The Pennsylvania State University
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CLOSING THE MACRO-MICRO LINK:

TESTING THE EFFECTS OF HOT SPOTS POLICING ON INDIVIDUAL ADOLESCENT OUTCOMES

A Dissertation in
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by
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ABSTRACT

The effect of punishment and punishment threats on offending and collateral outcomes is less straightforward than one may believe. Furthermore, these effects for adolescents who are at the peak of criminal involvement are largely unknown. In this dissertation, I examine the effect of a hot spots policing intervention, known as Operation Safe Streets, on perceptions of arrest risk, offending behavior, perceptions of police and the law, disorder, and rewards to crime. By exploiting the timing of Safe Streets, which began during ongoing data collection of the Pathways to Desistance Study, a sample of previously adjudicated adolescent offenders aged 14-17, I am able to estimate the effect of this intervention on these outcomes. Results suggest that Safe Streets did significantly increase perceptions of arrest risk for these adolescents. Safe Streets had some effect on offending, primarily by reducing individuals' frequency of offending but had little impact on the likelihood of engaging in crime. Finally, Safe Streets did not impact perceptions of procedural justice, police legitimacy or legal cynicism, nor did it have a large impact on the perceived rewards to crime. However, Safe Streets may have resulted in the spatial spillover of neighborhood disorder. My findings suggest that Safe Streets was effective at increasing adolescents' perceptions of the likelihood of arrest, but suggest these effects did not translate into large reductions in offending as was hypothesized. This finding, coupled with the lack of beneficial effects on perceptions of police and neighborhood disorder, suggest that hot spots policing interventions like Safe Streets should be utilized as just one aspect of crime-reductions strategies and interventions aimed to improve police-community relations.

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Chapter 1. INTRODUCTION

The field of Criminology has a long theoretical tradition dating back to the 18th century. Theories of the behavior of criminal justice actors, the criminal justice system as a whole, and the individual behavior of citizens including the causes and consequences of criminal activity comprise a majority of our theories. More recently in the past few decades, criminology has made significant advances in our understanding of these theories and has used their premises to develop solutions to reduce or prevent crime. One area focuses on police behavior and police strategies to solve, reduce and prevent crime. According to a recent National Academy of Sciences (2018) report, there are now a host of evidence-based policing strategies proven to reduce crime, many of which hold aspects of our oldest criminological frameworks at their core.

One of the most effective policing strategies to reduce crime is a proactive policing tactic known as hot spots policing (National Academies of Sciences, Engineering, and Medicine, 2018). Hot spots policing is defined as focusing police and crime prevention efforts in areas with high crime (i.e., hot spots) (Sherman, Gartin, & Buerger, 1989; Sherman & Rogan, 1995). A hot spot has been defined as "a geographical area of higher than average crime…relative to the distribution of crime across the whole region of interest (Chainey & Ratcliffe, 2013, pp.145-146). These areas are generally smaller than a neighborhood or community (Braga, Papachristos, & Hureau, 2012; Braga et al., 2014). Hot spots policing, then, is a strategy that exploits the concentration of crime in a very small percentage of locations within a city by increasing the level of police surveillance in these areas (National Academies of Sciences, Engineering, and Medicine, 2018). Hot spots policing is implemented widely with almost 90 percent of police departments employing hot spots policing in some form (PERF, 2008).

The strategy of hot spots policing is rooted in two criminological theories: Deterrence and Rational Choice. The most relevant aspect of Deterrence theory as it applies to policing is the idea that as potential offenders perceive high or higher perceptions of being detected for a crime, they will be less likely to engage in that crime. Similarly, Rational Choice theory proposes the importance of perceived risk of detection but adds more nuance regarding the need for this level of risk to outweigh the perceived rewards of that crime. Hot spots policing, therefore, is believed to be effective primarily through punishment and punishment threats because these increase potential offenders' perceptions of being caught if they decide to engage in crime.

Expanding beyond Deterrence and Rational Choice to theories of Broken Windows, Procedural Justice and Police Legitimacy, adds to our understanding of the ways in which hot spots policing may be effective at reducing crime. For example, Broken Windows theory suggests that by reducing crime and disorder, it is possible that communities can increase their levels of informal social control as residents feel safer being outside and participating in community events and activities (Kelling & Wilson, 1982). Perceptions of police and the law including procedural justice, police legitimacy and legal cynicism are other important mechanisms associated with offending. Though hot spots policing interventions are primarily designed to reduce crime through their deterrent effect, these alternative outcomes may be equally important for reducing crime. Improving, or at least by failing to harm, perceptions of police, including perceptions of how they handle interactions with citizens and more overarching perceptions of cynicism toward policing, hot spots policing may also increase compliance with the law, not through punishment threats, but by improved attitudes toward police and the law. Given the evidence from a host of experimental studies which suggest that hot spots policing has generally improved feelings of safety and not harmed perceptions of police and the law, these factors may also help to explain the effectiveness of hot spots policing for reducing crime. But these factors are important to study in concert. If perceptions of procedural justice, legal cynicism, police legitimacy, and disorder are related to future offending, a strategy which increases perceptions of arrest risk yet decreases these other perceptual outcomes may not decrease crime. The gains in a deterrent effect could be offset by reductions in police legitimacy or increases in legal cynicism. Strategies that impact these outcomes in this way may therefore have null effects on crime, or worse, may exacerbate crime problems.

These hypotheses regarding the mechanisms for why hot spots policing is effective are theoretically sound, but the results from existing research raise questions worthy of additional research. Highly rigorous experimental studies, such as randomized controlled trials (Braga & Bond, 2008; Braga et al., 2014; Ratcliffe, Taniguchi, Groff, & Wood, 2011; Sherman & Weisburd, 1995) and quasi-experimental designs (Lawton, Taylor, & Luongo, 2005; Sherman & Rogan, 1995), have demonstrated that hot spots policing causes a reduction in crime measured through official crime data or through residents calls for service (i.e., 911 calls or 311 non-emergency calls). The examinations of micro-level policies, typically done at the city or jurisdiction level, demonstrate that it is likely they can impact offending, however, individual-level offending behavior is almost never examined as nearly all studies rely on official crime data or calls for service. On the other hand, macro-level studies which have examined the impact of police more broadly, typically measured as police per capita or arrests per crime for each jurisdiction, have not found much support for the idea that police can impact perceptions of arrest risk (Kleck & Barnes, 2013, 2014; Kleck, Sever, Li, & Gertz, 2005) or crime (Lee, Eck, & Corsaro, 2016). These studies on macro-level police factors have also almost never examined individual-level offending behavior as an outcome.

The near exclusive focus of police on crime as measured by official crime data or calls for service makes it difficult to assess the potential spatial spillover of crime. If individuals seek to minimize their detection risk, it seems likely that potential offenders will choose to commit crimes in areas with lower likelihoods of detection. This could include in locations with fewer police per capita, in areas with lower arrest rates, or in areas which do not enact hot spots policing. In the latter case, many experimental studies have designed approaches to test the spatial spillover of crime by examining crime in areas immediately surrounding each targeted hot spot. However, if offenders relocate to areas outside of these ‘buffer zones’, particularly if they relocate to adjacent jurisdictions, the existing studies would not have captured this. Utilizing self-reported offending behavior would solve this problem by including all offenses that occurred before and after a hot spots policing intervention, regardless of the jurisdiction in which the crimes took place.

Additionally, criminological theories predict that large increases in police presence may result in more negative attitudes toward law enforcement, especially if these increases are viewed as unjust, or officers do not engage with citizens in procedurally just ways. Increased police presence may also increase perceptions of crime and disorder, causing individuals to decrease time or engagement within their communities. Our existing research suggests that interventions such as hot spots policing often do not impact citizens perceptions of law enforcement or community crime and disorder. However, these results may be premature. The existing research rarely follows the same citizens over time before and after the intervention, and almost exclusively focuses on law-abiding citizens, not those who are likely to experience direct contact with law enforcement. It remains unclear what effects these strategies have on adolescents as well as those previously or currently engaged in offending.

Viewing the totality of the existing evidence, questions remain about the potential for police to prevent crime through these different avenues. These divergent findings on the deterrent effects of police are concerning and have caused many criminologists to publish extensive reviews and theoretical pieces describing why or why not police can prevent crime. Unfortunately, the existing research suffers from methodological limitations making it impossible, given the current state of the literature, to end this debate. For example, studies addressing deterrence and studies of hot spots policing specifically suffer from the following limitations and gaps:

(1) Few studies exist which evaluate the effect of policing on perceptions of arrest risk, and none have focused on samples of those mostly likely to engage in crime when examining this relationship

(2) Few studies exist which examine the effect of policing on perceptions of police and related neighborhood outcomes for those most likely to be impacted by a change in policing

(3) Few studies have examined different theoretical expectations in a single analyses, for example, a study which examines perceptual deterrent effects and perceived rewards to crime alongside perceptions of police

(4) Few if any studies have evaluated the effect of police on individual-level offending data such as self-reported offending

(5) No study has examined *changes* in policing on *changes* in individuals' perceptions and offending over time

(6) Most of the existing studies suffer from an inability to address endogeneity between the predictor (i.e., police presence or hot spots policing) and the outcome (i.e., official crime or calls for service).

Lacking studies which address these limitations and enhance our theoretical understanding of the reviewed theories, it is not surprising that the jury is still out on whether or not police prevent crime and the extent of the potential collateral consequences for those likely to engage in crime.

Overall, these limitations are noteworthy because of the unanswered policy questions that remain and the gaps in the criminological literature. It remains unclear whether hot spots policing is associated with potential offenders' perceptions of arrest risk. Similarly, it is unclear if hot spots policing is associated with variation in individual-level offending. Finally, little is known regarding the collateral consequences of hot spots policing for juveniles and for offenders including impacts on perceptions of police and neighborhood disorder. These gaps in studies and limitations of existing work, if addressed, would advance our understanding of the deterrent effect of police, the efficacy of hot spots policing and our understanding of the potential unintended consequences of hot spots policing.

Contributions

This dissertation will contribute to the existing literature in a number of ways which can best be categorized by contributions to theory, policy and methods.

Theoretical Contributions

This dissertation adds to our theoretical understanding of offender decision-making and fills gaps in our understanding of Deterrence and Rational Choice theories. Understanding the effect of policing on perceptions of arrest risk is a mainstay in criminological theories such as Deterrence and Rational Choice, yet this association remains largely untested. Specifically, this study extends existing research on risk perceptions by focusing on individual-level data. *This dissertation will be the first study to examine the effect of a police intervention on perceptions of arrest risk on the same individuals over time*. This is important given recent research which suggests that comparing perceptions of arrest risk within individuals is preferable to making between-person comparisons. Understanding this effect for perceptions is extremely important for theory because, despite the strong theoretical backing of deterrence theory, there is a lack of evidence demonstrating that the criminal justice system can impact of explain individual differences in risk perceptions (Paternoster, 2010).

Additionally, the current dissertation will be amongst the first studies to test the deterrent effect of hot spots policing on self-reported offending. The existing literature, though methodologically rigorous and expansive, has not examined how hot spots policing impacts individual-level behavior. Because of this, it remains unclear how potential offenders respond to hot spots policing. For example, it may be the case that individuals respond to hot spots policing by deciding to they relocate their offenses to new and less policed areas. Though different approaches have been taken to assess if crime spills over to adjacent communities, no existing study has assessed this potential issue by examining individual-level behavior. This allows for an assessment of not only offending rates, but also if offending behavior changed in other ways, for example, if potential offenders decide to simply engage in fewer crimes but continue offending.

This dissertation also adds to the existing literature which has examined the impact of police on rewards to crime, procedural justice, police legitimacy, legal cynicism and neighborhood disorder. Few studies have examined how criminal justice factors may result in deleterious impacts on these other perceptual outcomes. For example, if increasing the odds of getting caught for a crime also increases ones' personal rewards (i.e., thrills) and social rewards (i.e., prestige or 'street cred') for engaging in crime, any increases to perceptions of arrest risk may be offset. Few studies have examined risk and rewards to crime in concert, and few if any have done so as a function of a policing intervention. Assessing both will improve the field's understanding of Rational Choice Theory and help us to determine the if theory of Deterrence, which ignores rewards to crime, is the best theoretical framework to use when discussing offender decision-making.

Policy Contributions

This dissertation will be amongst the first studies to examine the effects of an implemented policy on perceptions of arrest risk. The existing research on macro-level police factors such as police force size or arrest rates has examined individuals perceptions of the arrest rate for a community but not an individuals' perceptions of their own risk of arrest. The main question of interest for deterrence scholars and policy makers is not whether police presence can impact *individuals' perceptions of the average arrest rate*[1] but rather, whether police presence can impact *potential offenders' perceptions of their own arrest risk.* By assessing the former, this dissertation will actually be answering the primary policy question rather than studying a tangential issue. This is important to understand if criminal justice actors and policy makers want to understand precisely why policing strategies are effective and how much impact they have on perceived risk of arrest and subsequent crime. This will allow these actors to improve the efficiency of existing strategies and new crime-reduction interventions.

[1] That is not to say this question is unimportant. It may be important for its relationship with other outcomes such as police legitimacy and legal cynicism.

The current dissertation also adds to our knowledge regarding the possibility of deterring crimes *before* they are committed, rather than responding with arrest and punishment after the fact. Speaking in terms of policy, police can prevent crime through the threat of arrest (e.g., increased police presence due to hot spots policing), or through arrests (i.e., arresting an offender after they committed a crime). The ideal solution is the former, where police prevent crimes by increasing one's perception of arrest risk for a crime before the crime is actually committed. Criminal Justice expenditures for the United States are approximately $300 billion annually, and in most recent years, approximately half of that, around $142 billion spent annually on policing and the other half spent on courts and incarceration (Bureau of Justice Statistics, 2020). In the City of Philadelphia, the focus of this Dissertation, expenditures hover at around $7.5 million annually (Bureau of Justice Statistics, 2020). By deterring and preventing crimes before they occur, criminal justice expenditures for processing, sentencing and potentially subsequent incarceration or probation costs are saved. And by policing more efficiently by understanding what does and does not work can also help to save costs. More importantly, the measurable costs of victimization, such as replacing stolen goods and medical services, as well as the immeasurable costs of fear, trauma, and distress, are all saved.

Furthermore, by deterring crimes before they occur, those who decide not to engage in criminal activity can avoid criminal justice contact and the negative consequences of entering the criminal justice system. For these reasons, it is crucial to better understand if specific policing strategies such as hot spots policing are effective at impacting perceptions of risk, reducing subsequent crime, improving perceptions of police, the law and neighborhood safety. Overall, it is time to understand the mechanisms behind how policing may reduce crime and the potential costs and benefits of these strategies.

The current dissertation will also be one of the few hots spots policing evaluations which examines the effects of an intervention for an entire city, rather than estimating the effects in treated locations by comparing them to nontreated sites. This limitation of the experimental research on hot spots policing and other policing interventions has been raised in recent years (Nagin & Sampson, 2019). Without knowledge of the effects for an entire city, it means policy makers and police chiefs can not assess what the expected crime reductions are for their jurisdiction if they adopt specific interventions (Weisburd, 2018). This knowledge may help to increase buy-in from policy makers and criminal justice actors.

Finally, the focus on a sample of adolescent offenders allows for exploration of the effects of hot spots policing on perceptions of police and related outcomes for a population that is likely to actually experience a change in contacts with police as a result of the intervention. Prior work has almost exclusively focused on community residents over 18, and the use of telephone surveys has generally resulted in an overrepresentation of women and those not likely to experience police contacts or engage in crime. By understanding if hot spots policing impacts these outcomes for sample of potential offenders, we will have a better understanding of the potential collateral consequences of hot spots policing.

Methodological Contributions

The focus on individual-level data provides an opportunity to reduce some of the concern over endogeneity when examining the relationship between police and crime. Endogeneity is a problem because police interventions themselves may actually increase the ability of the police to detect crime through their own increased presence on the streets and also through increased citizen reporting of crime. If increased police presence as a result of hot spots policing, for example, means that officers uncover and subsequently record more crime, the actual reduction in the number of offenses committed may be larger than the one found from examining official data. By examining self-reported offending, there is a better chance of including all crimes committed before and after the intervention (rather than just those uncovered by police) and an increased likelihood of estimating a more accurate figure regarding the reduction in crime. With the use of individual self-report data, instead of conflating the possible positive effect on crime (due to a greater capacity for police to uncover crime) with the negative effect on crime (if there is truly a deterrent effect), the former can be ruled out.

