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The effects of focused deterrence on gang homicide: an evaluation of Rochester's ceasefire program

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THE EFFECTS OF FOCUSED DETERRENCE ON GANG HOMICIDE: AN EVALUATION OF ROCHESTER'S CEASEFIRE PROGRAM

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Abstract:

In the late 1990's, a problem-oriented policing initiative in Boston, "Operation: Ceasefire", achieved significant reductions in youth homicide by focusing on gang behavior. The program was driven by a concept known as "Focused Deterrence". Gang members are typically frequent offenders for whom general deterrence mechanisms have little effect. Additionally, the social norms of gangs often encourage offending behavior, making typical attempts to deter futile. Focused deterrence attempts to modify individual behavior and group norms with a credible and severe threat of collective punishment for an individual offending behavior. In "Operation Ceasefire", when a gang member committed a homicide, his gang was targeted for an "enforcement action" in which resources from many agencies across the criminal justice system were coordinated to severely punish the gang. Those enforcement actions were then advertised to other gangs as an example of what happens to gangs that commit homicides.

The success of the Boston program encouraged other jurisdictions across the country to implement their own versions of the "Ceasefire" project. In recent years, violence in Rochester, N.Y. came to be seen as consistent with the gang driven problem described in Boston and a version of Operation Ceasefire was implemented in October 2003. This study examines the "Ceasefire" program as implemented in Rochester, NY from October 2003 to December 2004. Using an interrupted time-series research design, the author finds limited but statistically significant reductions in homicides of Black Males ages 15-30 during the Ceasefire intervention period. Despite theses finding, increases in 2005 homicides of Black Males ages 15-30 have raised concerns about the effectiveness of the program. A Postscript examines the 2005 increase and considers explanations for the increase associated with potential theoretical and operational shortcomings in the Ceasefire program.

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INTRODUCTION

Since October 2003, the Criminal Justice System in Rochester, NY has collectively operated an acclaimed program "Operation: Ceasefire", which had great success in reducing youth homicide in Boston during the late 1990s. The program (from here on referred to as Ceasefire) drew upon research indicating much of Boston's youth violence problem involved a gang[1] component; victims or suspects were members or incidents were the direct product of gang activity. As a result, the program is meant to harness an important characteristic of offending patterns in young adults: offending in groups (Zimring, 1998).

There are two components to the program, enforcement actions and communications. The enforcement actions are strong multi-agency "crackdowns" on any member of a targeted group who is engaged in illegal activity. A group becomes targeted when someone in the group is involved in a homicide. The communications component (known as the "call-in") interacts with other gang members, warning them that if their crew is involved in a homicide, the whole crew will be subject to an enforcement action. Gang members obtain this message at a special meeting (the call-in) they are forced to attend due to their status as probationers or parolees. Those gang members are then asked to act as messengers to their gang. The overall objective of the program is to deter groups from violent activity initially through the threat of focused formal sanctions (the enforcement action) and to reinforce the deterrent effect through informal sanctions (peer pressure not to offend) generated from within the gang.

This study intends to evaluate the effectiveness of the Rochester Ceasefire program from October 2003 to December 2004, the first 15 months of the program. The author believes the evaluation will show evidence of the program's success but that the breadth of the success (i.e. violent crime overall) will be limited.

[1] For the purposes of this paper, the words "gang", "group", "crew", or "posse" will be used interchangeably with the following definition: "three or more individuals collectively engaged in criminal activity".

CHAPTER I

Background

The Roots of "Ceasefire"

The program evaluated in this study, Rochester Ceasefire, cannot be understood without understanding the original program upon which it is modeled. The Boston Gun Project was a problem-oriented policing initiative sponsored by the National Institute of Justice, and directed by David Kennedy, Anthony Braga, and Anne Piehl of the Kennedy School of Government at Harvard University. The project was guided by an inter-agency working group tasked with examining the nature of Boston's youth gun violence problem and developing intervention strategies to combat it. The working group members included representatives from the Boston Police Department, Suffolk County Probation, Suffolk County District Attorney's office, Bureau of Alcohol, Tobacco, and Firearms, and "Streetworkers"- a community outreach organization.

As the working group began to understand the problems of youth violent crime in Boston, they identified a correlation between the violence and a seemingly vast network of small, loosely organized gangs in Boston, particularly in the predominantly black neighborhoods of Roxbury, Dorchester and Mattapan. These gangs were not gangs in the traditional sense of the West Coast Bloods, Crips, or Latin Kings, but instead were groups of tight-knit youths from the same community that were collectively involved in criminal behavior, most typically drug sales. These local gangs seemed to be responsible for much of the problem of youth violence in Boston. As Kennedy notes (2001):

> *A relatively small number of youths were at high risk for both killing and being killed. They were gang members chronically in trouble with the authorities and known by working group members, often personally, because of their participation in gang activities, frequent arrests, status as probationers and prisoners, and visibility both on and off the streets.*

Indeed, the youths involved in the gangs were widely recognized across the Criminal Justice community. Many of the gang members were known across the police department, from administration to patrol officers for not only the violent crimes, but also for a continual stream of small-time drug offenses. Further, most gang members were well known by Probation and Parole officers as many were on one of the two forms of supervision. Finally, street outreach workers knew the gang members personally because of their imposing presence in the neighborhoods.

The working group also learned that the gang-related violence in Boston had created a culture of fear among Boston youths. The pervasive fear of victimization among youths in Boston had led many to carry firearms for protection, and thereby raising the volatility of an already dangerous community.

During the process of examining violent crime problem in Boston, the working group observed an event which eventually proved to be the foundation for the "Operation: Ceasefire" program. Boston Police and the Streetworkers organization combined forces to crack down on a gang that was acting especially violent in the Wendover Street area. To conduct the crackdown, the team drew upon all resources available to them, regardless of agency, pulling every "lever" available to them in the criminal justice system in order to round up members of the gang. The strategy used all of the gang members' contacts with the Criminal Justice system to the advantage of the police. The Boston Police worked with probation to strictly enforce probation conditions, they worked with the District Attorney to concentrate on the gang members' cases, and not offer plea bargains, and the police themselves applied constant pressure to gang member hangout locations.

A crucial element of the crackdown was honest conversations with gang members, either on the street with Streetworkers or at the time of arrest with police, explaining that the police were going to continue the crackdown on all the gang members until the violence stopped. Indeed, violence stopped, and, by the end of the crackdown, "…officers reported Wendover Street gang members actually pleading with them to remain at the end of the operation because the area was then so safe that they wanted it to stay that way." (Kennedy et. al, 2001)

From this experience, the working group learned two key lessons: First, that the "lever-pulling" gang crackdown strategy was effective in reducing violent crime in the neighborhood the gang was terrorizing. Second, that gang members feared such a crackdown, and when police and street outreach workers confronted gang members with the threat of such a crackdown, they reduced their levels of offending. These lessons formed the basis for their initiative to address violent crime in Boston, "Operation: Ceasefire".

The "Operation: Ceasefire" initiative which developed took the lessons learned in the Wendover Street experience and expanded them, attempting to make an impact on violent crime citywide by addressing all of the gangs in Boston.

The resulting process in the "Operation: Ceasefire" initiative was fairly simple. The first step was to identify a gang that was actively engaged in violence and undertake a crackdown on the gang members by using the "lever-pulling" strategy. The second step was to identify as many gangs in the city as possible, as well as identifying their membership and specifically, the members under supervision (Probation/Parole).

The third step was critical to the citywide success of the "Operation: Ceasefire" project. The working group held a series of meetings, known as "call-ins". Gang members were invited to these meetings; those under supervision were required to go to as a condition of their probation or parole. At the meetings, the gang members received an important message about their gang's behavior: violence was no longer tolerable, we know your gang is involved in violence, and if your gang continues to be involved in violence, we will use every tool we have to crackdown on your gang, just like we did to Wendover Street and Bowdoin Street (the Bowdoin Street gang was subject to a highly publicized and successful "pulling-levers" crackdown prior to the "call-in" meetings). The gang members then listened to representatives from the Boston Police, Probation, Parole, the District Attorney's office and the U.S. Attorney's office, who told the gang members about the tools their agency would use in the crackdown. In describing the tools available for the crackdown, each agency representative re-iterated the core message that "we are not putting up with this stuff [violence] any more", and that "if we focus on you, you can't win, so don't make us [act]" (Kennedy, 2001). The purpose of this communication strategy was to use the threat of a crackdown to deter the gangs, using the call-in attendees as messengers for their respective gangs.

The message in the meetings was not only a threat. At the end of the call-in, members of the “Streetworkers” group explained to the gang members that the entire community does not want to see its youth dying and that the “Ceasefire” project is not meant to punish, but to prevent more deaths. Further, the street outreach workers offered assistance to the gang members in obtaining employment and health services, as well as protection from other gangs. However, the key in the call-in meetings was to let the gang members know that the criminal justice system is watching their gang, and that “we [the criminal justice system] brought you Bowdoin Street. If this violence does not stop, you are next” (Kennedy, 2001). The ongoing fourth step in the process was to follow up with a “pulling levers” crackdown whenever gangs engaged in violence.

The results of the “Operation: Ceasefire” initiative have been highly touted at the national level for the initiative’s remarkable success. During the peak operation period of the program (June 1996-June 1998), “Operation: Ceasefire” is associated with a 63% reduction in youth homicides per month in Boston, a 32% decrease in shots fired per month, and a 25% reduction in the number of gun assaults (Kennedy, 2001). As a result of the success of “Operation: Ceasefire” in Boston, numerous other jurisdictions have attempted to implement “Ceasefire” programs, and the program is cited as an example program for jurisdictions seeking to reduce gun violence through the Department of Justice’s “Project Safe Neighborhoods” gun violence initiative.

Rochester, NY and Ceasefire

Rochester, NY is well suited to experiment with violence reduction programs. For a city of 218,000 residents, Rochester has an unusually high homicide rate. From 1995-

1999, the average homicide rate in Rochester was 21.34 per 100,000, higher than all cities in New York State. For the 95-99 period, the Rochester homicide rate more than doubled that of Boston's (9.42 per 100,000). Like Boston, homicides were disproportionately concentrated, both by geography and demography. Analysis of homicides by John Klofas of RIT found homicide victimization to be overwhelmingly concentrated in a geographic area known as "the crescent"- An area characterized by minority residents, high poverty, failing schools, unemployment, weak economic activity, and crime. According to Klofas, 80% of homicides (from 1991-2001) occurred in the "crescent", yet only 27% of the total Rochester population lives in that area. Even more startling, homicide victimization for young black males (15-30) in the crescent was 520 per 100,000, 65 times higher than the national homicide rate of 8 per 100,000. And young black males were not just being victimized; they were also perpetrating homicides at a similar rate. Reviews of homicide incidents conducted by Klofas and the Rochester Police Department from 2000-2003 indicated that a group dynamic was involved in many of the homicides. This group dynamic resembled that of Boston's: a loosely organized network of individuals engaged in drug sales and more serious criminal activity. The parallels of Rochester's violent crime problem to Boston made Ceasefire a logical and attractive program to experiment with.

The Rochester Criminal Justice community has attempted to implement Ceasefire on a number of occasions since reports of Boston's success began to circulate in 1998. The first version was initiated in 1998 by the U.S. Attorney's office of Western New York after members of the local criminal justice community learned about "Operation: Ceasefire" at a national conference. Like Boston, Rochester's early attempt at Ceasefire identified groups actively involved in violence. These groups were brought in to call-ins at community centers and given a message that was somewhat less clear than the Boston message. While the core message of "violence will no longer be tolerated, if your group continues to be involved in violence, the whole group will face new and serious punishments" was used, this early Rochester version relied heavily on community speakers (including ministers, activists, and outreach workers) to articulate the impact of violence on the community. While such testimony is powerful, it may have obscured the intended message.

The first iteration of Ceasefire in Rochester began to fall apart after 1998 for two reasons. First, the Criminal Justice agencies never performed an enforcement action on a group to use as an example, nor had they actually followed up on groups that received the message, but continued to engage in violence. The lack of enforcement credibility may have seriously undermined the deterrence effect the agencies were trying to generate. Secondly, the criminal justice agencies involved decided that the deterrence message would be more effective by communicating it beyond gang members and out to juveniles who had not yet begun to involve themselves in a gang or commit serious violent crime. Therefore, the group started to take the deterrence message to schools, speaking before audiences of elementary and middle school students. While the deterrence message may have some impact on younger children, evaluations of the D.A.R.E. program have shown that educating similar audiences about the risks of drug use has little impact on the child's future propensity for drug use (GAO, 2003). Likewise, the move toward a deterrence message in the schools got away from the focused deterrence model of Boston's Ceasefire, more closely resembling the broad and weak level of deterrence typically exerted by the Criminal Justice system, a level characterized by non-credible empty threats from an overburdened system.

The second iteration of Ceasefire in Rochester was implemented from 2002-2003, as a byproduct of a collaboration made possible through the federal Strategic Approaches to Community Safety Initiatives (SACSI) program. SACSI emphasized research-based interagency problem solving at the local level. The goal of the Rochester program was to use homicide research from John Klofas to drive homicide intervention strategies. Research findings showing the geographic, demographic, and group-related concentration of homicides convinced the SACSI team that the Ceasefire model was particularly applicable to the problem, and resurrected the Ceasefire program.

While the goal of the second version remained the same as before (homicide and violent crime reduction), Rochester again implemented the model in a slightly different way than the original Boston version. The second Rochester Ceasefire version focused on individuals (rather than gang members) on probation or parole that were believed to be at future risk to be victims or suspects in a violent crime according to their probation and parole officers. The goal of this second version was to create a threat of enforcement attention on high-rate offenders and generate a focused deterrent effect among the targeted individuals. Like the earlier Rochester iteration, the second Rochester Ceasefire failed to follow up on the sanctions that were being threatened during the call-in. While the second version deviated substantially from the original Boston model, the selection process for high-rate offenders showed anecdotal success: two individuals selected for the program were murdered before they took part in the Ceasefire meeting.

Finally, the most recent iteration of Ceasefire (the version that is the subject of this evaluation) originated in summer 2003 as an attempt to align the previous Rochester versions with the original design of the Boston model. This effort was led by Rochester Police Department together with John Klofas of Rochester Institute of Technology and David Kennedy of Harvard University. The goal of this iteration, (which is still under way and the author continues to play a role in the development and operation of the program) was to make a serious attempt to implement Ceasefire in a way that could meaningfully address the problem of group-related homicide, and hopefully replicate the same results as observed in Boston. As a result of the effort to mirror the original Boston program, the current Rochester Ceasefire is quite different from the earlier Rochester iterations.

Contrasting the Rochester Iterations

The first and probably most important difference between the current version of Rochester Ceasefire and the two earlier iterations is the use of enforcement to produce a legitimate and credible deterrence message. The current Ceasefire iteration was the only of the three Rochester versions to commence an enforcement action against a group for the purpose of having an example to show at the first call-in. For the first call-in of the current Ceasefire, a gang named "Thurston Zoo" was chosen as the example enforcement target due to one of their members' participation in a 2003 homicide. The enforcement action used was primarily drug-related in nature, using undercover officers to obtain multiple drug buys from many members of the group. Group members on probation or parole were watched closely, and a probationer in the group was violated when the opportunity arose. On the prosecution side of the enforcement action, a Assistant District Attorney was assigned to give specific attention to the cases (including no plea bargains) to ensure that the group members were not treated they way they would typically expect in the Criminal Justice system.

The enforcement action then served to bring legitimacy to the deterrence message because it allowed the speakers at the "call-in" to say: "we are serious, look what we did to Thurston Zoo, we will do that to your group to if your group is involved in a homicide." Since the initial enforcement action on Thurston Zoo, the Rochester Criminal Justice community has remained true to the Ceasefire threat by continuing to go after any group involved in a homicide. By December 2004, four enforcement actions had occurred against groups that were involved in homicide and three enforcement actions were pending. These enforcement actions involved the use long-term undercover narcotics investigations, prioritization of those cases by the District Attorney's office, tightened supervisory conditions of probationers and parolees in those groups, and in one enforcement action, a Federal consipracy prosecution. This commitment of resources to enforcement actions was a notable characteristic of the original Boston program that was missing in the earlier versions of Rochester Ceasefire.

Another distinguishing factor among the Rochester Ceasefire iterations was the clarity and credibility of the message. In the current program, the deterrence message communicated to "call-in" attendees was short and clear: "If your crew is involved in a homicide, all of the illegal activity of the crew will be targeted". This message represents a stark departure from the muddled messages of the earlier versions, which admonished the attendees not to engage in broad categories of offending, from violence to drug dealing to all offending for fear of increased attention from law enforcement. The strength of using homicide as the "trigger" for action by law enforcement is that the resources for meaningful enforcement actions are limited, and in order to deliver a credible deterrence message, law enforcement must follow through with the actions they threaten. The messages used in the earlier Rochester Ceasefire versions prohibited so much behavior as to make credible follow through with enforcement actions impossible. In this regard, the message in the current program represents the clearest and most credible deterrence message of the three Rochester Ceasefire programs.

Another unique characteristic of the current iteration (relative to the earlier Rochester models) was a focus on the development of gang intelligence. In the Boston Ceasefire and the first Rochester Ceasefire, groups or individuals were identified through an anecdotal process, gathering information from patrol officers. In the current Ceasefire, the Rochester Police Records system was used to correlate names and known group locations to identify likely members of groups. Markers used to confirm an individual's membership in a group included a combination of the following identifiers: self-identification as a member to a police officer during a contact, repeated police contacts with other known group members, repeated police contacts at known locations, and police identification of individual as a member of a certain group. In addition to review of police records data, qualitative data was gathered through intelligence meetings with select proactive police officers who were recommended by their superiors for their wealth of "street" knowledge, particularly of neighborhood gangs.

The process of implementing Ceasefire in Rochester evidences the difficulty of translating a program from one jurisdiction to another. Integrating strategic focus, organizational commitment, and agency resources can reshape a program in many unintended ways. Through this process, a model has emerged in Rochester that most closely resembles the original Boston program, and, as desired by the Criminal Justice partners involved in the program, a model with the greatest potential to address the problem of group-related homicide in Rochester.

CHAPTER II

Literature Review

The Ceasefire strategy is a multi-layered approach that seeks to incapacitate groups that commit homicide and deter other groups from involvement in future homicides. In Rochester as in many cities, the main goal of initiatives like Ceasefire is not simply to respond to crime, but to prevent it. The preventative aspects of Ceasefire come in two forms. First, the targeting of groups that have committed homicides has an ancillary preventative effect, as these gang members have already illustrated a propensity toward violence. Incapacitating these crews prevents further violence, but a great cost, as the enforcement actions against such crews require expenditures of time, labor, and capital.

The second, and primary, source of preventative power in Ceasefire comes from the communication of the Ceasefire message at the call-in. The goal of this message is to prevent future group-related homicides by deterring groups from violence through the credible threat of an enforcement action. In contrast to the enforcement action, the deterrence message can ideally be highly efficient as the resources cost needed to conduct a call-in are lower than the cost associated with an enforcement action, yet the potential of the call-in to prevent homicide is greater than an enforcement action simply to the population affected.

Clearly, the deterrent effect generated by the Ceasefire program is critical to the goal of the program, preventing group-involved homicides. Indeed, the success of the program is almost entirely dependent upon the generation of a deterrent effect among gangs. For this reason, the theory and empirical observations of deterrence effects are of necessary to examine for this study. This chapter examines the concepts and proofs of deterrence theories.

Foundations of Deterrence Theories

The criminal justice system exists not only to incapacitate and punish, but also to deter those who would consider crime with the threat of punishment. Deterrence theories derive from the philosophical foundations of law and justice, particularly, the social utility of punishment as a component of a justice system. In "On Crimes and Punishments", Cesare Beccaria (1764) is among the first to discuss the idea that when applied fairly, the threat of punishment can serve to deter offending behavior. He concludes that punishment for a violation of law must be swift, certain, transparent, and proportional in order for the act of punishment to be justified as socially useful (rather than a base act of violence by society against the condemned). By having certainty, proportionality, transparency, and swiftness in the application of punishment for law violations, a potential offender will always know what punishment one can expect from each particular type of crime. Societial knowledge about punishments and their consistent application, devoid of subjectivity or corruption, creates, according to Beccaria, fear of laws. The fear of laws has a socially useful purpose, to prevent future crime. Therefore, punishment for law violation which is swift certain, transparent, and proportional is justifiable as a restriction of individual rights because it serves to create fear of punishment in society, and thereby acts to prevent future crime.

> *Do you want to prevent crimes? See to it that the laws are clear and simple and that the entire force of a nation is united in their defense and that no part of it is employed to destroy them. See to it that the laws favor not so much classes of men as men themselves. See to it that men fear the laws and fear nothing else. For fear*

> *of the laws is salutary, but fatal and fertile for crimes is one man's fear of another.*

In "Principles of Morals and Legislation", Jeremy Bentham echoes Beccaria's sentiments on the utility of punishment, also arguing that the preventive effect punishment has upon society is a justification for the inherently evil act of punishment.

> *But all punishment is mischief: all punishment in itself is evil. Upon the principle of utility, if it ought at all to be admitted, it ought only to be admitted in as far as it promises to exclude some greater evil.*

Bentham, well known as one of the fathers of Utilitarianism, notes Beccaria as a major influence in his academic heritage. Bentham borrows from Beccaria the concept of utility maximization; that humans seek to maximize their individual pleasure and minimize their pain. Therefore, according to Bentham: "The value of the punishment must not be less in any case than what is sufficient to outweigh that of the profit of the offence" (Bentham, 1822). The notion of utility maximization is articulated in "On Crimes and Punishments", when Beccaria proclaims "La massima felicità divisa del maggior numero". Here Beccaria implies that because humans seek to maximize pleasure, good laws are one that should "create the greatest happiness shared by the greatest number".

The idea of social utility, shared by Beccaria and Bentham, is clearly evident in their ideas on the value of punishment in society. Both suggest that society gains a benefit by punishing an individual for crimes because the punishment imposes "pain" relative to the corresponding "pleasure" derived from committing the crime. The fear of "pain" therefore, prevents others in society from committing the same crime, and therefore, potential criminals are more likely to restrain from crimes than if no punishment were attached. This theory, as articulated by Beccaria and Bentham, comprises the fundamental theory of deterrence, also known as "simple deterrence" or "mere deterrence".

Despite the similarity of thought between Beccaria and Bentham, an important distinction must be made between the two when discussing the issue of the extent to which pain inflicted should exceed the benefit of the criminal offense. To Beccaria, the key element of effective deterrence is a system of laws that are clear, objectively and equally applied, and most importantly, proportional to the crime committed. Beccaria believed that man's fear of man produced violence, and that a society where man feared not other men, but laws, would be a society where most crime would be prevented. Therefore, the "pain" inflicted by punishment for crime must always be proportional to the crime committed, otherwise, the laws will appear unjust, and men will no longer fear laws.

To put Beccaria's assertions in context, *On Crimes and Punishments* was written at a time (1764) where the European aristocratic class was reinventing itself as the intellectual elite, persuading reform of disproportionate social treatment between the common man, aristocracy, and monarchy. Therefore, the equitable treatment of all in society becomes a primary issue of Beccaria's concern.

In contrast to Beccaria's notions of strict proportionality, Bentham's ideas about the use of punishment are informed much more by the fundamental concepts in Utilitarianism. Bentham's believed that the "pain" of the punishment "must be sufficient to outweigh" pleasure of the offense.

While the difference between Bentham and Beccaria may seem pedantic, it is in fact quite different. The implication of Beccaria's notions is that the justification for the punishment of the individual comes from the fear generated in society by the punishment, and while the generation of such fear has social value, such value should not come at the cost of fair and equal treatment, which is why the punishment should be proportional to the crime.

In contrast, Bentham uses basic utilitarian precepts to justify punishment of the individual. Because society is more likely to fear a punishment that is slightly more severe than the associated crime, society is more likely to be safe because fewer people will be willing to accept the risk of over-punishment. Therefore, the justification for the over-punishment of the criminal is in the benefit to society of crime reductions that occur from fear of over-punishment.

