered in the previous year. Strategies included (as appropriate to a particular victim) security upgrades, property/vehicle marking, "cocoon" watch, focused patrol, offender targeting, priority fingerprinting, and the loan of burglar alarms and vehicle location devices (Anderson, Chenery, and Pease, 1995). In a domestic violence reduction project, victims received wearable alarms linked to the police by cellular phone, responding officers received en route information on prior calls and on current court orders, victim service workers offered support and developed an action plan with the victim, and lecture and discussion sessions were held with the police to raise their awareness of the issue of domestic violence and the police role. Evidence from victim interviews indicated that the pendant alarms greatly increased the recipients' sense of security (Lloyd, Farrell, and Pease, 1994). One British program even targeted repeat victims of hate crimes at a housing authority estate (Phillips and Sampson, 1998).

In this country, researchers and public officials have also begun to recognize the potential benefits of working with repeat victims. A recent National Research Council conference on crime prevention featured a panel on repeat victimization, and the Justice Department's Office for Victims of Crime has included repeat victimization in its national evaluation plan. Earlier, Davis and Smith (1994a) reported the results of a field test of a crime prevention program administered to recent victims. One hundred ninety one New York City victims of robbery, burglary, and non-sexual assault were divided into two groups using a quasi-experimental design. One group received traditional crisis counseling while the other received instruction in crime prevention and was offered free upgrades of home security hardware. Relative to the crisis counseling group, victims akigned to the crime prevention training were significantly more likely to believe that the crime could have been avoided, had significantly greater knowledge of crime

prevention principles, and were significantly more likely to engage in precautionary behaviors. Victims who had experienced the crime prevention training had a 33% lower rate of revictimization than controls over the next twelve months. However, the sample was small and the difference only attained marginal statistical significance. Recently, the Police Executive Research Forum has conducted a field test in three cities of a program to reduce repeat burglaries (Clarke, Perkins, and Smith, 2001).

Repeat victimization is a relatively rare phenomenon in some populations (see, for example, the work of Shaw and Pease, 2000, in Scotland) and common in others (see the study of U.S. adolescents by Menard, 2000). But it is most common in households which make domestic violence reports to the police. In domestic cases, the risk of revictimization is high and immediate: Chances of a new incident are highest during the first 11 days following the initial incident (Lloyd, Farrell, and Pease, 1994).

The earliest program that worked with victims to prevent repeat incidents of domestic violence was begun in New York City in the mid-1980s. The New York Housing Police Department and Victim Services (now Safe Horizon) began the Domestic Violence Intervention Education Project (DVIEP) as a response to family violence hot spots in New York City. DVIEP crisis response teams, each consisting of a police officer and a social worker, were dispatched to follow up on the initial police response to domestic complaints. The teams provided victims with information on services and legal options and warned perpetrators (when they were present) of the legal consequences of continued abuse. The intent was to empower victims to increase the social and personal costs to perpetrators of abusive behavior. DVIEP is an early example of now-popular coordinated approaches to domestic violence that focus on both abusers and victims. Advocates have argued that such approaches hold the best hope of reducing recidivism in households experiencing domestic violence (Hart, 1992).

Elements of the approach begun in New York have been further developed widely in both England and in the U.S. For example, one English program offered victims wearable alarms linked to the police, access to counseling by victim caseworkers, and community meetings designed to raise awareness of domestic violence and the police role (Lloyd, Farrell, and Pease, 1994). In the U.S., a program in DuPage County, Illinois, combines a tough law enforcement approach to domestic violence with advocacy for the victims of violence. The advocates offer support, give women information about the legal system, and inform them about further counseling and advocacy services that are available (Weisz et al., 1995). The availability of federal funds under the Violence Against Women Act and the stipulation that jurisdictions develop a coordinated response to domestic violence have further encouraged the promotion of the empowerment in the U.S., especially in its use of advocates who are summoned to victims' homes by the police to provide crisis counseling.

The New York Experiments

Preliminary research conducted on the British multidisciplinary programs to reduce repeat domestic violence has suggested that they are effective in reducing repeat calls to the police (Kelly, 1999; Harmer, Griffiths, and Jenvood, 1999), although designs have been weak. In New York, there were similar early efforts to evaluate the effectiveness of the DVIEP model in reducing repeat instances of domestic violence, but results were inconclusive. Therefore, administrators decided to engage in more rigorously designed research to deterniine the effectiveness of follow-up visits and educational campaigns—two elements of the original New York program.

Over a decade between late 1987 and mid-1997, three separate field tests were conducted in New York City public housing projects. All three tested the same basic intervention model: Persons who reported family violence to the police were randomly assigned to receive or not to receive a follow-up visit from a domestic violence police officer and a social worker. The personal, follow-up visit that was tested was different from the normal intervention of the programs operated by the police department and Victim Services, which consisted more often of telephone calls or letters than of home visits. Researchers and administrators decided to test the personal follow-up visits because these were thought to be the strongest practical form of programmatic response that might be widely implemented. In the first two field tests, there was as well a second experimental treatment. Public housing units included in the studies were randomly assigned to receive or not receive education about domestic violence through brochures, posters, and public meetings. (The interventions are described in greater detail below.)

The sampling frame for the first experiment (hereafter referred to as "the 1987 domestic violence study") was households in designated public housing units in Manhattan where someone had called the police in response to a family violence incident (this could be violence between romantic intimates, sibling violence, elder abuse, or other forms of violence between persons related or living under the same roof). The incidents were minor in nature, many not involving violation of criminal statutes: Only 7% of the incidents resulted in arrests and just 14% of victims reported any form of injury. Four hundred thirty-five victims were randomly assigned to receive a home visit as a follow-up to the patrol response. The control group received only the initial police patrol response. Additional calls for police services were tracked for both groups over the next six months. At the end of the tracking period, researchers attempted to interview victims to ask about new abuse, about satisfaction with the police response, and about the victims' knowledge and use of social services. Interviews were completed with 72% of the sample. This unusually high success rate for a domestic violence criminal justice sample was due in part to the low level of transience among New Yorkers living in public housing.

According to law enforcement records, households that received either the home visit intervention or public education about domestic violence were more likely to call the

police during the subsequent six months than households that did not receive the interventions. Yet, according to victim survey data, there were no differences between the two groups in abuse during the six months following the trigger incident. In the literature on the effectiveness of arrest on curbing violence, victim reports and calls to the police are usually both treated as imperfect indicators measuring an underlying constrict of actual violence. However, the two measures clearly are not synonymous. Many victimizations—and especially many family violence victimizations—are not reported to the police (Straw and Gelles, 1990; Harris and Associates, 1979; Dutton, 1995). In a 1997 Criminology paper (Davis and Taylor, 1997), we interpreted our pattern of results to mean that the experimental interventions did not affect actual violence levels, but did increase victims' confidence in the police and made victims more willing to report violence when it occurred. Indeed, that explanation is consistent with theory on which field test was based: Program administrators had hoped that victims who received the intervention would call the police more often because they would gain confidence that the police would help.

We conducted a second experimental investigation (reported in an NU Research in Brief; Davis and Medina, 2001) of the same interventions several years later, this time using a sample of 402 public housing residents who had reported elder abuse incidents to the police. Like the cases in the first field test, incidents in this study (hereafter referred to as the "elder abuse" study) were relatively minor. Five percent of the abusers were arrested, just 4% of victims reported any injuries, and in only 22% of the cases was a crime alleged to have occurred (the remainder were labeled by the police as verbal or family disputes). Once again, law enforcement records for these households were tracked for the next six months. As in the first experiment, we were successful in interviewing more than 70% of victims in the sample at the end of the six-month tracking period. Also as in the first experiment, we found that victims who received the home visit intervention called the police sooner and more often than controls. (Unlike the earlier study, however, there were no differences between households assigned to the home visit condition and those not.) Survey results showed that victims who received both home visits and public education were significantly more likely to report new abuse relative to those who received neither home visits nor public education.