The current study also provides an example of an innovative way of revisiting existing data with a focus on their timing and historical context as it relates to criminal justice policies. Only a handful of studies have exploited natural variation in events during ongoing data collection to aid in estimating the causal effects of some policy or event. For example, White, Weisburd and Wire (2018) used the timing of the death of Freddie Gray which occurred during ongoing data collection of community residents to assess the impacts of police misconduct on obligations to obey the law (White, Weisburd, & Wire, 2018). Though innovative, that study utilized propensity score matching to compare two distinct samples of respondents pre and post treatment. The current study relies on the same respondents measured prior to and during a hot spots policing intervention to address some of the methodological limitations of the existing research.

Dissertation Objectives

The overarching objective of this dissertation is to better understand policing, offending and the mechanisms between police and offending. As discussed, this dissertation draws on theoretical frameworks which speak to the ways in which police can prevent and reduce crime and offending, as well as some of the potential collateral consequences of increased police presence. This includes Deterrence, Rational Choice, Procedural Justice, Police Legitimacy, Legal Cynicism, and Broken Windows. These theories, as well as the relevant literature will be reviewed in the next chapter. The first broad goal of this Dissertation is to provide new tests of these theories, some of which will be the first empirical assessments of certain aspects of these theories. The second objective is to improve our understanding of the effectiveness of hot spots policing in an effort to improve policy solutions to crime. Finally, the last objective of this dissertation is to assess the collateral consequences of a change in policing for a sample of respondents who are the most likely to be impacted by the police intervention in question.

Research Questions

1) Did Operation Safe Streets, a hot spots intervention, result in increased perceptions of arrest risk during the intervention, relative to periods prior to the intervention? (Study 1)
2) Did Operation Safe Streets result in decreased levels of offending during the intervention, relative to periods prior to the intervention? (Study 2)
3) Did Operation Safe Streets have an effect on additional collateral consequences, including perceptions of procedural justice, police legitimacy, legal cynicism, neighborhood disorder and rewards to crime? (Study 3)

The Current Studies

This dissertation is composed of six chapters, including three empirical chapters which examine the effect of Operation Safe Streets (henceforth Safe Streets) on a host of outcomes.

Following this introductory chapter, Chapter 2 discusses the theory and relevant literature, Chapter 3 explains the data used for the analyses, and provides an in-depth description of Safe Streets, the particular hot spots policing intervention which is examined. Chapter 4 is the first empirical chapter (Study 1), which assesses the effect of Safe Streets on perceptions of arrest risk. Next, Chapter 5 examines the effect on self-reported offending and related outcomes (Study 2). Chapter 6 examines alternative theoretically motivated outcomes expected to be impacted by hot spots policing (Study 3). Finally, Chapter 7 provides a summary of the findings from the three studies, discusses the generalizability and limitations of this dissertation, highlights some policy and research implications and concludes with a discussion of future directions and the main takeaways from this dissertation. Abstracts for each empirical chapter, discussed below, provide more detail on the three studies.

Study 1

Study 1 explores the impact of Safe Streets, a hot spots intervention in Philadelphia, on offenders' perceptions of arrest risk. Exploiting the timing of Safe Streets, which occurred during an ongoing longitudinal survey of high-risk offenders, the current study examines the impact of this intervention on changes in perceptions within person. Results show that Operation Safe Streets is related to a substantial increase in perceptions of arrest risk. The findings suggest that this hot spots intervention, which was not characterized by high levels of arrest and enforcement, did significantly increase perceptions of arrest risk in a sample of adolescent offenders. Several model specifications are examined, and falsification models are estimated to ensure confidence in these findings.

Study 2

Study 2 explores the impact of Safe Streets on individual self-reports of offending. Self-reported offending is measured retrospectively by aggregate reports of offending in the time since the last interview (i.e., wave-level analyses) and retrospectively by offenses mapped to the month in which they occurred (month-level analyses). First-difference models are used to examine the impact of Safe Streets on offending over time, net of time-stable individual traits and time-varying prior offending and contacts with the criminal justice system. The results suggest that the implementation of hot spots policing may have had a negative effect on offending, but the results are inconsistent across measurements of offending. To further explore the relationship between Safe Streets and offending, models are employed to examine the effects of the intervention on the ease in which one can acquire a firearm, the cost of firearms, peer offending, personal victimization and witnessing violence and the decision to engage in illegal work and illegal earnings. These results suggest that the intervention did significantly reduce some of these related outcomes. However, various falsification tests suggest that these results may be due to pre-existing trends in offending and these related behaviors. This study lends some support for the effectiveness of hot spots policing and suggests the possibility that hot spots policing can be effective at reducing crime. Nonetheless, the study raises questions as to whether these effects are above and beyond the effects that would be found over the same period without the intervention.

Study 3

Study 3 examines alternative mechanisms and collateral consequences that may be experienced as a result of increased police presence. This includes changes to perceptions of procedural justice, legal cynicism, legitimacy, neighborhood disorder, and personal and social rewards to crime. Within-person first-difference models which parallel the models from Study 1 are used to examine the effect of Safe Streets over time on these collateral consequences. The results show that Safe Streets had no effect on these outcomes. The null effects for perceptions of procedural justice, police legitimacy and legal cynicism, as well as the weak or null effects for personal and social rewards to crime, are promising. These findings suggest that Operation Safe Streets did not harm perceptions of police for these adolescents and did not make crime more thrilling or socially rewarding. However, the null effects and in some models, positive effects for perceptions of neighborhood disorder are somewhat discouraging as one goal of the intervention was to promote safer and less crime-ridden communities where residents could feel more comfortable outdoors.

Chapter 2. THEORY & PRIOR LITERATURE

Theory

There are several theories which speak to the relationship between police and crime, the relationship between police and collateral consequences, as well as the mechanisms which may explain the negative or positive association. These theories are each reviewed separately and then the next section examines the evidence which supports or refutes their claims.

Deterrence Theory

Deterrence theory is the most common theory applied to explain the relationship between police and crime. Traced back to the 18^{th} century, Deterrence theory has three main components; severity, certainty, and celerity (i.e., swiftness) of punishment (Beccaria, 1986; Bentham, 1988). The components of deterrence most relevant for police are the certainty and swiftness of punishment. While the swiftness of punishment had previously been discounted as a factor in police prevention of crime, it has been gaining some traction in recent years (see Mastrobuoni, 2019), however, this dissertation focuses exclusively on the role that police play in altering the *certainty of punishment* and does not assess the role of the celerity of punishment. The certainty of punishment is typically measured by assessing individuals' perceptions of their likelihood of arrest or apprehension but more broadly can include measures which assess how likely an individual perceives their chances of being punished. As such, measures of the certainty of punishment are discussed as the likelihood of arrest, apprehension, or punishment, btu the most commonly used language and measurement is the likelihood of arrest.

Certainty of punishment broadly is believed to be one of the key mechanisms through which the criminal justice system, typically police, can impact crime. As potential offenders perceive a greater probability of being caught for an offense, they are less likely to engage in that crime opportunity. This increase in certainty can be specific to a single offense, such as when an officer is nearby a location where an offender was contemplating committing a crime, or the types of crime, for example if an arrest for a motor vehicle theft increases one's perception of arrest for stealing a car in the future, but it can also relate more generally to global perceptions of certainty that prevent individuals from engaging in crime at all. Figure 2.1 provides a conceptual model of Deterrence theory, which includes the first pathway from police to perceptions of arrest risk and the second pathway from these perceptions to offending.

There are several different frameworks used to explain the various forms of deterrence and how they operate in practice. The most common distinction is between *specific* and *general* deterrence. Specific deterrence, also called special deterrence, is the effect that occurs after actually experiencing punishment (Andenaes, 1966). For example, after experiencing an arrest, an individual may increase their perceptions of arrest risk. On the other hand, general deterrence operates by demonstrating to others that individuals are caught and punished for crime. It has been defined as "the imposition of sanctions on one person [which] may demonstrate to the rest of the public the expected costs of a criminal act, and thereby discourage criminal behavior in the general population" (Nagin, 1978: p. 78). Specific and general deterrence are therefore typically focused on the effects of punishments, either as experienced by one's self, or as learned about because of the punishment of others.

Others discuss the difference between *direct* punishment experiences and avoidance, and *indirect* (vicarious) punishment experiences and avoidance (Stafford & Warr, 1993). This framework explicitly adds the importance of punishment avoidance (i.e., committing a crime and getting away with it). It also adds the notion that both direct and indirect punishment experiences can operate together and for the same individual. For example, being apprehended for a crime, and knowing that most often others are also apprehended, will have a larger deterrent effect than being apprehended and learning that others almost always get away with the crime. Though this framework is less often used compared to the distinction between general and specific deterrence, it has been well received and the added benefits of vicarious deterrence remain an important update to the deterrence literature (Paternoster & Piquero, 1995).

Finally, others have used a different framework regarding the role of criminal justice actors to discuss the different ways in which crime can be deterred. The deterrent effect that police can impart is generally believed to occur through two distinct avenues; through the police's role as apprehension agents where they arrest or apprehend individuals, or through their role as sentinels, where their mere presence can deter potential offenders from engaging in crime (Nagin, Solow, & Lum, 2015).

The former role of police as apprehension agents is focused on changing offenders' perceptions of apprehension risk through apprehension experiences. In this role, police may impart specific deterrence but they also may generate some additional general deterrent effects for those potential offenders who become aware of the apprehended offenders' recent arrest experience (vicarious deterrence) (Stafford & Warr, 1993). The latter role of police on the other hand, operates as an ex-ante intervention meaning it occurs before a crime takes place, where the threat of punishment *and not actual punishment* (i.e. arrest or incarceration) decreases offending (Nagin, 2013). Police engage in this role when engaged in foot patrol or car patrols for example.

Both of these roles are believed to deter crime by increasing potential offenders' perceptions of arrest risk. The hope is that this change in risk will occur as a result of punishment, such as apprehension, arrest, or citations, or through the mere threat of punishment, which may be signaled by an increase in police presence or a change in policy (e.g., DUI checkpoints).

These different roles are an additional way to think about deterrence, similar to the distinctions between specific and general deterrence, or direct and indirect punishment experiences. But these dichotomies often overlap. For example, when police arrest offenders, they are enacting their apprehension agent role and (presumably) resulting in a specific deterrent effect, but knowledge of this arrest may result in a general deterrent effect by increasing the arrest rate for a community and a vicarious deterrent effect if this offenders' accomplices or friends become aware of the arrest. Similarly, increasing police presence can have a general deterrent effect in the sense that potential offenders experience an increase in the likelihood of arrest, but it may also be thought of as a vicarious deterrent effect if potential offenders share their knowledge about increased police presence to each other. Moreover, if one experiences an increase in police presence in their community (i.e., the sentinel role of police), but then also learns of increased arrest rates in their community or the arrest of a friend (i.e., the apprehension agent role), they are experiencing general deterrence but through both roles of police.

Because of this overlap, these dichotomies are not mutually exclusive. As such, in this dissertation, the phrases *punishment* and *threat of punishment* are used to distinguish between direct punishment experiences such as arrest, and all other ways in which police behavior creates threats of punishment. It is also worth noting that punishment threats are the preferable form of deterrence as it means crimes are deterred *prior* to having been committed, rather than by seeking to deter past offenders (who necessarily caused a crime and a resultant victim) from committing new crimes.

To review, deterrence can occur by arresting or apprehending an individual (i.e., specific deterrence/apprehension agent role), by arresting an individuals' friends or accomplices (i.e., indirect/vicarious deterrence), or by increasing the threat of punishment by increasing officer presence (i.e., the sentinel role) or the rate of arrest for crimes in one's community (i.e., general deterrence). In all instances, it is believed that experiencing some change in risk is what has a deterrent effect, rather than the actual level of risk (Becker, 1968; Kahneman & Tversky, 1979). Regardless, of the role of police, perceptions of apprehension risk must be above the specific offenders' threshold of risk that they are willing to take (Nagin et al., 2015).

Rational Choice Theory

Beyond Deterrence, there is another related theory which can explain the association between police and crime; Rational Choice theory (Becker, 1968). A key aspect of Rational Choice Theory which distinguishes it from Deterrence Theory is the focus on the rewards to crime. The original theory is most often described in terms of the formula:

$$E(UC) = (1 - pc)\, U(R) + pc\, U(R - C)$$

where U is the expected utility from crime, pc is the chance of getting caught, R is the return to crime and C is the cost of crime or the punishment.

As you can see, in addition to the important role of the likelihood of getting caught, Rational Choice Theory incorporates information on the expected rewards of crime, as well as the expected punishments. Figure 2.2 provides a simplified conceptual model of Rational Choice theory. The current study does not delve into the effects of variation in punishment (i.e., incarceration versus probation or parole), but it does focus on the role of the rewards to crime.

The reward aspect of crime is often overlooked in studies of criminal decision-making (Piliavin, Gartner, Thornton, & Matsueda, 1986) but has gained more prominence in recent years (see for example Loughran, Paternoster, Chalfin, & Wilson, 2016; Matsueda, Kreager, & Huizinga, 2006; Thomas, Loughran, & Hamilton, 2020). The thrill or personal rewards, sometimes called intrinsic rewards of crime, are well noted in qualitative studies of crime such as robbery or burglary as reasons to engage in crime (Jacobs & Wright, 1999). The social benefits or prestige earned by engaging in crime are another type of reward often cited as a reason for why some offenders engage in crime (Moffitt, 1993).

Using a Rational Choice framework, it is important to acknowledge the rewards to crime as they may have a direct effect on decisions to offend. Additionally, these rewards may be directly impacted by the changes in apprehension risk. For example, the monetary rewards from crime may increase if crime becomes more difficult. Imagine a scenario where a certain amount of drugs yields a certain profit, but now, the supply of drugs is smaller. Customers may now be willing to spend more, yielding a higher profit margin for the seller. The thrill or intrinsic reward from crime may also change if crime becomes riskier. The rush of almost getting caught is often cited by offenders as one reason why they enjoy crime (Jacobs & Wright, 1999). Finally, just as the monetary or intrinsic rewards may be impacted by the risk of crime, social rewards may increase as one's peers perceive crime to be riskier, meaning only the 'toughest' or most brazen individuals will still choose crime, resulting in more prestige for those willing to continue taking risks.

Procedural Justice

There are several criminological theories which discuss other ways in which police and crime are related. First, the theory of Procedural Justice focuses on how police behave toward citizens and how perceptions of the procedures used by police or other law enforcement, and the resultant experiences with law enforcement relate to perceptions of police and the law. This framework also focuses on how these perceptions impact subsequent decisions to obey the law in the future (Sunshine & Tyler, 2003). Procedurally just perceptions or judgements occur when respondents perceive their encounters with police to be neutral, objective and respectful (Tyler, 1990, 2003, 2004), as well as fair and consistent (Sunshine & Tyler, 2003) and when the individuals are given a voice or hand in the decisions or final outcomes (Tyler, 1990). Perceptions of procedural justice are believed to be indirectly related to offending because these perceptions can translate into more positive views of police and perceptions of police legitimacy (Sunshine & Tyler, 2003; Tyler, 1990; Tyler & Huo, 2002) which subsequently relate to decreased future offending and increased compliance with the law (Fagan & Tyler, 2005; Higginson & Mazerolle, 2014; Paternoster, Brame, Bachman, & Sherman, 1997). Figure 2.3 presents a conceptual model of Procedural Justice.

Legitimacy

The related concept of Police Legitimacy is also an important potential mechanism explaining the relationship between police and crime. Police legitimacy is the idea that citizens view the police as worthy of authority and as such, worthy of obedience (Sunshine & Tyler, 2003). It does relate to the effectiveness of the law, for example how often police arrest offenders, but has more to do with how one perceives their interaction with law enforcement (Sunshine & Tyler, 2003). Legitimacy is believed to be impacted by perceptions of procedural justice (Tyler & Fagan, 2008). In the same way then, legitimacy is one possible mechanism to explain the relationship between police and crime. Figure 2.4 presents a brief conceptual model of the Legitimacy framework.

Legal Cynicism

The last theory regarding perceptions of police is legal cynicism. Legal cynicism is the belief that the law and the criminal justice system are illegitimate and ineffective at keeping citizens safe (Kirk & Papachristos, 2011). It is largely believed to be an orientation or cultural belief about criminal justice actors such as police. Within this framework, individuals who are cynical toward the law do tend to be law abiding citizens on average, but often feel that because the criminal justice system is so ineffective at keeping them safe and resolving their problems, they must solve their issues on their own. Perceptions of legal cynicism are created and altered from two specific factors: "(1) neighborhood structural conditions and neighborhood variation in police practices and (2) resident interaction with the police" (Kirk & Papachristos, 2011: p. 1198). These perceptions of legal cynicism then can spread further throughout a community via interactions between residents. Figure 2.5 presents a conceptual model of the Legal Cynicism framework.