Despite the difference between Beccaria and Bentham over the extent of acceptable punishment, both identify that a socially valuable use of punishment is to generate fear of punishment in society. This idea articulated by Beccaria and Bentham is known as “simple” or “mere deterrence”. The idea is referred to as “simple” because the idea expressed is empirically untenable. A definition of deterrence by Zimring and Hawkins (1973) illustrates the problem of “simple deterrence” theory:

> The theory of simple deterrence is that threats can reduce crime by causing a change of heart, induced by the unpleasantness of the specific consequences threatened

Andaenes (1966) suggests that mere deterrence is “the frightening effect of punishment.” As a phenomenon in society, deterrence is impossible to observe. One cannot possibly observe the “change of heart” that the Zimring definition suggests. For hundreds of years, the conundrum of deterrence as a conjecture rather than an empirically testable theory limited the ability of researchers to examine the concept scientifically. In

“Crime, Punishment, and Deterrence” Gibbs (1975) makes a substantial contribution to the conceptualization of deterrence effect in society. Gibbs argues that in order to study deterrence, the deterrence effect must be placed into a context.

> Since deterrence is inherently unobservable, rules of inference pertaining to it are unfalsifiable unless stated in the context of a theory. In turn, any deterrence theory necessarily makes assertions in which “deterrence,” “deters,’ or “deterred” is constituent term.

Gibbs notes that because the idea of deterrence must be placed into context in order to be empirically studied, a multitude of theories can be created explaining how all the different properties of punishments exert a deterrent effect on one or more individuals. The notion of contextual deterrence then finally provides a “context” for the examination and assessment of deterrence.

<u>Deterrence in Context</u>

Two primary categorical contexts of deterrence exist within the literature of deterrence theory, general deterrence and specific deterrence, and within these categories variations exist. The difference between general deterrence and specific deterrence has long been known. Bentham aptly noted in Principles of Penal Law “determent is equally applicable to the situation of the already-punished delinquent and that of other persons at large”. The first effect noted by Bentham is the general fear instilled in society from the threat of punishment. This effect is known in deterrence literature as “general deterrence”. The second effect noted by Bentham is the fear of future punishment which should be instilled into a criminal who has experienced punishment and been released. This second type of deterrent effect, known as specific deterrence suggests that criminal laws not only threaten the population at large from committing crimes, but also threaten individuals from committing future crime after they have already experienced punishment.

Within the broad theories of general and specific deterrence, a variety of distinctions have been made in regard to the range of deterrent effects sanctions have upon one or more individuals. The theory of "partial deterrence" (Zimring & Hawkins 1973) or "restrictive deterrence" (Gibbs 1975) suggests that the threat imposed by sanctions often causes one or more individuals to reduce their levels of offending (in severity, frequency, or both). Gibbs provides a strong example of this phenomenon:

> …all motorists exceed the vehicular speed limit occasionally. But they may have some sense (however dim) of a cumulative risk of punishment, and for that reason they do not violate speed regulations regularly, flagrantly (e.g., driving 60 miles per hour in a zone posted for 30), or uncritically (without regard to avoiding detection).

An important characteristic of the theory of "partial deterrence" is recognition that a sanction has not just an "all or nothing" deterrent effect, instead, some individuals will be deterred entirely, some will be not deterred at all, and some will be moderately deterred without ceasing offending. Partial deterrence can be equally applied to the theories of both general and specific deterrence. In the case of general deterrence, the threat of sanction may cause those who have offended one or more times to reduce the frequency in which they commit the crime to which the sanction is attached, and/or choose instead to commit crimes with a less harsh sanction. In terms of specific deterrence, individuals who have already been punished for the commission of a crime will be "partially deterred" if they commit the same crime with less frequency, or commit lesser crimes instead of committing the crime for which they were punished.

Another important distinction to make within the general deterrence theory is the notion of absolute versus marginal deterrence. The theories of absolute and marginal deterrence are useful when evaluating the deterrence effect of a sanction. Absolute deterrence theory suggests that given a crime, when a sanction is attached to its commission fewer incidences of the crime will occur than if no sanction is attached. Marginal deterrence theory suggests that given a crime, when a sanction exists for it and when that crime occurs in a community at certain rate, an increased sanction will produce a crime rate lower than the crime rate produced with the original sanction. The contribution of absolute and marginal deterrence to deterrence literature is the acknowledgement that while deterrence is an individual phenomenon, the phenomenon can be observed at the aggregate level when, holding constant other variables, crime rates vary with the existence or level of sanction imposed on the population. Therefore, a good deterrence policy should be both effective (exerting an absolute deterrent effect) as well as efficient (exerting a marginal deterrent effect).

The final distinction to address involves the literature on length of deterrent effect. Interrupted time-series studies (discussed later), done most notably by Sherman (1990) and Ross (1982) have observed temporal influence upon deterrent effects. In his studies of drunk-driving sanctions, Ross (1982) determined that sanction changes produced “initial deterrence”, however, the changes provide no long-term deterrence effects. Sherman (1990) articulates this phenomenon as “initial deterrence decay” and distinguishes between “initial deterrence” (the immediate deterrent effect which is created by a change in certainty or severity of punishment) and “residual deterrence” (the long-term deterrent effect of a sanction change which is weaker than the initial deterrent effect). While these effects could apply to both general and specific deterrence theories, research on this issue is constrained to general deterrence.

Empirical Evidence of Deterrence Theories

While many scholars have studied the idea of deterrence since Beccaria and Bentham, work on developing empirical proof of deterrence did not begin until the mid 1960s. Becker (1967) was the first to construct a mathematical formula of general deterrence. The basic formula is:

$$EU = pU(Y-f) +(1-p) U(Y)$$

In the model, $pU(Y-f)$ represents the utility of the benefits of committing a crime if punished, and $(1-p) U(Y)$ represents the utility of the benefits of committing the crime and avoiding punishment. Becker suggests that increasing cost variables p (the certainty of punishment) and f (the severity of punishment) decreases the expected utility (EU) of a crime, and therefore, changes in these variables can increase the deterrent effect an individual experiences (note the exclusion of celerity- a key element of punishment as articulated by Bentham).

Since the Becker model, research on general deterrence has expanded considerably. According to Nagin, (1998) empirical study of general deterrence has branched into three primary categories, ecological studies, interrupted time-series studies, and perceptual studies.

Deterrence Evidence in Ecological Studies

Research in the ecological study category examines aggregate data for negative relationships between crime rates and the sanction cost variables (certainty and severity). In these types of studies, sanction variables fall into two categories: prison-based and police-based.

In the category of prison-based sanction variables on crime rates, researchers looked for a negative relationship between crime rates and data gathered after the offender was found guilty (such as prison sentencing length and proportion of offenders for a crime which end up in prison). For example, Gibbs' 1968 study compared across states the impact of probability of imprisonment for homicide (derived by dividing the number of persons incarcerated for homicide in a state by the number of homicides reported in the state) and homicide imprisonment sentences on homicide rates. Similar studies done by Gray and Martin (1969) and Bean and Cushing (1971) verify Gibbs' finding of a negative association between homicide rate and the prison-based sanction variables, giving evidence to support the idea that increases in severity and certainty of punishment have a deterrent effect. Using the same general sanction variables, Antunes and Hunt (1973) Chiricos and Waldo (1970), Tittle (1969), and Logan (1971, 1972) have examined UCR index crimes. While these studies also found evidence for a deterrent effect when looking at both sanction variables for effect in homicide rate, all the studies only showed the negative association between the offenders-to-inmates ratio and the remaining index crimes.

One of the problems of these early ecological studies was the lack of control for socioeconomic factors influencing the data. Ehrlich (1973) and Forst (1976) both used functions that extensively controlled for such factors. Surprisingly, the studies had opposite conclusions. Ehrlich, analyzing data from the 1960’s, found statistically significant negative associations between the prison-based sanction variables and the index crime rate. Forst, who analyzed 1970’s data and used a function model similar to Ehrlich, found no negative associations between the index crime rate and the prison-based sanction variables.

While many of the prison-based ecological studies provide some evidence to suggest the existence of a general deterrent effect, many of the early ecological studies fail to control for another more difficult problem. Variables like the prison population and police presence or resources are endogenous to crime rates. Assuming clearance rates stay the same, as crime rates increase, more people will go to prison simply because more crime is being committed. Likewise, as crime rates increase, governments are likely to commit more resources to the problem. Because these variables are influenced by each other, variance attributed to a deterrent effect may be better explained by this phenomenon. Several studies discussed later including Sampson and Cohen (1988), Levitt (1997), and Marvell and Moody (1996) make stronger attempts to control for this problem, and their evidence for deterrent effects should therefore be considered more compelling.

The second category of ecological studies looks at police-based sanction variables. Where as the prison-based variables dealt with the deterrence through severity and certainty of punishment, research using police-based variables has primarily looked at variables that impact the certainty of punishment. The two major variables studied have been police resources and probability of apprehension. In 1974, Tittle and Rowe examined Florida index crime rates and arrest probabilities across counties and municipalities. The study found negative associations between index crime rates and arrest probabilities over .3. This finding led Tittle and Rowe to conclude that certainty of arrest is not an effective deterrent mechanism for a community when 30% or less of reported crimes result in an arrest.

Further research on the deterrent effect of certainty of punishment (in terms of probability of arrest) by Wilson and Boland (1978) clarified the relationship between police resources and deterrence. The study argued that changes in police resources have no direct deterrent effect because these resources are allocated through a bureaucratic process rather than at maximum efficiency. Instead, changes in police resources have an impact only when active enforcement exists. Wilson and Boland looked at 35 cities and examined the ratio of traffic offense arrests to traffic offenses as a measure of police aggressiveness. The study found that such an arrest ratio had a negative association with robberies, suggesting that police departments that aggressively enforce traffic violations (thereby increasing the certainty of punishment for such offenses) deter robberies.

The crucial idea that comes out of Wilson and Boland is that police aggressiveness can impact arrest rates, which, in turn, create a deterrence effect. Sampson and Cohen (1988) applied this idea to Wilson's theory of "Broken Windows" – that areas where minor crimes are condoned eventually breed more serious crimes. Sampson and Cohen studied 171 cities to determine if aggressive enforcement of minor crimes had indeed limited the amount of serious crimes relative to cities with more lax enforcement of minor crimes. Their study suggested that minor crime arrest rates are negatively associated with robbery crime rates.

The two most important recent studies done using police-based sanction variables have examined the question of whether police force size impacts crime rates. Research on police size impact on crime rates was virtually non-existent between 1978 and 1990, in large part due to the explication by Fisher and Nagin (1978) of the endogeniety problem. Levitt (1997) evades the problem by finding a change in police force size that cannot be attributed to crime rate changes. Levitt found that police forces increase on mayoral and gubernatorial election years, and after controlling for other factors, found that these police force increases are negatively associated with crime rates. Marvell and Moody (1996) found similar results using a Granger causality method whereby increases in police force were found to have negative association with future crime rates. Similar to the Marvell and Moody study, Corman and Mocan (2000) used data from New York City to examine the relationship between police size and crime rates on a monthly level while controlling for the endogenous relationship. The results of all three recent studies on the deterrent effect of police size were similar: a 10% increase in police size resulted in a 10% decrease in crime rates.

A review of the ecological study literature provides mixed evidence to support the theory of general deterrence, however, the evidence suggests that certainty of punishment in the form of higher arrest ratios (through aggressive policing and/or police force size) seems more likely than severity of punishment to produce a general deterrence effect.

Deterrence Evidence in Interrupted Time-Series Studies

The second body of research regarding general deterrence uses interrupted time-series studies. As this study employs the same methodological approach, the evidence for a general deterrence effect in interrupted time-series studies is of particular interest. Interrupted time-series studies examine two similar populations; the first population is subjected to a change in severity and/or certainty of a sanction, the second population remains the same. Evidence of a deterrence effect using this method requires a reduction in crime rates in the first population while no change in crime rates in the second population.

The classic example of interrupted time-series work is Kelling et al. al. (1974) In the well-known study, five patrol beats in Kansas City had two or three cars on each beat, five patrol beats had no cars, and five patrol beats had only one car. While the study found no evidence to support the deterrent effect of police presence, the study is often criticized, and according to Levitt (2002), "most researchers view this (Kelling et al. al 1974) experiment as inconclusive". Despite the Kansas City study, findings in interrupted time-series studies have provided evidence suggesting policies that increase sanction properties have a deterrent effect, but that effect does not remain constant on a permanent basis.

As discussed earlier, Ross (1982) studied changes in drunk-driving laws in Britain and Scandinavian countries where he observed a negative association between tougher sanction enforcement and drunken driving-related accident rates immediately after the implementation of such policies. However, those drunk driving-related accident rates eventually increased. Ross attributed the time-based change in deterrence effect to an initial overestimation of punishment risk followed by an eventual awareness of the absolute punishment risk. Ross (1982) estimated that the absolute risk for drunk driving was about 1 in 1000, and although that risk ratio was higher (2 in 1000) immediately after implementation of tougher sanctions (and related enforcement) the risk ratio was still low enough so as to exert limited real risk of punishment on offenders.

In Sherman's survey of police crackdowns (1990) he finds the same decay of deterrence effect, and while agreeing with Ross' conclusions about cause, also provides an alternative explanation. Sherman suggests that the initial deterrence effect produced by a change in sanction properties derives from a wide deviation in potential offenders' perception of risk of punishment. Over time, the deviation (or uncertainty) lessens, and potential offenders have a clearer understanding of the punishment risk associated with the crime. According to Sherman, the uncertainty of punishment risk generated in an initial deterrent effect is related to how effectively police can increase the perceived certainty of punishment. Given that police attempts to improve certainty of punishment impose resource costs, police are unable to permanently sustain the level of enforcement necessary to keep the likelihood of arrest at the level that produced the initial deterrence effect. As the level of enforcement drops, the likelihood of arrest drops, thereby decreasing punishment risk uncertainty for potential offenders. Depending on the situation, a residual deterrence effect may remain for a limited period (although weaker than the initial deterrence effect) after the level of enforcement has dropped from the level required to produce the initial deterrent effect.

For a policy to minimize the initial deterrence decay, and sustain a deterrent effect, enforcement must be intermittent rather than constant. If done frequently enough, variation in the level of enforcement will sustain the uncertainty of punishment risk that creates the initial deterrence effect, without imposing the additional cost of a constant enforcement increase.

Deterrence Evidence in Perceptual Studies

For a policy to truly deter an individual, he or she must believe that their punishment risk has increased to a level where criminal activity no longer becomes profitable. The area of perceptual deterrence literature examines the levels of punishment risk an individual believes exist when deciding to engage in a particular crime. As David Kennedy notes, (1997A) "it is fairly common for offenders to be ignorant of criminal justice policy and practice." Indeed, the most significant finding in perceptual deterrence studies (see Nagin and Paternoster 1993, 1994 {theft}, Paternoster and Simpson 1997 {corporate crime}, and Klepper and Nagin 1989A, 1989B {tax evasion}) is that individuals who have prior offending records seem to perceive lower punishment risks than individuals who have no prior records. A variety of theories exist to explain this phenomenon, however, little empirical proof exists to support any theory. The most popular explanation is put forth in the series of studies cited above. As individuals successfully commit crimes without apprehension, their internalized probability of punishment reduces. Therefore, prior offenders would have lower punishment risk perceptions than non-offenders. While this explanation is the most popular among competing theories, no strong evidence exists to explain the phenomenon.

Another relevant area of study within the perceptual deterrence literature is the perceived punishment risk associated with informal punishment sources after the imposition of formal punishments. The idea that an individual may be deterred from crime not only by the formal punishment response from the criminal justice system, but also from society at large for being a "criminal" has been hypothesized for a long time. Tittle (1968), Zimring and Hawkins (1973), Andaenes (1974), Gibbs (1975), and

Blumstein and Nagin (1976) all conceived of the power of societal forces as a deterrent to criminal behavior. Evidence for this phenomenon can be found in tax evasion studies by Klepper and Nagin (1989A, 1989B), where they found that individuals were more likely to evade tax payments when the enforcement mechanism was private rather than public. They suggest that when the enforcement mechanism becomes public, an additional cost of violating tax laws is added in the form of damage to reputation. When the enforcement mechanism is purely private, individuals only risk their money. Clearly, the potential damage of an individual's integrity in society acted as a deterrent effect above and beyond the monetary punishment of the formal sanctions. This concept is critical in Ceasefire, where an assumption exists that gang members will use informal sanction power in the form of "peer pressure" to prevent other members in their gang from committing homicides for fear of a collective retaliation against the gang by law enforcement (in the form of an enforcement action).

Nagin (1998) issues a caveat to this phenomenon, however. In order for an individual to perceive informal sanctions from society after a formal sanction, the criminal act for which the individual was formally sanctioned cannot be commonplace in society. As the experience of punishment becomes more commonplace in society (i.e. the number of individuals in society experiencing punishment increases) the social stigma attached to the crime decreases simply because a larger portion of the population has also committed the crime, and is therefore unlikely to stigmatize future offenders. The significance of Nagin's argument is realized in policy options where informal sanctions are utilized to produce a deterrent effect. Fortunately, homicide remains a rare event, and hypothetically some stigma still attaches even among gang members.

Literature Review Findings

Certainly, evidence exists to support deterrence theory from a Specific or General Deterrence perspective. This literature provides a variety of lessons that are relevant to Ceasefire. Clearly, certainty of the punishment threat matters a great deal, even more than the severity of the threat. Deterrence is a phenomenon that is time-sensitive and is subject to decay. As it is a largely internalized process by the potential offender, deterrence is subject to the offenders' perceptions of risk and severity, and that perception is influenced by past experience and police behavior. However, one of the most important pieces of Ceasefire, collective deterrence of groups, is largely unaccounted for by existing deterrence literature.

Focused Deterrence

Kennedy notes (2003) "deterrence…is the principal mechanism through which the central feature of criminal justice, the exercise of state authority, works- it is hoped- to diminish offending and enhance public safety". In the Criminal Justice system, the mechanism of deterrence is almost exclusively employed on an individual level, and for good reason- the Criminal Justice system functions though an individual case processing model. An incident occurs, a suspect is charged with a crime, the suspect is tried, and the suspect is disposed. A punishment given to a suspect has two deterrence functions, to deter that suspect from further offending, and to serve as an example to others of the punishment they would receive from committing the same offense. This focus is for good reason- As Kennedy (2003) put it, "a gang, after all, does not pull a trigger; some person does. A fraternity does not commit date rape, a person does".

An individual focus, however, fails to account for the context in which the individual offending is occurring. The context of youth violence in Boston was one of disputes fueled by alliances and beefs among 61 active gangs (Kennedy, 2003). Inevitably, individual offending behavior of those gang members was influenced partially by the gang's collective norms. As the individual behavior is a function of set of given norms, attempts to alter the individual behavior must also address the norms from which the behavior derives.

In contrast to the typical individual-only approach, the deterrent effect observed in Ceasefire appears to deter by addressing both the individual behavior and the social dynamic that influences it. This "Focused Deterrence" effect is what sets Ceasefire apart from other deterrence-based crime policy. This is, in effect, two layers of deterrence working in cooperation to address both individual behavior and group norms. In Ceasefire, an enforcement action commences against a group when someone in that group has been involved in a homicide. While such violent behavior may not be palatable to all group members on an individual level, the collective norms of the group have either condoned or failed to disapprove such violence. Until those group norms change, the group will continue to collectively condone violence, shaping the behavior of its' members. The group-focused enforcement action acts not only a punishment for individual and group deviance from societal norms regarding violence, but as an example meant to deter others.

Deterrence is generated through the call-in, where the consequences of homicide involvement are communicated to members of other groups. The examples of other groups that have been subject to enforcement actions as a result of their collective involvement in a homicide serve to deter on two levels: first, that the individual's actions have repercussions for the group, and second, that the group's lack of collective control over the behavior of its' members can have negative repercussions for everyone in the group.

The "Focused Deterrence" approach has substantial appeal from a policy perspective. Deterrence literature indicates that perceptions of punishment severity and risk vary substantially from person to person, and prior criminals tend to have a fairly realistic assessment of their actual levels (i.e. low risk of capture, and weak punishments). For criminals then, broad threats of punishment have little credibility, and they are less likely to be deterred than non-criminals. Groups and group members targeted by Ceasefire are likely to possess these perceptions, as most have frequent contact with the criminal justice system, often for drug dealing. "Focused Deterrence" compensates for these deterrence flaws in several ways.

For offenders engaged in criminality like drug sales, the perceived risk of punishment inherent in criminality is low. For each transaction that results in arrest, many more result in no punishment. Over time, experiences like this with the criminal justice system give unclear, even misleading signals to offenders. Ceasefire corrects this ambiguity with a clearly defined "line in the sand" that will produce punishment: someone in the group committing a homicide.

Of course, a "line in the sand" for behavior is only meaningful if the potential offenders see the punishment threat as credible. Too often the breadth and magnitude of the criminal justice system makes following up threats impractical- too many cases with too many different circumstances to dish out the same punishment every time for an offense. The clearly defined circumstances of Ceasefire allow credibility to be eventually built- If a homicide occurs and your group is involved, law enforcement will give special attention to all the members of your group that are involved in illegal activity. This punishment threat is one that law enforcement can actually follow through on- a limited number of events (group-involved homicides) and a narrowly defined target (members of said group that are engaged in criminal activity).

A secondary element of credible "Focused Deterrence" is adequately severe punishment. Just as prior offenders tend to have a low perception of punishment risk due to their experience with the criminal justice system, they also know that punishments are often hardly severe. The regular consequences for offenses that they regularly are arrested for will not serve to alter individual and group norms about violence. Instead, sophisticated and aggressive approaches such as use of federal law enforcement, cooperative patrolling with outside agencies, surveillance of activities, undercover narcotics operations, wiretaps, and conspiracy cases may be necessary. On the prosecution side, specialized attention including special prosecutors and limitations on plea bargains may be appropriate.

Deterrence is a concept that has no meaning without a context. Specific and General Deterrence illustrate the two ways which individual behavior can be deterred. For so long deterrence has been defined in simply these contexts, without regard to the social dynamics upon which individual behaviors are dependent. The context of "Focused Deterrence" whereby changes in punishment and punishment threats are intended to modify not only individual behavior, but also group norms, represents an important contribution to the theory behind one of the primary mechanisms for social control of deviant behavior.

CHAPTER III

Methodology

Research Goals

The Ceasefire program was implemented in response to a clear problem- violence in the City of Rochester, particularly homicide. The program is specifically tailored to address an important characteristic of Rochester's homicide problem: group-related homicides of Black men 15 to 30 (Klofas et al, 2001). Ceasefire is intended to incapacitate groups that are involved in homicides, and deter other groups from committing homicides. The goal of any evaluation research is to assess whether or not a program has achieved its' intended effect. In this research, the author attempts to ascertain whether the implementation of Ceasefire has produced decreases in violent crime, particularly in the areas for which the program is intended.

Outcome Measures: Individual vs. Aggregate

Programs such as Ceasefire are often implemented in response to rapid increases of a particular crime. These "spikes" place tremendous pressure on decision-makers from communities to respond immediately with a specialized plan to address the problem (i.e. "not business as usual"). Clearly, programs such as this have clear and limited goals: reduce the crime problem. Simple program goals would seem to make evaluations easy. Unfortunately, the timeframe of implementation makes the creation of appropriate research designs difficult. Instead, proof of success typically comes in output measures:

arrests made, grand jury referrals made, corners cleared, warrants served, amounts of drugs, guns, or money seized. These measures are readily accessible and demonstrate to the community that the criminal justice system has responded to the problem with additional special activity. These output statistics however, fail to speak to the outcomes desired and produced by the program- i.e. has the crime level of concern decreased, and is this decrease attributable to the implementation of the program?

Clearly, evaluation of the Ceasefire program requires the analysis of outcome measures. Outcome measures for evaluating Ceasefire could take two forms. The first would be from an individual offending perspective- has the person stopped offending after going to the call-in? Such an approach feels natural in criminal justice: crimes have a victim and an offender, so crime reduction starts at apprehending and punishing the offender. It logically follows that when evaluating a program, you might implement a controlled randomized experiment, comparing the offending of people who have attended a "call-in" (the treatment group) to a control group to see if levels of offending in the treatment group have decreased more than the control group. An evaluation of this type would be a strong research methodology for assessing changes in individual offending levels, (and may provide insight into Ceasefire's effects on group norms) but it fails to address the primary research goal of the evaluation- Comparative offending tells us nothing about changes in levels of aggregate crime.