We also conducted a third experiment (hereafter referred to as the domestic violence arrestee study), this time analyzing only the home visit intervention (and leaving out a public education treatment). This investigation, also conducted in a public housing setting, involved 197 victims of family violence. Unlike the earlier two studies, this field test was conducted on arrest cases. Twenty-two percent of abusers were charged with felonies, and the remainder with misdemeanors or violations. (The modal charge was third degree assault.) One in five of the victims reported some form of injury. We cbllected similar data to the other two experiments—new incidents reported on surveys or

reported to law enforcement agencies—six months after the trigger incident. Data from this study, for which we received no outside funds, are reported here for the first time.

Synthesizing the Findings

The first two studies in the series were consistent in the finding that households that were assigned to receive a home visit called the police more fkequently over the next six months than households that were assigned to a control condition. In the 1987 domestic violence study, the same was true for households assigned to the public education condition. In the first study, where we did not observe differences in violence reported on victim surveys, the results seemed to indicate that the interventions did not affect actual abuse, but encouraged victims to call the police when abuse occurred. However, in the second (elder abuse) study, where not only was more violence reported to the police, but more violence was reported on victim surveys by those who had received both interventions, the finding is more troubling. Since victim surveys are widely accepted as an indication of true incident rates, our results suggest that the interventions actually may have increased abuse, not just reporting of abuse.

As mentioned, there were differences between the samples, most notably in terms of age and nature of victidoffender relationship (see table, p. 11) that may account for the different results. The victims in the elder abuse study may have been involved in more of a dependent relationship with the abuser, making leaving a less likely option. If abusers of elderly relatives become angered by attempts to intervene, there may be no good escape for the victim.

The use of follow-up visits and public education to empower victims has become a common enhancement to the police patrol response to domestic violence, often funded by federal dollars. The intervention has usually been justified on the basis of reducing repeat incidents of violence. But at least some of our experimental data suggests that the intervention may be having the opposite effect. If that is the case, this is information that police departments and domestic violence coordinating councils need to know.

This paper reports on the results of a reanalysis that merges cases from the three experiments. The analysis conducted on the combined databases, with an N of nearly 1,000 cases, has sufficient statistical power to detect even small inter-group differences in outcome measures. It was our hope that by combining the databases, we would be able to develop consistent conclusions about the effects of the follow-up home visit and public education interventions. The three samples are diverse in terms of the nature of victim-perpetrator relationships (family violence or elder abuse) and types of incidents (minor incidents in two of the studies, many not rising to the level of crimes; assaults and other criminal incidents in the other). They also took place in a changing context, with a strong pro-arrest law going into effect in 1995, prior to the start of the elder abuse and domestic violence arrestee studies. The diversity in samples and context promotes greater

generalizeability of results from the combined analysis: Any results that hold up across the three studies would be robust and apply across a range of settings.

Methodology

Normally, meta-analytic techniques are used to synthesize the results of a series of experiments (e.g., Stanton and Shadish, 1997). However, in this case, we are in the fortunate position of having access to the original datasets and of having variables that are comparable across the sets. Therefore, we combined the databases from the three experiments to conduct a single analysis of the effects of the follow-up intervention. The sections below describe the program model that was the target of the evaluations, the process of creating comparable variables across the datasets, and the analysis strategy.

The Interventions

Home visit intervention. For designated incidents, a team consisting of a social worker and a police officer visited households within a few days of a domestic complaint. The team who visited the home attempted to edulcate the victim and perpetrator (if present) about the criminal and escalating nature of family violence and encourage the household to seek change through the use of courts and other services.

Home visits generally lasted 10-30 minutes, depending on whether the batterer was present and on the victim's receptiveness to assistance. During that time, victims were informed about services and about pursuing legal remedies, especially obtaining restraining orders. Referrals were made, most commonly to counseling services and support groups, drug and alcohol treatment programs for batterers, battered women shelters, home security improvement services, health care assessment and assistance, emergency financial assistance, assistance with relocating to other public housing units, and respite assistance for caregivers who batter seniors. On-the-spot crisis counseling was provided when indicated. (More details on thC home visits are available based on victim survey data in Davis and Medina, 2001, and Davis and Taylor, 1997.)

In cases where the complainant was not home in two tries, literature was left and/or phone contact made with the household. Contact was made with the household in 69% of cases in the 1987 domestic violence study and in 84% of the cases in the elder abuse study. (No information on outcomes of home visit attempts is available for the domestic violence arrestee study.) While it might be argued that this reduces the internal validity of the studies, we and other evaluators have argued (Davis and Smith, 1994b; Gartin, 1995) that the fact that an intended criminal justice intervention is not always actually delivered does not reflect a weakness of the experiment. The test was of a public policy intervention—a program to make *reasonable efforts* to conduct follow-up home visits within time and budgetary constraints. Only in a perfect world would every household have received the intended follow-up visit. Researching such a system might tell us about whether home visits work in theory, but would not inform us about a *public policy which attempts to conduct home visits.* This issue is discussed further in the conclusion.

Public education intervention. In the first two field tests, this intervention consisted of a leafleting and poster campaign and presentations at community and tenant association meetings. With cooperation of housing authority staff, leaflets were delivered to the door of every apartment in housing projects assigned to the public education treatment. Posters and additional leaflets were placed in common areas, such as around mailboxes and in housing managers' offices.

The leaflets contained information on the legal rights of victims, locations of emergency and long-term services, and the importance of the police in ending family violence. Presentations at community meetings were designed to familiarize housing project residents with the family violence team in the local Housing Police precinct and to educate the community about the nature of family violence.

The two interventions that made up the New York field tests were based primarily on an informational model: Making information available to victims about their rights and the services and legal options available to them was designed to empower victims. Empowered victims would then be emboldened to terminate untenable relationships or to work to end the battering within the context of the relationship. Secondarily, the interventions were designed to instill a sense of fear and circumspection in abusers.

Abstraction of Key Variables

We planned to use outcome measures based on criminal justice information sources and victim interviews. From criminal justice sources, each of the three databases contained the proportion of households reporting new domestic incidents to authorities within six months of the trigger incident, counts of the number of new calls for police services within six months following the trigger incident, and the time between the trigger incident and the first new incident of reported abuse.

In the first and third studies, victims were queried about the frequency and severity of violence in the past six months with the widely used Conflict Tactic Scale (CTS) (Straus, 1979). The scale includes the number of times victims were assaulted with a weapon; threatened with a weapon; hit, kicked, or shoved; had property damaged, or were harassed. Under the scoring system recommended by Straus (1979), weapon assault incidents were given weights of eight, followed by weapon threats (six), and hits, kicks or shoves (two). The rest of the items received a weight of one. In the elder abuse study, we used a variant of the CTS adapted by Pillemer and Finkelhor (1988) that included measures of physical, psychological, and financial abuse. The great majority of abusive incidents were minor in nature. Nearly all that resulted in injuries involved at most cuts or abrasions not requiring professional medical treatment.

From the abuse questions, we developed for all three datasets a measure of the proportion of victims reporting any new abuse and a measure of the frequency/severity of abuse. All three experiments also used similar versions of questions about victim use of social services (including legal assistance, victim services, church groups, courts,

shelters, and support groups) that generated counts of the number of services used, from 0 to 6.

The three datasets also had available comparably coded information on victim age and involvement in a romantic relationship with the abuser. These were important demographic variables to assess because the distributions of age and relationship to the abuser vaned across studies.