Prior theorizing has discussed legal cynicism as a cause and consequence of neighborhood crime rates, as well as a potential mechanism to explain why some neighborhoods have heightened rates of violence compared to others (Kirk & Papachristos, 2011). Legal cynicism may be a cause of crime in that it increases residents' likelihood of doing crime because they cannot rely on the police for help, and because they are less likely to call the police when crime occurs (Kirk & Matsuda, 2011). It can also be a consequence of crime because as residents see the police's inability to reduce or prevent crime, they may become more cynical. And finally, it can operate as a mechanism between neighborhood conditions and crime; neighborhood factors such as high rates of adolescents and young adults, or high levels of disorder and low social control, spur greater cynicism toward the law which results in more crime.

The original conceptualization of legal cynicism is a framework to describe 'anomie' or normlessness in regard to the law; citizens did not view the law as worthy enough for them to follow it (Sampson & Bartusch, 1998). But the more recent conceptualizations refer to legal cynicism as a 'cultural frame' and measure it using perceptions of the law and police (Kirk & Papachristos, 2011) rather than through social norms (Sampson & Bartusch, 1998). The current dissertation employs the more recent definition and relies on perceptions of the law and law enforcement because this dissertation is concerned with how these perceptions are altered by variation in police practices.

Broken Windows

Finally, one reason police may impact crime is not through their *direct* effect on crime, but rather by decreasing disorder and dangerousness in a community. Decreased disorder is believed to improve residents' perceptions of disorder and safety that results in increased time spent in communities and informal social control. This notion is put forth by Kelling & Wilson (1982) in their article on Broken Windows. These authors specifically highlighted the need to reduce social disorder in order to reduce crime. Social disorder is typically categorized as disorderly people, including individuals without shelter, intoxicated persons or persons harassing citizens, while physical disorder typically includes things such as abandoned buildings, trash and graffiti. By fixing the physical disorder (the "broken windows") and punishing the social disorder, it is hoped that the social disorder will decrease, and residents will begin to feel safe in their communities. Figure 2.6 provides a conceptual model of Broken Windows theory.

In the article, Kelling & Wilson (1982) explicitly discuss the role of police. The goal of Broken Windows is for police to help bolster the informal social control of communities. As such, the authors note that Broken Windows policing will be most effective in those communities which are 'marginally threatened' and not yet completely overridden with crime as these communities still have the capacity to engage in informal social control. Kelling & Wilson (1982) explicitly ask, "how can the police strengthen the informal social-control mechanisms of natural communities in order to minimize fear in public places?" They propose that one way to do so is by decreasing disorder. Therefore, Broken Windows policing is a form of policing where police are expected to address issues of disorder, for example, by closing down a bar which frequently has fights break out, or by issuing citations to illegal street vendors. This type of policing does not explicitly make a point of engaging with citizens, but citizens play a key role in its success. Broken Windows policing has taken many forms and ranges on a spectrum from informal enforcement of low-level offenses to formal enforcement such as stops and arrests.

Prior Literature

The 6 theories reviewed above have been examined in a host of empirical studies regarding the relationship between police and crime or offending behavior. First, I review the studies on Deterrence and Rational Choice Theory simultaneously given their overlap in the literature. Next, I discuss prior literature on Procedural Justice, Police Legitimacy and Legal Cynicism, and then present research on Broken Windows Theory. Within each section, I present the existing evidence in support or against these theories and discuss the limitations and note the gaps in the literature. Finally, I present additional evidence from existing empirical studies demonstrating when police have or have not been effective at reducing crime, focusing on the studies which have examined the direct effect of police on crime. Notably these studies do not fit into the sections relating to each theory as they do not assess specific theoretical mechanisms (i.e., perceptions of apprehension risk, procedural justice, legitimacy, cynicism and disorder). Finally, I give a concise summary of the noted gaps in these literatures.

Deterrence & Rational Choice

There is an expanse of studies which test Deterrence and Rational Choice. First, I review the studies which examine the first link in the theorized causal chain of these theories which focuses on the impact of police or police actions on perceptions of apprehension risk. Within this section, I first discuss the research on the impact of punishment, and then review the research on the threat of punishment on perceptions of punishment certainty. In the majority of studies, punishment certainty is measured as the risk of arrest. As such, I refer to this as perceptions of arrest risk unless there is variation in the measurement of the concept of perceptions of punishment certainty. Second, I review the research on the second link, which focuses on the effect of these perceptions on offending behavior.

The First Link – Police Effects on Perceptions

The first body of research examines the effect of experiencing punishment on perceptions of arrest risk, as well as the effects of punishment avoidance on perceptions of arrest risk. In this line of research, perceptions of arrest risk are measured after someone has been arrested for a crime (or gotten away with it) by responding to some variant of the question, "How likely are you to be arrested for committing crime?" Studies of this kind are somewhat new in the field of criminology. Scholars commented on the dearth of research regarding offending and punishment experiences on perceptions of arrest risk just 15 years ago (Pogarsky, Piquero, & Paternoster, 2004).

The earliest work on this topic has been supportive of a deterrent effect. For example, some have found that those who can evade punishment report lower perceptions of arrest risk (Paternoster & Piquero, 1995). Others have shown that, in samples of high school students, arrest experiences increase perceptions of arrest risk for various crimes (Pogarsky et al., 2004). In a sample of youth from high-risk neighborhoods, a positive association was found between punishment experiences and perceptions of arrest risk and a negative association was found between punishment avoidance and perceptions of arrest risk. However, on average, those who had reported engaging in crime reported lower perceptions than those who had no committed offenses (Matsueda et al., 2006). This finding supports Tittle's "shell of illusion," meaning that individuals who are naïve about crime will believe they are more likely to be arrested than those with experience offending (Tittle, 1980).

These studies have all demonstrated that punishment experiences increase an individual's perception of arrest risk and that punishment avoidance generally decreases that risk. But these studies have been critiqued due to their focus on nonoffending samples and relatively small sample sizes (Anwar & Loughran, 2011). More recent research has addressed these limitations. Lochner (2007) examined the relationship between individual arrest experiences and individuals' perceptions of their own arrest risk using the National Youth Survey (n=918) and the National Longitudinal Survey of Youth 1997 (n=4,621). Those who had recently been arrested reported higher perceived arrest risk, suggesting some evidence of a specific deterrent effect. Anwar & Loughran (2011) more recently examined a sample of adolescent offenders and found that on average, these individuals will update their perception of arrest risk by 6.3% compared to if they had not been arrested. This work also suggests that the effects of the rate of arrests to offenses (i.e., one's failure rate at avoiding arrest) is potentially more important than simply experiencing arrest or not when examining deterrent effects (Anwar & Loughran, 2011). This research has made an important contribution to the deterrence literature, demonstrating that perceptions of arrest risk are malleable to punishment experiences, but it remains unclear how variation in the threat of punishment without direct punishment experiences is related to perceived arrest risk.

The following section reviews studies which are concerned with the impact of punishment threats; these studies can be thought of as testing some combination of a general deterrent effect, vicarious deterrent effect and/or the deterrent effect of the sentinel role of police. First, I discuss the more common 'calibration studies' (Apel, 2013; Manski, 2004) which examine the association between punishment threats and perceptions of the certainty of punishment in a specific area (i.e., the arrest rate for a specific locale). Then I focus on studies which are more in line with tests of perceptual deterrence as they examine individuals' perceptions of their own likelihood of getting caught for committing a crime. I refer to these outcomes as perceptions of the arrest rate and perceptions of arrest risk, respectively.

The majority of studies on the deterrent effect of sanction threats have generally set out to test the assumption that respondents are aware of sanction threats or in other words that objective rates of arrest align with subjective perceptions of the arrest rate. These 'calibration studies' typically examine some variant of the outcome measured as, "Of the last 100 crimes committed in your city/county, how many resulted in the arrest of the offender?"

One of the first studies to test the relationship between official arrest rates and perceptions of the arrest rate was conducted by Claster (1967) who asked respondents to select the arrest rate for 6 specific crimes using a multiple-choice format. Both delinquents (those currently incarcerated at a juvenile training school) and nondelinquents (a sample of high school students) correctly estimated the arrest rate for only about 2 of 6 crimes, suggesting some awareness of sanction threats though not much. Notably the delinquents were more likely to overestimate the arrest rate while the non-delinquents were likely to underestimate it (Claster, 1967). Though difficult to assess with this cross-sectional data, it is possible that this overestimation by delinquents suggests some impact of their prior punishment on perceptions of punishment, which would be in line with a deterrent effect.

Erickson and Gibbs (1978) similarly explored the relationship between official arrest rates and perceptions of arrest rates using a random sample of 1200 Tucson residents in 1974 (Erickson & Gibbs, 1978). The correlation amongst official arrest rates and perceptions of arrest were calculated by comparing perceptions of the arrest rate for 10 different crimes (i.e., "of the last 100 cases of (crime type) committed… here in Tucson, what is your guess as to the number that resulted in the arrest of a suspect?") to official arrest rates (i.e., the number of arrests made). They found a strong correlation (r = 82) between the arrest rate and perceptions of the arrest rate. Parker & Grasmick (1979) also tested the relationship between arrest risk and perceptions of that risk by utilizing official arrest statistics. Adult respondents estimated the number of burglaries that resulted in arrest. On average, respondents reported that 28.5% of burglaries resulted in an arrest (median = 25%), while the official clearance rate was 13%. The authors conclude that these estimates suggest that individuals are not acutely aware of the arrest rate in their county. Others have examined the association between traffic citation rates measured as the number of traffic citations issued per day, and perceptions of those rates in a sample of adults (Cohen, 1978). Cohen found a weak pattern between the average number of citations given per day in the preceding three months, and respondents perceptions of citations awarded. On average, respondents perceived a much higher likelihood of receiving a citation than reality.

More recently, Scheider (2001) employed a randomized experiment to examine the effects of receiving knowledge of arrest rates on perceptions of the arrest rate for the average person. Respondents received prompts which varied the arrest rate for certain crimes, while the control group received no information regarding the arrest rate. This study found that knowledge of the arrest rate did in fact impact individuals' perceptions of the arrest rate for the average person. Because this was an experimental manipulation, it is not exactly considered a calibration study which is concerned with this relationship in the real world, but it does lend some support for the idea that objective arrest rates may impact perceptions of those rates.

The more recent work by Kleck and colleagues (2005; 2013; 2014) is often the work that is cited when demonstrating the general *lack of an association* between police in the aggregate and perceptions of the certainty of punishment. Kleck and colleagues examined the association between county-level clearance rates[2] and perceptions of the arrest rate (2005), as well as county-level police force size and perceptions of the arrest rate (2013; 2014). In the first study, the clearance rate for murders had a positive relationship with perceptions of arrests for homicide, while clearance rates for robbery, aggravated assault, and burglary were negatively associated with perceived arrest rates for these crimes. The relationships were small in

[2] The clearance rate for a given time period is created by taking the number of crimes cleared over the total crimes. Crimes can be cleared by arrests (when one or more persons is arrested, charged and referred to court) or by 'exceptional means' (when arrest is not considered possible but one of the following has occurred; (1) the investigation definitely established the identity of the offender, (2) there is enough information to support an arrest, charge, and turning over to the court for prosecution, (3) the exact location of the offender is known so that the subject could be taken into custody now, and (4) there some reason outside law enforcement control that precludes arresting, charging, and prosecuting the offender) (Summary Reporting System (SRS) User Manual, 2013).

magnitude and none reached statistical significance, offering no support for a relationship between clearance rates and perceptions of the arrest rate. When examining the impact of police force size on the same outcomes, Kleck and Barnes (2013) again found little evidence of an association between police and aggregate perceptions of the arrest rate (i.e., the average perceived arrest rate for all respondents within each county). Similarly, they found that police strength and actual arrest rates were not significantly related to perceptions of the arrest rate for any crime when examined at the individual-level (Kleck & Barnes, 2014). Importantly, within this research, the authors found no differences in the association between police and perceived arrest risk when disaggregating by those who had and had not previously been arrested (Kleck & Barnes, 2014). However, given the fact that this sample was collected via a random phone survey, it is unlikely that many of these arrests were for serious crimes or that many of the respondents were still engaged in offending behavior.

In general, the results of these calibration studies offer weak to null evidence that there is an association between macro-level police factors like arrest rates or police force size and perceptions of the arrest rate. These studies suggest that the average adult citizen is not very aware of the certainty of punishment in their county or community, nor are high school students or juvenile delinquents. However, it not required that perceptions of the certainty of punishment be perfectly correlated with objective risks, an idea that has now been referred to as the "strong form Nagin conjecture" (Pogarsky & Loughran, 2016). It is now generally accepted that there is not a perfect correlation between perceptions and reality (Nagin, 1998), but this observation does not rule out the potential for a deterrent effect (Pogarsky & Loughran, 2016).

One recent experimental study has demonstrated that there is a significant correlation between objective risk and perceptions of that risk. Barnum, Nagin and Pogarsky (2021) used experimental video vignettes to manipulate different risk factors for detection by police such as the speed limit, the speed of fellow drivers and police presence. Comparing estimates between persons of one's likelihood of getting caught by the police for speeding revealed patterns in line with calibration. Those who were given speeds which were more miles per hour over the speed limit reported higher estimates of the risk of getting caught compared to those who were only travelling a few miles over the speed limit. Within-person analyses also revealed that respondents' perceptions of getting caught were coherent; respondents on average rated scenarios in which they were passing traffic as the most likely to be caught while in scenarios where they were being passed they reported the lowest likelihood of being caught (Barnum, Nagin, & Pogarsky, 2021). This is one of the first studies to show a strong and significant positive relationship between objective risk of apprehension (i.e., getting caught for speeding) and perceptions of apprehension risk. It is worth noting that this study was experimental while the null effects from existing research have been conducted in the world without experimental manipulations.

There are several reasons why the relationships may be weak or null in the majority of these studies. One reason may be that the calibration studies on the relationship between punishment threats have examined clearance/arrest rates or police force size at the county level (Kleck & Barnes, 2005; Kleck et al., 2013, 2014; Lochner, 2007). Scholars have acknowledged that counties are not appropriate measures of risk at the locations where offenders are engaged in crime (Nagin 2013, p. 248). As previously discussed, "threat communication" is required in order for potential offenders to be aware of the risk of punishment (Apel, 2013; Pickett, Loughran, & Bushway, 2016) and this unit of analysis may simply be too large.

Moreover, the null findings may be due to the measures of the threat of punishment. Clearance rates and arrest rates have been heavily critiqued as a flawed measure of police effectiveness or objective apprehension risk (Braga & Apel, 2016; Cook, 1979, 1980; Nagin et al., 2015). Nagin and colleagues (2015) have extensively documented the issues with this measure, as have others (see Braga & Apel, 2016; Cook, 1979). Most importantly, the clearance or arrest rate necessarily does not include crimes that *have been deterred* as it only encompasses the rate of arrest for those crimes which have been commissioned. The clearance rate or arrest rate can not possibly include those deterred either by prior punishment experiences or by the threat of punishment. In addition, the denominator of the clearance rate (i.e., crime committed) does not capture offenses unknown to the police and may be downwardly biased. To put it simply, studies of this kind suffer from measurement error and therefore model misspecification resulting in inconsistent estimates of the true relationship between clearance rates or arrest rates and crime (Braga & Apel, 2016).

Additionally, these studies on the effect of punishment threats have generally examined samples of law-abiding citizens (Kleck & Barnes, 2013, 2014; Kleck et al. 2005). Reliance on this sample type has been noted as a major limitation (Nagin, Solow & Lum, 2015) and has been referred to as the *sampling criticism* in ongoing debates regarding the efficacy of existing literature on objective and subjective risks (Braga & Apel, 2016). Braga & Apel (2016) have noted that, even in large, nationally representative samples, the number of individuals who have engaged in serious crime (i.e., felonies) would likely fall in the single digits. Because of this, in recent years, some have suggested that the ideal sample then is “noninstitutionalized persons living in high-crime urban areas” (Kleck & Sever, 2018: p. 30). But it may be even more meaningful to examine samples of active or past offenders because this is the sample for which we want to understand how to increase perceptions of the certainty of punishment. For those not 'in the market' for offending, there is little reason to expect a strong correlation between objective levels and perceptions of those levels in the first place. There is even experimental evidence to suggest that this is the case (Block & Greedy, 1995). When comparing college students to incarcerated offenders, Block and Gerety found that offenders were more responsive to the certainty of punishment than the non-offender group.

Finally, and perhaps most importantly, the outcome in these studies has been measures of the arrest rate for an area rather than measures of one's personal apprehension risk. This important distinction has been made for over 40 years, yet the majority of the existing research has not examined personal risks of arrest; "To be consistent with the utilitarian perspective, measures of perceived certainty of punishment must be from the point of view of the respondent, but many researchers have measured the variable by asking respondents to estimate the certainty of arrest for "people in general" or for "people like yourself" (Silberman, 1976; Teevan, 1976a). The measure most appropriate for the utilitarian perspective, however, is *respondents' estimates of their own chances of being arrested*, and, thus, of their own potential costs" (Grasmick & Green, 1981: p. 3, emphasis added). Failure to differentiate between the "perceived personal risk" and the risk for "generalized others" (Saltzman, Paternoster, Waldo, & Chiricos, 1982) may be the most important limitation of the existing research.