The concept behind Ceasefire suggests that the deliverance of a focused deterrence message to violent groups using "messengers" ought to deter more group members than simply the messenger. As group-related violence accounts for a significant portion of all violence, we should expect to see decreases in aggregate levels of violence if Ceasefire were to have an effect. Therefore, the second type of outcome variable to consider for evaluation would be aggregate counts of crime over a period of time.

Research Design

As the goal of Ceasefire is to decrease aggregate levels of violence (both overall and in the M/B/15-30 demographic), then the outcome variables to study must be of an aggregate nature, in this case, counts of crime in a timeframe. For this type of research, two designs are good options: simple interrupted time-series analysis and randomized controlled experiments. In this instance, a randomized controlled experiment is an optimal methodology, but due to practical and political limitations associated with implementation, the author has chosen the alternatively acceptable simple interrupted time-series method.

Strengths of Randomized Controlled Experiments

The primary strength of this approach is the use of treatment and control groups to isolate the effects of the treatment (in this case the Ceasefire program) from other factors. For an evaluation of Ceasefire, a randomized controlled experiment would separate all of the violent gangs in Rochester into two groups: one group of gangs would be subject to the program (including call-ins and enforcement actions) and another group of gangs would not be subject to the program. If the treatment and control groups are indeed similar in nature, then change observed in the treatment group that is not observed in the control group can be directly attributable to the treatment. In other words, the control group accounts for the impact of intervening variables.

Weaknesses of Randomized Controlled Experiments

Unfortunately, the great strength of Randomized Controlled Experiments are also their weakness when it comes to implementation. A successful research design of this nature requires pre-planning before the treatment (the Ceasefire program) is implemented. For example, the first step in such an experiment would be to divide Rochester gangs into a treatment and a control group before implementing Ceasefire. This sorting process did not occur prior to the implementation in Rochester. At the time of implementation, evaluation research design was not a consideration, and therefore, this research design was not an option after implementation had occurred.

If a randomized experiment methodology were planned for this evaluation, it would have been fraught with problems. Ideally, gangs would be evenly distributed between the control and treatment groups in terms of gang violence propensity, with each group having a similar range of violent and non-violent gangs. Some gangs are easy to classify, as they maintain a persistent level of violence (or lack thereof, simply sticking to drug sales), most groups, however, "flare up" and "cool down" over time. Any results, therefore, of a randomized experiment would likely be skewed by imbalance in the levels of violence between the control and treatment group.

Additionally, the notion of selectively implementing a gang violence program may be unpalatable for the decision makers investing both political capital and agency resources in the effort. The selective implementation could also conceivably hinder the effectiveness of the program, especially considering the deterrence message is based upon consequences related to group-related homicide. If, over time, it becomes clear that only certain gangs (that are subject to the Ceasefire program) will face repercussions as the result of their involvement in a homicide, while other gangs (that are not subject to Ceasefire), face no repercussions, the credibility of the deterrence message for the gangs in the treatment group will inevitably decrease, thereby reducing the likelihood of the program having an effect.

Interrupted Time-Series Analysis

The most direct and appropriate approach to assessing Ceasefire is to conduct an interrupted time-series analysis. Typically, program evaluations of this nature use an interrupted time-series research design (Cook, 1979). The null hypothesis of this research design is simple- no difference in the time series is evident when comparing the period before the implementation of a program to the period after the implementation has occurred. Therefore, one would hope to disprove this null hypothesis in favor of showing a difference between the two periods. This difference provides evidence that the program may have had an effect on the time series. The benefit of interrupted time-series analysis for this evaluation is that is lacks the grouping selection problems that make a randomized experiment difficult in this situation. This design is very easy to implement, and the data necessary to complete it is readily available.

Weaknesses of Interrupted Time-Series Design

There are several weaknesses to interrupted time-series design for this evaluation. The first shortcoming of a time-series approach to analyzing this data is the length of the dataset. A common "rule of thumb" for time-series analysis is to have a minimum of 50 observations (Cook, 1979). This minimum is necessary to assess correlated error in the time-series. This dataset includes 60 observations, 15 of which are in the post-test period. The issue of a minimum number of observations is clearly a weakness in this case, as the number of observations in this data is very close to the minimum. This issue is of particular concern, as the need to lag the intervention variable (to test for program effect), will further reduce the number of observations.

As just noted, another weakness of Interrupted Time-Series Design is that of the placement of the break point in the time series. The placement of the break point is critical; effects can be minimized because a break point was placed too early or late in the time series, relative to the time at which the actual implementation of the program occurred. Placement of the break point is not always as simple as identifying the date when the program was implemented. The effect of a program can lag and/or anticipate any actual "start date" of a program. In other words, the effect of the program does not necessarily begin when the program starts. Braga et al. note “Implementation lags in policy interventions make it very difficult to say that a particular date defines the break between the pre-program and post-program periods, even when the implementation date is known” (Braga et al., 2003). Anticipatory effects are extremely unlikely to have occurred in this instance because the program was not made public prior to their implementation. Instead, one can plausibly envision lagged effects; the dissemination of the focused deterrence message takes time to diffuse into the targeted population, and therefore, it is unlikely that the program is in full effect on October 2003, the date of the first call-in.

In the case of this analysis, the first call-in for Ceasefire occurred on October 3, 2003. Given that the unit of analysis will be monthly counts, the break point is October 2003. The date is not perfect, however, due to the inexact nature of program effects. In Braga et al, “Testing for Structural Breaks in the Evaluation of Programs” (2003), A structural breaks methodology was applied to the Boston homicide time series to identify the optimal location of the break, thereby inferentially accounting for any lag. While this study does not use this methodology, it is of note that the 2003 study found the optimal break point (the break point where program effect became apparent) to be June 1996, whereas the 1998 interrupted time-series design used a break point of May 15, 1996- The day of the first call-in in Boston. Because of the uncertainty around when program effect actually begins to occur, this study will examine not only the October 2003 breakpoint, but also a variety of lagged intervention breakpoints.

Another issue involving interrupted time-series design is that of alternative program impact. In any given jurisdiction around the country, a variety of initiatives may be in place at any one time. A researcher evaluating the impact of a program must identify the (if any) programs in operation at the same time as the program of interest. After identifying competing programs, one must determine if these programs could have impacts that might account for impacts observed in the evaluation of the program of interest. In some cases, the presence of such programs may need to be accounted for in the analytical model. In the case of Rochester, one significant program has simultaneously co-existed with Ceasefire.

Project IMPACT was a program that ran intermittently from April 2004 through October 2004. The program consisted of co-operative joint tactical patrols between the Rochester Police Department, the Monroe County Sheriff's office, and the New York State Police. These tactical patrols focused on violence "hotspots" in the city, most of which are areas where gangs are active. When in action, these patrols drastically changed the active "force size" of officers on the road, adding as many as 80 additional officers. While the goal of this program was roughly in line with that of Ceasefire (violence reduction), the means were drastically different. The IMPACT patrols focused on heavy patrol-type activities including clearing corners where drug dealing was suspected, traffic checkpoints, and bike patrols. If this change in patrol behavior were to affect change in levels of violence, it would likely be due to the incapacitation effects produced by the patrol's arrests. Due to the intermitted nature of the patrol, a simple proxy variable is insufficient to assess the potential impacts of the IMPACT patrol on violence. The author has attempted to examine the effects of the patrol on violent crime through its output products (felony and misdemeanor arrests) to determine if changes in arrests are associated with reductions in violence. To account for potential lag effects of mass arrests, the author has also examined the monthly counts of criminals sent to state prison.

The last major issue of concern, stochastic error, is perhaps the most important. There are two processes that exert influence over time-series data, deterministic and stochastic. The deterministic component of a time-series is normally distributed, independent of error forces, and can be somewhat reliably predicted. The deterministic component of time-series data contains the information needed to assess, for instance, program impact. The other part of the time series, the stochastic component, deals with sources of error in the time-series. By convention, these sources of error are collectively called "noise", but this "noise" has three distinct sources. These noise sources must be adequately controlled for in interrupted time-series analyses in order to get only the impact of the program.

The first source of noise in a time-series is a trend. Over time, chronological data tend to exhibit a generally linear pattern up or down. In most cases, trend is easily discerned through visual inspection; however, in data sets of short duration or high variability, trend may be difficult to see. As a describable systematic source of error, trend can be controlled for in an interrupted time-series analysis.

The second noise source comes from the effects of seasonality. In general, property crime and violent crime peak in opposite times of the year; property crime spikes in the winter, while violent crime problems are at their apex in the summer months. These seasonal patterns repeat themselves every year (to varying degree) and despite their annual repetition, are often misunderstood by criminal justice policy-makers as a sign that, come October, they really have been doing something right to combat violent crime. Like trend, seasonality has a systematic structure that can be accounted for in statistical analysis.

The last source of noise in a time series is completely random. If you control for trend and seasonality in a time-series, you still inevitably get a normally distributed series of random shocks in the data. While the errors should follow a normal distribution, they are unsystematic, and have no structure that can be integrated into a model for analysis. Instead, random error is compensated for using statistical methods.

The author used autocorrelation frequency plots to assess the structure of noise in the dependent variables of this dataset. This tool was unable to adequately diagnose a noise structure that ARIMA modeling can control for. That ACF plots did not show a clear stochastic noise structure does not necessarily mean those structures do not exist in the dataset, especially considering the dataset's limited number of observations. In lieu of ARIMA modeling, we have included two independent variables in the dataset, a simple linear trend variable and a variable measuring mean monthly temperature as a proxy for seasonal effects.

Analysis Tools

To conduct this analysis, the author will use variety of tools of use in an interrupted time-series analysis. First, a simple pre-test post-test comparison is employed to examine basic changes in the level of homicide during the intervention period. The next level of analysis involves t-testing of the pre-test and post-test dependent variable means using the intervention variable and the lagged intervention variables. The final two steps of the analysis involve performing a correlation matrix to inform variable selection for a multiple regression analysis.

Dependent Variables

Clearly, homicide in general and in the M/B/15-30 demographic are the prime variables where program effect would occur, but several other variables are also worth examining. If groups are successfully being deterred from committing homicides, they are receiving and understanding the message communicated at the call-in- if someone in your group commits a homicide, the whole group gets special attention. For the message to have success, violence-conducive behavior would have to be altered, particularly gun carrying and trigger-pulling. It seems likely that crimes involving similar behaviors as homicides- other gun-related violence- could decrease as a result of a successful Ceasefire program. Therefore, the author has obtained over five years of monthly counts (2000- partial 2005) of victims[2] of the following crimes from the records management system of the Rochester Police Department: Homicide, Homicide of Black Male 15-30 (M/B/15-30), Gun Assault 1st Degree[3], Gun Assault 1st Degree of M/B/15-30, Gun Robbery 1st Degree[4], Gun Robbery 1st of M/B/15-30.

[2] An inevitable disconnect exists when examining crimes- should the unit of analysis be the incident or the victim? When counting homicides, however, victims rather than incidents are counted, by convention. We will also, therefore, count victims of gun assaults and gun robberies for analysis purposes.
[3] As defined in NYS Penal Law, Title H, Article 120.10
[4] As defined in NYS Penal Law, Title H, Article 160.15

Independent Variables

The independent variables used in this evaluation fall into two categories: intervention variables and alternative explanatory variables. In addition to the basic intervention dummy variable (which runs 15 months from October 2003 to December 2004), the author will examine dummy variables lagged from one to four months to account for the uncertainty of when Ceasefire actually began to take effect.

The alternative explanatory variables are proxies for a wide variety of phenomenon that could have impact upon the dependent variables. As previously noted, monthly mean temperature serves as a proxy for seasonal variation and a simple linear trend variable represents the possibility of a broad linear trend in the dataset. The monthly unemployment rate acts as a proxy for the influence of economic conditions on violent crime, and monthly counts of felony arrests , misdemeanor arrests, and state prison convictions are meant to account for changes in policing behavior, specifically, the IMPACT patrols of summer 2004.

Hypothesis

The Ceasefire program attempts to reduce homicide in two ways: incapacitation of groups who engage in homicide, and deterrence of groups who have the potential to commit homicide. A group, rather than individual, focus is a prudent approach if the following assumption- groups and group members are involved in many of the total number of homicides each year- is true. Unfortunately, this supposition is difficult to conclusively assess in the instance of Rochester. Prior to the implementation, no up-to-date intelligence was routinely captured and analyzed, so it is impossible to say, prior to the implementation of Ceasefire, how many homicides truly involved groups or group members.

We must rely on proxy measures, for instance, victimization levels in the M/B/15-30 demographic. From 2000-2003, this "high-risk" demographic accounted for 45% of all homicide victims (N=179), and 56% of all known homicide suspects (N=109) in Rochester. Of the over 700 gang members now in the Rochester Police Department's gang database, 86% are Black Males, ages 15-30. Prior research also indicates that group involvement is a key characteristic of youth violence (Zimring, 1998). Clearly, this demographic is disproportionately at risk for engagement in violence in the city of Rochester. If the assumption of group involvement is true, it is reasonable to believe that the Ceasefire program could have powerful incapacitation effects and broad deterrent effect causing declines in homicide within the target demographic[5], and by extension, in all demographics. Additionally, if homicides decline, it is reasonable to assume crimes that look similar in nature to homicides- severe gun assaults and robberies- would also decline, as the Ceasefire program is really deterring violent behaviors among groups.

The author, therefore, hypothesizes that a causal negative relationship exists between the Ceasefire intervention and all six dependent variables (Homicides, Gun Assaults and Gun Robberies in both the M/B/15-30 demographic and among all victims).

[5] It is important to note that the Ceasefire program draws no racial distinction in enforcement actions or call-ins. All members of a group, regardless of race or ethnicity, are candidates for a call-in, or if their group is involved in a homicide, an enforcement action. The M/B/15-30 is used for evaluation purposes because blacks are disproportionately represented in victimization, offending, and gang involvement relative to other races. The demographic then serves as a particularly tight measure from which to observe the potential impacts of a highly focused initiative.

Using the tools mentioned earlier, the author seeks to disprove the following null hypotheses:

For t-tests:

$$Ho: \mu(\text{pre-test}) = \mu(\text{post-test})$$

$$Ha: \mu(\text{pre-test}) \neq \mu(\text{post-test})$$

For multiple regression analysis:

$$Ho: \beta(\text{dependent}) = 0$$

$$Ha: \beta(\text{dependent}) \neq 0$$

<u>Definitions</u>

ALL ASS: Total monthly counts of Assault 1st victims in which a gun was used during the event. (DEPENDENT)

ALL HOM: Total monthly counts of homicide victims. (DEPENDENT)

ALL ROB: Total monthly counts of Robbery 1st victims in which a gun was used during the event. (DEPENDENT)

FELONYAR: Monthly counts of felony arrests made by the Rochester Police Department in Rochester, NY. (INDEPENDENT)

INT: A dummy variable representing the Ceasefire intervention, a value of 1 indicates the presence of the intervention. In this variable, the Ceasefire intervention runs 15 months from October, 2003 to December, 2004. (INDEPENDENT)

LAGS (INT_1): A dummy variable representing the Ceasefire intervention, a value of 1 indicates the presence of the intervention. This variable is lagged 1 month, making the Ceasefire intervention run 14 months from November, 2003 to December, 2004. (INDEPENDENT)

LAGS (INT_2): A dummy variable representing the Ceasefire intervention, a value of 1 indicates the presence of the intervention. This variable is lagged 2 months, making the Ceasefire intervention run 13 months from December, 2003 to December, 2004. (INDEPENDENT)

LAGS (INT_3): A dummy variable representing the Ceasefire intervention, a value of 1 indicates the presence of the intervention. This variable is lagged 3 months, making the Ceasefire intervention run 12 months from January, 2004 to December, 2004. (INDEPENDENT)

LAGS (INT_4): A dummy variable representing the Ceasefire intervention, a value of 1 indicates the presence of the intervention. This variable is lagged 4 months, making the Ceasefire intervention run 11 months from February, 2004 to December, 2004. (INDEPENDENT)

MB HOM: Monthly counts of Black Male homicide victims ages 15-30 in the city of Rochester, NY. (DEPENDENT)

MB ROB: Total monthly counts of Black Male Robbery 1st victims, ages 15-30, in which a gun was used during the event. (DEPENDENT)

MB ASS: Total monthly counts of Black Male Assault 1st victims, ages 15-30, in which a gun was used during the event. (DEPENDENT)

MEANTEMP: Monthly mean temperature values in Rochester, NY. (INDEPENDENT)

MISDARR: Monthly counts of felony arrests made by the Rochester Police Department in Rochester, NY. (INDEPENDENT)

STATEPRI: Monthly counts of state prison sentences issued in Monroe County, NY. (INDEPENDENT)

TREND: Simple equal interval linear trend variable, running from .08 to 5 in chronological order. (INDEPENDENT)

UNEMPL: Monthly unemployment rate in the city of Rochester, NY. (INDEPENDENT)

CHAPTER IV

Results and Findings

Descriptive Statistics

This analysis assumes normal distributions of the six dependent variables. Although several variables exhibit signs of positive skewing[6], the data appear to be roughly normal. Indeed, a Poisson distribution may be most appropriate for this data set, however, the analytical tools associated with this distribution are beyond the purview of this analysis. See Braga et al. (2001) for advanced methods assuming a Poisson distribution with data similar to that being examined in this evaluation.

Pre-Test Post-Test Analysis

The length of the dataset for all variables is 60 cases. The data is organized by monthly counts starting from January 2000 and ending in December 2004. The first Ceasefire call-in occurred in October 2003, marking the official start of the program and post-test period. The pre-test period, January 2000 to September 2003, covers 45 months. The post-test period, October 2003 to December 2004, covers 15 months. The table below shows the monthly means for the pre and post-test periods of each dependent variable.

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[6] See Exhibit I of Appendix for Descriptive Statistics and Histograms.

	Pre-Test Monthly Mean (1/00-9/03)	Post-Test Monthly Mean (10/03-12/04)	% Change from Pre-Test Mean
MB_HOM	1.71	1.00	-41.56%
ALL_HOM	3.64	3.40	-6.71%
MB_ASS	1.96	1.80	-7.95%
ALL_ASS	3.22	3.27	1.38%
MB_ROB	9.02	7.80	-13.55%
ALL_ROB	41.13	50.93	23.82%

Overall, there seems to be a modest decline in the mean number of monthly homicide victims during the post-test period, but a rise in mean monthly victims of 1st degree gun assaults and 1st degree gun robberies. In the M/B/15-30 demographic, decreases are evident in homicide, gun assaults and gun robberies during the post-test period. The decrease within homicide is particularly large, in both percentage and real terms, when you consider the relative infrequency of the event within the context of the temporal scale of analysis (month). The difference in direction of the percentage changes when comparing the M/B/15-30 means to the total victimization means suggests that a change in victimization level among the M/B/15-30 demographic may have been occurring that was not occurring in other demographics.

As discussed in Chapter III, there is reason to believe that the full deterrent effect of Ceasefire may have lagged behind its implementation date of October 2003. Indeed, a distinct natural break occurs in the monthly counts of M/B/15-30 homicide victims as of January 2004. From January 2004 to April 2004, no M/B/15-30 homicides occurred. No similar stretch of four months without a M/B/15-30 homicide exists within the dataset. If a three-month lag occurred and actual program effects began to become evident in January 2004, then years can also be used as a measure to conduct basic pre and post-test analysis.

	2000	2001	2002	2003	00-03 Average	2004	2004 % Change from Average
MB_HOM	15	22	15	31	21	9	-56.6%
ALL_HOM	40	41	41	57	45	36	-19.6%
MB_ASS	22	21	30	21	24	21	-10.6%
ALL_ASS	28	42	38	44	38	42	10.5%
MB_ROB	114	112	98	120	111	79	-28.8%
ALL_ROB	490	479	480	626	519	540	4.1%

The above table provides yearly counts for the six dependent variables. The column "00-03 Average", is analogous to a pre-test mean if we assume a three-month lag in program effect. 2004 essentially is the post-test period. This version of the pre-test post-test analysis yields essentially similar results to the initial analysis- a modest decrease in total homicides during the post-test period and small increases in both gun assaults and gun robberies. Likewise, the M/B/15-30 demographic shows a large decrease in homicide and modest decreases in gun assaults and robberies during the post-test period.

<u>T-Tests</u>

Given that the basic pre-test post-test analysis indicated some differences in the pre and post-test means of the dependent variables, the next step of the analysis was to assess the statistical significance of these differences. To do this, independent sample T-Tests were conducted on all six dependent variables, using the intervention dummy variable to group pre-test and post-test data. To examine the potential for program effect lag, T-tests were also run by groupings according to four separate lagged intervention variables, which lagged from one through four months. All statistics examined assumed no equal variance between the pre and post-test groups.

The initial T-test of the original intervention variable (see Figure C in appendix) showed one statistically significant difference between the pre-test and post-test means. During the intervention period, total gun robberies increased by an average of 9.8 per month. After lagging the intervention one month, t-tests indicate a statistically significant average decline of .83 M/B/15-30 homicides per month during the intervention period. No other statistically significant differences in means were observed during the one-month lag t-tests.

The t-tests performed using a two month lagged intervention variable for grouping showed no statistically significant results, although MB HOM was very close (.059) to showing a .83 decrease in monthly homicides. When performed with the three-month lagged intervention variable, a statistically significant average reduction of 1.03 M/B/15-30 homicides per month was evident in the MB HOM variable during the intervention period. No additional differences in means of significance existed. The last set of t-tests, grouped by the four-month lagged intervention variable exhibited two statistically significant changes in mean during the intervention period. MB HOM decreased by an average of .89 homicides per month during the intervention period, and MB ROB decreased by an average of 3.19 gun robberies per month.

The t-tests indicate several important points- that no statistically significant decrease in level of total gun assault, gun robbery, or homicide occurred after the implementation of Ceasefire, regardless of when potential effects may have begun to take effect. Therefore, if the Ceasefire program were to have had impact on overall incidents of gun assaults, gun robberies, and homicides, these impacts were limited at best, and most likely, negligible.

In the "high-risk" M/B/15-30 demographic, however, there was persistent evidence of a significant decline in the number of monthly homicides after lagging the intervention one month. The peak of statistically significant declines occurred during the third month lag, yet another variable, MB ROB showed a statistically significant decline with MB HOM when t-tests were run using the four-month lagged intervention variable. All together, the t-tests suggest that if Ceasefire had impact, it happened in and only in the desired "key demographic", and that some program effect lag may have occurred. What t-test analysis fails to account for is the influence of additional factors.

Correlation Matrix

As discussed in Chapter III, the author selected a variety of independent variables that hypothetically might influence the dependent variables in this analysis. A correlation analysis using Pearson's r was performed to assess whether relationships do exist between the independent and dependent variables, and to examine the nature and strength of those relationships. Of primary interest was evidence of negative correlations between any of the dependent variables and any of the five intervention variables. Such evidence furthers the possibility of disproving the null hypotheses of this experiment (i.e. that the Ceasefire intervention had no effect on our dependent variables), and informs the next step of the analysis, namely, which dependent variables are worth examining through multiple regression, and which independent variables ought to be included in those regression models.

Three of the dependent variables, ALL HOM, MB ASS, and ALL ASS, exhibited no signs of statistically significant correlation with any of the five intervention variables, nor any correlation approaching statistical significance. As the relationships between these dependent variables and the intervention variables do not seem to be statistically significant, they were excluded from further analysis.