Table:
Comparison of Key Variables across Studies

Variable	Study 1 Domestic Violence N = **434**	Study 2 Elder abuse **N=406**	Study 3 Domestic violence **N = 197**
Demographics			
% romantic relationship	42%	19%	**69%**
Age	Mean = 39	Mean = 66	**Mean = 32**
Abuse reports to police			
% new report to police	38%	33%	**23%**
No. new reports to police	Mean = 0.86	Mean = 0.61	**Mean = 0.32**
Days to abuse (failures)	Mean = 50.4	Mean = 63.5	Mean = **92.1**
Abuse reports on survey			
% new report on survey	43%	34%	**28%**
Frequency/severity score	Mean = 7.19	Mean = 6.85	**N/A**
Services used (mean)	2.3	0.7	**1.8**

Outcome Measures

Prevalence of new abusive incidents. The basic measure we used to analyze effects of treatments upon recidivism was the proportion of cases in which new abusive incidents were reported to the police or to research interviewers. These data were analyzed using logistic regression models.

Frequency of new abusive incidents. It has been argued that the prevalence of an event may not be a good yardstick for understanding the impact of a social intervention and in particular criminal justice sanctions. Famngton (1987) argued that a single offense during a follow-up period does not necessarily mean the failure of a rehabilitation program or lack of deterrence since the intervention may still have caused fewer crimes per person. The CTS used in our study actually confounds frequency and severity of incidents (since some incidents are weighted more heavily than others). In addition to learning whether the interventions affected the proportion of households reporting a new abusive incident,

we also wanted to know whether the interventions mitigated or exacerbated the frequency or severity of incidents.

The CTS measure has a highly skewed distribution (most cases have no repeat incidents, while a few have many and severe repeats). When dealing with such data, the use of linear regression or ANOVA models can result in inefficient, inconsistent, and biased estimates since these tests assume a multivariate normal distribution of the experimental data. Therefore, we used negative binomial regression, developed to model distributions of failures where a majority of the sample does not fail at all during the time observed, while only a handful failed more often. The use of negative binomial was also appropriate because of high levels of overdispersion, i.e. conditional variances are greater than conditional means.

Latency to new abusive incidents. In addition to examining whether the interventions affected the proportion of households reporting new incidents and the fiequency/severity of incidents reported, we were also interested in knowing whether the interventions affected the time to the first new incident of abuse following the trigger incident. The time-to-failure analysis uses a Cox semi-parametric regression model to compare differences between treatment conditions in terms of hazard rates (hazard rates are estimates of the relative risk of failure per unit time, given that the case has survived until a particular instant). Our model introduces independent variables representing treatment designations and study (first, second, or third) in order to estimate whether the baseline hazard fimction is dependent on the level of each independent measure.

Use of social services. Use of services is a count variable with negatively skewed distributions similar to counts of new abusive incidents. These data were analyzed using negative binomial regression models, analogous to that described above for frequency of new abuse.

Results

Analytic Plan

The first and second studies each used a factorial design with home visits and public education as treatments. In the third study, the home visit was the single independent variable. Therefore, our primary analysis was with data from the first two studies, to determine whether the two factors separately or in concert affected recidivism and service utilization. Following presentation of the results of the two-study analysis, we present the more limited analyses that examine the effects of home visits based on data from all three studies.

For analysis of all outcome measures using the various techniques described above, we included terms representing the two experimental treatments and designation of which of the three studies each case came from. We also included nature of victirdoffender relationship and victim age as covariates.[1] Each table reporting multivariate results contains two statistical models—the first with only main effects and the second with main effects and terms representing interactions between treatments.

Two-Study Recidivism Results

Tables 1, 2, and 3 consider the direct and interactive effects of offering follow-up home visits and a public education program, based on the 1987 domestic violence and elder abuse studies that included both of these treatments.

Table 1 summarizes the new abuse measures for both the police reports and victim interviews broken down by levels of public education and home visits and by study. When the data from the two studies are combined (rightmost columns), the highest rates of officially-reported victimization occur in the cell which received both home visit and public education treatments. This is true for prevalence and frequency measures based on official data. It is also true for prevalence of abuse based on survey data. The only

[1] We compared case characteristics between experimental and treatment groups for each of the three studies. In the 1987 study, there were no significant differences between those assigned to public education or control in terms of victim and perpetrator age or gender; relationship between victim and perpetrator; prior arrests and complaints; and length of relationship. There were no significant differences between those who were and were not assigned to receive home visits based on victim or perpetrator age; relationship; victim gender; prior arrests and complaints; and length of relationship. There were significant differences between those assigned and not assigned to home visits based on perpetrator gender (25% of home visit perpetrators were female compared to 16% of controls). In the elder abuse study, experimental and control groups did not differ on victim injury; victim and perpetrator age; victim gender; perpetrator race; relationship between victim and perpetrator; and existence of an order of protection against the abuser. There were significant differences at the .05 level in terms of victim race (37% of victims in the home visit group were Hispanic versus 26% in the control group) and perpetrator gender (71% of pqetrators in the home visit group were male compared to 62% of control perpetrators). In the domestic violence arrestee study, no significant differences were detected between those assigned and not assigned to receive a home visit in terms of victim age and education; previous complaints filed with the police; nature of relationship between victim and perpetrator; and charge class (felony or misdemeanor).

exception is frequency of new abuse based on survey data, where the highest rate was among those receiving both treatments and the lowest rate was among those receiving a home visit, but not public education.

Table 2 reports the results from the multivariate tests of the two randomly assigned treatments based upon the police records. The upper portion of the table presents main effects of the two treatments. The results are remarkably consistent across measures. In five out of six comparisons, assignment to home visit or public education treatments led to significantly *greater* prevalence and frequency of police reports as well as *shorter* times to failure. The only comparison that was not statistically significant was time to failure according to levels of public education. But even this comparison just missed attaining statistical significance (p-value =.06). Beside the significant treatment effects we also found that increasing age of victims exerted a small but significant increase in the odds of failure. Every additional year in age added one percent to the odds of failing. In addition, those assigned to the elder abuse study reported significantly lower rates of failure on average compared to those in the 1987 domestic violence study.

The lower portion of Table 2 presents a model that includes interaction effects. In this model, all six main effect comparisons between levels of home visits and levels of public education reach statistical significance. The interaction of the two, however, was not significant, indicating that the effects of home visits and public education were additive.

Table 3 reports the results for the multivariate test of the treatment effects based upon the victim reports. Analysis of main effects presented in the top portion of the table shows significant increases in the frequency of abuse for those who were assigned to receive home visits (compared to those assigned not to receive home visits) and for those assigned to receive public education (compared to those assigned not to receive public education). Prevalence results are in the same direction, but not statistically significant. In addition, increasing age did not affect the prevalence of new abuse, but did significantly increase the frequency of abuse.

The lower portion of Table 3 displays interaction effects. With the interaction model, both treatment effects lead to non-significant increases in the prevalence of victimization, and the interaction of the two treatments produced a trend toward increased abuse that was nearly significant (p-value = 0.09).

Three-Study Recidivism Results

Table 4 reports bivariate results based on the official and victim interview data from all three studies. In 10 out of the 12 within-site bivariate treatment comparisons, the experimental group has greater prevalence, frequency, or shorter times-to-failure than the control group. Four of these 12 comparisons reached traditional levels of statistical significance. The four significant results were based upon officially recorded failures, and three of the four significant differences were found among the elder abuse experiment comparisons: Among the elder abuse victims, those assigned to receive home visits had

significantly greater failure rates regardless of the measures' parameter (prevalence, time-to-failure, or frequency).