A handful of studies have explicitly tested the association between police and individuals' perceptions of their own likelihood of arrest (i.e., how likely is it that you would be caught for breaking into a car?). These studies do not seek to test if objective punishment rates and subjective perceptions of these rates align, as in the calibration studies reviewed above, but instead focus on the perceptual deterrent effect of various measurements of punishments and punishment threats on individuals' perceptions of their own likelihood of being arrested.

Two of the studies previously reviewed (Claster, 1967; Scheider, 2001) examined perceptions of one's own likelihood of arrest in addition to perceptions of the arrest rate. Though delinquents in Claster's (1967) study were more likely to overestimate the arrest rate, they also perceived lower likelihoods of getting arrested personally for each crime compared to the nondelinquents. Claster speculated this was due to a sort of 'magical immunity' where the delinquent respondents believed they would be better than the average person at escaping arrest. This complements Tittle's work discussed previously. Scheider (2001) also found that the arrest rate was less likely to predict one's perceptions of their own arrest risk than it was to predict the arrest rate for the average person. These two studies demonstrate the importance of distinguishing between studies which measure perceptions of the arrest rate, versus perceptions of one's own likelihood of arrest, as the resultant conclusions are often inconsistent. For example, others have found that county-level arrest rates are positively related to an individuals' perception of their own likelihood of arrest for motor vehicle thefts (Lochner, 2007), a finding counter to research on the relationship between county-level clearance rates and perceptions of the arrest rate (Kleck et al., 2005). Nonetheless, this effect was largely diminished once individual characteristics (e.g., race, age, income) and population density were controlled.

Only one study has examined how a change in police policy impacts perceptions of arrest risk.[3] Terpstra and colleagues (2019) examined the effects of a surge in traffic checkpoints on

[3] There are a host of studies which have examined the impact of changes in DUI sanction severity on perceptions of arrest risk and punishment severity ((Ross & Voas, 1990). Because these studies did not examine an increase in police stops or police presence, they are not reviewed here but it is worth noting that they have generally demonstrated an increase in perceived arrest risk. Related work has also has found that changes in sentencing severity once an individual reaches adulthood are also perceived by respondents (Hjalmarsson, 2009).

perceptions of citation risk in the Netherlands using surveys of motorists before and after the intervention. This study found that perceptions of apprehension risk for nearly all offenses decreased in both locations, but these perceptions decreased significantly less in the location which experienced more frequent traffic checkpoints, suggesting a deterrent effect of increased police stops (Terpstra, van Velthoven, & van Wijck, 2019).

The work by Barnum and colleagues reviewed above (2021) and Tersptra and colleagues (2019) are the best examples of examining the relationship between variation in policing and perceptions of individual's personal apprehension risk. However, both of these studies focused on traffic offenses rather than more serious crimes. Furthermore, and unfortunately, the latter study by Terpstra and colleagues (2019) is the only study to examine the effect of *changes* in punishment threats on perceptions of one's personal apprehension risk. Deterrence scholars have long articulated that objective levels of the certainty of punishment may not be as important as changes to these levels over time (Ariely, Loewenstein, & Prelec, 2003; Chiricos & Waldo, 1970). This idea is sometimes referred to as the "weak-form Nagin conjecture" (Pogarsky & Loughran, 2016), meaning that, as opposed to the "strong-form", objective rates and subjective rates do not need to be perfectly aligned but they must be positively associated.

There is recent evidence to suggest that it is not the level of risk reported by potential offenders that is important, but rather relative risk. Scholars have now noted that risk perceptions are 'coherently arbitrary' (Thomas, Hamilton, & Loughran, 2018), and that simply assessing positive or negative changes within person is more meaningful than between person analyses (Pogarsky & Loughran, 2016; Thomas et al., 2018). No single study has examined the impact of change in police policy on the same individuals over time.

Overall, the existing research on the link between police and perceptions of apprehension risk is complex in many ways and offers somewhat divergent findings across studies. In one regard, it may be that perceptions of one's apprehension risk are impacted by direct experiences through arrest (Anwar & Loughran, 2011; Lochner, 2007; Matsueda et al., 2006), but are only weakly impacted by police presence or the arrest rate in the area (Lochner, 2007; Terpstra et al., 2019). Or it may be that police presence and the arrest rate in an area is not a deterrent as individuals are so unaware of the certainty of punishment meaning that it cannot possibly have a deterrent effect (Kleck & Barnes, 2013, 2014; Kleck et al., 2005). The studies which examined direct punishment experiences, like the effects of arrest, are often more sophisticated and as such, this relationship has been well established. However, regarding the studies that do exist on the threat of punishment, it is difficult to reach a conclusion due to several limitations in the existing studies and overall dearth of research on this relationship. The existing research on punishment threats is typically cross-sectional in nature, examines general population samples, relies on suboptimal units of analysis, and asks respondents to report on their perceptions of the aggregate arrest rate rather than perceptions of their own likelihood of arrest given the commission of a crime. In regard to policy, this means that existing studies have not examined the outcome of interest for the population of interest.

The Second Link –Perceptions on Offending

The relationship between perceptions of arrest risk and offending is more established than the first link. More studies have tested this association and found convincing evidence that perceptions of arrest risk are in fact related to offending behavior in the way that is predicted by

Deterrence and Rational Choice Theories. The next section reviews these studies and discusses their strengths and limitations.

The oldest set of studies on this topic examined the relationship between perceptions of arrest risk and offending by showing negative correlations between past offending behavior and current perceptions of arrest risk. For example, Waldo & Chiricos (1972) found in a sample of undergraduates that those who perceived the lowest arrest rates for theft and marijuana use were more likely to engage in these behaviors. However, these relationships were stronger when examining the perceived likelihood of arrest “for someone like yourself” rather than the average arrest rate overall. Of those who thought that for someone like them, arrest for marijuana use was likely, 0% engaged in using marijuana, but of those who thought it was unlikely, 39% had used marijuana. Similarly, only 41% of those who thought arrest for petty theft was likely had stolen something compared to 62% of those who thought arrest for this crime was unlikely (Waldo & Chiricos, 1972). These results were deemed suggestive of a deterrent effect. Importantly they demonstrate the stronger effect for perceptions of arrest for one’s self or someone like one’s self, rather than general arrest rates.

Jensen, Erickson, & Gibbs (1978) found an inverse relationship between perceptions of one’s personal arrest risk and self-reported offending in a sample of high school students. Notably, this study also examined perceptions of the aggregate risk (i.e., “Out of every 10 kids who commit an offense, how many get caught?”) and found that perceptions of the aggregate risk have a weaker relationship with offending than perceptions of one’s personal risk. This finding again aligns with prior critiques raised regarding the (preferred) use of personal arrest risk over perceptions of the arrest rate when studying deterrence. Others have found that this relationship holds for some subgroups but not others. Grasmick and Milligan (1976) found an inverse relationship between perceptions of arrest and self-reported offending for older adults (over 25), but amongst adults 18-25, this relationship was not found. The authors suggest that younger individuals are less responsive to deterrent messages, suggesting an interaction between perceptions of arrest risk and age (Grasmick & Milligan, 1976).

The results of these studies (and others not included in this review) on average lend support for a significant inverse relationship between perceptions of arrest risk and offending. But these studies have important limitations, the most problematic being the lack of an examination of these relationships over time. By using cross-sectional data, researchers were only able to examine associations between past offending and current perceptions of arrest risk.

The first longitudinal study on the relationship between perceptions of arrest risk and offending was not conducted until 1982 (Saltzman et al., 1982). The researchers set out to address the main limitation of existing research – that perceptions were compared to *past* offending behavior. These researchers deemed the prior research as tests of the 'experiential effect,' meaning the effect of prior offending behaviors on perceptions. This research, though described here, is more in line with the research discussed above on punishment avoidance, however, in the literature it is referred to as the earliest work on the perceptions and offending link and as such, is reviewed in this section. Though the authors critiqued the existing 'deterrent' research, Saltzman and colleagues (1982) found results consistent with a true deterrent effect in their study. The results showed that the deterrent effect does exist, though it is weak, but importantly, this relationship is distinct from the experiential effect (Saltzman et al., 1982).

After the criticisms raised by Saltzman and colleagues (1982), the research on the link between perceptions of arrest risk and offending has become more sophisticated and has been able to address the concerns over the appropriate causal ordering, and has begun to include more controls for potential confounders (Nagin, 1998). Some of the first attempts at addressing this issue used current perceptions on intentions to offend in the future. Studies by Bachman, Paternoster, & Ward (1992), Grasmick & Bursik (1990), and Paternoster & Simpson (1996) have all found a negative association between perceptions of arrest risk and hypothetical future offending (i.e., intentions to break the law). These associations have been found in samples of college student regarding sexual assault (Bachman et al., 1992), for tax fraud, petty theft and drunk driving in samples of adults (Grasmick & Bursik, 1990), and for white collar crime in samples of graduate students and corporate executives (Paternoster & Simpson, 1996).

Recently, scholars have conducted true longitudinal studies on the relationship between perceptions of arrest risk and offending. For example, examining adolescents from the Pathways study over time showed that a one standard deviation increase in perceptions of arrest risk is related to reductions in the number of different crimes one commits, and reduces acquisitive crimes (i.e., theft and robbery), drug crimes and violent crimes by 21%, 14% and 15%, respectively (Loughran et al., 2016). Also using the Pathways study, Thomas & Vogel (2019) found that increasing perceptions of arrest risk over time were related to reductions in offending, however the effect was small. Perceptions of arrest risk explained about 4% of the reductions in offending, and resulted in declines in offending of only 5%.

Recently, the importance of rewards to crime has gained attention in the criminological literature. For example, in the study just discussed, Loughran and colleagues (2016) found that, although perceptions of arrest risk are inversely associated with offending, personal and social rewards played a larger role in explaining offending behavior. This idea is reinforced by the finding that, though perceptions of arrest risk explained a small amount of the decline in offending over time, declining personal and social rewards to crime as respondents age was found to reduce crime by approximately 17% (Thomas & Vogel, 2019). These findings align with other work which has also found the greater relative importance of rewards to crime over risks of apprehension (Matsueda et al., 2006).

These studies have also demonstrated that these factors may interact. In general, higher risks are related to greater monetary rewards (Slovic, Finucane, Peters, & MacGregor, 2004) and common sense says this is also generally true of crime. In addition, when crime becomes more risky, it may also become more intrinsically rewarding as the thrill or rush is one reason offenders give for engaging in crime (Jacobs & Wright, 1999). Because of these factors, it is important to examine rewards when testing the relationship between risk and offending, but it is also important to explore how risk may impact rewards directly and subsequently impacts offending.

In summary, there is evidence suggesting that perceptions of arrest risk are related to later offending behavior. Once longitudinal studies were employed, confidence in this relationship was increased. Most recently, studies have included rewards and other covariates and still found support for a deterrent effect of perceptions on later offending. The general conclusion then is that there is likely a significant inverse relationship between perceptions of arrest risk and future offending, but it may vary across populations (e.g., adolescents versus adults; new offenders versus frequent offenders) and is usually stronger when measuring one’s perception of their personal risk of arrest rather than their perception of the average arrest risk for a group or locale.

Procedural Justice, Police Legitimacy and Legal Cynicism

The majority of work which has examined Procedural Justice, Police Legitimacy and Legal Cynicism has examined these factors in concert as they are all related as causes and consequences of each other. For example, research has found that procedurally just treatment by police and other criminal justice actors is related to perceptions of legitimacy. The most well-known study of this relationship explored these hypotheses using mail and phone surveys of New York residents and found evidence of the association between treatment by police and legitimacy (Sunshine & Tyler, 2003). This study also found that legitimacy was predictive of compliance with the law and cooperation with police. As such, legitimacy is often viewed as the mechanism explaining the association from perceptions of experiences with police and subsequent compliance with the law (Sunshine & Tyler, 2003).

Recently, scholars have found that the relationship between procedural justice and legitimacy is invariant, meaning it operates similarly across different individuals and in different scenarios (Wolfe, Nix, Kaminski, & Rojek, 2016). However, on average, African-Americans and Hispanics report lower police legitimacy than Whites (Tyler & Jackson, 2014).

The related concept of legal cynicism (a negative attribute) has similarly been found to be related to increased offending (Kirk & Papachristos, 2011). Kirk & Papachristos (2011) examined the association between levels of legal cynicism and homicide in Chicago neighborhoods. The relationship between concentrated poverty and homicide was partially mediated by legal cynicism, and this effect was strongest in communities with higher rates of adolescents as this subgroup had the highest rates of cynicism toward the law.

The existing evidence is generally supportive of the associations between procedural justice, police legitimacy and compliance with the law but these relationships may not be causal; it is not clear from the existing research that changing police practices and increasing procedurally just actions will actually translate into changes in perceptions of procedural justice, legitimacy and subsequent compliance with the law (Nagin & Telep, 2017). This is an important and large gap in the research on the effects of police on procedural justice perceptions and subsequent offending behavior.

In addition to the theoretical studies discussed above, a host of studies have examined these outcomes when evaluating specific police strategies. For example, two experimental studies have directly examined the effects of hot spots policing on perceptions of procedural justice, both for adults over 18. Kochel and Weisburd (2017) found that in hot spots targeted with directed patrol (i.e., an increase in police presence via some combination of vehicle patrols and foot patrol amongst other activities), residents from hot spots reported smaller increases in perceptions of procedural justice compared to residents in locations which received standard police practices or targeted problem-solving policing, suggesting some negative impacts of heightened police presence. However, once the intervention ended, perceptions improved to levels consistent with residents in the other sites suggesting a lack of long-term impacts (Kochel & Weisburd, 2017). Others have found hot spots policing to have no effect on these perceptions. For example, Ratcliffe and colleagues (2015) found no effect of foot patrol, offender-focused or problem-solving policing at hot spots, on perceptions of procedural justice for community residents, nor was there any impact on overall satisfaction with police services.

Less is known regarding the impact of hot spots policing on perceptions of procedural justice for juveniles. Weisburd and colleagues (2008) examined the impact of a community-oriented and problem-solving police intervention on juveniles' perceptions of procedural justice (Weisburd, Morris, & Ready, 2008). Perceptions of procedural justice for juveniles who lived in locations which experienced this new policing strategy were not significantly different from the responses of those who experienced no change in policing.

Additional studies have examined related outcomes of police legitimacy, satisfaction with police and willingness to cooperate with police. Some have found that perceptions of police are not impacted by hot spots policing, even for those community members likely to interact with police did not perceive any changes to police tactics or officer demeanor after the intervention began (Braga & Bond, 2009). Similarly, Weisburd and colleagues (2011) examined the effects of a hot spots policing intervention which increased police presence by about 3 hours per week at each targeted hot spot on perceptions of legitimacy amongst residents and business owners. The intervention focused on addressing disorder via warnings but issued citations and arrests when warnings were not sufficient. The results of the telephone survey suggest that the intervention had no effect on perceptions of police legitimacy, which was counter to expectations and concerns that increased office presence and order-maintenance policing would result in deleterious consequences (Weisburd, Hinkle, Famega, & Ready, 2011).

However, others have found that intensive crackdowns on problems such as drugs and disorder are related to increased fear of crime amongst residents (Hinkle & Weisburd, 2008). This particular intervention itself did reduce disorder (Weisburd et al., 2006) and disorder was found to be positively related to fear of crime (Hinkle & Weisburd, 2008), however, these improvements appeared to be offset by the heightened fear that resulted from increased officer presence. Concerns like this have been raised by opponents of hot spots policing. For example, some have argued that hot spots policing may negatively impact collective efficacy amongst residents, and if this occurs, any reductions in crime may be negated by these deleterious impacts (Rosenbaum, 2006).

Nevertheless, when taken together, these existing studies have generally concluded that hot spots policing interventions do not result in negative consequences for community members

(Braga & Bond, 2009; Kochel & Weisburd, 2017; National Academies of Sciences, Engineering, and Medicine, 2018; Weisburd et al., 2011). However, there is not much evidence to suggest that they have any positive effects either and the dearth of research on long-term impacts, as well as jurisdiction-wide impacts is problematic (National Academies of Sciences, Engineering, and Medicine, 2018), as is the lack of research on effects for juveniles or those most likely to experience a change in policing or police contact as a result of the intervention. The above studies have relied on community or school samples, resulting in respondents who are generally law-abiding citizens, or unlikely to have had direct contact with police, especially as a result of the intervention. For example, Kochel & Weisburd (2017) found that less than 36% of survey respondents at treated sites had been stopped by police during the intervention, making the majority of respondents those who are not the intended targets of deterrent messages or who experienced police contact. This limitation is perhaps the most important gap in the existing research on the impact of police on perceptions of procedural justice, police legitimacy and legal cynicism. Given the limitations of the existing research, the current evidence is taken with caution, particular because it is contrary to the theoretical expectations.