ALL ROB was the only dependent variable to correlate to the original intervention variable, INT, at a weak[7], but statistically significant, level. At r=.303, the direction of the correlation is opposite of what would be expected had the Ceasefire program been successful at reducing gun robberies. Instead, increases in gun robberies seem to be weakly related to the intervention period. This relationship may be the result of a collinearity problem, explained by a temporal pattern inherent in the ALL ROB variable. Both ALL ROB and INT are positively correlated to TREND at statistically significant levels. The strength of the relationship between ALL ROB and TREND is low (r=.300), but the relationship between TREND AND INT is high (r=.734). Because the intervention occurred in the temporal end of the dataset, the possibility exists that the relationship between ALL ROB and INT could be the result of the influence TREND exerts on both variables. If the relationship were spurious, we might expect to see a similar dynamic in the relationships between ALL ROB and the other intervention variables. While there are high levels of correlation between the other intervention

[7] By social science convention, strength of correlation is defined by quintile ranges and their accompanying descriptors: very weak (r=00-.29), weak (r=.30-.49), moderate (r=.50-.69), strong (r=.70-.89), and very strong (r=.90-1.00).

variables and TREND, there are no statistically significant relationships between ALL ROB and the other intervention variables. Even though the relationships lack significance, the change in direction and strength of the relationships is worth noting. When examining the non-significant relationships between ALL ROB and the lagged intervention variables, a pattern of decline is evident. From LAGS (INT_1) to LAGS (INT_3), the already weak positive relationship with ALL ROB decreases, until the relationship between ALL ROB and LAGS (INT_4) is negative and weak. The author suspects this pattern is consistent with the collinearity issue related to TREND. The INT variable covers the period from October 2003 through December 2004, which is essentially five seasons, two of which are winter. In general, Robberies tend to occur more often in winter months[8], so the correlation between INT and ALL ROB includes one additional season's worth of Robberies. As the intervention variables are lagged, the temporal effect of the extra winter season is diminished, and with it, the statistically significant positive relationship observed in the relationship between All ROB and INT. Due to the likely spurious relationship with the INT variable, ALL ROB is excluded from further analysis.

Another correlation of note involving ALL ROB was a weak negative correlation with MISDARR. Such a relationship could conceivably validate a "broken windows" approach to police activity. If Robbery offenders are also engaged in lesser misdemeanor crimes, then policing focus on misdemeanor offenses (causing an increase of misdemeanor arrests) could yield Robbery offenders, and thereby reduce the number of Robberies. This is an unexpected and ancillary finding to the focus of this study;

[8] While no statistically significant relationship exists between ALL ROB and MEANTEMP in this data, the direction of the relationship is negative.

however, it is of interest due to the potentially intervening effects of the IMPACT focused patrols performed jointly by State Police, Monroe County Sheriffs and the Rochester Police Department during the summer of 2004. This patrol could have produced the type of focus on lesser offenses necessary to increase misdemeanor arrests. Alternatively, ALL ROB and MISDARR are both correlated to TREND, again illustrating the likely spurious relationship between ALL ROB and MISDARR.

Whereas the ALL ROB variable had a statistically significant relationship with the original intervention variable, the MB ROB variable was observed to have a weak, negative relationship (r=-.269) with LAGS (INT_4) of statistical significance. Unlike the ALL ROB variable, MB ROB did not have a relationship with TREND of statistical significance, making collinearity due to temporal factors not an issue[9]. In addition to the statistically significant relationship with LAGS (INT_4), the author observed a weak, negative relationship (r=.-245) between MB ROB and LAGS (INT_3) that approaches statistical significance (p= .066). No other relationships of significance were observed between MB ROB and the other independent variables.

The only other dependent variable to have a significant relationship with one of the intervention variables was MB HOM, with a weak, negative relationship (r=-.271) with LAGS (INT_3). A significant relationship was also observed between MB HOM and MEANTEMP (r=.318). These relationships suggest a significant decrease in M/B/15-30 homicides occurred during the intervention period, (lagged three months), and that homicides in the demographic increased as temperature increased.

[9] Other independent variables of a temporal context were also examined. MEANTEMP does not have a statistically significant relationships with any of the intervention variables nor MB ROB, although MB ROB has a weak, negative relationship with MEANTEMP that approaches significance (p=.065).

Multiple Regression

As a result of the correlation matrix, several relationships worth examining through regression analysis were identified. The dependent variables MB HOM and MB ROB have significant (or near-significant) relationships with the intervention variables LAGS (INT_3) and LAGS (INT_4), as well as the proxy independent variable for seasonality, MEANTEMP. Therefore, the following models were evaluated:

Model 1- MB HOM = constant + LAGS (INT_3) +MEANTEMP

Model 2- MB HOM = constant + LAGS (INT_4) +MEANTEMP

Model 3- MB ROB = constant + LAGS (INT_3) +MEANTEMP

Model 4- MB ROB = constant + LAGS (INT_4) +MEANTEMP

All four models had fairly weak explanatory power, with no R-square value higher than R^2=.153. These low values indicate a substantial amount of variance within both MB HOM and MB ROB that regression analysis failed to account for, either due to limitations of OLS in the context of time-series data[10], or the influence of unaccounted for predictor variables. The author attempted to examine the data using ARIMA modeling, but was unable to identify representative error structures consistent with the three sources of noise that ARIMA corrects for: auto-regression, integration, and moving averages.

The primary goal of this study, however, was not to create a predictive model, but to evaluate the extent to which the Ceasefire intervention is responsible for changes in

[10] OLS modeling may produce incorrect results when dealing with time-series data, as autocorrelation in the time-series violates the assumption of independence in the error term of the regression model ("Chapter 9", 2003).

homicide, gun assault 1st, and gun robbery 1st, particularly in the "high-risk" M/B/15-30 demographic. For this purpose, the Beta statistic is of greatest interest, as it provides an assessment of the variance in the dependent variable explained solely by the predictor variable of interest, specifically, the intervention variable. In Model 1, the Beta for LAGS (INT_3) is $\beta = -.250$, and is, for practical purposes, statistically significant at $p=.052$. For Model 2, the Beta value of LAGS (INT_4) at $\beta=-.231$, is comparable to Model 1 but not statistically significant ($p=.074$). Model 3 has a Beta of $\beta=-.269$, similar to the Beta of Model 4 ($\beta=-.268$), both of which are statistically significant. Probability plots of residuals for the four models suggest that the models are a reasonably good fit for the data (See Appendix E). There does appear to be some slight oscillation, however, possibly due to seasonal fluctuations. While these models have included temperature as a proxy variable for seasonality, advanced time-series models control for these factors more effectively than OLS for the purposes of predictive modeling, but are out of the scope of analytical methods for this study.

As both Model 1 and Model 3 (the models using LAGS (INT_3) produced statistically significant Beta values, it seems likely that as of January 2004, the Ceasefire intervention was having measurable effect upon both the MB HOM and MB ROB variables. The Betas indicate that from January 2004 (three months after the intervention was implemented) to December 2004, homicides and gun-involved Robbery 1st incidents involving Black Males, ages 15-30 declined by an average of 25% and 26.9% per month, respectively.

If the intervention was having effect on the dependent variables by January 2003, one would expect to observe statistically significant Beta values for LAGS (INT_4) as well. While this was true for regression model 4 (involving MB ROB and LAGS (INT_4)), regression model 2 produced a Beta for LAGS (INT_4) that was not statistically significant. One possible reason for this phenomenon is a convergence of infrequent events and small sample size. The sample size for both Models 2 and 4 was 56 cases, only barely the accepted minimum needed to perform regression analysis. However, while the average monthly number of events during the four month lagged intervention period was 6.27 for MB ROB, the average for MB HOM was only .82 per month. The total number of events during the four month lagged intervention period for MB ROB was 69, but only 9 for MB HOM. Low sample size does not seem to have been a problem in Model 4, where events were much more frequent, but the infrequency of events may have posed too much of a problem for the regression model to handle in the case of Model 2. Indeed, the results of Model 2 may be substantively significant, but OLS regression models seem to be an insufficient tool to ascertain the model's statistical significance given the limitations of the data.

To validate the variable selection method used for the regression models (i.e. t-tests and correlations informing choice of variables to use), the author conducted five regressions on all six dependent variables- one for each intervention variable- including all independent variables in the model using the ENTER method. The results of the 30 regressions mirror the results observed using the approach to variable selection employed by the author. For the dependent variables MB ASS and ALL ASS, no significant relationship with an intervention variable was observed in any model. This finding is in keeping with the decision to exclude these variables from regression analysis as a result of their lack of correlation with any of the intervention variables. ALL ROB exhibited statistically significant beta values with the three and four month-lagged intervention variables, but in both regression models also showed a statistically significant relationship with the TREND variable. Given the previously discussed collinearity problems between ALLROB, TREND and the intervention variables, the author believes this finding can be reasonably discounted. The "lumped" regression models showed one other result that was not produced by the author's model selection method. The ALLHOM regression model where the three month-lagged intervention variable was used showed a statistically significant relationship between the two variables (β =-.429, p=.052). The only other independent variable in this model with a relationship of statistical significance with ALLHOM was MEANTEMP, which was expected given the widely known correlation between temperature and violence. Nevertheless, a statistically significant correlation between ALLHOM and LAGS (INT_3) did not exist in the correlation matrix, yet in the regression model this relationship exists. The author suspects this change is once again due to the involvement of the TREND variable. In the correlation matrix, LAGS (INT_3) exhibited a strong correlation with TREND and TREND exhibited a moderate correlation with ALLHOM. Therefore, the finding of a statistically significant relationship between ALLHOM and LAGS (INT_3) in the regression model should be discounted as a product of collinearity.

CHAPTER V

Implications of Findings, Limitations of Study,
and Recommendations for Further Research

Implications of Findings

Program Goals and Evaluation Objectives

In the fall of 2003, the Rochester criminal justice community implemented Ceasefire for the purpose of responding to increases in gang-involved violent crime. Ceasefire had clearly definable goals: to reduce homicide among Black Males ages 15-30, and by extension, reduce gun violence in the demographic. As homicide and gun violence victimization and offending disproportionately involves Black Males ages 15-30 (and much of the violence in this demographic involved group dynamics), it was believed programmatic focus on this problem would yield reductions that would have an effect on total homicides and gun violence.

This study sought to evaluate Rochester's success or failure in achieving the goals of the Ceasefire program. In keeping with the program goals, this study assessed the impacts of the Ceasefire program on homicide and gun violence in the M/B/15-30 demographic as well as overall homicide and gun violence.

Were Ceasefire to achieve an optimal expected level of success in Rochester, an evaluation would have observed a "tipping point" effect in crime rates, where groups would be deterred from not simply from committing homicides but from engagement in homicide-producing behavior (i.e.-gun violence). This reduction in violent behavior would obviously reduce levels of offending and victimization in the M/B/15-30 demographic, but because that demographic comprises such a large portion of all homicide and gun violence, reductions in total levels of homicide and gun violence would also be observed. The homicide-gun violence link is implied in the deterrence message; One deters behavior not events, so in order to avoid an enforcement action, a group must refrain not just from committing homicides but from gun violence in general (as any shooting can easily become a homicide). This “tipping point” effect would have been the expected result of successful Ceasefire program, however, this research does not confirm the occurrence of this effect. Instead, the evaluation has found desirable but modest results of limited scope.

Evidence for Satisfaction of Program Goals

A variety of measures indicate that the Ceasefire program has had meaningful effect in reducing homicide in the M/B/15-30 demographic. Simple measures show reductions of 41% in M/B/15-30 homicides, 8% in M/B/15-30 gun assaults, and 14% in M/B/15-30 gun robberies during the period Ceasefire was active (10/03-12/04) compared to the pre-test period (1/00-9/03). Statistically significant differences from pre-test to post-test means among M/B/15-30 homicides at one, three, and four-month intervention lags were observed using t-tests. Regression modeling indicated significant average monthly reductions of 25% for M/B/15-30 homicide and of 27% for M/B/15-30 gun robberies.

Evidence against Satisfaction of Program Goals

The evaluation did not find much evidence to validate the "tipping point" hypothesis that reductions in M/B/15-30 homicides would produce reductions in M/B/15-30 gun violence, and overall homicide and gun violence victimization. The simple pre-test post-test measures indicated a decrease of 6.7% in the post-test period, but increases of 1.38 and 23.82 in total gun assault and gun robbery, respectably. The T-tests conducted also did little to validate the "tipping point" hypothesis. The only crime variable to show statistically significant decreases was the M/B/15-30 homicide variable, and, in contrast to the hypothesis, a statistically significant increase of total gun robberies was observed (9.8%) during the post-test period using a one month lag. Regression modeling showed only decreases in M/B/15-30 homicide and gun robbery, indicating no meaningful effects on total homicide and gun violence levels.

The findings suggest Ceasefire had effect in the area which it was primarily intended for (M/B/15-30 homicide victimization), however, there is limited support to suggest that the program was able to effect broader gun violence, showing a reduction only in gun robbery victimization in the M/B/15-30 demographic. The lack of a broad, "tipping-point" effect raises questions about the findings of this study. Deterrence occurs in a behavioral context; it is impossible to deter events, but it is possible to deter the behavior that produces the event. In this case, the behavior that produces a homicide is essentially the same as the behaviors that produce gun robberies or gun assaults, so efforts to deter homicides should also deter these similar behaviors. The products of that deterrence should be decreased homicide and gun violence, yet while homicides (in the M/B/15-30 demographic) decreased, gun violence in the demographic has not. This divergence is of concern, and will be examined more thoroughly in Chapter VI.

Limitations of Study

The implications of the findings of this study have meaning not simply for Rochester crime policy, but perhaps for "focused deterrence" as a theoretical concept. The limitations and weaknesses of this study are therefore of particular interest when considering its' implications. These limitations can be categorized, as they relate to either issues of research design or limitations of statistical tools and data.

Limitations of Research Design

The fundamental challenge of examining the existence of deterrence is the one noted by Gibbs (1975), that deterrence, as phenomena, requires a context in which to be studied. The interrupted time-series research design employed by this study implies that decreases in levels of crime must be attributed to the deterrent effect that Ceasefire is intended to produce (when all recognizable factors are accounted for). The study does not measure the extent of deterrence produced, or if any deterrence was produced in the first place, it simply assumes that the Ceasefire program should, when implemented, cause reductions in specific crimes, and those reductions should be attributable to the intended deterrent characteristics of the program.

On a more practical level, the interrupted time-series research design was not the optimal design for this study. Given the political and time constraints, this research design was the only option available, but a randomized control group study would have been a superior approach to this evaluation. This issue was previously discussed in Chapter III, but the primary advantage to control groups is the enhanced ability to control for unanticipated factors that might influence the program's effect on the test population. A control group design would not have solved the problem of measuring actual deterrence but it would have improved the confidence in the validity of the research findings.

Limitations of Statistical Tools & Data

The statistical toolset typically employed for time-series analysis does not, generally, respond well to limited datasets. This dataset of monthly crime counts consisted of 60 observations, 15 being in the post-test period, and only 12 in the three-month lagged intervention variable that showed a statistically significant relationship with M/B/15-30 homicide. These numbers approach the bare minimum counts necessary to conduct statistical analysis, however is of even more concern given the numerical size of each observation. Gun assault, gun robbery, and homicide, especially, are fairly rare events. Any random fluctuation in such a limited dataset of rare events could produce a misleading effect when applying statistical methods.

The problem of limited data raises questions about the appropriate tools to apply to this dataset. The author attempted to conduct ARIMA analysis of the data set, but was unable to decipher the noise structure of the crime data when examining ACF and PACF plots. The existence of correlations between several of the crime variables and the mean monthly temperature independent variable undermines the ACF and PACF plots, as seasonal correlation was clearly observed. The ACF and PACF plots may have failed as a diagnostic tool because of the limited size of the dataset. The ACF and PACF plots attempt to find noise patterns by lagging residuals, an approach made difficult by only 60 observations.

The limited observations and rarity of events raise an inevitable question about the appropriateness of t-tests and beta values. This study assumes a normal distribution, in large part because the data used are parameter values- the study is only interested in examining what happened from 2000-2004. However, given the peculiar characteristics of this data, perhaps for statistical analysis purposes, tools using Poisson distributions would have been more appropriate. In his reviews of Boston Ceasefire data, Braga employed log-linear models (2001), and the Bai-Perron method of using Wald tests (2003) to identify structural breaks in time-series data, a method that is robust when dealing with potentially non-normally distributed data.

Besides data length issues, the other primary limitation of this study, in terms of statistical tools and data, is one of independent variable selection. The regression models used in this analysis considered a limited amount of alternative causal variables, and given the complexity of forces that impact violent crime rates, other unidentified factors that are potentially meaningful might exist. Medical care, for example, could greatly impact homicide counts from one year to the next if critically injured gunshot victims are operated upon. Another potential factor is police activity. In the summer of 2004, the

Rochester Police Department teamed with the Monroe County Sheriff's Department and the New York State Police to conduct periodic focused patrol details in the most violent neighborhoods in the city, neighborhoods where gangs are disproportionately concentrated. This detail was not added into the evaluation primarily due to timing problems (the details ran periodically and could not match up to the unit of observation used in the study), as well as initial reviews of pre and post test data indicated limited or no impact on the crimes examined in this study. This detail and other police of varying scope have the potential to greatly impact infrequent events such as gun violence.

Recommendations for Further Research

For the first year of Ceasefire in Rochester, this study's findings are encouraging early indicators of a successful program; statistically significant monthly reductions of the target crime attributable to the Ceasefire program. While the study does not presume to be the conclusive evaluation of the Ceasefire program in Rochester, its findings validate (all other things being equal) the continuation of the program. However, there remains much to investigate, and the issues, questions, and findings of this research provide a useful stepping-stone for future evaluation research of Rochester's Ceasefire program and of the theoretical concept of focused deterrence.

Apply Different Statistical Tools to Dataset

The positive finding of this study (statistically significant reductions in M/B/15-30 homicide in the post-test period), is tempered by a variety of shortcomings, namely a short dataset of rare events and limits on research design and statistical analysis tools. The evaluations conducted on the Boston Ceasefire program utilized a 96 observation dataset (with a 24 observation post-test period) as well as statistical tools (log-linear regression informed by ARIMA modeling, and Bai-Perron structural breaks methods) beyond the purview of this study. For data as observed in Boston and Rochester, Braga's methods are optimal. The log-linear models compensate for two primary weaknesses of this study, limited size of dataset and rarity of events (through a Poisson distribution and log-odds ratios) and the potential interference of time-series-related noise (through ARIMA modeling). The Bai-Perron method removes the weaknesses associated with dummy variables in this situation by testing for Wald statistics of significance across all possible break points of a series to test for breaks in the structure of the time-series as opposed to a change in parameters at the time of introduction of the dummy variable to the time-series. More conclusive findings involving the Ceasefire data will require approaches such as these over a longer period of study.

Qualitative Examination of Focused Deterrent Effects

Perhaps the area in most need of further research is the existence and extent of Ceasefire's deterrent effect. No research has been done to examine what, if any message is communicated from the call-in attendees to their fellow crew members, likewise, it is unclear whether the enforcement actions add meaningful credibility to the message and whether the enforcement actions are known to crew members that do not attend the call-ins. If crews are aware that other crews are being taken off of the streets, do the crews associate this police activity with Ceasefire? Questions of this nature require a qualitative approach to examining the perceptions of crew members. Issues of punishment risk, intra-crew communication, peer pressure to offend, cohesiveness of the group unit, and the function of the "messenger" as a communication mechanism are among the critical questions to understanding how the assumed deterrence effects of Ceasefire actually work in practice. Qualitative efforts such as focus groups, surveys, or ethnographic observation of crew members could contribute greatly to the understanding of the individual and group behaviors that Ceasefire is intended to deter.

Re-Assessment of Characteristics of Violent Crime

As a program, Ceasefire is an attempt to solve the problem of violence by addressing one of its' primary characteristics, gang involvement. This characteristic matters because it is a substantial and definable pattern among a disparate set of characteristics that comprise the entirety of a communities' violence problem. One can never hope to programmatically address all facets of violence in a community, much less, effectively, but patterns such as gang involvement provide law enforcement with an targeting opportunity, a means to concentrate resources for improved effectiveness and efficiency.

It is, of course, possible that the major characteristics of violence in a community will change over time. Programs that target gang violence no longer remain relevant when the gang component of violence subsides. If violence dynamics change and no one is paying attention, police anti-violence strategies designed around a previous set of violence characteristics are left wasting precious resources and making no impact on the problem. For this reason, continuous review of the nature and characteristics of crime problems are essential to the prevention of the problem.

In Rochester, strong partnerships exist between researchers and the Criminal Justice community. Through the process of implementing Ceasefire, practitioners have depended upon research to inform problem identification, implementation and evaluation. After just over a year of operation, the Ceasefire program was due for assessment of its success or failure in addressing the problem of gang homicide. Initial results indicate modest success in addressing the defined problem. The next step is to re-visit the initial problem identification stages of the effort to examine whether or not the problem of gang homicide still exists as a primary characteristic of local violence. Through continuous data-driven examination of Rochester's violence characteristics, the local criminal justice system will have the knowledge necessary to adjust their strategies and efforts to achieve their real goal, meaningful long-term reductions of violence in Rochester.

CHAPTER VI

Postscript

The data reviewed for this study cover a time period from January 2000-December 2004, with the Ceasefire program active from September 2003 through December 2004. Within the intervention period, the study observed a statistically significant average monthly reduction in M/B/15-30 homicides. As of July 2005, the Rochester Ceasefire program remains in place, yet homicide statistics have changed dramatically. Through the end of July 2005, 28 homicides have occurred in Rochester, 14 of which were victims in the M/B/15-30 demographic. By contrast, 22 homicides occurred through the end of July 2004, with only 5 in the demographic. From 2000-2004, Rochester averaged 25 total homicides and 11 M/B/15-30 homicides through the end of July. Rochester homicide is at its second-highest level since 2000, exceeded only by the anomalous year of 2003.

The 2005 data look nothing like the remarkable reductions of 2004, despite the ongoing efforts of the Rochester Ceasefire program. When these most recent seven months of homicide data are taken into consideration, the findings of this study inevitably come into question, and by extension, the effectiveness of the Rochester Ceasefire program. This chapter is intended to consider some of the possible explanations why Ceasefire has failed to produce in 2005, and what fixes may need to occur to get back on the right track.

Regression to the Mean

The simplest explanation for the 2005 increase in homicide is that Ceasefire had no actual effect in 2004, and that both the 2004 decreases and the 2005 increases so far are variance around the mean level of homicide in Rochester. This explanation is a real possibility; the statistically significant findings of this study were observed when using a lagged intervention variable that essentially used the twelve months of 2004 as the intervention period.

In terms of yearly counts, the 2004 M/B/15-30 homicide total (nine) is much lower than the totals in any of the previous four years examined in the study (2000-2003). This seemingly meaningful drop, however, may be a function of the length of the data set rather than trend. For example, in January 2005, 3 of 6 homicides were in the M/B/15-30 category. While three additional homicides may not seem to make a difference, homicides are rare statistical events, and even that small of a number could produce findings that are not statistically significant. Likewise, statistical significance says nothing about substantive significance, and since we possess no direct evidence of deterrence of M/B/15-30 homicides, it is entirely possible that the statistical significance observed is simply a coincidental byproduct of random variation that happens to align with an intervention that has a short evaluation timeframe. Further, 2004 is well within two standard deviations of the four-year yearly average of M/B/15-30 homicide, suggesting that 2004 may have been only a statistical fluctuation.

A possible explanation for why the 2004 fluctuation occurred is related to Ceasefire, but not necessarily the product of the Ceasefire program. The decline may be related to a well-known sociological phenomenon- the “Hawthorne effect[11]”. the idea behind the Hawthorne effect is that research subjects under observation change their behavior. Applied to Ceasefire, it is conceivable that research attention to the issue of gang homicide altered policing behavior, which in turn, altered offending behavior. So it may not have been the actual Ceasefire program that caused the reductions, but simply the focus on gang violence and the resulting changes of police behavior that caused the anomalous 2004 declines to occur.

If indeed the dramatic reductions of 2004 were simply anomalous, then, distressingly, the implication is no changes in crime rate occurred. A finding of this nature would shed doubt upon Ceasefire as a mechanism to deliver focused deterrence, and/or upon focused deterrence as a viable context of deterrence theory.

Systems/Implementation Explanations for the Increase

The most obvious and perhaps the most convincing explanation for the 2005 failures of Ceasefire is an institutional inability to implement the program as designed. There is evidence to suggest that the Rochester program has not followed through operationally, and Kennedy attributes the eventual failure of Boston’s Ceasefire to a breakdown in the operational process (2002). If a breakdown in the process occurred, it might have occurred in one (or more) of three main components of the overall Ceasefire process: inter-agency communication, conducting enforcement actions, or communicating the deterrence message to crews.

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[11] The term “Hawthorne effect” refers to a series of industrial management studies performed by Roethlisberger & Dickson from 1927-1932 (1939). These studies observed that regardless of physical and environmental to the workplace, worker productivity improved under observation.