The rightmost columns of Table 4 show the multi-study comparisons between those assigned to home visits to those who were not. A greater proportion of those assigned to home visits reported new violence, according to both official data and survey data. Those assigned to home visits also reported a higher frequency of abuse to the police and had shorter time to failure. The differences between the frequency of incidents and time-to-first-failure by treatment group were both statistically significant. Those assigned home visits had a mean rate that was about 63% greater than those not assigned to receive home visits, and those assigned home visits failed for the first time on average about 12 days before those not assigned to receive home visits. However, neither the prevalence measure based on official data nor the prevalence measures based on victim reports attained statistical significance.

The three-study multivariate analyses confirmed most of the bivariate analysis. Table 5 reports the results using official data. The multivariate analysis displayed in Table 5 showed that those assigned to receive home visits reported new abuse sooner and reported more frequent abuse than those not assigned to receive home visits. The same was true for the prevalence measure, but the effect attained only marginal statistical significance ($p = .065$).

In all of the models, victims in both the elder abuse and domestic violence arrestee studies were significantly less likely to fail on average compared to those in the 1987 domestic violence experiment. For the time to failure and the prevalence analysis, older victims were less likely to be victimized than younger victims. For the prevalence analysis, those with missing relationship status were significantly less likely to fail compared to the not mamed victim/offender dyads.

The lower portion of Table 5 adds an interaction term between treatment and study for each of the three outcome measures based on official data.[2] In the analyses with interaction terms, those assigned to home visits had a higher frequency of failure and shorter times to failure than households not assigned to the home visit condition. The results for the prevalence measure were in the same direction but, as in the model above without interaction terms, did not approach statistical significance. None of the treatment x study interaction terms were significant. Also as in the upper portion of the table, victims in the elder abuse study reported fewer failures on all three measures relative to victims in the 1987 domestic violence study.

Table 6 reports the analysis of prevalence of new abuse based on survey results. The proportion of victims reporting new abuse did not differ significantly according to whether victims were assigned to receive a home visit or not. Like the majority of other

[2] A test of the proportionality of the hazard rates by assigned treatment found that they were not proportional overtime. Therefore, the time to failure model with interaction terms adds an additional time-dependent parameter that addressed the non-proportionality problem.

comparisons, however, those assigned home visits had greater odds of victimization. In fact, for both outcome measures (victim interviews and police reports), victims assigned home visits had similarly greater odds (+29% and +28%) of failing compared to the control group. In addition, those assigned to the domestic violence arrestee treatment, particularly those in the control group, had smaller odds of victimization than those assigned to the 1987 control group.

Service Use Results

Table 7 reports the frequency of services received by the victims across the studies and treatment groups. The first set of comparisons shows the percentages of victims receiving different amounts of service by study and by whether or not the victims were assigned to get home visits. The second set of comparisons reports the percentage of victims receiving different amounts of service by study and by whether or not the victims were assigned to receive public education programming. Across the three studies, about one in three victims received at least one service after the initiating incident, but just about 5% received more than four services. On average each victim received about 1.6 services between the initial incident and the final victim interview.

The vast majority of victims in both the 1987 and the arrestee studies received at least one treatment and two-thirds received two or more services. The mean number of services received was 2.26 in the 1987 and 1.81 in the arrestee study. Among the victims in the elder abuse study, only a slight majority of victims received one or more services. The mean number of services received between the initial incident and the final victim interview by the elder abuse victims was just 0.74. Table 7 also shows that the difference in the number of services received according to experimental treatments within each study was not significantly different. Thus, on average, victims received about the same amount of services regardless of their assigned treatment.[3]

Table 8 reports the results from a multivariate negative binomial regression model of the quantity of services received. The model on the left compares quantity of services received between those who received home visits and those who did not across the three studies (victim relationship to abuser, victim age, and study were statistically controlled). The first model shows that the victims assigned to receive home visits only received slightly more services than those not receiving home visits. The model on the right, based only on the 1987 study and elder abuse data, considers whether assignment to public education adds to the quantity of services received. In this model, neither home visits nor public education nor the joint effect of both treatments significantly increased the number

[3] The between study conclusions regarding the number of services are based upon a three-way Bonferroni Multiple Comparison Test that was run in conjunction with a General Linear Model that also controlled for treatment assignment. All three comparisons reached statistical significance with p-values < 0.02. The treatment comparison (home visits vs. no home visits) was not significant (b= .16; p-value = ,124).

of services received by the victims. Thus, there is no evidence that either of the experimental interventions increased victims' use of services.

Home Visit Interventions Received versus Assigned

The consistent higher reporting of new abuse by victims assigned to receive a home visit is, perhaps, surprising, since the intervention was brief. Moreover, not every household assigned to the home visit condition received personal contact from the field team: In the 1987 study, 31% of households assigned to the home visit condition in fact received only literature because residents were not home during the two home visit attempts. In the elder abuse study, 16% received only literature.

In Table 9, we present a dose-response analysis, comparing recidivism rates between households where personal contact was established through a home visit and households assigned to receive home visits that received only literature. We would expect that if the home visit intervention is truly causing an increase in reported abuse between experimental and control households, then those households where personal contact was made should report more subsequent abuse than households where literature was left after unsuccessful home visit attempts.

The figure shows that in five of six comparisons those cases assigned to the home visit condition that received personal contact reported more abuse both to police and to research interviewers on the six-month survey. This was true in both studies for frequency of complaints made to the police, in both studies for fi-equencyheverity of abuse reports made to research interviewers, and in the 1987 study for prevalence of abuse reports made to research interviewers. The only exception to the pattern was for prevalence of abuse reported to research interviewers in the elder abuse study. Thus, we confirm that more potent "doses" of the home visit lead to higher rates of reporting of new abuse.

Discussion

The results of the reanalysis of data from three separate field tests of the same interventions unequivocally demonstrate that the interventions cause an increase in reporting of new abusive incidents to authorities and to research interviewers. On 14 out of 14 measures of new abuse included in the two reanalyses—both those based on official reports and on victim interviews—we found that those groups assigned to receive home visit or public education interventions reported more abuse than control groups. In seven out of 10 comparisons in the three-study analysis, differences between treatment levels reached statistical significance. In two of the four comparisons performed in the two-study analysis, results attained statistical significance. The fact that the findings were so consistent across the three studies indicates that increased reporting of abuse is not idiosyncratic to one of the samples, but holds across the three different types of samples used in these studies. However, we do not know the extent to which the results are applicable to similar programs in other places or to populations outside public housing.

Does increased reporting necessarily mean increased abuse, or could it be that persons who were assigned to the intervention groups had become more sensitized to abuse? In the report on the elder abuse results contained in Davis, Medina, and Avitabile (2000), some data are presented that argue against the sensitization hypothesis. But in the end, we cannot know for sure which alternative is correct. We were not expecting to find increased reports of abuse and so did not design the studies to distinguish between increases in abuse and increased sensitivity to abuse.

Going into these studies, we had assumed that the effects of the interventions would be to empower victims through information about their situation, available services, and legal options. The program logic model posited that new abuse would decline as victims extracted themselves from self-defeating relationships or worked with social services and criminal justice staff to develop strategies to end the abuse while staying in the relationship. However, we found no evidence that those who received the interventions were more likely to avail themselves of social or legal services, so the intervention could not have worked—at least not in the way intended.