Broken Windows

The studies on Broken Windows theory are focused on the relationship between disorder and crime or are evaluations of police interventions which incorporate elements of Broken Windows policing. First I review the studies on the relationship between disorder and crime and then I discuss the findings from evaluations of Broken Windows policing interventions.

The research on the relationship between disorder and crime is divergent. Some have found significant positive associations between disorder and crime (Branas et al., 2016; Keizer, Lindenberg, & Steg, 2008; Skogan, 1990), but others have found no relationship (Harcourt, 2001; Taylor, 2001). Some have found associations when examining serious crimes like robbery and homicide but not for lower-level offenses like burglary and theft (Freedman & Owens, 2011). And still others have concluded that disorder is largely irrelevant and rather it is just the social cohesion amongst residents and informal social control (i.e., collective efficacy) that can explain associations between neighborhood structural factors and crime (Sampson & Raudenbush, 1999).

A recent review of Broken Windows theory concluded that there is not a significant relationship between disorder and offending, or disorder and fear (one of the mechanisms through which broken windows policing is believed to reduce future crime) (O'Brien, Farrell, & Welsh, 2019). Another extremely comprehensive review of the relationship between disorder and crime states that the 'causal link' between disorder and crime has been disproven (National Academies of Sciences, Engineering, and Medicine, 2018). Though this association between disorder and crime may not exist, it is also important to explore the effects of various Broken Windows policing interventions on crime.

Broken Windows policing is a tactic that over 75% of police departments employ in some capacity (Mastrofski & Fridell, 2016). Though it was originally formulated as a way to improve disorder and decrease citizen fear of their communities, it has often manifested in a strategy which focuses primarily on increased arrest and less on improving communities for their residents. [4] The strategies range on a spectrum from serious enforcement to more community

[4] The distinction between Broken Windows policing, as discussed here and sometimes referred to as "Order-Maintenance policing" and Zero-Tolerance policing is important. These distinctions have become increasingly blurred in the policing literature making it difficult to define and evaluate Broken Windows policing. Though these are often used interchangeably, Zero-Tolerance policing refers to policing with high levels of enforcement for all crimes, regardless of how small. Safe Streets was designed in a way akin to Broken Windows policing and designed with the intention of not increasing enforcement, which is the opposite of a Zero-Tolerance design.

policing, with the former (unfortunately) being the more common (National Academies of Sciences, Engineering, and Medicine, 2018).

A commonly cited example of Broken Windows policing coupled with hot spots policing is an intervention which took place in Lowell, Massachusetts. The hot spots policing intervention focused on reducing disorder and was successful at reducing both physical and social disorder measured via calls for services regarding disorder and systematic social observations of disorder (Braga & Bond, 2008). Other variants of Broken Windows policing have targeted specific problems of disorder, such as homelessness, by arresting individuals, increasing police presence and issuing citations (Berk & MacDonald, 2010). This intervention was found to decrease crime in Los Angeles' Skid Row, without displacing crime to adjacent communities.

Because Broken Windows policing is believed to reduce crime by impacting disorder and subsequent levels of informal social control and fear of crime, it is important to examine these potential mechanisms. Using interviews with 'key community members', meaning those who would likely have contact with the police and who spend a great deal of time in the community, Braga & Bond (2009) found that the same intervention which reduced disorder was found to reduce residents perceptions of disorder and increase perceptions of police presence, but had no impact on fear of crime or perceptions of police behavior (Braga & Bond, 2009). The multifaceted foot patrol, offender-focused and problem-solving policing at hot spots discussed previously also found no effect on perceived physical or social disorder, as was expected (Ratcliffe et al., 2011).

Weisburd and colleagues (2011) similarly examined the impact of a hot spots policing intervention which employed aspects of Broken Windows policing on fear of crime, perceptions of crime and disorder and collective efficacy. It was found that fear of crime, collective efficacy and perceptions of crime and disorder were not significantly impacted, leading the authors to conclude that there were no impacts on any of these outcomes (in addition to police legitimacy discussed above) for residents living in locations targeted by increased officer presence and enforcement (Weisburd et al., 2011). Importantly, the authors reached the conclusion that "ordinary citizens are not very aware of police activities unless they are directly impacted by them through interactions with the police" (p. 314).

Unfortunately, few studies overall have examined how Broken Windows policing impacts residents perceptions of their community (i.e., fear, disorder, collective efficacy, informal social control) which are the mechanisms believed to be behind its success (National Academies of Sciences, Engineering, and Medicine, 2018). At the present, there is insufficient evidence to make a conclusion about the effect of Broken Windows policing on these outcomes (National Academies of Sciences, Engineering, and Medicine, 2018).

Overall, there is evidence that certain forms of Broken Windows policing may reduce some forms of crime, but two recent meta-reviews reach the same conclusion; the form of Broken Windows policing that focuses on increasing arrests for low-level offenses is not effective at reducing the more serious crime it aims to reduce (Braga, Welsh, & Schnell, 2015; National Academies of Sciences, Engineering, and Medicine, 2018). Unfortunately, the variation across implementations of this style of policing makes it difficult to assess what (if any) elements may be successful at reducing crime.

It appears that neighborhood disorder and 'broken windows' are not great predictors of crime rates, however, implementing certain types of Broken Windows policing may be effective at decreasing crime and improving other factors in one's community. Though these conclusions may appear to refute each other, both of these findings can coexist. One way to better understand the discrepancy in addition to the areas of overlap is to explicitly assess disorder and residents' perceptions of their communities as an outcome and a mechanism when examining the effects of police on crime. Unfortunately, these mechanisms have gone largely unexplored and when they have been examined, they have been tests on a sample of generally law-abiding adult citizens rather than on the populations most impacted by changes in policing or most likely to engage in crime.

Evidence of the Crime-Reducing Effects of Police

The studies reviewed above focus on tests of specific theoretical mechanisms. In addition to these studies, there is an additional literature that focuses on the role of police on crime but is generally not concerned with testing these mechanisms. This research almost always examines official crime or calls for service and does not examine self-reported offending as an outcome. Though there are many policing strategies designed to reduce crime, some of which are more effective than others (see the National Academies of Sciences, 2018 review for the most recent and comprehensive review of policing strategies), this dissertation is concerned with the crime-reducing effects of police by focusing on examining the effects of a policing intervention defined as both a hot spots policing intervention and a drug crackdown. As such, I first review the evidence on macro-level police factors like police force size and arrest rates, and explore the impacts of these two policing strategies.

The majority of studies have examined the deterrent effect of police by looking at the relationship between police force size and crime rates. As of 2016, there were approximately 62 studies on the relationship between police force size and crime (Lee et al., 2016). The majority of these studies did not find a deterrent effect of police. For example, Greenberg, Kessler, & Loftin

(1983) examined the effects of police employment on violent and property crime in cities and suburbs. They found no effect of police on crime, nor did Zedlewski (1983) when examining the relationship between police force size and official police data from the Uniform Crime Report. From this meta-review, Lee and colleagues (2016) conclude that there is a small negative nonsignificant effect of police size on crime.

Unfortunately, the majority of research that has tested the effects of increased police presence largely cannot disentangle the effect of police from preexisting trends toward increased crime (as the latter often is the reason for the increase in police hiring) due in part to the focus on official crime data as the outcome. The “simultaneous two-way relationship” between police presence and crime has made research in this area limited or weak at best (Kleck & Barnes, 2014).

One of the most famous studies which seeks to overcome this limitation is Levitt’s 1997 study which used election years as an instrument to examine the relationship between police force size and crime rates (Levitt, 1997). This study found that police force size is negatively related with crime rates, but more recent critiques of this work have raised questions surrounding the assumptions of the design, calling into question the findings. A similar approach which has garnered less criticism has been employed in three studies which utilize federal police hiring grants (COPS grants) to measure the relationship between police force size and crime (Evans & Owens, 2007; Worrall & Kovandzic, 2007, 2010). Two of these studies have also yielded significant reductions in crime as a result of more police (Evans & Owens, 2007; Worrall & Kovandzic, 2010) but one did not find a deterrent effect (Worrall & Kovandzic, 2007). Others have exploited shocks which have resulted in variation in police presence. Following a terrorist attack, police in Buenos Aires, Argentine implemented police protection in Jewish institutions across the city. This newfound police presence resulted in a large reduction in the number of motor vehicle thefts in the area, suggesting a deterrent effect of police (Di Tella & Schargrodsky, 2004). From this research, it appears that even when issues of endogeneity have been addressed, the results of different studies yield different conclusions making it difficult to draw a conclusion regarding the deterrence effect of police force size or police presence.

Alternatively, one could examine individual-level data on arrests to examine if police impact individual offending behavior. Only one study on police force size and crime has done so. Tauchen and colleagues (1994) utilize data from the Philadelphia Cohort data which followed males born in 1945 for 7-8 years when they were ages 19 through 27 (see Wolfgang, Figlio, & Sellin, 1972). This study examined the effect of police expenditures per crime on official crime records for these males, and found that police expenditures are related to a decrease in arrests, suggesting a general deterrence effect (Tauchen, Witte, & Griesinger, 1994). While this study improved upon the remaining literature on this topic, it still only captured offenses known to police, rather than self-reported offenses. As such, there is still a direct association between the independent variable and measurement error in the dependent variable.

Generally, the divergence in findings across studies has made it difficult to draw conclusions about the effects of police on crime (Telep & Weisburd, 2012). Some have argued that this relationship is null to weak and not worthy of further exploration (Lee et al., 2016) but others strongly believe that police can have a deterrent effect (Durlauf & Nagin, 2011). Examining the evidence of specific police strategies does help to lend further support for the hypothesis that police can in fact deter crime.

In addition to the research on police force size, deterrence studies have examined the impact of clearance rates on crime. Clearance rates are generally measured as the number of crimes cleared by arrest over the number of crimes. There is less research in this area than the research on police force size but again, there is mixed support for the deterrent effect. For example, Chilton (1982), Greenberg & Kessler (1982) and Greenberg, Kessler, & Logan (1979), find no relationship when examining the relationship between clearance rates and crime in cities during the late 1960s into the 1970s.

Yet some support for the effects of clearance rates on crime has been found when differentiating by crime type. For example, when analyzing robbery, burglary and larceny, significant and substantial effects were only found for robberies (Decker & Kohfeld, 1990). Furthermore, some research has explored whether clearance rates must reach a certain threshold before resulting in deterrent effects. For example, Tittle & Rowe (1974) found that when clearance rates surpassed 30%, a marginal general deterrent effect was present. A replication and extension of this study which examined clearance rates and crime in cities and counties within two large states again found support for this threshold effect, but concluded it is largest in small cities (Brown, 1978). This finding has also been supported by more recent research which also suggested that the deterrent effect is stronger and more prevalent in small cities (Chamlin, 1991).

These studies all share the same problem of having a direct relationship between the independent and dependent variable. In fact, studies of clearance rates on crime have been cited as problematic given that the numerator of the dependent variable (the number of crimes) is the denominator in the independent variable (the clearance rate) (Chiricos & Waldo, 1970). Additionally, crime rates and clearance rates may be related given that decreases in crime may result in higher clearance rates as officers have more time to dedicate to each case (Brown 1978).

Both the research which has examined police force size and clearance/arrest rates as predictors of crime suffers from several limitations. Similar to studies which examined perceptions of arrest risk, most studies on police force size or clearance rates and crime also use the county as the unit of analysis. Such large units of analysis are less than ideal as offenders are unlikely to detect variation in police presence or arrest risk at such large a unit. The concerns over the arrest rate and clearance rates as measures of punishment threats have previously been discussed but these concerns are present in these studies as well.

Furthermore, these studies have examined official crime rates as the outcomes of interest. The lack of research on self-reported offending for studies which examine macro-level police factors makes it difficult to assess the true deterrent effect of police. The focus on official data means there is certainly endogeneity between the predictor and outcome. Without individual-level self-reported offending data, it is near impossible to assess if potential offenders commit crimes in jurisdictions with lower levels of police presence or if individuals commit less crimes (i.e., restrictive deterrence), but nevertheless continue offending (Jacques & Allen, 2014). This reliance on official data also means that the dependent variable in these studies does not include crimes unknown to police. This limitation is problematic for most criminology studies; however, it is especially detrimental when the number of police in general likely directly impacts the probability of crime detection and recording by police.

These studies of macro-level police factors including police force size and clearance rates or arrest rates have provided little evidence that police can deter crime. A focus on micro-level policies, such as hot spots policing, does offer support that police can deter crime. The relationship between hot spots policing and crime is well established in the literature; hot spots policing is now known as one of the most effective crime reduction strategies (Braga et al., 2014; Skogan & Frydl, 2004). The evidence for this strategy's effectiveness is strong as it is based on randomized-controlled trials and quasi-experimental designs. Meta-reviews and meta-analyses have demonstrated the robustness of these findings across different study types and study sites (see Braga, Turchan, Papachristos, & Hureau, 2019 for more information). For example, in a hot spots policing evaluation in Lowell, Massachusetts, Braga and Bond (2008) revealed a statistically significant reduction in robbery calls for service (41.8 percent) and non-domestic assaults (34.2 percent) relative to the control locations. This study also was specifically designed to examine spatial spillover effects and concluded that there was no evidence of crime displacement, and that sustained decreases in violence may have persisted for up to 6 months (Braga & Bond, 2008).

Additional studies have found reductions in crime incidents as well. For example, a hot spots policing intervention in Jacksonville, Florida reduced the number of street violence incidents over 3 months by 33%. There was, again, no evidence for the displacement of crime measured via crime incidents (Taylor, Koper, & Woods, 2011). Not only does the existing research suggest that hot spots policing leads to a reduction in official crime without displacement of crime to adjacent communities, but the evidence additionally proposes that there may even be a diffusion of crime-reducing benefits to neighboring areas (National Academies of Sciences, Engineering, and Medicine, 2018).

Though these hot spots policing evaluations are typically highly rigorous designs, there are still limitations. The lack of research on self-reported offending for hot spots policing studies makes it even more difficult to assess the true deterrent effect of police. Similar to the macro-level studies of police, there is likely endogeneity between crime and hot spots policing and the outcomes in these studies does not include crimes unknown to police. Most recent studies have taken approaches to handle the possibility that crime is displaced to adjacent areas and assess if this is the case, but these studies are still constrained by the assumptions they make regarding offender behavior. Without individual-level self-reported offending data, it is near impossible to assess if potential offenders commit crimes in adjacent jurisdictions without hot spots policing or adapt to the increase in police presence in other ways. This limitation is especially detrimental for hot spots policing evaluations as the presence of hot spots policing likely directly impacts the probability of crime detection and recording by police. One reason may be that citizens perceive the increased police attention and become more proactive about reporting crime. Suggestive evidence of this has been found in at least one study where large significant reductions in crime were found, but only weak and non-significant reductions in calls for service (Taylor et al., 2011). Finally, and most importantly, these studies have generally not examined the mechanisms for why they are or are not effective, nor have they explored the potential collateral consequences for those most likely to be impacted by changes in policing.

Overall, there is again ambiguous results from the existing research on the deterrent effect of police. Hots spots policing is believed to be an effective strategy at reducing crime but the research on police force size or arrest rates has shown little significant association with crime. For all areas of research, the existing studies are limited because they have not examined individual-level outcomes making it unclear if punishments and threats of punishment by police are actually deterring offending at the individual-level. This limitation has also made it practically impossible to assess the mechanisms which may explain changes in crime and to examine other potential collateral consequences.

Overall Summary of Prior Literature

Above I have reviewed the theories and prior literature on Deterrence, Rational Choice, Procedural Justice, Police Legitimacy, Legal Cynicism and Broken Windows, as well as the existing evidence regarding the effects of police on crime. I have discussed the limitations of the existing research in great detail and the gaps that still remain. These gaps can be more succinctly summarized as follows:

1) There are a lack of studies on the perceptual deterrent effect of the threat of punishment by police on perceptions of personal apprehension risk for individuals on the margins for offending. No single study has examined the impact of a police crackdown or hot spots policing intervention on these perceptions within-person over time.
2) Few if any studies on the deterrent effect of police, including hot spots policing evaluations, have examined individual-level self-reported offending over time.
3) Relatively few studies on the effects of police have examined perceptions of procedural justice, police legitimacy, legal cynicism or neighborhood disorder and very few if any have examined these outcomes for individuals who are the most likely to have experienced police contact or increased police presence as a result of a police intervention.