Inter-agency Communication

In Ceasefire, the term “pulling levers” describes the concept of concentrating diverse criminal justice resources on a specific target. This idea is the cornerstone of Ceasefire, so the ability to collectively concentrate resources is a critical task that partner agencies must quickly learn. The tasks that each agency must individually perform are not new- the District Attorney’s office understands how not to offer a plea bargain, Probation and Parole understand how to tighten their supervision conditions, etc. It is the strategic leverage of those resources in a coordinated fashion that is the difficult part. Coordinating the resources requires a great deal of inter-agency communication.

Some communication issues are general and simple (what means of communication should be used? How frequently do we need to communicate?) but most are fairly complex and challenging. Through 2005, two inter-agency communication issues have proved problematic for Rochester, the criteria for commencing enforcement actions, and the target identification and enforcement action oversight process.

Inter-Agency Communication:
Defining Criteria for Commencing Enforcement Actions

In Rochester, there appears to be a lack of clarity between agencies about the criteria that determines if a group should be the subject of an enforcement action. In theory, enforcement actions are precipitated by a “gang homicide”, unfortunately, confusion exists over how to define a “gang homicide”. This communication breakdown is significant because it may mean groups who deserve to be enforcement action targets are not receiving attention because of the confusion. If groups who deserve to be targeted are not targeted, law enforcement is not following through on the punishment threat delivered in the deterrence message. If the punishment message is not credible, other gangs will not be deterred from committing homicides and Ceasefire will have failed.

The issue of criteria in Rochester Ceasefire is related to a national debate regarding the classification of “gang crime”. (Maxson & Klein, 1990) The issue of what constitutes a “gang crime” is relevant to Rochester Ceasefire, because the uncertainty over how to define a “gang homicide” is the crux of this particular inter-agency communication problem. Out of both the national and local debate, two definitions for “gang crime” emerge. One definition suggests that any crime where a victim or a suspect is a gang member should count as a “gang crime”. The other definition relies upon the context of the incident (i.e. were the instigating factors a product of gang activity) to determine if a crime is a “gang crime”. For the issue of “gang homicide” Rochester initially adopted the first definition, but over time, moved toward the second definition. As of July 2005, the Ceasefire administrative group had not come to a consensus on which definition would guide Ceasefire for the future.

The lack of definitional clarity in Rochester has the capacity to limit program effectiveness, but the choice of which definition to use also has some significant implications for the program:

1. **Resource and expenditure issues:** The first definition is broader than the second, meaning agencies will need to do more enforcement actions under the first definition than they would under second. More enforcement actions means agencies that are responsible for much of the enforcement action work (the police) will have to commit more resources to Ceasefire under the first definition than under the second definition. These agencies may not have (or may be unwilling to commit) additional resources, forcing the existing resources to do more that may reduce their effectiveness.

2. **Message continuity issues:** The difference in definition is important because it is directly related to the Ceasefire deterrence message. At the call-in, the message delivered to gang members is “if one of your gang members commits a homicide,

> the entire gang gets special attention from law enforcement". The first definition of what provokes an enforcement action is perfectly symmetrical with the deterrence message, the second definition, however, is not. The second definition indicates the event must be gang precipitated, meaning a gang member, on his own, could commit a homicide and that homicide would not count as a "gang homicide". The deterrence message makes no such distinction. It is possible that use of the second definition could hurt the credibility of the deterrence message by not going after all homicides in which a gang member is involved.

Clearly, a common definition is essential for Ceasefire. The lack of a clear definition produces confusion, and as a result of that confusion, the administrative group may not be targeting gangs that deserve to be targeted. When other gangs see that gangs involved in homicides have not been targeted, the punishment threat will lose credibility, and in turn, the effectiveness of Ceasefire will be compromised. To fix the problem, Rochester ought to adopt the first definition. While the first definition is more resource intensive than the second definition, it is a much better fit to the deterrence message. The second definition is contrary to the deterrence message, and also risks damaging the credibility of the program. In a sense, adopting the second definition would almost be as bad as having no definition at all.

<u>Inter-Agency Communication:
Identifying Target Crews and Overseeing Enforcement Actions</u>

The second inter-agency communication problem in Rochester Ceasefire is the identification of target crews and coordinating of enforcement actions against them. To target a crew, Ceasefire's administrative group reviews homicides and receives intelligence about the presence of crew involvement in the homicide. If crew involvement exists, and the homicide fits the appropriate criteria (as previously discussed), the crew involved is targeted for an enforcement action, and a working group is established to implement the enforcement action.

While this process sounds simple and fluid, inter-agency communication breakdowns have caused this process to come to a near standstill. To make the process work, the administrative group must: pay continual attention to potential target cases, have continual engagement and participation by members of the administrative group, and have agency representatives at the working group table who have the authority to implement the enforcement action. Each of the three components of the targeting and operations process is problematic in Rochester:

1. **Continual attention to potential target cases-** The administrative group reviews case information on new homicides that occur during the two weeks prior to the meeting. In such a short time span, many cases have not yet been fully investigated, so gang connections remain unclear. As new information on unclear cases has emerged over time, the administrative group has not returned to those cases to make a decision regarding an enforcement action. In fact, because gang involvement has not been readily apparent in many cases, the eventual decision to target a crew is made informally by those coordinating enforcement actions at RPD after consulting with homicide investigators. The results of those decisions are sometimes shared with the administrative group and sometimes not shared. The de-formalization of the decision-making process is a result of lack of long-term attention by the administrative group to homicide cases where additional information is needed to make a decision. Because this devolution of power has occurred, the administrative group essentially has no meaningful decision-making power in the target selection process.

2. **Participation by administrative group members-** The administrative meetings are for agency leaders and occur every two weeks. When an agency head is unable to attend, they send a representative. The representatives may not have a complete understanding of Ceasefire, may not fully understand the context and relevance of information discussed at the meeting, and have no authority to make decisions for the agency they represent. This lack of understanding is detrimental because important information discussed at a meeting may not get back to the agency head and agendas at the administrative meeting are slowed down each time an agency head is not present to participate in important decisions.

3. **Power at the working group table-** Early in the Ceasefire process, a formal working group meeting was commissioned to implement an enforcement action against a target group. Agency heads were asked to send representatives to the working group meetings who were able to deal with the day-to-day implementation of the enforcement actions. The representatives who would attend the early working group meetings possessed little knowledge about Ceasefire and had little authority to return to their agency and carry out the tasks asked of them by the working group. As a result of the group's ineffectiveness, enforcement actions began to be coordinated out of RPD (as they bore the most burden for the enforcement action) with informal communication with other agencies, as needed.

Collectively, the three major problems associated with the process of target selection and enforcement action oversight could have had a major adverse impact on the success of Ceasefire. Fixing these problems must be a priority in Rochester, and good solutions to the problems are, as of this writing, being implemented. Rochester is implementing an administrative meeting agenda with greater structure, to include a review of all open homicide cases at administrative meetings. In the future, the administrative group will classify homicide cases as "Ceasefire-eligible", Not Ceasefire-eligible", and "Unknown", and will review all information on enforcement actions underway for "Ceasefire-eligible" homicides. The administrative group will also be updated on the investigative progress of all "Unknown" cases, so that the group can collectively make a decision on an enforcement action as soon as sufficient information is available. Additionally, a formal working group model is to be re-formed with high-ranking agency officials who have the knowledge and authority to carry out enforcement actions as intended. The working group will receive direction from and report directly to the administrative group. Given the existing problems associated with the target identification and enforcement action oversight process, Rochester's solutions, if implemented, should resolve these problems effectively.

Inter-Agency Communication: Summary

If problems with inter-agency communication are a cause of Ceasefire's 2005 failure, the lack of clear criteria for commencing enforcement actions, and the degradation of the target identification and enforcement action oversight process are, in large part, responsible. The communication breakdowns in these critically important aspects of Ceasefire are fixable. By agreeing on a "gang homicide" definition, and following through on the oversight process reforms already implemented, Rochester should be able to correct weaknesses in this area and reclaim the successes of 2004.

Conducting Enforcement Actions

Executing enforcement actions is another component of the Ceasefire process in which problems have occurred, problems that may be related to the failure of Ceasefire in 2005. While the problems related to administration and oversight of the enforcement actions are a concern, the actual execution of enforcement action operations are a separate issue with separate problems. The problems experienced by Rochester (as observed by the author) include: limitations of investigational strategies, overburdened officers, and conflicting organizational goals.

Conducting Enforcement Actions: Limitations of Investigational Strategies

Disproportionately, the burden of carrying out an enforcement action is placed upon the police department with other agencies playing supporting roles. Probation and Parolees typically account for no more that 25% of the members of a group- a limited target for Probation and Parole resources. Prosecutorial partners can only work after cases have been made against the crew members. The police have little option but to conduct the bulk of the case work associated with the enforcement action. Because they play a central role, police effort is a significant factor in the success or failure of an enforcement action.

Through July 2005, all enforcement actions conduced by the Rochester Police Department have almost exclusively involved narcotics investigations, with virtually no involvement by patrol resources. In these investigations, narcotics officers have used three levels of investigation. Of the six enforcement actions done by Rochester (through July 2005), each level has been used twice.

- **Simple Narcotics Investigation:** This type of investigation involves purchasing illegal narcotics from crew members by a combination of confidential informants (C.I.s) and undercover officers. The goal of the investigation is to buy narcotics from as many crew members as possible, and eventually conduct a "sweep" where all of the investigation targets are picked up and charged with drug possession or sales. This investigation type has the initial dramatic benefit of the "sweep", but the nature of the charges (non-violent felony or misdemeanor offenses) is not substantially different from typical drug sentences an offender might experience. For a crew member who has already been through the criminal justice system as a result of his crew's drug activity, the charges produced from this investigation type may not be a sufficiently severe punishment threat.

- **Wiretap Investigation:** This level of investigation uses wiretaps and camera surveillance to complement the undercover narcotics investigations. Because of the additional information they provide, these tools can be useful to build more substantial narcotics cases against crew members. Wiretaps require constant observation, however, and take officers away from other methods of building narcotics cases. In practice, once a wiretap is commenced, existing cases against crew members are strengthened but few new cases are built. While the approach yields quality cases, it does not always target a large number of crew members.

- **Violent Crime Task Force Investigation:** The most successful Rochester enforcement actions have used the investigative strategy of targeting crews with the Federal Racketeer Influenced and Corrupt Organization (RICO)

> Act[12]. Rochester is able to use Federal laws through the Violent Crime Task Force (VCTF), which is comprised of RPD officers and Federal law enforcement agents. The investigations are long-term, utilizing undercover narcotics cases, electronic surveillance methods, and historical records to prove conspiracy to engage in racketeering activity. This strategy is distinctive because the investigative effort is thorough. The VCTF works only on these investigations, allowing for patience and creativity. The combination of patience and unique investigative resources result in investigations that produce serious cases[13] against large quantities of gang members.

Clearly, the local/federal investigatory partnerships involving conspiracy cases are the most powerful tools currently available in the Rochester Ceasefire toolbox. That tool is, however, the most resource intensive and the slowest of the three major investigative strategies. Unfortunately, all three investigative strategies possess some sub-optimal trait, whether it be weak cases, not enough cases, or too lengthy. Any of these three weaknesses are a problem. Over time, crew members may observe these negative traits and interpret them as evidence of lack of follow-through on the punishment threat.

Conducting Enforcement Actions: Overburdened Officers

The narcotics officers in a police department are a desirable yet scarce resource. They are highly skilled and have the capacity to conduct unique and useful investigations, but narcotics units are non-essential police functions and have low staffing levels (relative to primary police functions such as patrol or criminal investigations). As a result, they are overburdened with work responsibilities. In Rochester, the large workloads carried by narcotics officers have negatively impacted the effectiveness of Ceasefire enforcement actions. In addition to enforcement actions, investigative burdens

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[12] RICO, which is Title IX of the Organized Crime Control Act of 1970, allows federal law enforcement to impose sanctions for "any person employed or associated with any enterprise engaged in…a pattern of racketeering activity" (Title 18 U.S.C 1962). In United States v. Turkette (452 U.S. 576), the Supreme Court confirmed that the term "enterprise" covered illegitimate organizations such as gangs.

[13] Arguably the most serious charges of all investigative strategies, as all charges are Federal charges.

for narcotics officers include following up drug hotline complaints, support of major crimes investigations (undercover surveillance of suspects, narcotics investigations into conspirators in a murder case to create leverage), and other long-term investigations. As a result, narcotics officers are forced to juggle a multitude of investigative priorities without giving any investigation a thorough effort.

The investigations necessary to make an enforcement action a success require great skill and creativity. The level of demand placed upon the officers forces them to maximize their efficiency by clearing their plates of responsibility for cases in the quickest way possible. The operational mindset to "just get it done" is a perfectly reasonable approach to an overwhelming workload, but lumping enforcement actions in with the regular workload shortchanges the extra effort and special nature of enforcement actions that differentiate them from typical police behavior. To truly validate the punishment threat of the Ceasefire deterrence message, enforcement actions require quality cases on most, if not all, members of a target crew. The current lack of singular focus upon enforcement action investigations in Rochester has, in many instances, resulted in sub-optimal enforcement actions.

Conducting Enforcement Actions: Conflicting Organizational Goals

Another problem associated with the execution of enforcement actions is the conflict of goals between Ceasefire and traditional narcotics investigations. Narcotics investigations place a premium on the identification and prosecution of high-value targets. In contrast, Ceasefire enforcement actions are meant to utilize narcotics enforcement as a mechanism for incapacitating violent gangs. This disparity in goals is relevant because an enforcement action conducted under the goal structure of regular narcotics investigations will not yield the effect desired for the Ceasefire program.

High-value targets (drug kingpins) tend to be well insulated from their illegal businesses. Catching high-value targets typically involves a technique known as "flipping" whereby cases are first developed against subordinates or associates of the high-value target. The subordinates are offered reduced sentences in exchange for their cooperation and participation in the case against the high-value target.

The tactic of "flipping" is a ubiquitous investigatory concept, but when paired with the typical narcotics mindset of going after high-value targets, cases against lesser targets are generally exchanged for information against the high-value target rather than being vigorously prosecuted. This poses a problem in Ceasefire enforcement actions where the goal is not to take down a single high-value target, but to take down an entire group. The enforcement action targets may not necessarily the typical targets of narcotics investigation. The members of a target crew will typically be low-level drug dealers, the type of individual that law enforcement might try to "flip" to gain information about a higher-value target. Despite the inclination to "flip" such crew members, such individuals must be vigorously prosecuted. The punishment threat of Ceasefire tells offenders the whole crew will be subject to enforcement (if the crew commits a homicide), and for that threat to be credible, the enforcement actions must re-enforce the threat. Unfortunately, this goal conflict has occurred in Rochester in several enforcement actions, with the investigators unable to break away from the typical narcotics enforcement mindset.

Conducting Enforcement Actions: Fixing the Problems

Because the problems associated with conducting enforcement actions are so similar, one common “fix” may effectively solve the three problems of investigatory limitations, overburdened officers, and conflicting organizational goals. To achieve an optimal level of effectiveness with enforcement actions, it may be necessary to assign a narcotics team (or teams) specifically and exclusively to Ceasefire. Enforcement actions (as evidence of the punishment threat) are most effective when they produce the results that only major investigations like RICO cases can achieve, but those cases require time and effort. The creativity and focus needed for such investigations are not possible when Ceasefire is lumped in with a plethora of other competing responsibilities. A specifically assigned “Ceasefire team” would also have the benefit of breaking out of the typical organizational mindsets of narcotics officers. The team could benefit from training about the Ceasefire program, and the operational mindset of the team could be better aligned with the goals of Ceasefire by removing the team from a typical narcotics caseload.

Perhaps too, it has been a mistake to rely solely on the limited resources of narcotics officers to conduct enforcement actions. Patrol resources have not (as of yet) been efficiently coordinated into Ceasefire enforcement actions. Police frequently respond to calls for service, special details, or other independent proactive police work in gang territories, yet none of that policing behavior has been integrated into enforcement actions. In a coordinated fashion, patrol resources could be effectively directed into a targeted group’s territory. Those resources may contribute significant gun or drug arrests to the enforcement action, but may have a simpler use- moving the crew into houses where narcotics officers can generate and execute search warrants.

Conducting Enforcement Actions: Summary

Weaknesses in the execution of enforcement actions may be related to the failure of Rochester Ceasefire in 2005. The problems of investigational limitations, overburdened officers, and conflicting organizational goals are closely related to one another, and may have cumulatively damaged the effectiveness of the deterrence message by not sufficiently following through on the punishment threat. The specific assignment of a RPD narcotics team exclusively to enforcement action could fix the problems associated with the implementation of enforcement actions. A separate unit would free officers from the other investigatory burdens, enable the use of the optimal investigatory strategy, and align the unit goals with the goals of the Ceasefire program.

Communicating Deterrence Message

The delivery of the deterrence message is of critical importance to the success of Ceasefire. While communication of the deterrence message is critical, it is also difficult, and Rochester has had problems effectively communicating the deterrence message to gangs. Communication problems have occurred with: the selection of call-in attendees, the content and number of call-in presenters, and in other efforts to communicate the deterrence message to gang members.

Communicating Deterrence Message: The Selection of Call-in Attendees

The most obvious way in which call-in communication failures occur is by inviting inappropriate attendees to the call-in. Gang intelligence is critical to Ceasefire in several ways, but the choice of call-in attendees is perhaps the most important. When non-members are invited, the idea of attendees as "messengers" falls apart, because the attendee is not a crew member. Because the message is not being delivered to the correct audience, no deterrence can be generated. Worse, the effort is less likely to be credible if the target population (gang members) becomes aware that law enforcement cannot differentiate gang members from non-gang members. Inevitably, intelligence will be incorrect at some point, and while it is impossible to know many intelligence failures it takes to damage the effectiveness of the message, continued failures over time may have serious negative consequences.

To minimize intelligence failures, Rochester developed a robust system of intelligence gathering, cross-referencing report information with observations from street officers. Despite this system, the occurrence of incorrect call-in attendee choices could be a possibility, particularly early in the program when intelligence-gathering efforts were not yet fully implemented. Intelligence failures could still occur in Rochester because the intelligence gathering system is dependent upon human factors. The research analyst (a position occupied by this author) is solely responsible for the vetting of all intelligence, and specifically, for each call-in's attendee list. The analyst's intelligence and attendee selection protocols (or deviation from) play a central role, and breakdowns at this stage of the process could completely undermine the goals of Ceasefire if the wrong individuals are invited to the call-in.

If problems associated with the selection of call-in attendees are to blame for Ceasefire's lack of 2005 success, the fixes to the problems are well within reach. Inviting the appropriate people to call-ins is a critically important part of Ceasefire, because communication of the deterrence message depends almost entirely upon the call-in attendees. The current system in Rochester for vetting call-in candidates needs more checks and balances against intelligence mistakes than it currently has. To resolve this problem, a three-person committee should be all be reviewing the same intelligence and collectively making decisions about an individual's fitness to attend the call-in. Additional review of intelligence information by other people will minimize the mistakes that a singe individual might make in this critical position.

Communicating Deterrence Message: Content and Number of Presenters

Successful communication of the deterrence message at the call-in requires two equal things- the correct attendees to receive the message and presenters who can articulate the deterrence message effectively. Since Ceasefire's implementation, problems have occurred with both the content of call-in presenters' messages and with the number of presenters at the call-in. At the call-ins to date, as many as 20 different presenters have spoken and the number of presenters per call-in has varied from three to eight. The speakers all have the same basic task of delivering the deterrence message, but with so many different presenters, at least one presenter is bound to get it wrong.

Rochester presenters have been "off-message" on a variety of instances, owing either to a lack of preparation, alternate agenda (notably promoting the saving power of Jesus Christ), or inability to grasp the correct message. Indeed, presenter communication of the message has been a frequent problem in Rochester. Typical mistakes include substituting all violence for homicide in the message (as the precipitating event for an enforcement action) or espousing a vague and general "we are watching you, and are coming after any illegal activity you are involved in" message. While the occasional deviation from intended messages is to be expected, the continual barrage of disparate messages from speakers may produce serious cognitive dissonance for attendees. If the attendee does not understand the deterrence message, they will be unable to deliver the message to their fellow crew members. In recent call-ins, Rochester has attempted to correct this problem by allowing for only the same four speakers to present at call-ins: the police Lieutenant in charge of the homicide unit, the District Attorney, and two police Investigators. Since changing the presenter composition, the message delivered at the call-in (anecdotally) contains fewer mistakes than in previous call-ins.

The problem of presenter communication errors may have contributed to Ceasefire's lack of 2005 success, but as of this writing, Rochester appears to have remedied the problem by limiting the number of presenters and closely controlling their message at the call-in. Limiting the number of presenters reduces the potential for communication mistakes, and ensures that all speakers repeat the key deterrence message themes without deviation. In addition to these solutions, the Rochester administrative group should constructively review the performance of each presenter after the call-in. An evaluative process of the call-in deterrence message will ensure that the message remains clear over time.

Communicating Deterrence Message: Other Communication Efforts

In addition to the call-in, Rochester has attempted to communicate with gang members in several other ways. A weakness of the call-in as a communication mechanism is the reliance of indirect communication with the target population. The successful communication of the message depends upon the attendee acting as a messenger to a broader audience, so the effects produced by Ceasefire are only as good as your messengers. On two occasions, Rochester has implemented special details intended to directly communicate the Ceasefire deterrence message to gang members. Like the call-in, these efforts to communicate the deterrence message have had problems.

The first detail was conducted by the Tactical Unit of the Rochester Police Department in cooperation with New York State Parole and the Monroe County Probation Department in October 2003. Officers were instructed by supervisors about Ceasefire and the deterrence message, and given target areas to work in. The target areas were gang locations, and officers were provided with name and address information on selected members of that gang who were under supervision. The detail was intended to seek out gang members on the corners and deliver the deterrence message in addition to contacting the gang members under supervision. A very similar detail, underway in Rochester as of May 2005, conducted the same activities, but also focused on getting in between gangs that were actively engaged in disputes.

Unfortunately, the details had the same communication problems inherent as the call-in. The details ask police officers (who typically have no familiarity with the Ceasefire program) to deliver a deterrence message that they may not fully understand. Like the call-in presenters, the officers are bound to get the message wrong, and the collective effects of that miscommunication at best create no beneficial deterrence effect, and, at worst, undermine the message through misstatements and falsehoods (in the context of Ceasefire).

In the scope of efforts to communicate the deterrence message, the Ceasefire details have had limited effect, but the problems associated with the details may have contributed to Ceasefire's lack of 2005 success. A few small measures are necessary to fix the problems related to communication of the deterrence message by the Ceasefire details. The special details are a good complement to the call-ins, but greater training is necessary to reduce communication errors. Rochester has provided laminated cards with the message to officers, and has specifically trained some officers working the details, but room for error still exists. Future details should clearly delineate which personnel will deliver the Ceasefire message. Only those personnel should be involved in message delivery, and those people should be specifically trained and provided with support materials (laminated cards). In addition, the details should be periodically reviewed (perhaps by members of the administrative group) to ensure message integrity. These efforts will help to control quality and content of the deterrence message, and thereby ensure the appropriate message is communicated to the intended audience.

Communicating Deterrence Message: Summary

The delivery of the deterrence message is essential to Ceasefire's success. Since Ceasefire's inception, Rochester has had problems effectively communicating the deterrence message to gangs, and those problems (associated with the selection of call-in attendees, the content and number of call-in presenters, and other efforts to communicate the deterrence message to gang members) may have played a role in the program's 2005 decline. To fix these problems, greater oversight is needed, controlling the specifics of who receives the message and what the content of the message needs to be.

Summary of Systems/Implementation Explanations for 2005 Homicide Increase

Implementing programs is an inexact science, and the problems Rochester has experienced with inter-agency communication, conducting enforcement actions, and communication of the Ceasefire message, are reasonable challenges to the optimal implementation of Ceasefire. If the 2004 reductions experienced in M/B/15-30 homicides were not the product of random variation, these problems may be likely explanations for why Ceasefire has failed to affect homicides in the M/B/15-30 demographic in 2005. Besides regression to the mean and systems and implementation problems, an explanation for the 2005 decline could be theoretical weakness in the concept of focused deterrence.