There is a literature on desistence of domestic violence that may be useful as a lens through which to analyze the interventions. That literature suggests—-contrary to common myth—that careers of batterers are likely short or very sporadic (Feld and Straus, 1989; Maxwell, Garner, and Fagan, 2001; Langan and Innes, 1986; Quigley and Leonard, 1996). For those batterers who do not desist or reduce their abusive behavior over a period of time, Fagan (1989) argues that social and legal sanctions may be effective. According to Fagan, actions by victims that raise the personal or social costs to the batterer may promote a reduction or cessation in abuse. The intervention we ex'amined might be expected to promote desistance by empowering women to leave the relationship, demand change under threat of leaving, or inflict shame on the abuser.

Moreover, the mere physical presence of a police officer might have been expected to directly stigmatize those abusers who were present at the time of the home visit.

But Fagan also warns that sanctions may backfire and produce increased abuse, especially among those with chronic histories of abuse. That may be what happened in our sample, where it is possible that the combined interventions may have increased new abusive incidents by inciting abusers. We do not have direct evidence on this point since we did not interview abusers.

There is, however, some precedent for iatrogenic outcomes resulting from attempts to intervene in domestic violence. One is from the work of Ford (1991). He reports results from a true experiment that batterers who were prosecuted to conviction were significantly angrier than men whose cases were diverted or dropped.

Dugan, Nagin, and Rosenfeld (1999) found suggestions of an iatrogenic effect of victim services on domestic homicide rates. Dugan, et al. regressed domestic homicide rates on several measures including availability of services for victims. They found that the presence of hotline and legal advocacy services were associated with lower homicide rates of married males. However, availability of counseling services was *positively* correlated with homicides of both married males and unmarried females.

After years of debate, a recent report found a positive effect of arrest on reduction of intimate violence in NIJ's Spouse Abuse Replication Project (Maxwell, Gamer, and Fagan, 2001). However, Sherman's (1992) analysis of the SARP data found that among unemployed spouse abusers in Milwaukee, arrest actually *increased* the annual frequency of reported violence by 43%. For every 1,000 unemployed suspects, an arrest policy might cause more than 200 more reported acts of domestic violence per year. Sherman's analysis of data from two other SARP sites also indicated that arrest increases violence among unemployed spouse abusers

Finally, in one of the better-designed studies of batterer intervention, Harrell (l991) reported that a larger proportion of men assigned to batterer intervention programs committed new abuse compared to men assigned to a control group. Differences on two of the three measures attained statistical significance.

We caution the reader to consider several facts in interpreting our work. We tested specific elements of an intervention strategy based on empowering victims. The fact that we found these elements to increase reports of new abuse does not invalidate the empowerment model. All we can say is that the follow-up home visits as they were conducted for our research increased reports of new abuse. Had the visits been structured differently or had the follow up contact used telephone or letter modalities (as was the case with the program in regular use in New York), we might have found a different pattern of results.

Some might argue against combining data from the studies at all. Although the ikerventions were similar, the populations and offenses were quite different from one study to the next. Also, one of the outcome measures (frequency/severity of abuse) was

defined somewhat differently in the elder abuse study than in the other two. These are important differences. Nevertheless, we believe that combining the studies and finding clear differences by treatment increases the generalizeability of the findings. And including relevant demographics and study identity variables in the analyses should control for inter-study differences.

Our results strongly suggest the need for monitoring and careful supervision of programs using the techniques tested here until new and more comprehensive research can be conducted:This research should include measures of awareness of what constitutes abuse and qualitative interviews with victims who report new incidents of abuse to attempt to tease out the difference between actual abuse and increased sensitivity. It should also rank new incidents on a seventy scale to determine whether differences between treatment groups were due to serious incidents (which anyone would agree constitutes abuse) or to minor incidents (which some might not construe as abuse). Finally, the research ought to include interviews with abusers as well as victims to determine how abusers' attitudes toward victims, their emotional states, and their motivation to commit abuse might have been impacted by the interventions. This is research that would be difficult to do well because the abusers may prove difficult to locate, may be unwilling to be interviewed, and would have strong incentives not to give honest responses to all questions. Moreover, there are serious human subjects issues that would have to be addressed and resolved since we now suspect that victims in such a study who received interventions might be placed in some jeopardy. But such research would point the way forward out of the untenable position we are now in where well-intentioned services may place victims at risk. In the end, it is important that this research be conducted in order to provide the most effective and responsible service possible to domestic violence victims.

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Appendix: Tables

Table 1. Base Rates of Officially Recorded and Victim Reported Failure by Treatment Groups

	1987 DV		Elder Abuse		Both Studies	
	Public Education		Public Education		Public Education	
	No	Yes	No	Yes	No	Yes
Prevalence of Police Reports						
No Home Visit Grp.	32%	39%	**19%**	**34%**	**27%**	**37%**
Assigned Home Visits Grp.	37%	45%	39%	36%	38%	41%
Mean Frequency of Police Reports						
No Home Visit Grp.	**0.42**	**0.77**	**0.30**	**0.59**	**0.37**	**0.68**
Assigned Home Visits Grp.	**0.90**	1.35	**0.82**	0.69	0.86	1.04
Prevalence of Victimization						
No Home Visit Grp.	42%	39%	36%	26%	40%	33%
Assigned Home Visits Grp.	38%	52%	37%	37%	38%	45%
Mean Frequency of Victimization						
No Home Visit Grp.	6.59	7.15	5.25	**3.53**	5.99	5.48
Assigned Home Visits Grp.	**4.66**	**7.45**	4.78	**8.05**	**4.72**	**7.73**

Bolded values in boxes were statistically different from each other at p-value <0.05

Table 2. Rates of Officially Recorded Failures (2 study comparison)

	Prevalence (a)				Time-to-Failure (b)				Frequency (c)			
Model I. Direct Effects	b	s.e.	P-value	(Exp)b	b	s.e.	P-value	(Exp)b	b	s.e.	P-value	(Exp)b
Relationship (not married)												
Married	0.17	0.18	0.331	1.19	0.12	0.14	0.412	1.12	-0.15	0.16	0.349	0.86
Missing	-0.27	0.37	0.462	0.76	-0.25	0.30	0.415	0.78	0.03	0.19	0.890	1.03
Victim Interview	-0.29	0.16	0.078	0.75	0.00	0.13	0.984	1.00	-0.25	0.34	0.457	0.78
Victim's Age	**0.01**	**0.01**	**0.041**	**1.01**	**0.01**	**0.01**	**0.012**	**1.01**	**0.01**	**0.01**	**0.013**	**1.01**
Study (1987 DV)												
Elder Abuse	**-0.57**	**0.22**	**0.008**	**0.56**	**-0.41**	**0.17**	**0.015**	**0.66**	**-0.66**	**0.18**	**0.000**	**0.52**
Assigned Treatment (No Home Visit Grp.)												
Receive Home Visits	**0.34**	**0.15**	**0.026**	**1.40**	**0.29**	**0.12**	**0.018**	**1.33**	**0.57**	**0.14**	**0.000**	**1.76**
Assigned Treatment (No Public Education)												
Receive Public Education	**0.34**	**0.15**	**0.027**	**1.40**	0.22	0.12	0.068	1.25	**0.39**	**0.15**	**0.008**	**1.47**
Constant	**-1.45**	**0.35**	**0.000**	**0.23**					**-1.18**	**0.34**	**0.000**	**0.31**
Alpha									**1.81**	**0.46**	**0.000**	
Sigma									0.23	0.52	0.661	
Model II. Direct and Interaction Effects												
Study (1987 DV)												
Elder Abuse	**-0.57**	**0.22**	**0.008**	**0.56**	**-0.41**	**0.17**	**0.015**	**0.66**	**-0.67**	**0.18**	**0.000**	**0.51**
Assigned Treatment (No Treatment Grp.)												
Home Visits Only	**0.51**	**0.22**	**0.020**	**1.66**	**0.30**	**0.12**	**0.014**	**1.35**	**0.81**	**0.21**	**0.000**	**2.24**
Public Education Only	**0.35**	**0.15**	**0.022**	**1.42**	**0.24**	**0.12**	**0.048**	**1.27**	**0.64**	**0.23**	**0.005**	**1.90**
Home Visits + Public Ed.	-0.33	0.30	0.273	0.72	-0.30	0.24	0.210	0.74	-0.44	0.28	0.124	0.65
Constant	**-1.45**	**0.35**	**0.000**	**0.23**					**-1.30**	**0.34**	**0.000**	
Alpha									**1.81**	**0.46**	**0.000**	
Sigma									0.23	0.52	0.661	