These gaps make it hard to assess the deterrent effect of police, particularly the role of punishment threats or the impact of hot spots policing on perceptions of apprehension risk, offending, and other mechanisms or collateral consequences of policing. For hot spots policing specifically, this means that there is no comprehensive assessment of the explanation for why hot spots policing reduces crime, and no single study which has examined these important perceptual and behavioral outcomes in concert. As previously stated, the current study seeks to address these limitations by examining the impact of a hot spots policing intervention known as

Operation Safe Streets on these individual outcomes by examining individual level data on previously adjudicated adolescents followed over time from the Pathways to Desistance Study.

Figures

Figure 2.1 Deterrence Theory – Path Diagram

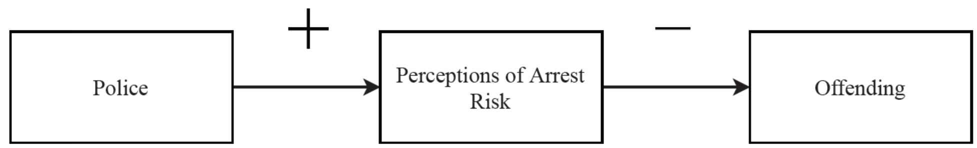


Figure 2.2 Rational Choice Theory – Path Diagram

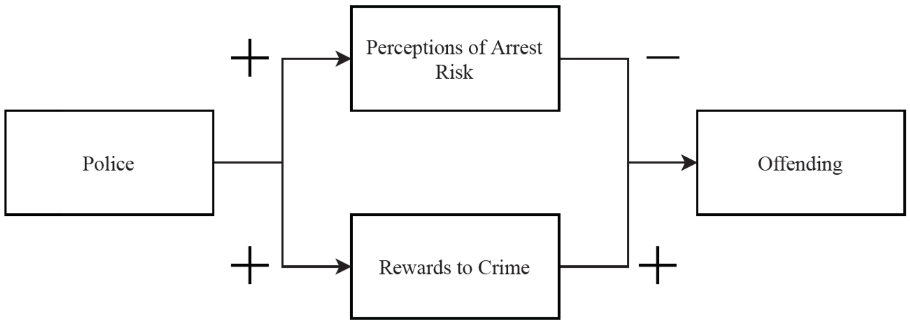


Figure 2.3 Procedural Justice – Path Diagram

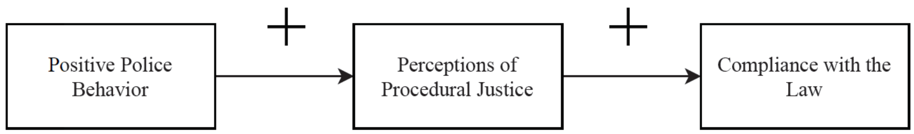


Figure 2.4 Police Legitimacy – Path Diagram

Figure 2.5 Legal Cynicism – Path Diagram

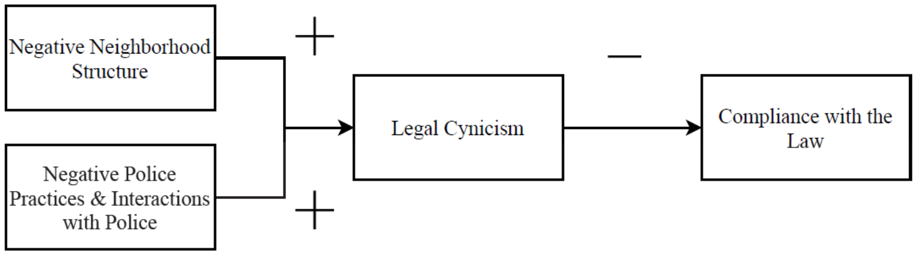
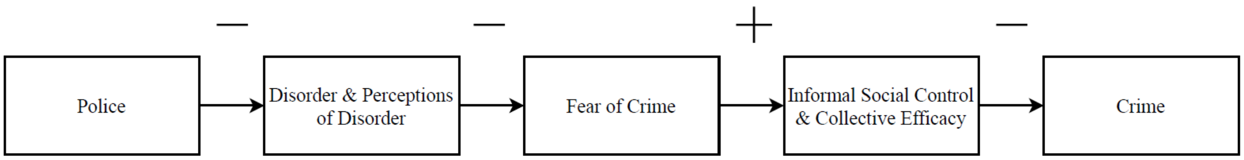


Figure 2.6 Broken Windows Theory – Path Diagram



Chapter 3. DATA & STUDY DESIGN

Pathways to Desistance

The focal data for this dissertation come from the Pathways to Desistance study, henceforth referred to as the Pathways study. The Pathways study is a longitudinal survey of adolescent offenders aged 14 to 16 at the time of their adjudication in Philadelphia and Phoenix counties. Baseline interviews with recently adjudicated adolescents (i.e., those formally processed in the criminal justice system) were conducted from November 2000 to January 2003. Follow-up interviews began in May 2001 and were conducted in 6-month intervals for the first three years and annually for years 4-7. This resulted in 11 total survey waves. Most respondents were found guilty of felonies, though some respondents were charged with misdemeanor property crimes, sexual assaults and weapons offenses. The total sample consists of 1,354 adolescents.

The current dissertation utilizes the data from respondents from Philadelphia county only (n=700).[5] The analytic sample is limited to interviews conducted from baseline through December 31, 2005. Additionally, survey data from periods when respondents spent less than 10% of their days in the community since the last interview were dropped from the analyses to ensure exposure to the intervention.[6] Interviews conducted in wave 8 and beyond were also dropped as these interviews were conducted at 1-year intervals rather than 6-month intervals. The modal number of interviews per respondent is seven. In addition to survey data, this dissertation uses information on official criminal history, collected alongside survey data in the

[5] Philadelphia County and the City of Philadelphia are coterminous, therefore, all youth from Philadelphia in the survey are within the jurisdiction of the Philadelphia Police Department at baseline.
[6] Though this restriction was done for substantive and theoretical reasons, the patterns are consistent when the entire sample is included, however, the effect sizes are generally reduced by approximately 50 percent.

Pathways study via FBI records and juvenile and adult court records for each respondents' home jurisdiction (Schubert et al., 2004).

Notably, some of the data is collected via Life-Event Calendars in order to map important events, such as offending experiences, to the precise months in which they occurred. Life-Event Calendars, also known as Life-History Calendars, are tools used to collect accurate retrospective data on events. Life-Event Calendars have been used and validated for collecting information on antisocial behavior and offending (Horney, Osgood, & Marshall, 1995). The strategy typically uses an interactive calendar to help the respondent visualize dates and identify events in relation to other meaningful dates, such as birthdays or in the case of the Pathways study, in relation to instances of arrest or incarceration. In the current dissertation, I examine aggregate reports of offending as defined by any events since the last interview. I also use the Life-History Calendar data to examine monthly rates of offending and the types of offenses endorsed during each month. Information on all of the items used, how they are measured, and which study they are examined in is available in Table 3.1, and descriptive information on the sample is available in Table 3.2.

At baseline, 86% of the 700 respondents are male and are on average 16 years of age. Most respondents are Black (72%), followed by Hispanic (15%), White (10%) and other races (3%). At baseline, respondents report that they are arrested for approximately 30% of the crimes they commit at an average of about three arrests per respondent. A crime variety score of 30% denotes that on average, respondents committed about 7 to 8 unique crime types (i.e., 30% or 8 of 24 possible crime types) upon entry into the study.

Over time across survey waves, the study sample has a high retention rate. Approximately 93% of the Philadelphia sample was interviewed at follow-up waves 1 and 2, and over 86% was retained for waves 3-7. Waves 8-10, which are not included in these analyses, had consistently high retention rates of 85%, 82% and 79%, respectively. Table 3.2 demonstrates that the sample characteristics remained consistent, with no significant differences in the demographic makeup of the sample over time. However, it is worth noting that this sample became less criminally involved, as is expected from a highly delinquent sample during the peak of the age-crime curve (Hirschi & Gottfredson, 1983). The prevalence who were arrested, and the number of arrests at each period declined starkly from baseline. Criminal variety scores also declined, and the percentage of time spent in the community (i.e., not incarcerated) increased at nearly each follow-up time period.

Operation Safe Streets

On May 1st, 2002, Operation Safe Streets (henceforth Safe Streets) was implemented in Philadelphia, Pennsylvania. Two uniformed officers were stationed at 200-300 hot spots for near round-the-clock presence. Philadelphia Police targeted all of the most problematic drug hotspots, generally street corners or specific addresses. The targeted locations were selected based on crime data, arrest data, firearm seizures and informant information (crimesolutions.gov). Safe Streets was devised to increase officer presence in an effort to decrease drug offending in problematic areas, specifically open-air drug markets. During its tenure, the intervention was always categorized by heightened police presence; Safe Streets was implemented at full force for the first year when additional patrols were mandatory for officers, but after approximately 1-year, Safe Streets shifts became voluntary. After approximately 18 months the intervention decreased substantially and ultimately ended after about 2 years.

As opposed to some more popular hot spots policing interventions, Safe Streets was not categorized by high levels of enforcement. While this is not typical of hot spots policing strategies, any focus of police attention and resources to small geographic units is considered a type of hot spots policing (Braga, Turchan, Papachristos, & Hureau, 2019a). In fact, the evaluation of Operation Safe Streets is one of the key interventions included in systematic reviews of this type of policing intervention.

According to documents received from the Philadelphia Police Department, the intervention was explicitly intended to increase perceived arrest risk, and prevent narcotics sales, through an increase in police visibility. The stated goal was to specifically aim to reduce crime through increased officer presence rather than arrests. This goal were also stated publicly; Police Commissioner Sylvester Johnson dismissed the use of arrest as a feasible solution to the ongoing drug problems (Smith, 2002).

In order to increase its impact and gain citizens cooperation with police, Safe Streets was highly publicized. Press coverage began on the first day, with a press conference by Mayor Street of Philadelphia. The intervention was continually portrayed in the media, typically in popular newspapers including the *Philadelphia Inquirer* and the *Philadelphia Daily News*, as well as other online news outlets, during the first months of the intervention. These articles highlighted the benefits of the intervention to community members (e.g., Moran et al., 2002; Police One, 2002) and the increased visibility of police (e.g., Police One, 2002) and included quotes by residents, addicts, city officials, and public health employees noting the surge in police presence and change in crime in the targeted locations (Gordon, 2003; Smith, 2002). For example, one self-proclaimed active drug user commented on the presence of police, saying "They got two cops on pretty much every drug corner in Kensington…" (Moran et al., 2002).

Mayor Street also hosted several "Operation Safe Streets Rallies" in the initial months of the intervention in the affected communities to further promote the message of increased police presence and renewed attention to the neighborhoods most affected by drug problems (Philadelphia Higher Education Network for Neighborhood Development, 2002).

A formal evaluation of Safe Streets was conducted shortly after the intervention took place. Lawton, Taylor & Luongo (2005) examined reported crime in the first 18 weeks of the intervention compared to 121 weeks of crime data prior to Safe Streets. ARIMA models were employed to examine the effect of the intervention on the city as a whole, as well as comparisons between treated and untreated (albeit lower crime) hot spots and their surrounding areas. City-wide analyses found violent crime, homicide and drug crimes were reduced by approximately 16, 0.5 and 10 crimes per week during the intervention, respectively; however, these effects did not reach statistical significance. At targeted hot spots, violent crimes decreased an average of one crime per week, with a spatial diffusion of benefits resulting in 0.3 less crimes per week in adjacent areas. Drug crimes were reduced by approximately 3 crimes per week. Overall, the evidence from this evaluation suggests that the program was effective at reducing violent and drug crimes, with larger effects when examining the targeted hot spots rather than the city as a whole. As noted by the authors, the results on the analysis of the spatial spillover of crime remain ambiguous, as results varied by model specification; some models suggested the displacement of crime into adjacent areas, and other models suggested a diffusion of benefits (Lawton et al., 2005).

Table 3.3 presents information regarding the timing of interviews in relation to Safe Streets. Time *t* refers to the first treated wave. Given the staggered nature of the Pathways data collection, not all respondents first experienced the treatment at the same wave. In Panel A of

Table 3.3, information is presented based on the number of interviews conducted before Safe Streets began and the number at each wave. For example, 88 respondents were first interviewed after the implementation of Safe Streets, while the initial interview for 612 respondents took place prior. Of the 612, 257 were interviewed once before Safe Streets began and the remaining 206, 143 and 6 persons were interviewed two, three and four times before Safe Streets, respectively. The majority of respondents were also interviewed at multiple time points during Safe Streets; the modal number of interviews during Safe Streets is three. Survey data from interviews conducted after Safe Streets ended are also included in the analysis, with approximately 3 interviews per person in the post-treatment period, defined as 18 months after Safe Streets began.

Table 3.4 presents descriptive statistics on the sample, similar to Table 3.2, but instead examines them in relation to the timing of Operation Safe Streets. Throughout the analyses, time t represents the first period after Safe Streets was implemented; periods t-3 to t-1 include data on interviews conducted prior to Safe Streets; periods t1 through t3 include interviews during the first 18 months of the intervention, and t4 through t6 denote periods after Safe Streets. Prior to the intervention (periods t-3, t-2 and t-1), the sample is slightly more male, and has higher levels of arrest, arrest rates, and offending behavior, as well as more engagement in diverse crime types, compared to the first treatment wave. T-tests reveal significant differences ($p<0.05$) exist between the first treatment wave and previous waves for the percentage of males, percentage of respondents who were arrested, the arrest rate, the number of arrests, percentage who offended, and variety scores. This denotes slightly greater attrition for males in the sample and also highlights general patterns of desistance from crime over time, as has been previously noted in this dataset, both in Table 3.2 and in other studies (Steinberg, Cauffman, & Monahan, 2015).

These patterns persist across waves; following the implementation of Safe Streets, respondents have a lower prevalence of arrest, arrest rates, number of arrests, offending prevalence and criminal variety scores compared to the first treatment wave. Importantly, the lower prevalence of arrests and the significantly lower average number of arrests in the first treatment wave compared to prior periods suggests that Safe Streets, as intended, was not a high-enforcement and arrest-based strategy. Though official arrest data is unavailable from this time period, this simple comparison suggests that Safe Streets, as intended, was not characterized by an increase in arrest for the adolescents in this sample. In addition, Appendix Table A.1 presents first-difference models which predict arrest and the number of arrests for respondents in the sample after Safe Streets began, with basic controls for offending and incarceration experiences. These results suggest a small but *negative* effect of Safe Streets on the prevalence of arrest, and a *negative* and significant decrease in the number of arrests after Safe Streets began. From these analyses, it appears likely that Safe Streets was true to its goal of not increasing enforcement.

Research Design & Identification Strategy

Analytic Strategy

In order to estimate a causal effect, first-difference linear regression models are presented which predict changes in individuals' perceptions over time to isolate the effect of Safe Streets. All within-person time-stable traits are controlled for, leaving the ability to estimate the effect of treatment, as well as the effect of any time-varying factors related to the outcomes of interest, which may include arrest and offending experiences. Because this study is concerned with the effect of Safe Streets for the period when respondents *first* experience this increase in police presence, first-difference models as opposed to fixed-effects models are used. First-difference models report the change on perceptions from periods of no treatment to treatment, rather than the change in perceptions from one's average across all time periods. However, in order to show the consistency across models, parallel fixed-effects regression models are included as sensitivity analyses within each chapter.

First-difference models examine the change in the outcomes from untreated to treated periods. Instead of reliance on Stata's built-in capabilities for panel analyses, change measures are created by subtracting the most recent wave from the current wave on all measures. This makes it possible to preserve more cases than the alternative approach using the time-series operators in Stata which would drop 2 observations for each missing wave. As such, a respondent who completed the survey in waves 1 and 3 is preserved in the current approach (subtracting their wave 1 score from wave 3 score), rather than dropping observations from waves 1 and 3 for this respondent. Results were consistent across modeling strategies, with slightly larger effects when using the built-in time-series operators in Stata, but the main models rely on the approach that preserves more data and thus has more power.

All available observations are retained in each model so sample sizes differ slightly across analyses. However, all models remove observations from periods when respondents were not in the community (i.e., they were incarcerated or detained in a facility) for at least 10% of the days since the prior interview for analyses at the wave-level, and at the month-level, all analyses drop months when respondents were not in the community for the majority of the month. It is important to note that change scores were created prior to dropping these periods in Studies 1 and 3 which examine perceptual outcomes. This was done for two reasons. Studies 1 and 3 are concerned with changes from any prior period, regardless of incarceration status, on one's current perception, so long as they are exposed to treatment for at least some time during the recall period. In other words, changes in perceptions of arrest risk or perceptions of police from an incarcerated period to a non-incarcerated and treated period are preserved. Changes from a non-incarcerated period to an incarcerated period are dropped. Creating change scores for the dependent variables prior to dropping waves when respondents were incarcerated maximizes the number of cases in the study.