Theoretical Explanation for the Increase

The explanations offered in this chapter for Ceasefire's 2005 failure have thus far dealt with potential problems in the operationalization and implementation of Ceasefire. The failure of Ceasefire, however, when taken together with the eventual failure of Boston, is cause for concern that the failures may not be due to problems with implementation, but weaknesses inherent to focused deterrence. Two hypothetical problems may exist in the theory: The assumption that punishment can be consistent and credible, and the unanticipated way in which group behavior expedites deterrence decay.

Theoretical Explanation for the Increase: Credibility and Consistency Problems

Focused deterrence requires a credible punishment threat, and a credible punishment threat depends upon the consistent application of punishment to targeted groups. To be deterred, non-target groups must perceive that they will get the punishments they are threatened with if they commit a homicide, In order to maintain that perception, actual punishments must be certain, meaning that when a group commits a homicide, the group must always receive those punishments. Hypothetically, the delivery of consistent punishments is possible, but in practice, criminal justice systems may not possess the resources and the level of coordination necessary to produce consistent punishment. The system is designed to process individual cases based on their specific merits. To ensure justice, no one agency controls the entire process, and the agencies involved generally act independent of one another. In a sense, the criminal justice system ensures that punishments are not consistently applied, because no two cases are the same, and following that logic, no two cases should receive the exact same punishment. If the assumption that criminal justice systems can deliver consistent punishments is false, it would represent a major weakness in the theory of focused deterrence, as punishment consistency and credibility are necessary pre-conditions of generating deterrence.

In contrast to the problems associated with systems/implementation, theoretical problems may be impossible to fix. Nevertheless, by understanding the problems, the application of focused deterrence (Ceasefire) may be able to be re-structured to account for theoretical weaknesses in a more effective way. If true punishment consistency cannot fully be obtained, credibility may still be able to be produced by creating the perception that punishments are generally consistent. In order to create the perception of punishment consistency, law enforcement must only threaten punishments that can actually be delivered. If law enforcement “writes checks they can’t cash”, or over-promises punishments, all credibility is lost.

Re-orienting Ceasefire to account for punishment consistency problems is quite possible. The avenue to correct this problem is through the enforcement actions. Typical investigations-focused enforcement actions are always partially a roll of the dice: no reliable or predetermined outcomes exist. Because no consistent punishment can be assured, this type of enforcement action may not generate a credible punishment threat. In the typical enforcement actions, the potential exists for very severe punishments of a group, but that potential is not always realized. Creating the perception of punishment consistency may require efforts in addition to investigations. For the purposes of Ceasefire, consistency is easiest to produce when it is clearly demonstrable and can be communicated.

For example, if a homicide occurred and police knew gang X was involved, a consistent response would include leaving a single patrol officer at the corner 24 hours a day to deter foot drug traffic from gang X's territory. RPD could communicate with gang X, explaining that an officer will be stationed there to disrupt drug traffic at this location for a time period (six months) because of their involvement in the homicide. The message could also be communicated (as part of a punishment threat) to other groups, and the other groups could also visibly see the officer in gang X's territory all the time. By having that consistency, RPD could start to generate credibility with their punishment threat, and assuming the threat was sufficiently severe, could start to deter behavior leading to homicide.

Of course, an officer on a street corner is not a severe punishment to a group. The officer might deter some drug sales, but generally would not impose much punishment on the group's members. So the officer on the corner would have to be combined with the narcotics investigations and the no-plea bargain policy from the District Attorney's office to create sufficient severity. But all of these punishments would need to be communicated to the target group **while** the enforcement action was ongoing, as well as all other groups in the city. While letting a group know that they are currently being investigated for narcotics may not prove to be fruitful, the communication works to prove the consistency, and therefore, credibility of the police.

The police need not say they are conducting a narcotics operation against a group, but could communicate to all groups: "group X was involved in (insert the specific homicide) and now they are at the top of our priority list and will be for the next six months. Among other things we will be doing during that time, you will be able to see a police officer in front of where they hang out, and no members of that crew will receive plea bargains for offenses they commit. We may do investigations on the group during that time, and we will communicate those results to you. Any other gangs that commit a homicide will get the same treatment."

While this is just an example, it serves to demonstrate an important point- that a credible punishment threat can be created through a combination of separate punishments with different purposes. In this example, the officer on the corner illustrates the certainty and consistency of the punishment, while the no-plea bargains and periodic investigations of groups represent the severity of the punishment. The current investigative-driven approach to enforcement actions cannot possibly be visible or consistent enough to illustrate to potential offenders that the threat of punishment is certain. Only a group-focused punishment threat that includes elements of consistency, certainty and severity will deter acts of group violence.

Theoretical Explanation for the Increase: Group Role in Deterrence Decay

One of the aims of focused deterrence is to harness group peer pressure for the purpose of re-enforcing the deterrence message. Theoretically, the deterrence message is communicated among the group, and group members pressure other members not to offend for fear of punishment for the whole group. If this aspect of the theory is true, the inter-group communication process exerts powerful influence over members of the group. This communication process is not well understood, and while it is beneficial for the purposes of initially generating deterrence, it may also play a role in accelerating the decay of deterrence effects.

In the initial phases of Ceasefire, focused deterrence should produce strong effects. Ceasefire represents a new operational approach to gang homicide, and the criminal justice system is reacting in an aggressive fashion. Gang members are faced with intense scrutiny of their gangs, unfamiliar punishment threats, and some evidence (through early enforcement actions) that law enforcement is for real. If focused deterrence works, the effects should be strongest at this point because gang members are uncertain about actual punishment risks and may take law enforcement claims of credibility at face value. Assuming the inter-group communication process works, these heightened punishment risks would be communicated back to the group, and group members would pressure other members to not commit homicide.

Over time, however, gang members have the opportunity to re-assess punishment risks associated with Ceasefire. If the actual punishments delivered by law enforcement have been less than what was threatened, future punishment threats are not likely to be seen as credible. Likewise, if law enforcement has failed to follow through on punishing gangs involved in homicide, the credibility of the punishment will decline. All offenders eventually re-assess punishment risk, and as a result, some offenders may no longer be deterred. This phenomenon is known as "deterrence decay".

Focused deterrence effects start to decay when gang members become aware that law enforcement punishment threats are not as certain or severe as advertised. Just as the inter-group communication process played a role in generating deterrence, it most likely influence deterrence decay. The process most likely influences deterrence decay by accelerating the rate of decay. Once information contrary to the deterrence message is introduced into a crew from a credible source (perhaps a cousin who is in another group that was involved in a homicide, but was not punished), the new information should circulate through the group quickly, and group members will convince other members that the punishment threat associated with Ceasefire is not credible. Further, it may be that "tipping points" exist in gang members willingness to perceive punishment threats as credible. After a certain point, the inter-group communication process may permanently discount the credibility of threats from law enforcement. In other words, once the gang decides the deterrence message is not credible, there may be no way to reverse that perception, and therefore, not ability to deter.

It may not be possible to "fix" the problem of group behavior expediting deterrence decay. The inter-group communication process that, at first, generates deterrence, eventually works to undermine deterrence. This problem appears to simply be an unintended consequence of the long-term application of focused deterrence.

If punishment threats cannot maintain long-term credibility (as discussed earlier), the group dynamic of focused deterrence will accelerate deterrence decay rendering

Ceasefire ineffective. This weakness of focused deterrence has a significant implication- perhaps Ceasefire (as an application of focused deterrence) only has a limited shelf life as a program. As a short-term remedy to a serious homicide problem, Ceasefire can be effective, but perhaps it is not an acceptable long-term strategy to reduce homicide.

All of this analysis is, of course, conducted from a perspective grounded in criminological theory. The suggestions offered in this chapter are consistent with an understanding of Deterrence theory. However, issues related to focused deterrence are also issues of group behavior and the modification of behavior. These issues are, at their core, fundamentally issues for the field of Psychology. Perhaps review of existing research on behavioral modification (for individuals and groups) could inform both problems observed in focused deterrence theory. A review of psychology literature may find, for example, that behavior is best reinforced intermittently rather than consistently, indicating the need for an entirely different "fix" for the problems of focused deterrence rather than the ones prescribed in this chapter. The lessons taken from other complimentary fields may provide solutions necessary to ensure the viability of focused deterrence theory.

Theoretical Explanation for the Increase: Summary

This chapter reviewed two potential flaws in focused deterrence: The possibly invalid assumption that punishment can be consistent and credible, and the way in which group behavior expedites deterrence decay. Unlike earlier problems discussed in this chapter, these theoretical problems have no clear fixes. The second problem (group dynamics accelerating deterrence decay) is dependent upon the first problem (maintaining credible long-term punishment threats). The first problem can be addressed by improving offender's perceptions of punishment consistency and credibility. By aligning punishment threats to actual punishments, and by adding punishments that demonstrate consistency, offender perceptions of punishment consistency may be able to be altered. If these improvements are made, the second problem may not be an issue. If not, the combinations of both the first and second problems may mean Ceasefire is at best a short-term strategy to employ.

CONCLUSION

The Ceasefire program was implemented in Rochester, NY as a response to the city's high rates of homicide victimization. Research into Rochester's homicide problem revealed very high concentrations in victimization and offending by Black Males ages 15 to 30. In many cases involving Black Male (ages 15 to 30) homicide victims or suspects, evidence of gang affiliation existed. Ceasefire was implemented in the hope that its focused deterrence based approach to violence prevention could ameliorate Rochester's problem of gang homicide.

The goal of this study was to evaluate Rochester N.Y.'s focused-deterrence based program, Ceasefire. The dataset used in the study were monthly victimization counts of Homicide, Assault 1st, and Robbery 1st incidents from January 2000 to December 2004. The study examined overall monthly victimization counts and victimization counts of the M/B/15-30 demographic. Multiple regression analyses indicated statistically significant reductions in average monthly Homicide and Robbery victimizations of the M/B/15-30 demographic during the intervention period (October 2003 to December 2004) when compared to a four-year pre-intervention period (January 2000 to September 2003).

While these findings are indicative of success of the Ceasefire program in Rochester, the findings should be interpreted with caution. Method choices were appropriate for this study, but were constrained by limitations of the data in addition to the nature of the Rochester Ceasefire program. Over a longer period of observation, data used in this study might prove more amenable to alternate methods, and may or may not yield contrasting results to this study.

As an evaluation, this study offers little concrete evidence of the effectiveness of Rochester’s Ceasefire program. While the study does find evidence of success in the first 15 months of the program (using less than optimal methods), Rochester’s 2005 homicide levels have risen, and victimization in the M/B/15-30 demographic has increased substantially. A variety of reasons exist to explain the homicide increase, but nevertheless, it has occurred, and casts doubt upon the effectiveness of Rochester’s Ceasefire program for the future.

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APPENDIX

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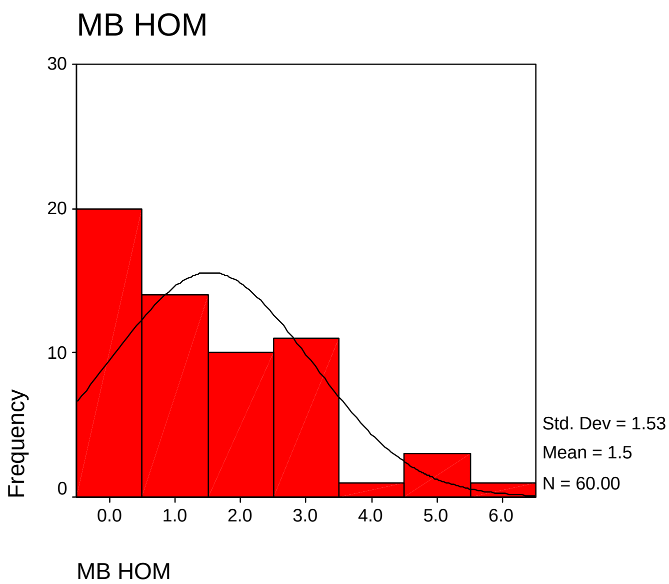
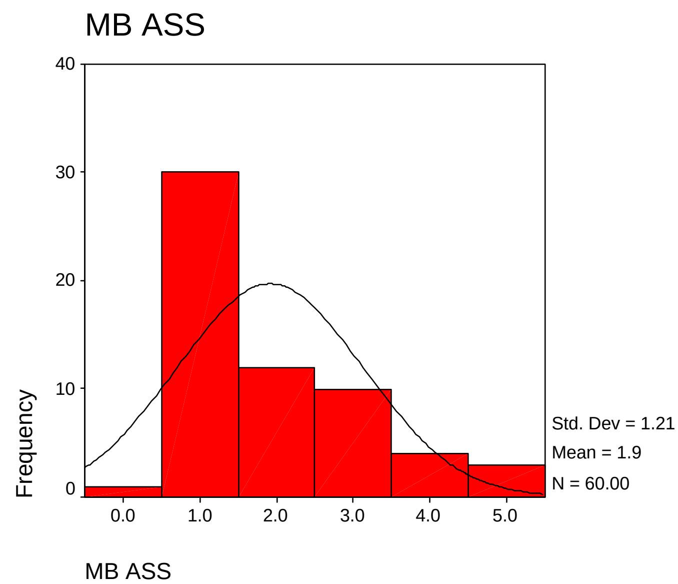
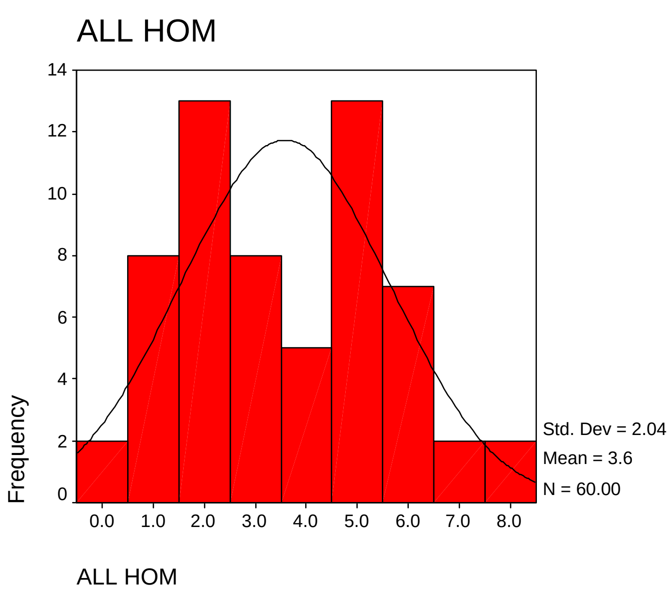
Appendix A: Descriptive Statistics

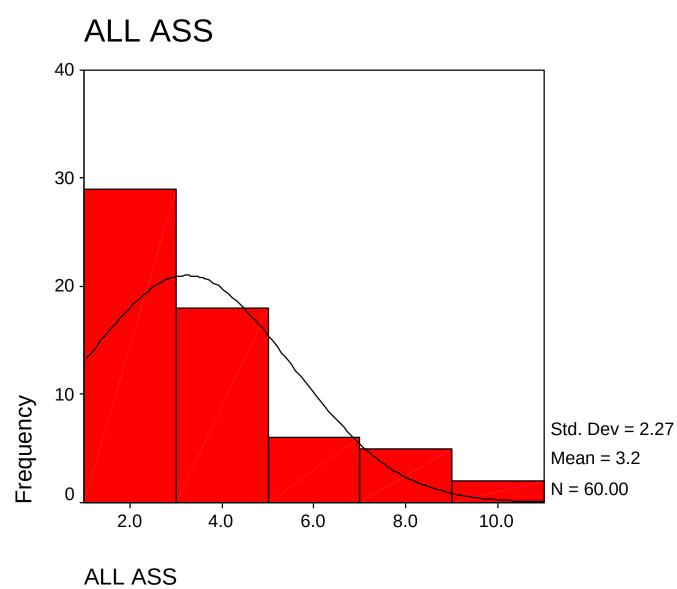
		MB HOM	ALL HOM	MB ASS	ALL ASS	MB ROB	ALL ROB
N	Valid	60	60	60	60	60	60
	Missing	0	0	0	0	0	0
Mean		1.53	3.58	1.92	3.23	8.72	43.58
Median		1.00	3.00	1.00	3.00	8.00	40.00
Mode		0	2	1	2	4	35
Std. Deviation		1.535	2.036	1.211	2.273	4.665	14.109
Skewness		.925	.218	1.052	1.314	.666	.675
Std. Error of Skewness		.309	.309	.309	.309	.309	.309
Kurtosis		.331	-.873	.282	1.322	.633	-.186
Std. Error of Kurtosis		.608	.608	.608	.608	.608	.608
Range		6	8	5	9	24	59
Minimum		0	0	0	1	0	21
Maximum		6	8	5	10	24	80

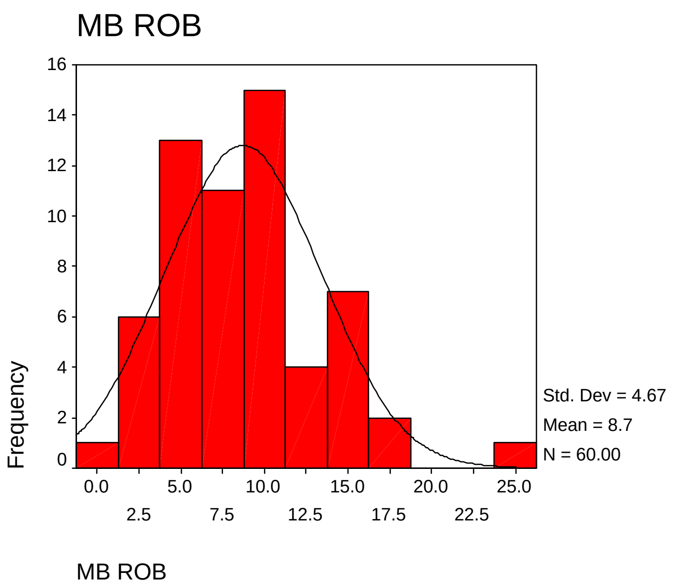
Appendix B: Frequency Histograms of Dependent Variables










MB ROB
16
14
12
10
8
6
4
2
0
Frequency
Std. Dev = 4.67
Mean = 8.7
N = 60.00
0.0
5.0
10.0
15.0
20.0
25.0
2.5
7.5
12.5
17.5
22.5
MB ROB

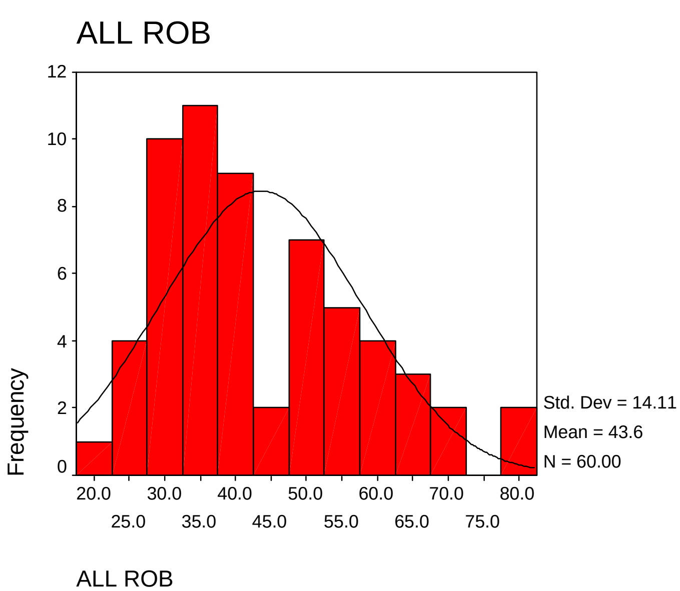

ALL ROB
12
10
8
6
4
2
0
Frequency
Std. Dev = 14.11
Mean = 43.6
N = 60.00
20.0
30.0
40.0
50.0
60.0
70.0
80.0
25.0
35.0
45.0
55.0
65.0
75.0
ALL ROB

Figure C: Independent Samples T-Tests (Grouping by Intervention Variables)

Independent Samples T-Tests (INT)

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
MB HOM	Equal variances assumed	2.065	.156	-1.574	58	.121	-.71	.452	-1.616	.193
	Equal variances not assumed			-1.821	32.172	.078	-.71	.390	-1.506	.084
ALL HOM	Equal variances assumed	.058	.810	-.400	58	.691	-.24	.611	-1.468	.979
	Equal variances not assumed			-.377	21.933	.709	-.24	.648	-1.588	1.099
MB ASS	Equal variances assumed	.002	.963	-.428	58	.670	-.16	.364	-.884	.572
	Equal variances not assumed			-.431	24.330	.670	-.16	.361	-.900	.589
ALL ASS	Equal variances assumed	.029	.865	.065	58	.948	.04	.683	-1.323	1.412
	Equal variances not assumed			.066	24.544	.948	.04	.675	-1.347	1.436
MB ROB	Equal variances assumed	.015	.904	-.877	58	.384	-1.22	1.394	-4.012	1.568
	Equal variances not assumed			-.909	25.609	.372	-1.22	1.345	-3.988	1.544
ALL ROB	Equal variances assumed	1.955	.167	2.424	58	.018	9.80	4.043	1.708	17.892
	Equal variances not assumed			2.147	20.079	.044	9.80	4.564	.281	19.319

Independent Samples Test (INT_1)

		Levene's Test for Equality of Variances		t-test for Equality of Means					95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
MB HOM	Equal variances assumed	1.620	.208	-1.795	57	.078	-.83	.461	-1.750	.096
	Equal variances not assumed			-2.069	28.262	.048	-.83	.400	-1.645	-.009
ALL HOM	Equal variances assumed	.035	.852	-.720	57	.475	-.45	.628	-1.711	.806
	Equal variances not assumed			-.688	20.304	.499	-.45	.658	-1.823	.918
MB ASS	Equal variances assumed	.011	.915	-.262	57	.794	-.10	.375	-.849	.653
	Equal variances not assumed			-.262	21.615	.796	-.10	.376	-.880	.683
ALL ASS	Equal variances assumed	.024	.876	.294	57	.770	.21	.701	-1.198	1.610
	Equal variances not assumed			.299	22.233	.768	.21	.691	-1.226	1.639
MB ROB	Equal variances assumed	.012	.914	-1.353	57	.181	-1.91	1.411	-4.735	.916
	Equal variances not assumed			-1.433	23.914	.165	-1.91	1.333	-4.660	.841
ALL ROB	Equal variances assumed	.111	.741	1.556	57	.125	6.66	4.277	-1.908	15.222
	Equal variances not assumed			1.519	20.930	.144	6.66	4.383	-2.460	15.775

Independent Samples Test (INT_2)

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
MB HOM	Equal variances assumed	1.002	.321	-1.740	56	.087	-.83	.478	-1.791	.126
	Equal variances not assumed			-1.979	24.164	.059	-.83	.421	-1.701	.036
ALL HOM	Equal variances assumed	.028	.868	-.634	56	.529	-.41	.653	-1.721	.894
	Equal variances not assumed			-.591	17.775	.562	-.41	.700	-1.885	1.058
MB ASS	Equal variances assumed	.023	.881	.031	56	.975	.01	.387	-.763	.787
	Equal variances not assumed			.030	19.068	.976	.01	.393	-.810	.834
ALL ASS	Equal variances assumed	.021	.886	.270	56	.788	.19	.722	-1.250	1.640
	Equal variances not assumed			.267	19.150	.793	.19	.731	-1.333	1.723
MB ROB	Equal variances assumed	.160	.691	-1.692	56	.096	-2.45	1.451	-5.362	.452
	Equal variances not assumed			-1.842	22.326	.079	-2.45	1.332	-5.216	.306
ALL ROB	Equal variances assumed	.003	.959	.994	56	.324	4.38	4.408	-4.447	13.213
	Equal variances not assumed			.986	19.261	.336	4.38	4.445	-4.912	13.678

Independent Samples Test (INT_3)