Notes: a= ahgistic Regression Model; &Cox setrd-Parametric Regression Model; FNegative Binominal Regression M e 1

The models with the interaction tarns also contain all of the direct effects, but they are not reported here because they are areMy reported in the direct effects model. Bolded coeEcients are statistically signlfrcant at pvalue ~0.05

Table 3. Rates of Victimization (2 study comparison)

	Prevalence (a)				Frequency (c)			
Model I. Direct Effects	b	s.e.	P-value	(Exp)b	b	s.e.	P-value	(Exp)b
Relationship (not married)								
Married	0.34	0.20	0.084	1.41	0.09	0.12	0.485	1.09
Missing	-0.08	0.48	0.873	0.93	**-0.82**	**0.39**	**0.034**	**0.44**
Victim's Age	0.00	0.01	0.739	1.00	**-0.02**	**0.00**	**0.000**	**0.98**
Study (1987 DV)								
Elder Abuse	-0.26	0.24	0.293	0.78	**-0.75**	**0.16**	**0.000**	**0.47**
Assigned Treatment (No Home Visit Grp.)								
Receive Home Visits	0.25	0.17	0.153	1.28	**0.41**	**0.11**	**0.000**	**1.51**
Assigned Treatment (No Public Education)								
Receive Public Education	0.05	0.17	0.773	1.05	**0.43**	**0.10**	**0.000**	**1.54**
Constant	-0.37	0.40	0.354		**1.28**	**0.24**	**0.000**	
Alpha					**0.08**	**0.03**	**0.003**	
Sigma					**1.83**	**0.07**	**0.000**	
Model II. Direct and Interaction Effects								
Study (1987 DV)								
Elder Abuse	-0.25	0.24	0.312	0.78	**-0.93**	**0.27**	**0.001**	**0.40**
Assigned Treatment (No Treatment Grp.)								
Home Visits Only	0.25	0.18	0.158	1.28	-0.12	0.25	0.624	0.89
Public Education Only	0.02	0.17	0.911	1.02	-0.16	0.26	0.545	0.86
Home Visits + Public Ed.	0.59	0.35	0.088	1.81	0.62	0.35	0.074	1.86
Constant	-0.36	0.40	0.360		**1.64**	**0.45**	**0.000**	
Alpha					0.33	0.31	0.289	
Sigma					**1.68**	**0.16**	**0.000**	

Notes: a= zbgstic kgpmon Me]; ?Negative Binolrdnal Regression Me 1

The mdek with the interaction term also contain all of the dmct effects, but they are not reported here because they are already reported m the direct effects model. Bolded coefficients are statistically sigmficmt at pvalue c0.05

Table 4. Rates of Officially Recorded and Victim Reported Failures by Treatment and Study

	1987 DV		Elder Abuse		DV Arrestee			Multi-Study	
Full Sample	No Home Visits	Home Visits	No Home Visits	Home Visits	No Home Visits	Home Visits		No Home Visits	Home Visits
N with Police Information	205	223	181	191	69	108		455	522
Prevalence of Recidivism	35.6%	40.8%	**27.1%**	**37.7%**	24.6%	22.2%		30.5%	35.8%
(a) Within study P-value	0.269		0.029		0.710			0.081	
(a) Between study P-value							0.001		
Mean Frequency of Recidivism	**0.58**	**1.13**	**0.45**	**0.75**	0.33	0.31		**0.46**	**0.73**
(b) Within study P-value	0.001		0.013		0.779			0.000	
(b) Between study P-value							0.000		
Mean N of Days to First-Incident	157.8	146.1	**151.6**	**134.4**	163.9	158.2		**156.2**	**144.2**
(c) Within study P-value	0.096		0.015		0.955			0.032	
(c) Between study P-value							0.001		
Victim Interview Sample									
N with Victim Interviews	144	164	123	149	32	54		303	371
% of Study Sample	69.6%	72.2%	**64.8%**	**73.9%**	42.7%	44.3%		63.4%	66.7%
(a) Within study P-value	0.539		0.047		0.826			0.261	
(a) Between study P-value							0.000		
Prevalence of Victimization	41.0%	45.1%	30.9%	36.9%	25.0%	29.6%		35.1%	39.5%
(a) Within study P-value	0.463		0.298		0.644			0.240	
(a) Between study P-value							0.013		

Notes: a = Pearson Chi-Square = GLM F-test; c=Kadatleier Breslow Test;
Bolded percentages are statisticy different from each other at p-value <0.05

Table 5. Rates of Officially Recorded Failures (3 study comparison)

	Prevalence (a)				Time-to-Failure (b)				Frequency (c)			
Model I. Direct Effects	b	s.e.	P-value	(Exp)b	b	s.e.	P-value	(Exp)b	b	s.e.	P-value	(Exp)b
Relationship (not married)												
Married	0.21	0.17	0.216	1.23	0.14	0.13	0.285	1.15	0.06	0.18	0.740	1.06
Missing	**-0.58**	**0.29**	**0.050**	**0.56**	-0.41	0.25	0.099	0.67	-0.47	0.25	0.061	0.62
Victim Interview	-0.21	0.16	0.187	0.81	0.04	0.13	0.756	1.04	-0.07	0.15	0.629	0.93
Victim's Age	**0.01**	**0.01**	**0.046**	**1.01**	**0.01**	**0.00**	**0.014**	**1.01**	**0.01**	**0.00**	**0.009**	**1.01**
Study (1987 DV)												
Elder Abuse	**-0.52**	**0.21**	**0.012**	**0.59**	**-0.38**	**0.16**	**0.022**	**0.69**	**-0.64**	**0.17**	**0.000**	**0.53**
DV Arrestee	**-0.49**	**0.24**	**0.042**	**0.62**	-0.35	0.20	0.079	0.71	**-0.76**	**0.24**	**0.001**	**0.47**
Assigned Treatment (No Home Visit Grp.)												
Receive Home Visits	0.26	0.14	0.065	1.29	**0.24**	**0.11**	**0.036**	**1.27**	**0.47**	**0.13**	**0.000**	**1.60**
Constant	**-1.38**	**0.27**	**0.000**	**0.25**					**-0.90**	**0.28**	**0.001**	**0.41**
Alpha									1.85	0.44	0.000	
Sigma									0.21	0.54	0.702	
Model II. Direct and Interaction Effects												
Study (1987 DV)												
Elder Abuse	**-0.54**	**0.21**	**0.009**	**0.58**	**-0.40**	**0.17**	**0.017**	**0.67**	**-0.58**	**0.24**	**0.015**	0.56
DV Arrestee	-0.43	0.24	0.078	0.65	-0.29	0.20	0.156	0.75	-0.38	0.38	0.326	0.69
Assigned Treatment (No Treatment Grp.)												
Home Visits + 1987 DV	0.17	0.16	0.282	1.19	**0.52**	**0.20**	**0.012**	**1.68**	**0.59**	**0.19**	**0.002**	1.80
Home Visits + Elder Abuse	0.32	0.30	0.295	1.37	0.25	0.25	0.313	1.28	-0.65	0.46	0.158	0.52
Home Visits + DV Arrestee	-0.37	0.42	0.382	0.69	-0.33	0.36	0.353	0.72	-0.09	0.28	0.755	0.92
Time x Treatment					**-0.33**	**0.36**	**0.353**	**0.72**				
Constant	**-1.38**	**0.27**	**0.000**						**-0.94**	**0.28**	**0.001**	
Alpha									**1.88**	**0.44**	**0.000**	
Sigma									0.13	0.84	0.872	