In Study 2, this measurement is altered to examine only changes between non-incarcerated periods as the outcomes are behaviors that can only occur if respondents had spent time outside of incarcerated settings. The aim of Study 2 is not to examine the effect of the intervention for those who were incarcerated, but to examine the effect of the intervention on changes in offending behavior from the most proximate period prior to the intervention when respondents were capable of offending to the first treated period when respondents were capable of offending. Further discussion of the sensitivity of different models to these decisions are discussed within each empirical chapter.

In each study, the primary first-difference models begin by examining the effect of Safe Streets net of common trends in perceptions over time (Britt, 1994). These baseline models are presented to demonstrate the effect of the relationship prior to the inclusion of covariates because it is possible and perhaps likely that the covariates of arrest, offending and street time have also been impacted by the treatment. Recent discussions of experimental and quasi-experimental evaluations of criminal justice interventions have highlighted the potential implications of post-treatment conditioning (i.e., controlling for covariates that may be impacted by the treatment itself) (Doleac, Temple, Pritchard, & Roberts, 2020). Because of this, models are first presented without these controls, however, given the theoretical importance of conditioning on these covariates in studies of perceptions of arrest risk particularly, as well as in studies of perceptions of police and rewards to crime, as well as the interest in the effects of this policy net of arrest, models are then presented which include these covariates.[7]

After baseline models are presented, controls for relevant time-varying factors (e.g., arrest or offending) at each time period are added. In some instances, the effects of the intervention are interacted with time-varying factors like arrest or one's arrest rate (rate of arrests to offenses). All models control for age at last birthday to account for aggregate trends in perceptions over time and all standard errors are clustered on the individual. These more nuanced modeling decisions are discussed within the *Analytic Strategy* section of each study.

Identifying Assumptions

The staggered design of the Pathways to Desistance Study helps to rule out the possibility of an endogenous relationship between Safe Streets and perceptions. Because of this, the current study can minimize survey-design effects (i.e., responses changed as a result of repeated surveys over time) because Safe Streets was experienced at different survey waves for respondents. Additionally, the current design helps to rule out the possibility of an unmeasured exogenous shock which could impact perceptions. For example, if a specific highly publicized crime or event occurred during the intervention period and all respondents were interviewed immediately following this event, there may be the concern that the effect of Safe Streets and this event were conflated. Due to the staggered nature of the survey, post-intervention survey waves range from the first to fourth interview, and the timing is such that the first interview for each respondent after Safe Streets ranged from 30-373 days after the implementation.

[7] Arrest and offending are not included as covariates in Study 2.

It is also unlikely that any results would be detecting anticipation effects. Anticipation effects are effects of treatment that begin before treatment actually occurs as respondents begin to change their attitudes or behaviors as a result of the impending treatment or intervention. For example, imagine an experiment designed to assess the pain reducing effects of aspirin. If respondents who were given an aspirin began to feel less pain within 30 seconds of receiving the drug, this would likely be due to anticipation effects because enough time had not passed for the drug to take effect. This would be similar to a 'placebo' effect where respondents who received a placebo noted declines in their pain. Note however that anticipation effects are effects that occur just before actually receiving treatment, rather than effects of treatment despite never receiving any treatment.

Because of these reasons, the research design makes it possible to estimate causal effects of Safe Streets. But additional efforts were also taken to increase confidence in the current research design. A search for alternative crime-reducing interventions or changes in policing during the study period revealed several programs enacted prior to and following the end of Safe Streets. A Business Improvement District was implemented from 1999 to 2002 (See Hoyt, 2005) which was prior to the intervention so it is not likely that this intervention impacted the estimates. Four additional crime prevention strategies were implemented in Philadelphia around this time; a program to inform and educate citizens about law enforcement duties and procedures in 2005; a CCTV program in July-November 2006 (See Ratcliffe, Taniguchi, & Taylor, 2009); a Strategic Anti-Violence Unit in 2006 to offer CBT to high risk probationers (See Sherman, 2007); and Operation *Safer* Streets, a multifaceted police intervention designed to reduce and prevent gun violence, in 2006 (See City of Philadelphia, 2007). To avoid conflating the effects of these interventions with the effects of Safe Streets, the study period ends on December 31, 2005.

The current study rules out endogeneity and spuriousness, survey-design effects, anticipation effects, and effects that could be attributed to other interventions. Ruling out these factors, coupled with a within-person longitudinal designs, garners confidence in the identification strategy and the ability to estimate causal effects of Safe Streets on the perceptual and behavioral outcomes studied in this dissertation.

Tables and Figures

Table 3.1. Variable Descriptions

Variable Names	Description	Coding	Availability	Study
Independent Variable				
Operation Safe Streets	Measure of whether interview date falls in June 2002 or later	Yes (1); No (0)	Wave (0-10) & Month (1-84)	1,2,3
Dependent Variables				
Perceptions of Arrest Risk (Self)	"How likely is it that you would be caught and arrested for the following crimes?" (fighting, armed robbery, stabbing someone, breaking and entering, stealing, vandalism and motor vehicle theft)	0-10 with 10 being 100% certainty of being caught	Wave (0-10)	1
Perceptions of Arrest Risk (Others)	"How likely is it that kids in your neighborhood would be caught and arrested for the following crimes?" (fighting, armed robbery, stabbing someone, breaking and entering, stealing, vandalism and motor vehicle theft)	0-10 with 10 being 100% certainty of being caught	Wave (0-10)	1
Self-Reported Offending	Dichotomous indicator of offending	Yes (1); No (0)	Wave (0-10) & Month (1-84)	2
Self-Reported Variety Score	Percentage of delinquent acts endorsed of 24 crimes	0-100%	Wave (0-10) & Month (1-84)	2
Self-Reported Frequency of Offending	Number of Self Reported Offenses (truncated at 99%)	Count	Wave (0-10)	2
Gun Availability	If a young person in this neighborhood wants to buy a gun, he/she can	5 Point Likert Scale; (1) strongly agree to (5) strongly disagree	Wave (0-10)	2
Gun Costs (9mm & .38)	How much would it cost to buy a [99mm/.38] gun?	Amount in Dollars ($)	Wave (0-10)	2
Peer Delinquency	How many of your friends have; Purposely damaged or destroyed property that did not belong to them, hit or threatened to hit someone, sold drugs, gotten drunk once in a while, carried a knife, carried a gun, owned a gun, gotten into a physical fight, stolen something worth more than $100, taken a motor vehicle or stolen a car, been hurt in a fight, and, gone in or tried to go into a building to steal something?	Average of 12 items; (1) None of them (2) Very few of them (3) Some of them (4) Most of them (5) All of them	Wave (0-10)	2
Exposure to Violence (Self)	Have you been chased where you thought you might be seriously hurt?, Have you been beaten up, mugged, or seriously threatened by another person?, Have you been raped, had someone attempt to rape you or been sexually attacked in some other way?, Have you been attacked with a weapon, like a knife, box cutter, or bat?, Have you been shot at?, and, Have you been shot?	Count of the prevalence of 6 items	Wave (0-10)	2

Exposure to Violence (Witnessed)	Have you seen anyone get chased where you thought they could be seriously hurt?, Have you seen anyone else get beaten up, mugged, or seriously threatened by another person?, Have you seen someone else being raped, an attempt made to rape someone, or any other type of sexual attack?, Have you seen someone else get attacked with a weapon, like a knife, box cutter, bat, chain, or broken bottle?, Have you seen someone else get shot at? , Have you seen someone else get shot?	Count of the prevalence of 7 items	Wave (0-10)	2
Illegal Work (Monthly)	Have you made money other ways, including from activities that are illegal?	Yes (1); No (0)	Month (1-84)	2
Illegal Earnings (Monthly)	Total amount of money made from illegal work during month x	Amount in Dollars ($)	Month (1-84)	2
Perceptions of Procedural Justice of Police (Direct Experience)	During your last contact with the police when you were accused of a crime, how much of your story did the police let you tell?, Of the people you know who have had a contact with the police (in terms of crime accusation), how much of their story did the police let them tell?, The police treat me the same way they treat most people my age, Over the last couple of years, the police have been treating me the same way they always treated me in the past, During my last encounter with the police, they treated me in the way that I expected they would treat me, During my last encounter with the police, they treated me in the way that I thought I should be treated, Even after the police make a decision about arresting me, there is nothing I can do to appeal it, Even after the police make a decision about arresting me, someone in higher authority can listen to my case, and even in some cases, change the decision, Police considered the evidence/viewpoints in this incident fairly, Police overlooked evidence/viewpoints in this incident, Police were honest in the way they handled their case, Police used evidence that was fair and neutral, Police made up their mind prior to receiving any information about the case, Think back to the last time the police accused you of doing something wrong: Did the police treat you with respect and dignity or did they disrespect you?, Did the police show concern for your rights?	Average of 14 items; (1) Strongly disagree (2) Somewhat disagree (3) Neither agree nor disagree (4) Somewhat agree (5) Strongly agree	Wave (0-10)	3
Legitimacy	I have a great deal of respect for the police; Overall, the police are honest; I feel proud of the police; I feel people should support the police; The police should be allowed to hold a person suspected of a serious crime until they get enough evidence to charge them; The police should be allowed to stop people on the street and require them to identify themselves.	Average of 6 items; (1) Strongly disagree (2) Somewhat disagree (3) Somewhat agree (4) Strongly agree	Wave (0-10)	3
Legal Cynicism	Laws are meant to be broken; It is okay to do anything you want; There are no right or wrong ways to make money; If I have a fight with someone, it is no one else's business; A person has to live without thinking about the future.	Average of 5 items; (1) Strongly disagree (2) Somewhat disagree (3) Somewhat agree (4) Strongly agree	Wave (0-10)	3

Neighborhood Disorder	Cigarettes on the street or in the gutters; Garbage in the streets or on the sidewalk; Empty beer bottles on the streets or sidewalks; Boarded up windows on buildings; Graffiti or tags; Graffiti painted over; Gang graffiti; Abandoned cars; Empty lots with garbage; Condoms on sidewalk; Needles or syringes; Political messages in graffiti; Gangs (or other teen groups) hanging out; Adults hanging out on the street; People drinking beer, wine or liquor; People drunk or passed out; Adults fighting or arguing loudly; Prostitutes on the streets; People smoking marijuana; People smoking crack; People using needles or syringes to take drugs	Average of 21 items; Never (1) to Often (4)	Waves (0-10)	3
Personal Rewards to Crime	How much 'thrill' or 'rush' is it to do any of the following things (fighting, robbery with a gun, stabbing someone, breaking into a store or home, stealing clothes from a store, vandalism and auto theft)?	Average of 7 items; (0) no fun or kick at all to (10) a great deal of fun or kick	Waves (0-10)	3
Social Rewards to Crime	If one were to steal, fight or rob; Other people my age will respect me more, I'll get more respect from adults in my neighborhood, people my age will be afraid to mess with me, I'll impress my boyfriend (or girlfriend), and, I can get back at someone	Average of 15 items; (1) strongly disagree to (4) strongly agree	Waves (0-10)	3
Additional Outcomes for Robustness Checks				
Perceptions of Arrest Risk (Self) for Aggressive Crimes	How likely is it that you would be caught and arrested for the following crimes?" (fighting, stabbing someone, vandalism)	0-10	Wave (0-10)	1
Perceptions of Arrest Risk (Self) for Income Crimes	How likely is it that you would be caught and arrested for the following crimes?" (armed robbery someone, breaking and entering, stealing, and motor vehicle theft)	0-10	Wave (0-10)	1
Procedural Justice of Police (Others)	Police treat males and females differently, Police treat people differently depending how old they are, Police treat people differently depending on their race/ethnic group, Police treat people differently depending on the neighborhoods they are from.	Average of 5 items; (1) Strongly disagree (2) Somewhat disagree (3) Neither agree nor disagree (4) Somewhat agree (5) Strongly agree	Wave (0-10)	3
Neighborhood Disorder (Collateral)	See Neighborhood Disorder	Average of 4 questions, Never (1) to Often (4)	Wave (0-10)	3

Alternative Predictor for Sensitivity Tests				
Non-Lagged Interview Date (Wave)	Measure of whether interview date falls in May 2002 or later	Yes (1); No (0)	Wave (0-10)	1
Non-Lagged Interview Date (Month)	Measure of whether interview month is May 2002 or later	Yes (1); No (0)	Month (1-84)	2
Placebo Measures				
Placebo Interview Date	Measure of whether interview date falls in June 2001 or later (1 year early); or January 2001 or later (6 months early)	Yes (1); No (0)	Wave (0-10)	1,2,3
Social Costs to Crime	I would be suspended from school, I would lose respect from my close friends, I would lose respect from my family members, I would lose respect from neighbors or other adults, I would lose respect from my girlfriend/boyfriend, and, It would make it harder to find a job	Average of 6 items ranging from (1) very unlikely to (5) very likely.	Wave (0-10)	1
Control Variables				
Age	Age in years at time of interview	Age	Wave (0-10)	1,2,3
Prior Arrests	Dichotomous indicator for arrest	Yes (1); No (0)	Wave (0-10) & Month (1-84)	1,2,3
Number of Prior Arrests	Number of arrests in recall period	Count	Wave (0-10)	1
Arrest Rate	Number of arrests in recall period over the number of self-reported offenses	Percentage	Wave (0-10)	1
Street Time	Percentage of time living in the community (compared to a secure setting)	Percentage	Wave (0-10) & Months (1-84)	1,2,3
Main Location	Primary location for each month indicating if it is in the community or a secure location	Yes (1); No (0)	Months (1-84)	2

Note. In several models, offending, variety scores, and frequency of offending are also included as control variables. These items are described in the dependent variables section above.

Table 3.2. Sample Demographics by Wave

	Baseline	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6	Wave 7
Age	16.12	16.52	17.07	17.61	18.09	18.52	19.06	19.94
Male	86.43%	84.52%	83.30%	84.18%	83.72%	82.60%	82.20% [a]	84.64%
Black	71.71%	70.04%	68.04%	69.17%	72.23%	70.80%	73.00%	70.22%
White	10.29%	10.71%	12.99%	11.97%	10.86%	11.80%	10.80%	11.29%
Hispanic	15.29%	16.47%	16.29%	16.02%	14.61%	15.20%	14.00%	15.67%
Other	2.71%	2.78%	2.68%	2.84%	2.30%	2.20%	2.20%	2.82%
Arrested	100.00%	15.67% [a]	22.06% [a]	20.28% [a]	25.05% [a]	21.00% [a]	20.80% [a]	28.84% [a]
Arrest rate	30.10%	70.41% [a]	91.01% [a]	75.67% [a]	80.59% [a]	104.48% [a]	71.61% [a]	81.49% [a]
Number of arrests	2.97	0.19 [a]	0.26 [a]	0.27 [a]	0.32 [a]	0.27 [a]	0.27 [a]	0.41 [a]
Offended	99.57%	54.89% [a]	48.45% [a]	44.83% [a]	43.72% [a]	35.20% [a]	35.80% [a]	42.32% [a]
Crime variety	29.87%	6.51% [a]	6.07% [a]	5.83% [a]	5.40% [a]	4.15% [a]	4.13% [a]	5.36% [a]
Street time	100.00%	54% [a]	68% [a]	84% [a]	86% [a]	88% [a]	87% [a]	85% [a]
n	700	504	485	493	479	500	500	319

Notes. Sample sizes *are* reported for the maximum number of interviews for each time period; item missingness on specific variables (max reductions in reported N's < 10 for all variables but arrest rate). Arrest rate is substantially reduced as this item only captures those who offended in the recall period.

[a] Significant differences (p <0.05) from means at baseline

Table 3.3. Interview Information by Safe Streets Timing

	t-7	*t-6*	*t-5*	*t-4*	*t-3*	*t-2*	*t-1*	***t***	***t1***	***t2***	***t3***	***t4***	*t5*	*t6*	*t7*	Total Persons
Panel A - Number of Interviews Before Safe Streets for Study 1 and Study 3																
0	-	-	-	-	-	-	-	88	38	49	18	63	54	38	38	88
1	-	-	-	-	-	-	257	134	163	157	41	183	178	158	10	257
2	-	-	-	-	-	206	206	127	150	138	28	149	143	108	1	206
3	-	-	-	-	143	143	143	116	117	112	12	114	93	-	-	143
4	-	-	-	6	6	6	6	6	5	3	-	5	1	-	-	6
Total Interviews	0	0	0	6	149	355	612	471	473	459	99	514	469	324	49	700 (3,980)
Panel B - Number of Interviews Before Safe Streets for Study 2																
0	-	-	-	-	-	-	-	87	34	51	53	53	60	61	-	87
1	-	1	1	2	-	2	68	36	37	40	39	44	42	-	-	74
2	1	1	5	4	17	245	159	185	189	193	189	187	98	-	-	274
3	-	1	1	10	166	99	108	134	128	128	130	124	-	-	-	175
4	-	-	4	90	61	65	73	77	77	68	73	-	-	-	-	90
Total Interviews	1	3	11	106	244	411	408	519	465	480	484	408	200	61	0	700 (3,801)

Notes. These time periods are used for descriptive purposes only and data are not analyzed in this way in first-difference models.