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
MB HOM	Equal variances assumed	2.202	.144	-2.090	55	.041	-1.03	.492	-2.013	-.042
	Equal variances not assumed			-2.535	23.880	.018	-1.03	.405	-1.865	-.191
ALL HOM	Equal variances assumed	.142	.707	-.991	55	.326	-.67	.673	-2.015	.682
	Equal variances not assumed			-.942	16.293	.360	-.67	.708	-2.165	.832
MB ASS	Equal variances assumed	.127	.723	-.570	55	.571	-.23	.400	-1.029	.573
	Equal variances not assumed			-.603	18.757	.554	-.23	.378	-1.019	.564
ALL ASS	Equal variances assumed	.207	.651	.254	55	.801	.19	.744	-1.303	1.680
	Equal variances not assumed			.243	16.409	.811	.19	.778	-1.457	1.835
MB ROB	Equal variances assumed	.228	.635	-1.877	55	.066	-2.82	1.501	-5.824	.191
	Equal variances not assumed			-2.059	19.856	.053	-2.82	1.368	-5.672	.039
ALL ROB	Equal variances assumed	1.345	.251	.202	55	.841	.93	4.617	-8.320	10.187
	Equal variances not assumed			.237	22.310	.815	.93	3.940	-7.231	9.098

Independent Samples Test (INT_4)

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
MB HOM	Equal variances assumed	1.953	.168	-1.732	54	.089	-.89	.516	-1.927	.141
	Equal variances not assumed			-2.098	20.370	.049	-.89	.426	-1.780	-.006
ALL HOM	Equal variances assumed	.016	.900	-.724	54	.472	-.51	.703	-1.918	.900
	Equal variances not assumed			-.672	14.089	.512	-.51	.757	-2.132	1.114
MB ASS	Equal variances assumed	.010	.920	-.600	54	.551	-.25	.417	-1.087	.586
	Equal variances not assumed			-.619	15.871	.545	-.25	.405	-1.109	.608
ALL ASS	Equal variances assumed	.143	.707	.039	54	.969	.03	.778	-1.528	1.589
	Equal variances not assumed			.037	14.287	.971	.03	.826	-1.739	1.799
MB ROB	Equal variances assumed	.197	.659	-2.053	54	.045	-3.19	1.556	-6.313	-.075
	Equal variances not assumed			-2.243	17.170	.038	-3.19	1.424	-6.196	-.192
ALL ROB	Equal variances assumed	2.798	.100	-.247	54	.806	-1.19	4.821	-10.857	8.473
	Equal variances not assumed			-.312	22.057	.758	-1.19	3.822	-9.116	6.733

Figure D: Correlation Matrix

		MB HOM	ALL HOM	MB ASS	ALL ASS	MB ROB	ALL ROB	UNEMPL	TREND	INT	LAGS (INT,1)	LAGS (INT,2)	LAGS (INT,3)	LAGS (INT,4)	MEAN TEMP	STATEP RI	FELONY AR	MISD ARR
MB HOM	Pearson Correlation	1	.685(**)	-.122	.095	.142	.058	.018	-.009	-.202	-.231	-.227	-.271(*)	-.229	.318(*)	.150	.102	.076
	Sig. (2-tailed)	.	.000	.355	.471	.278	.659	.891	.946	.121	.078	.087	.041	.089	.013	.251	.437	.563
ALL HOM	Pearson Correlation	.685(**)	1	-.090	.036	.041	.056	.169	.042	-.052	-.095	-.084	-.132	-.098	.264(*)	.125	.080	-.009
	Sig. (2-tailed)	.000	.	.495	.785	.756	.672	.197	.749	.691	.475	.529	.326	.472	.042	.342	.542	.945
MB ASS	Pearson Correlation	-.122	-.090	1	-.116	-.043	-.059	.032	-.023	-.056	-.035	.004	-.077	-.081	-.240	-.112	-.048	.184
	Sig. (2-tailed)	.355	.495	.	.378	.743	.657	.810	.863	.670	.794	.975	.571	.551	.065	.394	.715	.159
ALL ASS	Pearson Correlation	.095	.036	-.116	1	-.010	-.131	.067	.162	.009	.039	.036	.034	.005	.167	-.189	-.168	-.017
	Sig. (2-tailed)	.471	.785	.378	.	.942	.318	.608	.215	.948	.770	.788	.801	.969	.201	.149	.200	.895
MB ROB	Pearson Correlation	.142	.041	-.043	-.010	1	.650(**)	-.114	-.074	-.114	-.176	-.220	-.245	-.269(*)	-.240	-.101	-.082	-.081
	Sig. (2-tailed)	.278	.756	.743	.942	.	.000	.387	.573	.384	.181	.096	.066	.045	.065	.443	.531	.538
ALL ROB	Pearson Correlation	.058	.056	-.059	-.131	.650(**)	1	.154	.300(*)	.303(*)	.202	.132	.027	-.034	-.202	-.050	-.202	-.362(**)
	Sig. (2-tailed)	.659	.672	.657	.318	.000	.	.240	.020	.018	.125	.324	.841	.806	.121	.704	.122	.004
UNEMPL	Pearson Correlation	.018	.169	.032	.067	-.114	.154	1	.734(**)	.316(*)	.286(*)	.255	.242	.180	.044	-.103	-.533(**)	-.243
	Sig. (2-tailed)	.891	.197	.810	.608	.387	.240	.	.000	.014	.028	.053	.070	.185	.736	.433	.000	.062
TREND	Pearson Correlation	-.009	.042	-.023	.162	-.074	.300(*)	.734(**)	1	.750(**)	.737(**)	.722(**)	.706(**)	.688(**)	.018	-.322(*)	-.597(**)	-.630(**)
	Sig. (2-tailed)	.946	.749	.863	.215	.573	.020	.000	.	.000	.000	.000	.000	.000	.889	.012	.000	.000
INT	Pearson Correlation	-.202	-.052	-.056	.009	-.114	.303(*)	.316(*)	.750(**)	1	.955(**)	.910(**)	.864(**)	.817(**)	-.094	-.252	-.291(*)	-.630(**)
	Sig. (2-tailed)	.121	.691	.670	.948	.384	.018	.014	.000	.	.000	.000	.000	.000	.474	.052	.024	.000
LAGS (INT,1)	Pearson Correlation	-.231	-.095	-.035	.039	-.176	.202	.286(*)	.737(**)	.955(**)	1	.953(**)	.905(**)	.856(**)	-.113	-.245	-.310(*)	-.640(**)
	Sig. (2-tailed)	.078	.475	.794	.770	.181	.125	.028	.000	.000	.	.000	.000	.000	.395	.062	.017	.000
LAGS (INT,2)	Pearson Correlation	-.227	-.084	.004	.036	-.220	.132	.255	.722(**)	.910(**)	.953(**)	1	.950(**)	.899(**)	-.111	-.201	-.248	-.590(**)
	Sig. (2-tailed)	.087	.529	.975	.788	.096	.324	.053	.000	.000	.000	.	.000	.000	.405	.129	.061	.000
LAGS (INT,3)	Pearson Correlation	-.271(*)	-.132	-.077	.034	-.245	.027	.242	.706(**)	.864(**)	.905(**)	.950(**)	1	.947(**)	-.076	-.160	-.241	-.563(**)
	Sig. (2-tailed)	.041	.326	.571	.801	.066	.841	.070	.000	.000	.000	.000	.	.000	.574	.233	.071	.000
LAGS (INT,4)	Pearson Correlation	-.229	-.098	-.081	.005	-.269(*)	-.034	.180	.688(**)	.817(**)	.856(**)	.899(**)	.947(**)	1	.005	-.184	-.215	-.539(**)
	Sig. (2-tailed)	.089	.472	.551	.969	.045	.806	.185	.000	.000	.000	.000	.000	.	.969	.175	.111	.000
MEAN TEMP	Pearson Correlation	.318(*)	.264(*)	-.240	.167	-.240	-.202	.044	.018	-.094	-.113	-.111	-.076	.005	1	-.041	.186	.282(*)
	Sig. (2-tailed)	.013	.042	.065	.201	.065	.121	.736	.889	.474	.395	.405	.574	.969	.	.756	.156	.029
STATE PRI	Pearson Correlation	.150	.125	-.112	-.189	-.101	-.050	-.103	-.322(*)	-.252	-.245	-.201	-.160	-.184	-.041	1	.268(*)	.203
	Sig. (2-tailed)	.251	.342	.394	.149	.443	.704	.433	.012	.052	.062	.129	.233	.175	.756	.	.038	.121
FELONY AR	Pearson Correlation	.102	.080	-.048	-.168	-.082	-.202	-.533(**)	-.597(**)	-.291(*)	-.310(*)	-.248	-.241	-.215	.186	.268(*)	1	.493(**)
	Sig. (2-tailed)	.437	.542	.715	.200	.531	.122	.000	.000	.024	.017	.061	.071	.111	.156	.038	.	.000
MISD ARR	Pearson Correlation	.076	-.009	.184	-.017	-.081	-.362(**)	-.243	-.630(**)	-.630(**)	-.640(**)	-.590(**)	-.563(**)	-.539(**)	.282(*)	.203	.493(**)	1
	Sig. (2-tailed)	.563	.945	.159	.895	.538	.004	.062	.000	.000	.000	.000	.000	.000	.029	.121	.000	.

** Correlation is significant at the 0.01 level (2-tailed).
* Correlation is significant at the 0.05 level (2-tailed).

Figure E: Selected Multiple Regression Models

Regression: MB HOM = constant + LAGS (INT, 3) + MEANTEMP

Model Summary(b)

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.390(a)	.152	.121	1.461

a Predictors: (Constant), MEANTEMP, LAGS(INT,3)
b Dependent Variable: MB HOM

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	20.737	2	10.369	4.856	.011(a)
	Residual	115.298	54	2.135		
	Total	136.035	56			

a Predictors: (Constant), MEANTEMP, LAGS(INT,3)
b Dependent Variable: MB HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	.467	.624		.749	.457
	LAGS(INT,3)	-.947	.476	-.250	-1.988	.052
	MEANTEMP	.026	.012	.282	2.242	.029

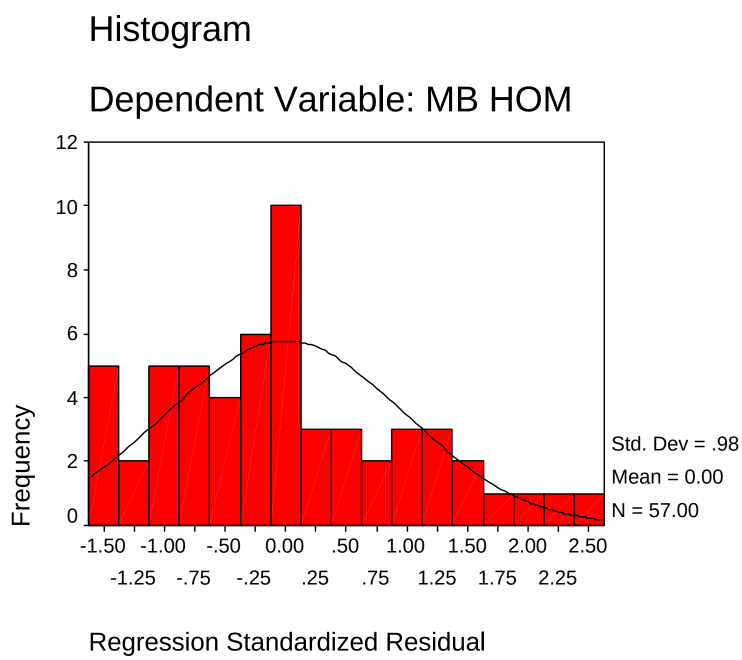
a Dependent Variable: MB HOM

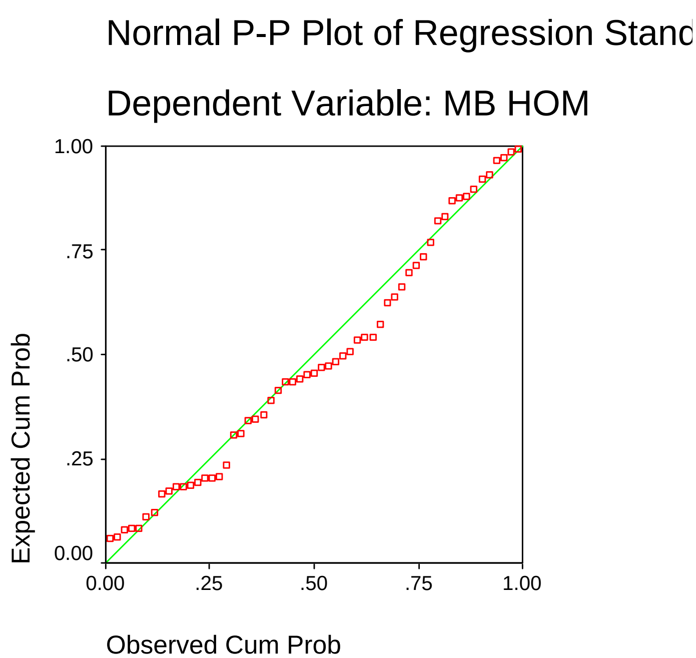
Residuals Statistics(a)

	Minimum	Maximum	Mean	Std. Deviation	N
Predicted Value	-.03	2.42	1.56	.609	57
Residual	-2.28	3.67	.00	1.435	57
Std. Predicted Value	-2.610	1.403	.000	1.000	57
Std. Residual	-1.563	2.515	.000	.982	57

a Dependent Variable: MB HOM

Residual Plots: MB HOM = constant + LAGS (INT, 3) + MEANTEMP





Regression: MB HOM = constant + LAGS (INT, 4) + MEANTEMP

Variables Entered/Removed(b)

Model	Variables Entered	Variables Removed	Method
1	LAGS(INT,4), MEANTEMP(a)	.	Enter

a All requested variables entered.
b Dependent Variable: MB HOM

Model Summary(b)

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.384(a)	.148	.116	1.467

a Predictors: (Constant), LAGS(INT,4), MEANTEMP
b Dependent Variable: MB HOM

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	19.792	2	9.896	4.595	.014(a)
	Residual	114.137	53	2.154		
	Total	133.929	55			

a Predictors: (Constant), LAGS(INT,4), MEANTEMP
b Dependent Variable: MB HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	.304	.618		.491	.625
	MEANTEMP	.029	.012	.308	2.433	.018
	LAGS(INT,4)	-.899	.494	-.231	-1.822	.074

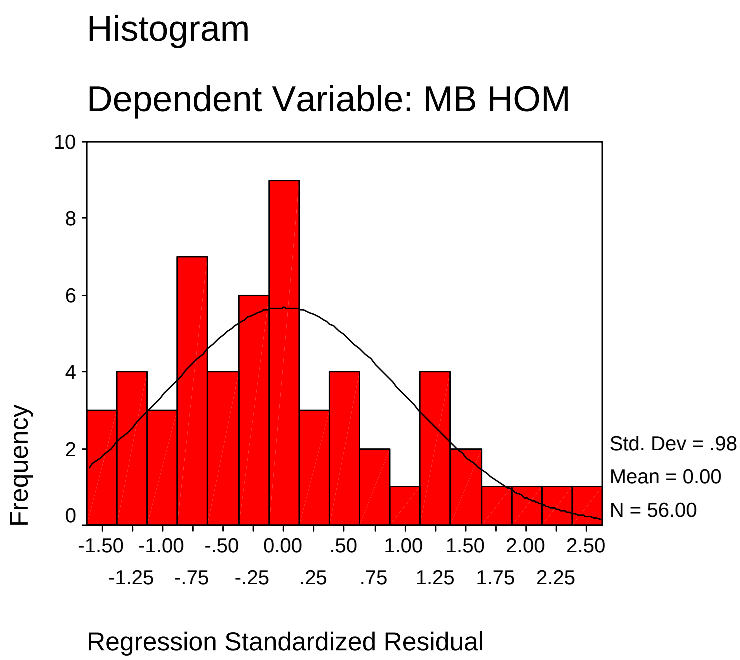
a Dependent Variable: MB HOM

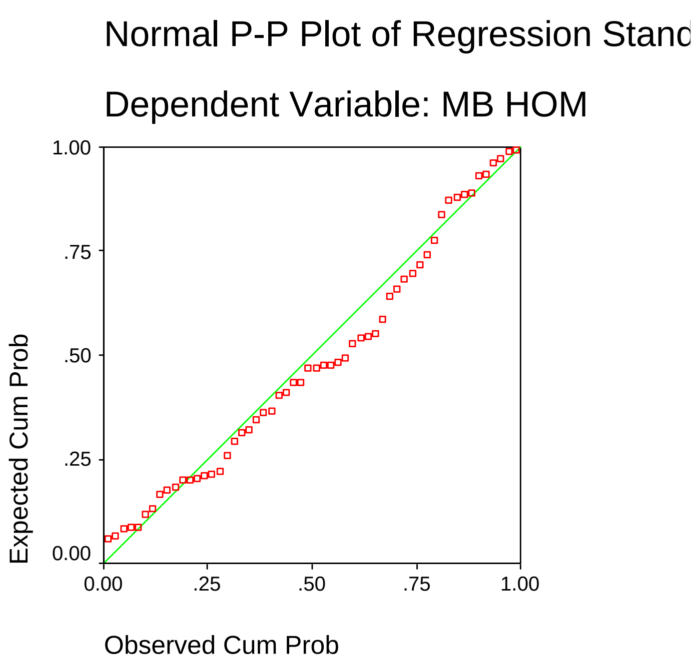
Residuals Statistics(a)

	Minimum	Maximum	Mean	Std. Deviation	N
Predicted Value	.03	2.42	1.54	.600	56
Residual	-2.28	3.68	.00	1.441	56
Std. Predicted Value	-2.508	1.477	.000	1.000	56
Std. Residual	-1.553	2.505	.000	.982	56

a Dependent Variable: MB HOM

Residual Charts: MB HOM = constant + LAGS (INT, 4) + MEANTEMP





Regression: MB ROB = constant + LAGS (INT, 3) + MEANTEMP

Variables Entered/Removed(b)

Model	Variables Entered	Variables Removed	Method
1	LAGS(INT,3), MEANTEMP(a)	.	Enter

a All requested variables entered.
b Dependent Variable: MB ROB

Model Summary(b)

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.391(a)	.153	.122	4.426

a Predictors: (Constant), LAGS(INT,3), MEANTEMP
b Dependent Variable: MB ROB

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	191.233	2	95.616	4.882	.011(a)
	Residual	1057.644	54	19.586		
	Total	1248.877	56			

a Predictors: (Constant), LAGS(INT,3), MEANTEMP
b Dependent Variable: MB ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	13.711	1.890		7.256	.000
	MEANTEMP	-.087	.036	-.306	-2.434	.018
	LAGS(INT,3)	-3.083	1.442	-.269	-2.138	.037

a Dependent Variable: MB ROB

Residuals Statistics(a)

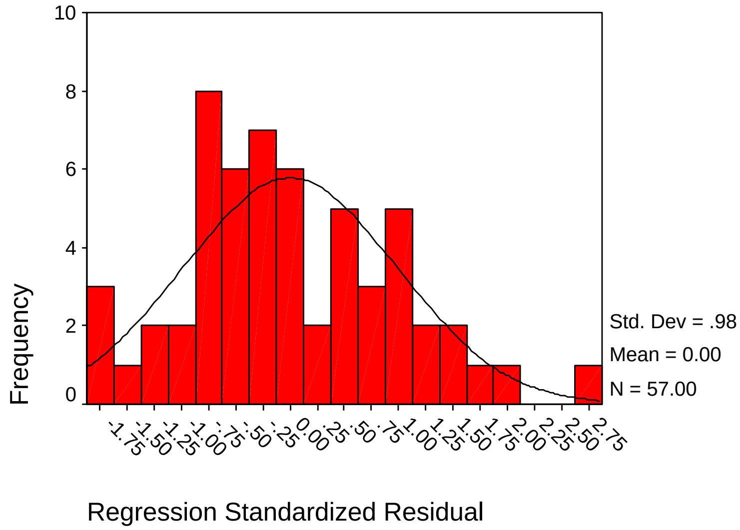
	Minimum	Maximum	Mean	Std. Deviation	N
Predicted Value	4.71	12.13	8.81	1.848	57
Residual	-8.20	12.25	.00	4.346	57
Std. Predicted Value	-2.215	1.801	.000	1.000	57
Std. Residual	-1.854	2.767	.000	.982	57

a Dependent Variable: MB ROB

Residual Plots: MB ROB = constant + LAGS (INT, 3) + MEANTEMP

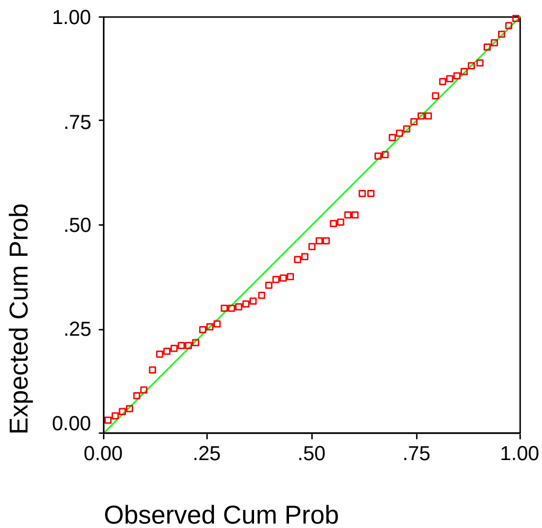
Histogram

Dependent Variable: MB ROB



Normal P-P Plot of Regression Stand

Dependent Variable: MB ROB



Regression: MB ROB = constant + LAGS (INT, 4) + MEANTEMP

Variables Entered/Removed(b)

Model	Variables Entered	Variables Removed	Method
1	LAGS(INT,4), MEANTEMP(a)	.	Enter

a All requested variables entered.
b Dependent Variable: MB ROB

Model Summary(b)

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.393(a)	.154	.122	4.458

a Predictors: (Constant), LAGS(INT,4), MEANTEMP
b Dependent Variable: MB ROB

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	192.156	2	96.078	4.834	.012(a)
	Residual	1053.398	53	19.875		
	Total	1245.554	55			

a Predictors: (Constant), LAGS(INT,4), MEANTEMP
b Dependent Variable: MB ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	13.448	1.879		7.157	.000
	MEANTEMP	-.081	.036	-.286	-2.265	.028
	LAGS(INT,4)	-3.176	1.500	-.268	-2.118	.039

a Dependent Variable: MB ROB

Residuals Statistics(a)

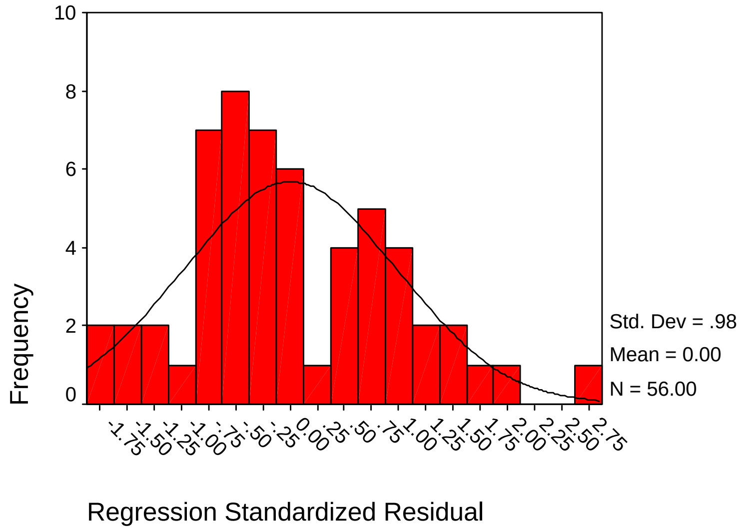
	Minimum	Maximum	Mean	Std. Deviation	N
Predicted Value	4.74	12.06	8.84	1.869	56
Residual	-8.17	12.38	.00	4.376	56
Std. Predicted Value	-2.192	1.720	.000	1.000	56
Std. Residual	-1.832	2.777	.000	.982	56

a Dependent Variable: MB ROB

Residual Charts: MB ROB = constant + LAGS (INT, 4) + MEANTEMP

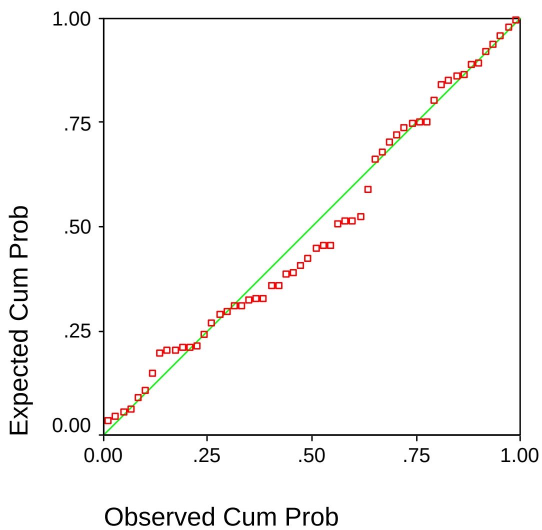
Histogram

Dependent Variable: MB ROB



Normal P-P Plot of Regression Stand

Dependent Variable: MB ROB



Appendix F: Multiple Regression Models Using MB HOM, all Intervention Variables and all other Independent Variables