Notes: a= bgstic Regession Model; b=Cox Sani-Pammetric Regession Model; FNegaIive Binominal Fkgression Model

?he models with the interaction tams also contain all of the direct effects, but they are not reported here because they are already reported m the direct effects model. Bolded coefficients are statktically signifimt at pvalue ~0.05

Table 6. Prevalence of Victimization (3 study comparison)

	Prevalence (a)			
Model I. Direct Effects	b	s.e.	P-value	(Exp)b
Relationship (not married)				
Married	0.26	0.19	0.170	1.29
Missing	-0.14	0.48	0.772	0.87
Victim's Age	-0.01	0.01	0.293	0.99
Study (1987 DV)				
Elder Abuse	-0.16	0.24	0.501	0.85
DV Arrestee	**-0.80**	**0.27**	**0.004**	**0.45**
Assigned Treatment (No Home Visit Grp.)				
Receive Home Visits	0.25	0.16	0.136	1.28
Constant	-0.39	0.36	0.270	0.68
Model II. Direct and Interaction Effects				
Study (1987 DV)				
Elder Abuse	-0.16	0.24	0.492	0.85
DV Arrestee	**-0.80**	**0.28**	**0.005**	**0.45**
Assigned Treatment (No Treatment Grp.)				
Home Visits + 1987 DV	0.24	0.21	0.244	1.27
Home Visits + Elder Abuse	0.06	0.35	0.860	1.06
Home Visits + DV Arrestee	-0.01	0.56	0.991	0.99
Constant	-0.39	0.36	0.271	0.675

Notes: a= Logistic Regression Model
The models with the interaction terms also contain all of the direct effects, but they are not reported here because they are already reported in the direct effects model.
Bolded coefficients are statistically significant at p-value <0.05

Table 7. Quantity of Services Received by the Victim by Study and Treatment Groups

	1987 DV					Elder Abuse					DV Arrestee		
	Home Visits		Public Education			Home Visits		Public Education			Home Visits		
N Services	%No	%Yes	%No	%Yes	%Total	%No	%Yes	%No	%Yes	%Total	%No	%Yes	%Total
0	4.8	5.4	5.2	5.1	5.1	56.5	51.7	54.5	53.3	53.9	12.5	11.1	11.6
1	38.8	35.5	38.7	35.4	37.1	29.8	25.2	26.9	27.7	27.3	40.6	27.8	32.6
2	23.1	22.9	24.5	21.5	23.0	9.7	13.6	11.2	12.4	11.8	25.0	35.2	31.4
3	14.3	15.1	12.3	17.1	14.7	2.4	6.8	4.5	5.1	4.8	15.6	14.8	15.1
4	8.8	9.0	8.4	9.5	8.9	1.6	2.7	3.0	1.5	2.2	3.1	7.4	5.8
5	5.4	6.0	6.5	5.1	5.8						3.1	0.0	3.5
6	4.1	4.2	4.5	3.8	4.2								
7		1.8		1.8	1.0								
8													
9	0.7		0.7		0.3								
N	147	166	155	158	313	124	147	134	137	271	32	54	86
Chi-Sqr.	4.16		6.17			4.77		0.89			2.38		
P-value	0.84		0.63			0.31		0.93			0.80		
Mean	2.21	2.31	2.17	2.35	2.26	0.63	0.84	0.75	0.74	0.74	1.66	1.91	1.81
Std Dev.	1.59	1.64	1.53	1.69	1.61	0.88	1.07	1.02	0.96	0.99	1.18	1.23	1.21

Table 8. Quantity of Service Received by the Victim

	Three Studies (a)			Two Studies (a)		
Model I. Direct Effects	b	s.e.	P-value	b	s.e.	P-value
Relationship (not married)						
Married	0.11	0.08	0.148	0.10	0.08	0.224
Missing	-0.07	0.20	0.728	-0.06	0.21	0.774
Victim's Age	0.00	0.00	0.490	0.00	0.00	0.453
Study (1987 DV)						
Elder Abuse	**-1.13**	**0.10**	**0.000**	**-1.14**	**0.11**	**0.000**
DV Arrestee	-0.28	0.12	0.023			
Assigned Treatment (No Home Visit Grp.)						
Receive Home Visits	0.12	0.07	0.087	0.11	0.08	0.150
Assigned Treatment (No Public Education)						
Receive Public Education				0.05	0.07	0.498
Constant	**0.64**	**0.13**	**0.000**	**0.61**	**0.14**	**0.000**
Alpha	0.06	0.04	0.177	0.08	0.05	0.097
Model II. Direct and Interaction Effects						
Study (1987 DV)						
Elder Abuse	**-1.27**	**0.15**	**0.000**	**-1.14**	**0.11**	**0.000**
DV Arrestee	-0.36	0.20	0.073			
Assigned Treatment (No Treatment Grp.)						
Home Visits + 1987 DV	0.05	0.09	0.538			
Home Visits + Elder Abuse	0.13	0.25	0.597			
Home Visits + DV Arrestee	0.24	0.16	0.134			
Assigned Treatment (No Treatment Grp.)						
Home Visits Only				0.10	0.11	0.352
Public Education Only				0.04	0.11	0.718
Home Visits + Public Ed.				0.02	0.15	0.895
Constant	0.14	0.39	0.719	**0.61**	**0.14**	**0.000**
Alpha	0.05	0.04	0.201	0.08	0.05	0.097

Notes: a=Negative Binominal Regression Model

The models with the interaction terms also contain all of the direct effects, but they are not reported because they are redundant to the direct effects model. Bolded coefficients are statistically significant at p-value <0.05

Table 9. Recidivism Among Households Assigned to Home Visit Condition According to Whether Contact Was Made or Literature Left

	Contact Made	Literature Left
Elder abuse study		
Frequency: reports to police	0.69	0.80
Prevalence: reports to interviewers	41%	36%
Freauencvheveritv: reoorts to interviewers	7.03	9.34
1987 DV study		
Frequency: reports to police	0.81	1.25
Prevalence: reports to interviewers	35%	48%
Freauencv/severitv: reoorts to interviewers	3.55	7.05

METHOD APENDIX

In this appendix, we provide details on sampling, case assignment process, and interview techniques for the three samples.

1987 Domestic Violence Study

Sampling Frame Sixty-four public housing projects (total population 93,000) were matched in pairs for size and demographic characteristics (projects ranged in size from 100 households to more than 2,000 households). The projects were all within **the** bounds of Manhattan's 23rd, 25th, and 32nd precincts. One member of each pair was randomly assigned to receive the public Education intervention and the other served as a control.

Four hundred thirty six households that reported incidents of domestic violence within the designated public housing projects were randomly assigned either to receive follow-up home visits or to a control condition. The 436 cases were fairly equally divided among romantic intimates (40%), children abusing parents (35%), and others (25%). (The last category consisted primarily of siblings abusing other siblings.) In 21% of the cases, the perpetrator had been arrested. The remaining 79% of the cases were incidents of family violence and/or disturbances in which no arrests were made. Victims were overwhelmingly female (87%), while perpetrators were predominantly male (79%).