Regression: MB HOM = constant + INT + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.486(a)	.236	.133	1.429

a Predictors: (Constant), MISDARR, STATEPRI, UNEMPL, MEANTEMP, INT, FELONYAR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	32.755	7	4.679	2.292	.041(a)
	Residual	106.178	52	2.042		
	Total	138.933	59			

a Predictors: (Constant), MISDARR, STATEPRI, UNEMPL, MEANTEMP, INT, FELONYAR, TREND
b Dependent Variable: MB HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.953	2.769		-.344	.732
	UNEMPL	-.237	.236	-.234	-1.004	.320
	TREND	.749	.396	.711	1.891	.064
	INT	-2.154	.810	-.613	-2.659	.010
	MEANTEMP	.024	.012	.266	1.950	.057
	STATEPRI	.031	.022	.186	1.394	.169
	FELONYAR	.007	.007	.185	1.062	.293
	MISDARR	-.001	.002	-.123	-.632	.530

a Dependent Variable: MB HOM

Regression: MB HOM = constant + LAGS (INT, 1) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.493(a)	.243	.139	1.424

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	33.167	7	4.738	2.338	.038(a)
	Residual	103.376	51	2.027		
	Total	136.542	58			

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.490	2.785		-.176	.861
	UNEMPL	-.215	.241	-.213	-.895	.375
	TREND	.641	.389	.598	1.645	.106
	MEANTEMP	.024	.012	.258	1.915	.061
	STATEPRI	.037	.023	.209	1.605	.115
	FELONYAR	.006	.006	.164	.972	.336
	MISDARR	-.002	.002	-.167	-.837	.406
	LAGS(INT,1)	-2.097	.795	-.586	-2.637	.011

a Dependent Variable: MB HOM

Regression: MB HOM = constant + LAGS (INT, 2) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.504(a)	.254	.149	1.426

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	34.545	7	4.935	2.427	.032(a)
	Residual	101.679	50	2.034		
	Total	136.224	57			

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-1.613	2.785		-.579	.565
	UNEMPL	-.305	.255	-.300	-1.197	.237
	TREND	.831	.432	.757	1.925	.060
	MEANTEMP	.024	.013	.255	1.891	.064
	STATEPRI	.047	.024	.264	1.968	.055
	FELONYAR	.007	.007	.187	1.098	.278
	MISDARR	-.001	.002	-.104	-.524	.603
	LAGS(INT,2)	-2.316	.832	-.630	-2.783	.008

a Dependent Variable: MB HOM

Regression: MB HOM = constant + LAGS (INT, 3) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.570(a)	.325	.228	1.369

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	44.172	7	6.310	3.366	.005(a)
	Residual	91.863	49	1.875		
	Total	136.035	56			

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-1.517	2.692		-.564	.576
	UNEMPL	-.374	.242	-.363	-1.546	.128
	TREND	.982	.399	.872	2.464	.017
	MEANTEMP	.027	.012	.290	2.243	.029
	STATEPRI	.051	.023	.285	2.226	.031
	FELONYAR	.006	.006	.165	1.048	.300
	MISDARR	-.001	.002	-.106	-.564	.576
	LAGS(INT,3)	-2.847	.773	-.751	-3.683	.001

a Dependent Variable: MB HOM

Regression: MB HOM = constant + LAGS (INT, 4) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.566(a)	.320	.221	1.377

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	42.871	7	6.124	3.228	.007(a)
	Residual	91.058	48	1.897		
	Total	133.929	55			

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND
b Dependent Variable: MB HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.881	2.713		-.325	.747
	UNEMPL	-.409	.255	-.390	-1.600	.116
	TREND	.990	.411	.862	2.407	.020
	MEANTEMP	.035	.012	.379	2.860	.006
	STATEPRI	.047	.023	.262	2.033	.048
	FELONYAR	.005	.006	.119	.765	.448
	MISDARR	-.001	.002	-.119	-.628	.533
	LAGS(INT,4)	-2.900	.819	-.745	-3.542	.001

a Dependent Variable: MB HOM

Appendix G: Multiple Regression Models Using ALL HOM, all Intervention Variables and all other Independent Variables

Regression: ALL HOM = constant + INT + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.415(a)	.172	.060	1.974

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	42.025	7	6.004	1.541	.174(a)
	Residual	202.559	52	3.895		
	Total	244.583	59			

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.747	3.825		-.195	.846
	UNEMPL	.545	.326	.405	1.671	.101
	TREND	-.352	.547	-.252	-.644	.522
	MEANTEMP	.036	.017	.300	2.115	.039
	STATEPRI	.019	.030	.088	.629	.532
	FELONYAR	.010	.009	.202	1.118	.269
	MISDARR	-.005	.003	-.327	-1.618	.112
	INT	-.411	1.119	-.088	-.367	.715

a Dependent Variable: ALL HOM

Regression: ALL HOM = constant + LAGS (INT, 1) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.427(a)	.182	.070	1.972

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	44.248	7	6.321	1.626	.149(a)
	Residual	198.294	51	3.888		
	Total	242.542	58			

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.191	3.858		-.049	.961
	UNEMPL	.429	.333	.318	1.286	.204
	TREND	-.093	.539	-.065	-.173	.863
	MEANTEMP	.036	.017	.296	2.114	.039
	STATEPRI	.015	.032	.064	.476	.636
	FELONYAR	.010	.009	.200	1.142	.259
	MISDARR	-.005	.003	-.320	-1.547	.128
	LAGS(INT,1)	-1.104	1.101	-.232	-1.002	.321

a Dependent Variable: ALL HOM

Regression: ALL HOM = constant + LAGS (INT, 2) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.432(a)	.187	.073	1.985

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	45.286	7	6.469	1.642	.146(a)
	Residual	197.058	50	3.941		
	Total	242.345	57			

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-1.016	3.877		-.262	.794
	UNEMPL	.368	.355	.271	1.037	.305
	TREND	.031	.601	.021	.052	.959
	MEANTEMP	.038	.018	.303	2.156	.036
	STATEPRI	.024	.033	.100	.715	.478
	FELONYAR	.010	.009	.198	1.116	.270
	MISDARR	-.004	.003	-.284	-1.365	.178
	LAGS(INT,2)	-1.144	1.159	-.233	-.988	.328

a Dependent Variable: ALL HOM

Regression: ALL HOM = constant + LAGS (INT, 3) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.495(a)	.245	.137	1.924

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	58.881	7	8.412	2.273	.044(a)
	Residual	181.329	49	3.701		
	Total	240.211	56			

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.521	3.782		-.138	.891
	UNEMPL	.239	.340	.174	.703	.485
	TREND	.366	.560	.244	.654	.516
	MEANTEMP	.041	.017	.334	2.437	.018
	STATEPRI	.026	.032	.110	.810	.422
	FELONYAR	.010	.009	.193	1.157	.253
	MISDARR	-.004	.003	-.304	-1.529	.133
	LAGS(INT,3)	-2.162	1.086	-.429	-1.991	.052

a Dependent Variable: ALL HOM

Regression: ALL HOM = constant + LAGS (INT, 4) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.507(a)	.257	.148	1.920

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	61.102	7	8.729	2.368	.037(a)
	Residual	176.898	48	3.685		
	Total	238.000	55			

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL HOM

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	.056	3.781		.015	.988
	UNEMPL	.254	.356	.182	.715	.478
	TREND	.326	.573	.213	.569	.572
	MEANTEMP	.049	.017	.393	2.836	.007
	STATEPRI	.021	.032	.090	.668	.507
	FELONYAR	.008	.009	.156	.958	.343
	MISDARR	-.005	.003	-.325	-1.644	.107
	LAGS(INT,4)	-2.098	1.141	-.404	-1.839	.072

a Dependent Variable: ALL HOM

Appendix H: Multiple Regression Models Using MB ASS, all Intervention Variables and all other Independent Variables

Regression: MB ASS = constant + INT + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.440(a)	.194	.085	1.159

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	16.764	7	2.395	1.784	.110(a)
	Residual	69.820	52	1.343		
	Total	86.583	59			

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	.185	2.245		.082	.935
	UNEMPL	-.086	.191	-.108	-.450	.655
	TREND	.279	.321	.335	.867	.390
	MEANTEMP	-.028	.010	-.382	-2.724	.009
	STATEPRI	-.017	.018	-.127	-.925	.359
	FELONYAR	-.002	.005	-.063	-.351	.727
	MISDARR	.004	.002	.509	2.551	.014
	INT	-.108	.657	-.039	-.165	.870

a Dependent Variable: MB ASS

Regression: MB ASS = constant + LAGS (INT, 1) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.437(a)	.191	.080	1.166

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	16.386	7	2.341	1.722	.125(a)
	Residual	69.343	51	1.360		
	Total	85.729	58			

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.008	2.281		-.004	.997
	UNEMPL	-.029	.197	-.036	-.145	.885
	TREND	.140	.319	.164	.438	.664
	MEANTEMP	-.027	.010	-.375	-2.690	.010
	STATEPRI	-.014	.019	-.103	-.767	.447
	FELONYAR	-.002	.005	-.070	-.401	.690
	MISDARR	.004	.002	.502	2.441	.018
	LAGS(INT,1)	.246	.651	.087	.377	.708

a Dependent Variable: MB ASS

Regression: MB ASS = constant + LAGS (INT, 2) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.434(a)	.189	.075	1.171

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	15.963	7	2.280	1.662	.140(a)
	Residual	68.606	50	1.372		
	Total	84.569	57			

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.025	2.287		-.011	.991
	UNEMPL	-.011	.210	-.014	-.053	.958
	TREND	.095	.355	.110	.267	.790
	MEANTEMP	-.026	.010	-.358	-2.547	.014
	STATEPRI	-.014	.019	-.099	-.709	.481
	FELONYAR	-.003	.005	-.091	-.512	.611
	MISDARR	.004	.002	.507	2.442	.018
	LAGS(INT,2)	.421	.684	.145	.616	.541

a Dependent Variable: MB ASS

Regression: MB ASS = constant + LAGS (INT, 3) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.448(a)	.201	.087	1.168

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	16.831	7	2.404	1.761	.117(a)
	Residual	66.889	49	1.365		
	Total	83.719	56			

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.384	2.297		-.167	.868
	UNEMPL	-.106	.206	-.131	-.513	.610
	TREND	.299	.340	.339	.880	.383
	MEANTEMP	-.029	.010	-.397	-2.816	.007
	STATEPRI	-.009	.020	-.062	-.446	.657
	FELONYAR	-.001	.005	-.041	-.240	.811
	MISDARR	.005	.002	.531	2.592	.013
	LAGS(INT,3)	-.104	.659	-.035	-.158	.875

a Dependent Variable: MB ASS

Regression: MB ASS = constant + LAGS (INT, 4) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.450(a)	.203	.087	1.179

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	16.985	7	2.426	1.745	.121(a)
	Residual	66.729	48	1.390		
	Total	83.714	55			

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	-.424	2.322		-.183	.856
	UNEMPL	-.087	.219	-.105	-.400	.691
	TREND	.243	.352	.268	.691	.493
	MEANTEMP	-.029	.011	-.402	-2.801	.007
	STATEPRI	-.009	.020	-.066	-.470	.641
	FELONYAR	-.001	.005	-.043	-.253	.801
	MISDARR	.005	.002	.530	2.589	.013
	LAGS(INT,4)	.059	.701	.019	.084	.933

a Dependent Variable: MB ASS

Appendix I: Multiple Regression Models Using ALL ASS, all Intervention Variables and all other Independent Variables

Regression: ALL ASS = constant + INT + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.368(a)	.136	.019	2.250

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	41.373	7	5.910	1.167	.338(a)
	Residual	263.361	52	5.065		
	Total	304.733	59			

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	5.515	4.361		1.265	.212
	UNEMPL	-.542	.372	-.361	-1.457	.151
	TREND	1.018	.624	.652	1.631	.109
	MEANTEMP	.016	.020	.120	.824	.413
	STATEPRI	-.021	.035	-.086	-.604	.549
	FELONYAR	-.008	.010	-.141	-.763	.449
	MISDARR	.003	.003	.158	.767	.447
	INT	-1.655	1.276	-.318	-1.297	.200

a Dependent Variable: ALL ASS

Regression: ALL ASS = constant + LAGS (INT, 1) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.277(a)	.076	-.055	2.334

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	22.099	7	3.157	.580	.769(a)
	Residual	266.884	49	5.447		
	Total	288.982	56			

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	5.264	4.588		1.147	.257
	UNEMPL	-.398	.412	-.265	-.966	.339
	TREND	.558	.679	.340	.821	.415
	MEANTEMP	.018	.021	.129	.854	.397
	STATEPRI	-.024	.039	-.094	-.624	.536
	FELONYAR	-.011	.011	-.186	-1.008	.319
	MISDARR	.003	.004	.202	.919	.363
	LAGS(INT,3)	-.429	1.317	-.078	-.326	.746

a Dependent Variable: ALL ASS

Regression: ALL ASS = constant + LAGS (INT, 2) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.321(a)	.103	-.020	2.295

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	30.965	7	4.424	.840	.560(a)
	Residual	268.696	51	5.269		
	Total	299.661	58			

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	5.759	4.491		1.282	.206
	UNEMPL	-.480	.388	-.321	-1.238	.221
	TREND	.788	.628	.496	1.256	.215
	MEANTEMP	.019	.020	.141	.963	.340
	STATEPRI	-.024	.037	-.093	-.657	.514
	FELONYAR	-.011	.010	-.189	-1.034	.306
	MISDARR	.003	.003	.182	.842	.404
	LAGS(INT,1)	-.975	1.282	-.184	-.761	.450

a Dependent Variable: ALL ASS

Regression: ALL ASS = constant + LAGS (INT, 3) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.296(a)	.088	-.040	2.318

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	25.834	7	3.691	.687	.682(a)
	Residual	268.580	50	5.372		
	Total	294.414	57			

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	5.601	4.526		1.238	.222
	UNEMPL	-.418	.415	-.280	-1.008	.318
	TREND	.635	.702	.393	.905	.370
	MEANTEMP	.018	.020	.134	.902	.372
	STATEPRI	-.027	.038	-.103	-.695	.491
	FELONYAR	-.011	.011	-.187	-.997	.324
	MISDARR	.003	.004	.188	.852	.398
	LAGS(INT,2)	-.639	1.353	-.118	-.473	.639

a Dependent Variable: ALL ASS

Regression: ALL ASS = constant + LAGS (INT, 4) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.313(a)	.098	-.034	2.329

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	28.233	7	4.033	.744	.636(a)
	Residual	260.320	48	5.423		
	Total	288.554	55			

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ASS

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	5.580	4.587		1.217	.230
	UNEMPL	-.561	.432	-.365	-1.300	.200
	TREND	.920	.695	.546	1.323	.192
	MEANTEMP	.021	.021	.156	1.023	.312
	STATEPRI	-.021	.039	-.082	-.553	.583
	FELONYAR	-.010	.010	-.176	-.980	.332
	MISDARR	.003	.004	.193	.887	.379
	LAGS(INT,4)	-1.455	1.384	-.255	-1.051	.298

a Dependent Variable: ALL ASS

Appendix J: Multiple Regression Models Using MB ROB, all Intervention Variables and all other Independent Variables

Regression: MB ROB = constant + INT + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.354(a)	.125	.008	4.647

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	161.105	7	23.015	1.066	.399(a)
	Residual	1123.078	52	21.598		
	Total	1284.183	59			

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	24.221	9.005		2.690	.010
	UNEMPL	-.636	.768	-.207	-.829	.411
	TREND	.636	1.289	.199	.494	.623
	MEANTEMP	-.065	.041	-.233	-1.598	.116
	STATEPRI	-.059	.072	-.118	-.822	.415
	FELONYAR	-.005	.021	-.045	-.243	.809
	MISDARR	-.003	.007	-.099	-.477	.635
	INT	-3.475	2.635	-.325	-1.319	.193

a Dependent Variable: MB ROB

Regression: MB ROB = constant + LAGS (INT, 1) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.418(a)	.174	.061	4.500

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	218.158	7	31.165	1.539	.175(a)
	Residual	1032.792	51	20.251		
	Total	1250.949	58			

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	25.206	8.804		2.863	.006
	UNEMPL	-.702	.761	-.229	-.922	.361
	TREND	.795	1.231	.245	.646	.521
	MEANTEMP	-.074	.039	-.265	-1.883	.065
	STATEPRI	-.030	.072	-.057	-.421	.675
	FELONYAR	-.001	.020	-.007	-.038	.970
	MISDARR	-.006	.007	-.178	-.854	.397
	LAGS(INT,1)	-4.880	2.514	-.451	-1.942	.058

a Dependent Variable: MB ROB

Regression: MB ROB = constant + LAGS (INT, 2) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.453(a)	.206	.094	4.457

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	256.980	7	36.711	1.848	.099(a)
	Residual	993.296	50	19.866		
	Total	1250.276	57			

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	23.606	8.703		2.712	.009
	UNEMPL	-.874	.797	-.284	-1.096	.278
	TREND	1.182	1.350	.355	.876	.385
	MEANTEMP	-.080	.039	-.284	-2.043	.046
	STATEPRI	-.021	.074	-.039	-.283	.778
	FELONYAR	.004	.020	.033	.188	.852
	MISDARR	-.005	.007	-.150	-.732	.467
	LAGS(INT,2)	-5.845	2.602	-.525	-2.247	.029

a Dependent Variable: MB ROB

Regression: MB ROB = constant + LAGS (INT, 3) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.460(a)	.212	.099	4.482

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	264.637	7	37.805	1.882	.093(a)
	Residual	984.240	49	20.087		
	Total	1248.877	56			

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	23.770	8.811		2.698	.010
	UNEMPL	-.857	.792	-.274	-1.082	.284
	TREND	1.103	1.305	.323	.845	.402
	MEANTEMP	-.072	.040	-.253	-1.811	.076
	STATEPRI	-.015	.075	-.028	-.205	.838
	FELONYAR	.001	.020	.005	.029	.977
	MISDARR	-.005	.007	-.149	-.732	.468
	LAGS(INT,3)	-5.897	2.530	-.514	-2.331	.024

a Dependent Variable: MB ROB

Regression: MB ROB = constant + LAGS (INT, 4) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.468(a)	.219	.105	4.501

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	272.976	7	38.997	1.925	.086(a)
	Residual	972.578	48	20.262		
	Total	1245.554	55			

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND
b Dependent Variable: MB ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	24.655	8.866		2.781	.008
	UNEMPL	-1.085	.835	-.339	-1.300	.200
	TREND	1.266	1.344	.362	.942	.351
	MEANTEMP	-.060	.040	-.212	-1.492	.142
	STATEPRI	-.019	.075	-.035	-.254	.800
	FELONYAR	-.001	.020	-.009	-.056	.955
	MISDARR	-.004	.007	-.125	-.616	.541
	LAGS(INT,4)	-6.308	2.675	-.531	-2.358	.022

a Dependent Variable: MB ROB

Appendix K: Multiple Regression Models Using ALL ROB, all Intervention Variables and all other Independent Variables

Regression: ALL ROB = constant + INT + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.402(a)	.162	.049	13.759

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	1900.083	7	271.440	1.434	.212(a)
	Residual	9844.500	52	189.317		
	Total	11744.583	59			

a Predictors: (Constant), INT, MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	60.834	26.662		2.282	.027
	UNEMPL	-.178	2.273	-.019	-.078	.938
	TREND	1.729	3.815	.178	.453	.652
	MEANTEMP	-.122	.120	-.145	-1.017	.314
	STATEPRI	.072	.212	.048	.339	.736
	FELONYAR	.008	.063	.023	.125	.901
	MISDARR	-.020	.020	-.200	-.982	.331
	INT	1.771	7.800	.055	.227	.821

a Dependent Variable: ALL ROB

Regression: ALL ROB = constant + LAGS (INT, 1) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.430(a)	.185	.073	13.622

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	2143.985	7	306.284	1.650	.143(a)
	Residual	9464.151	51	185.572		
	Total	11608.136	58			

a Predictors: (Constant), LAGS(INT,1), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	63.166	26.652		2.370	.022
	UNEMPL	-1.074	2.302	-.115	-.466	.643
	TREND	4.331	3.727	.438	1.162	.251
	MEANTEMP	-.161	.119	-.189	-1.353	.182
	STATEPRI	.138	.218	.085	.634	.529
	FELONYAR	.035	.062	.099	.569	.572
	MISDARR	-.027	.021	-.273	-1.320	.193
	LAGS(INT,1)	-7.652	7.609	-.232	-1.006	.319

a Dependent Variable: ALL ROB

Regression: ALL ROB = constant + LAGS (INT, 2) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.449(a)	.201	.089	13.358

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	2246.655	7	320.951	1.799	.108(a)
	Residual	8922.241	50	178.445		
	Total	11168.897	57			

a Predictors: (Constant), LAGS(INT,2), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	63.336	26.085		2.428	.019
	UNEMPL	-1.440	2.389	-.156	-.603	.549
	TREND	5.267	4.045	.530	1.302	.199
	MEANTEMP	-.190	.118	-.225	-1.613	.113
	STATEPRI	.122	.222	.076	.548	.586
	FELONYAR	.051	.061	.148	.839	.406
	MISDARR	-.028	.020	-.282	-1.369	.177
	LAGS(INT,2)	-11.664	7.798	-.351	-1.496	.141

a Dependent Variable: ALL ROB

Regression: ALL ROB = constant + LAGS (INT, 3) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.515(a)	.266	.161	12.908

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	2953.367	7	421.910	2.532	.026(a)
	Residual	8163.686	49	166.606		
	Total	11117.053	56			

a Predictors: (Constant), LAGS(INT,3), MEANTEMP, STATEPRI, UNEMPL, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	62.797	25.377		2.475	.017
	UNEMPL	-2.553	2.280	-.274	-1.120	.268
	TREND	7.791	3.758	.765	2.073	.043
	MEANTEMP	-.181	.114	-.215	-1.589	.119
	STATEPRI	.177	.216	.109	.819	.417
	FELONYAR	.056	.058	.157	.955	.344
	MISDARR	-.027	.020	-.270	-1.375	.175
	LAGS(INT,3)	-19.171	7.286	-.560	-2.631	.011

a Dependent Variable: ALL ROB

Regression: ALL ROB = constant + LAGS (INT, 4) + MEANTEMP + STATEPRI + UNEMPL + FELONYAR + TREND

Model Summary

Model	R	R Square	Adjusted R Square	Std. Error of the Estimate
1	.558(a)	.311	.211	12.624

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND

ANOVA(b)

Model		Sum of Squares	df	Mean Square	F	Sig.
1	Regression	3456.902	7	493.843	3.099	.009(a)
	Residual	7649.312	48	159.361		
	Total	11106.214	55			

a Predictors: (Constant), LAGS(INT,4), MEANTEMP, UNEMPL, STATEPRI, FELONYAR, MISDARR, TREND
b Dependent Variable: ALL ROB

Coefficients(a)

Model		Unstandardized Coefficients		Standardized Coefficients	t	Sig.
		B	Std. Error	Beta		
1	(Constant)	67.138	24.864		2.700	.010
	UNEMPL	-3.730	2.341	-.391	-1.593	.118
	TREND	9.590	3.770	.917	2.544	.014
	MEANTEMP	-.127	.113	-.150	-1.124	.267
	STATEPRI	.172	.211	.106	.819	.417
	FELONYAR	.050	.057	.139	.884	.381
	MISDARR	-.026	.019	-.259	-1.360	.180
	LAGS(INT,4)	-24.236	7.502	-.684	-3.230	.002

a Dependent Variable: ALL ROB