Assignment Process and Case Intake During the sampling period, all family violence reports received through 911 were assigned to one of the two levels of the home visit intervention according to a log sheet that was pre-numbered with sequential ID numbers and a corresponding treatment group designation for each number. The treatments were pre-assigned using a table of random numbers. When the DVIEP team reviewed incident reports each day, one member of the team placed a call to the research department. For each incident, the DVIEP staff member gave the incident number, which a researcher entered on the next available line on the assignment sheet. The researcher then gave the DVIEP staff member the treatment designation corresponding to that line on the assignment sheet. This procedure was designed to protect against overrides of assignments by DVIEP staff, who might have been concerned that certain cases receive home visits regardless of the outcome of the case assignment process.

Survey Methodology Six months after the trigger incident, efforts were made to interview victims by telephone. If telephone contact was unsuccessful, multiple written invitations to participate were mailed. If no response was received, up to three visits were made to the victim's home to conduct a face-to-face interview. Victims were paid $15 to complete the 30-minute questionnaire.

These procedures resulted in an interview rate of 72%. Six percent of the 436 victims refixed to be interviewed. For the remaining 22% of the sample not interviewed, no contact with the victim was ever made. Comparisons of victims who were interviewed and those not interviewed revealed no significant differences in terms of victim or perpetrator age, victim or perpetrator gender, nature of relationship, type of incident (complaint versus arrest), or whether victim and perpetrator shared a domicile. Further, completing a victim interview was not significantly associated with assignment to wither public education or home visit treatments.

Outcome Measures Collected from Criminal Justice Records Each sampled case was entered into a personal computer system database. The database was used to track additional incidents that originated from the same addresses as those in the sample over a six month period.

Elder Abuse Study

Sampling Frame The study incorporated dual sampling frames to assess the effects of the public education and follow-up home visit interventions. Sixty public housing projects in the borough of Manhattan located within three Police Service Areas (PSAs) comprised the sampling frame for the public education intervention. A public housing PSA is a police administrative area similar to a precinct. PSA 4 covers all of lower Manhattan; PSA 5 covers mid-town to 125th Street; and PSA 6 covers 125th Street to the Northern tip of Manhattan.

The sampling frame for the follow-up home visit included residents of the sixty housing projects 55 years or older Eligible cases consisted of those classified by police as domestic incidents involving persons 55 years and older, who reported an incident of elder abuse to the police between 1/1/96 and 10/30/96. During that time, 439 cases were taken into the sample, an average of 1.3 cases per day. Thirty-four of these cases were dropped because they did not meet the study's requirements. The majority of the cases dropped were child custody disagreements (11) or cases classified as unfounded (8). Other reasons for dropping cases were language barriers, incorrect documentation of age, incorrect documentation of address, and errors in the police report.

Fifty-three percent of the perpetrators were children of the victims, 19% were grandchildren, 16% were romantic intimates (either spouses or boyhends/girlfriends), and the remainder were other relatives. Most perpetrators (66%) were male, while the overwhelming majority of victims were female.

A plurality (49%) of the trigger incidents involved only verbal arguments. Most of the others were classified by the police as family disputes (15%) and misdemeanor offenses (9%). Just a few incidents were serious enough to deserve their initial

classification as felony offenses (3%) or the arrest of the offender (6%). Physical injuries were reported by the police in 4% of incidents and hospitalization of the victim in just 3%.

Assignment Process and Case Intake Housing projects in each PSA were sorted into pairs matched for demographic similarities. Then, using a random number scheme, one member of each of the 30 resulting pairs was assigned to receive the public education treatment and the other was assigned to the control condition.

Cases were drawn from complaints of elder abuse made to the Housing Police Department (HPD) in the three participating PSAs. Domestic violence counselors screened police files daily for victims over 55 years of age. Cases were randomly assigned to the treatment group based on the Domestic Incident Report (DIR) number assigned by the police. Odd numbered DIRs were assigned to receive follow-up home visits, while even numbered DIRs were assigned to the control group. Cases were then entered into a case log which was used to trigger home visits by the DVIEP team and interviews by a research assistant.

In randomized experiments, the application of treatments is often imperfect. In this experiment, three cases designated as controls by the random assignment process received home visits when a DVIEP supervisors overrode the intended designation. In these three cases, supervisors were concerned about possible harm to the victim if the intervention was withheld. More common were cases assigned to receive home visits in which victims were not at home during either of the two attempts that were scheduled for each household. Only 49.8% of the victims assigned to the home visit group received the full intervention. Nevertheless, in an additional 22.5% of the cases, our records document some level of interaction between the home visit group and the family unit, mostly through phone conversations with the victim or personal contacts with some of the family members during the attempts to reach the victim.

Survey Methodology Six months from the date of the trigger incident, we began attempts to interview victims. The principal means of conducting interviews was by phone. We received assistance from NYCHA in locating victims who had moved. We sent teams of interviewers to victims' homes if telephone attempts failed. If the home interviews attempts also failed, we mailed letters offering first $25 and then $50 for completion of an interview.

The completion rate was 69%. Fourteen percent of the sample refused to be interviewed. We lost 3% of our sample to out of town moves and 2% to death and illness. The remainder of those not interviewed could not be contacted by phone or visits to their homes. Interview non-completion varied significantly by treatment group: 27% of the home visit group did not complete the survey compared to 35% of the control

group (p=O.O8). We ran a model to predict non-completion of the interviews using age, race and ethnicity of the perpetrator, type of relationship, seriousness of the triggering incident, and the interventions as the covariates. Only being in the home visit group was a significant predictor of not completion of the survey.

Outcome Measures Collected from Criminal Justice Records The DVIEP databases maintained on households at each of the three participating PSAs were searched to determine if new incidents of abuse were reported for households in our sample within, six-months following the trigger incident. When new cases were found, we recorded complaint date, type of abuse reported, charge, whether an arrest was made, use of weapons, and injury.

Domestic Violence Arrestee Study

Sampling Frame This study involved only the follow-up home visit intervention. There was no public education component to the study. The sampling frame consisted of all family violence arrests by the police in Manhattan's PSA2 Housing Police district between 10/20/95 and 6/2/96.

Twenty-two percent of the 197 cases involved felony charges and 78% misdemeanor or violation charges. The most common charge was third degree assault. One in five victims reported some form of injury. A large percentage (69%) of the cases involved intimate partners, and the overwhelming majority (88%) of victims were women. The average age of the victims was 32 years.

There were no significant differences between home visit and control groups on victim age, victim education, prior complaints to the police, nature of victidoffender relationship, or charge class.

Assignment Process and Case Intake During the sampling period, all PSA2 family violence arrests were assigned to receive a home visit or to the control group. The assignment was made using a log sheet that assigned each line to one or the other treatments. Treatment designations on the sheet were generated from a table of random numbers. Once each day, a DVIEP staff member called the research office to receive group assignments for each new arrest case. Each case enumerated by the DVIEP staff member was entered by a researcher on the next available line of the assignment sheet. The researcher informed the DVIEP staff member of the treatment designation for the case.

Survey Methodology Six months after the trigger incident, attempts to interview victims by telephone were begun. Up to five attempts were made to contact victims, dispersed between daytime and evening hours. Because the study was conducted without grant funds, no resources were available to send letters offering monetary incentives or to conduct visits to victims' homes for those victims not reached by phone. The success rate for victim interviews was 44%.

Outcome Measures Collected from CriminalJustice Records Six months following the trigger incident, the PSA2 domestic violence database was searched to determine if new incidents of abuse were reported for sampled households. For any new incidents recorded, we recorded date of incident, type of abuse reported, charge, whether an arrest was made, and type of foice used (e.g., punching, use of weapon, threats, etc.).