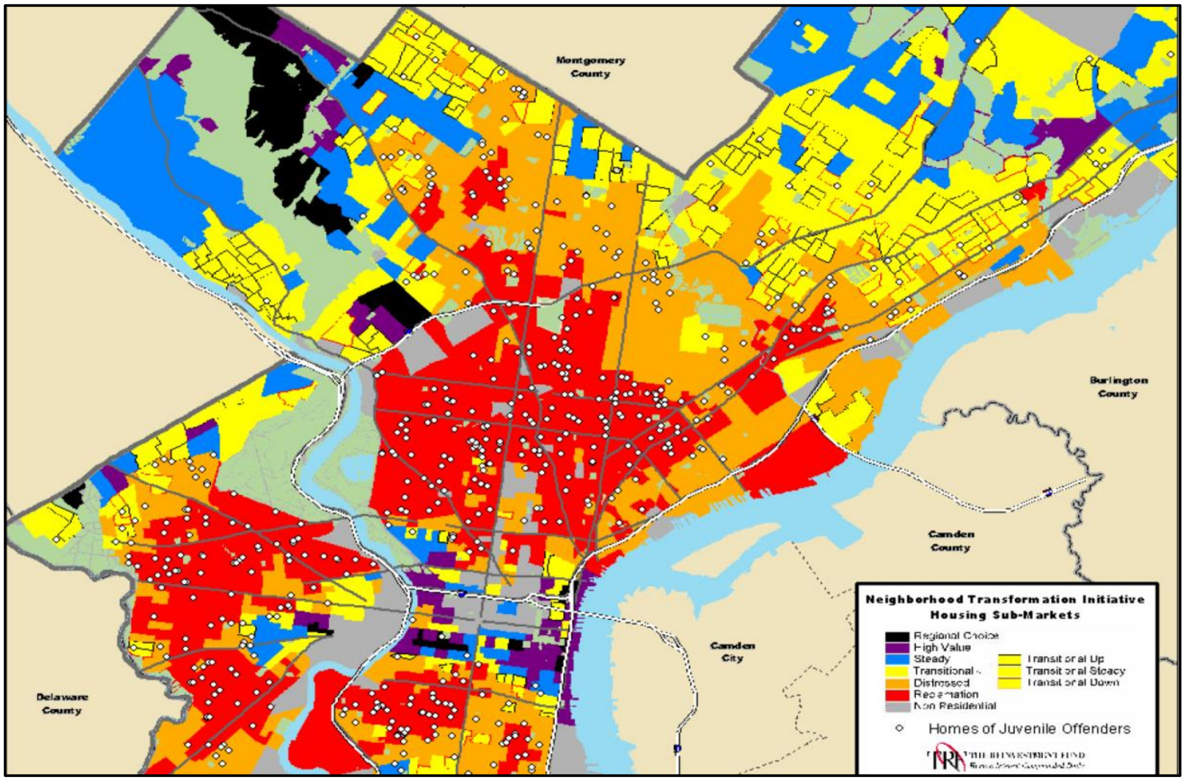
Table 3.4. Sample Demographics by Safe Streets Timing

	Baseline	*t-3*	*t-2*	*t-1*	***t***	***t1***	***t2***	***t3***	*t4*
Age	16.12	15.77 [a]	16.21 [a]	16.49 [a]	**16.94**	**17.53** [a]	**17.90** [a]	**18.21** [a]	18.59 [a]
Male	86.43%	87.92%	89.30% [a]	87.58% [a]	**82.38%**	**83.30%**	**82.35%**	**74.75%**	83.85%
Black	71.71%	67.11%	69.58%	72.22%	**67.52%**	**72.09%**	**70.81%**	**62.63%**	71.60%
White	10.29%	12.75%	10.14%	9.48%	**12.74%**	**10.99%**	**12.20%**	**14.14%**	11.09%
Hispanic	15.29%	16.78%	16.62%	15.36%	**16.77%**	**14.80%**	**14.60%**	**18.18%**	15.18%
Other	2.71%	3.36%	3.66%	2.94%	**2.97%**	**2.11%**	**2.40%**	**5.05%**	2.14%
Arrested	100.00%	95.97% [a]	64.51% [a]	50.33% [a]	**37.15%**	**22.83%** [a]	**21.35%** [a]	**21.21%** [a]	26.26% [a]
Arrest rate	30.10%	29.53% [a]	23.27% [a]	18.86%	**14.66%**	**11.25%**	**7.54%** [a]	**7.01%**	10.30%
Number of arrests	2.97	2.35 [a]	1.65 [a]	1.46 [a]	**0.92**	**0.29** [a]	**0.25** [a]	**0.25** [a]	0.36 [a]
Offended	100%	98% [a]	82% [a]	74% [a]	**60%**	**45%** [a]	**41%** [a]	**42%** [a]	38% [a]
Crime variety	29.87%	26.96% [a]	19.71% [a]	16.27% [a]	**12.18%**	**5.57%** [a]	**4.88%** [a]	**4.92%** [a]	4.78% [a]
Street time	100%	98% [a]	78%	71% [a]	**80%**	**81%**	**86%** [a]	**91%** [a]	86% [a]
n	700	149	355	612	**471**	**473**	**459**	**99**	514

Notes. Periods during Safe Streets are noted in bold. Sample sizes are reported for the maximum number of interviews for each time period; item missingness on specific variables (max reductions in reported N's < 10 for all variables but arrest rate). Arrest rate is substantially reduced as this item only captures those who offended in the recall period.

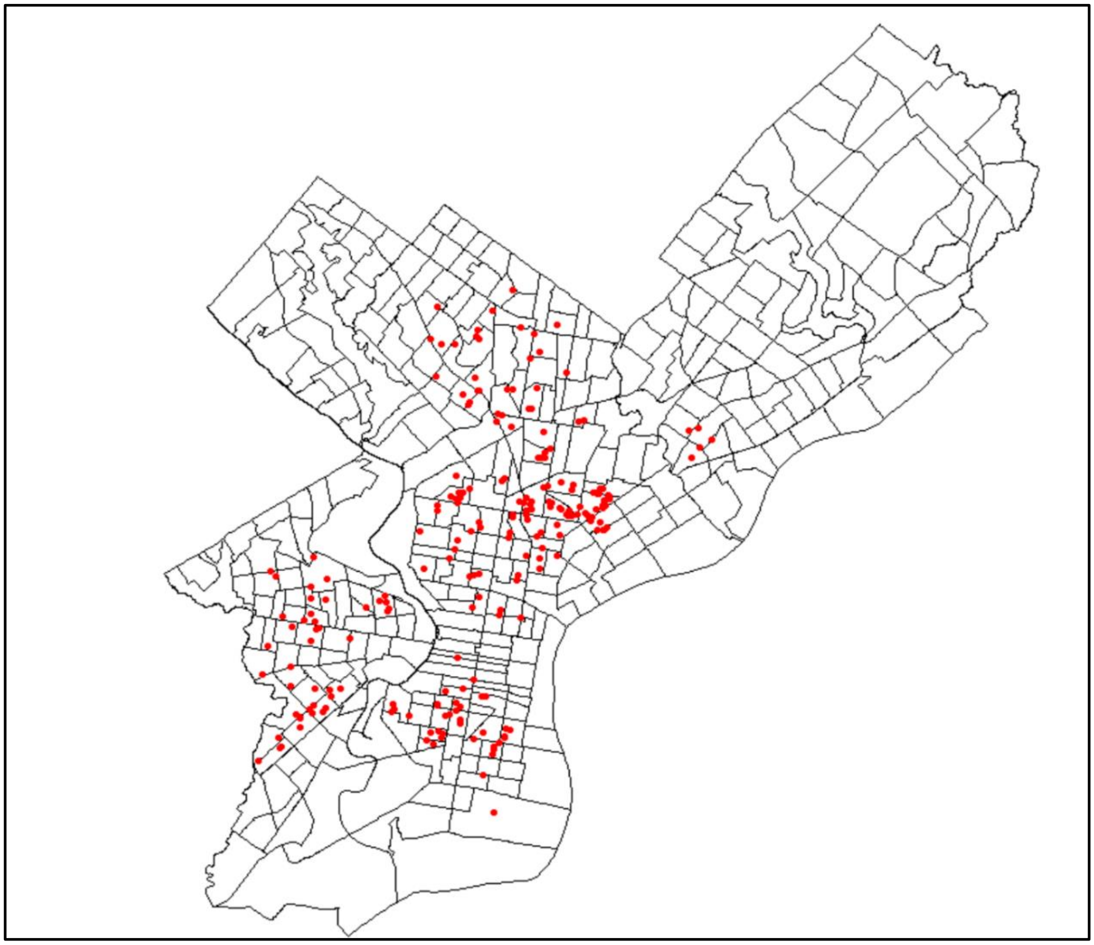
[a] Significant differences (p <0.05) from means at time t

Figure 3.1 Map of Original Home Addresses of Philadelphia Respondents



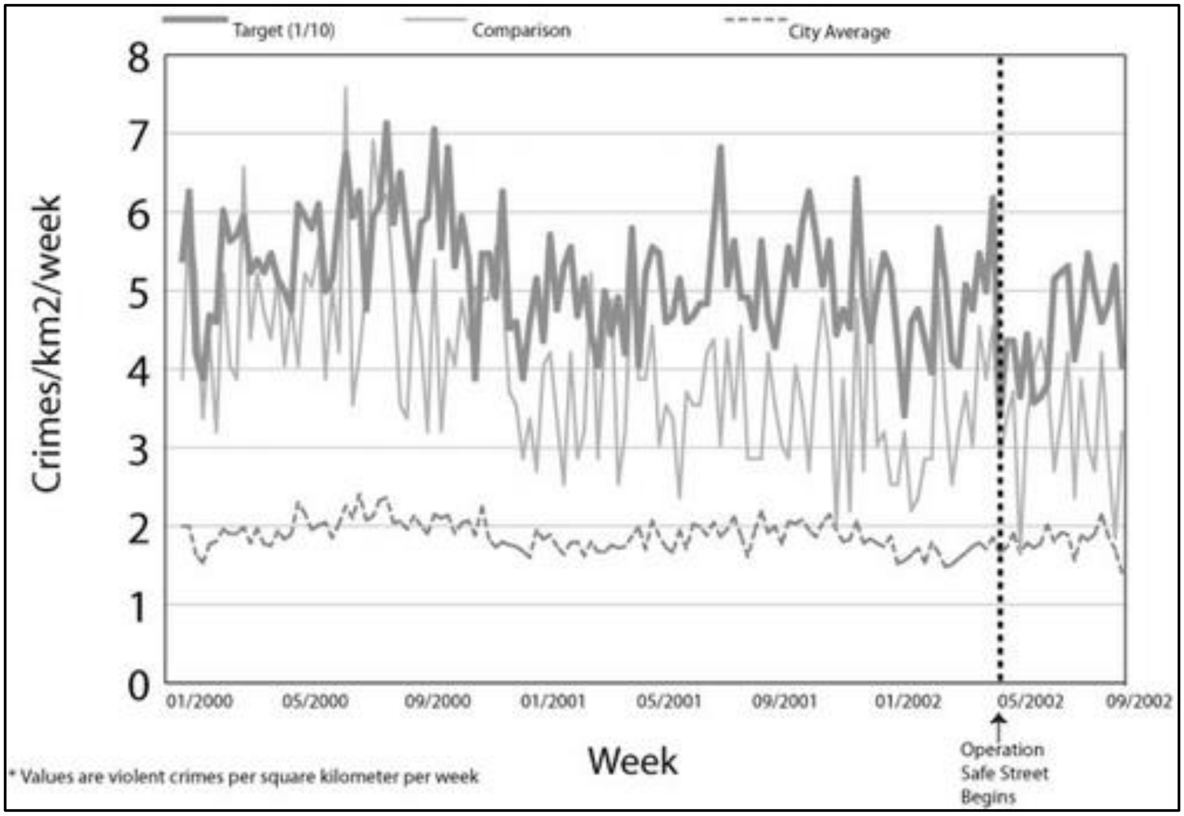
Source: Mulvey, Edward P. & Schubert, Carol A. (2012). Pathways to Desistance. American Society of Criminology. Chicago, Illinois.

Figure 3.2 Map of Original Safe Streets Targeted Sites



Note. Locations were provided to Rebecca Bucci courtesy of the Philadelphia Police Department

Figure 3.3 Trends in Crime Surrounding Operation Safe Streets



Note. Philadelphia weekly violent-crime rates: citywide, target areas (N = 213), and comparison areas (N = 73).
Source: Lawton, Taylor & Luongo, 2005

Chapter 4. STUDY 1

"The decisive factor in creating the deterrent effect is, of course not the objective risk of detection, but the risk as it is calculated by the potential criminal."

Andenaes (1966; p. 963)

The hypothesized causal link between police and offending is the impact that police have on potential offenders' perceptions of arrest risk (Corsaro & Engel, 2015). In other words, in order for police to be effective deterrents, the police must communicate the certainty of arrest either through punishments or punishment threats, which in turn, alters potential offenders' perceptions of arrest risk (Waldo & Chiricos, 1972). Without this impact on perceptions of arrest risk, the criminal justice system will not have a deterrent effect (Andenaes, 1966; Nagin, 1998).

In order for perceptions to be altered, police must communicate the threat of punishment so that offenders become aware of the increase in risk (Geerken & Gove, 1975). If police can increase offender certainty of punishment and subsequently perceptions of arrest risk, to sufficient levels, police should theoretically decrease crime. Some prior research has demonstrated that punishment experiences like arrest are related to increases in perceptions of arrest risk. But in regard to police effects through the *threat of punishment* without actual punishment, this assumption regarding the role of police on perceptions remains to be tested outside of lab-based experiments (e.g., Barnum et al., 2021). In general, the lack of studies that directly test how the criminal justice system impacts perceptions of arrest risk has been defined as the "missing link" in deterrence research (Kleck et al., 2005). The empirical research on the relationship between police and perceptions is dwarfed by the theoretical discussions and debates surrounding the effects of punishment on perceptions (and subsequent criminal behavior) (see Volume 15, Issue 3 of Criminology & Public Policy for one example).

Given the inherent benefits of deterring crime prior to its occurrence rather than arresting offenders after the commission of a crime (Nagin et al., 2015), it is important to understand whether punishment *threats*, such as the level of police presence, are related to variation in perceptions of arrest risk. Despite the ongoing debates on the deterrent effect of police via punishments and punishment threats, no single study has addressed the limitations and critiques of the existing research so the question of whether criminal justice factors can (perceptually) deter potential offenders remains open to debate. The current study seeks to examine the causal effect of Safe Streets, a hot spots policing intervention, on perceptions of one's own likelihood of arrest risk; the first path in the causal chain described by Deterrence and Rational Choice theories. In this chapter, I test the effect of hot spots policing on individuals' perceptions of their own arrest risk.

Theoretical Expectations for Operation Safe Streets

Given the existing research, theory, and frameworks regarding the role of the police, there are expectations of the potential effect of Safe Streets on perceptions of arrest risk. First, because Safe Streets itself has been found to reduce violent crime (Lawton et al., 2005), theory suggests that this negative relationship is likely due, at least in part, to the impact of this intervention on perceptions of arrest risk. Furthermore, given that Safe Streets was not designed as a high-enforcement or high-arrest strategy, it is expected that the effects of Safe Streets will be found even when individual-level controls for arrest are included. Finally, given the importance of threat communication as a goal of the intervention, and the anecdotal evidence that residents perceived changes in policing and the media attention surrounding Safe Streets, theory suggests that perceptions of arrest risk will be impacted.

Hypotheses

H4.1: Operation Safe Streets will lead to an increase in perceptions of arrest risk of one's self in the period (wave) immediately following its implementation.

H4.2: Operation Safe Streets will lead to an increase in perceptions of arrest risk of others in the period (wave) immediately following its implementation

H4.3: Operation Safe Streets will have a larger effect on perceptions of arrest risk of one's self than perceptions of arrest risk of others.

Study Contributions

It is unclear from the existing perceptual deterrence research if police can impact perceptions of one's own likelihood of arrest, and additionally, if the mere threat of punishment can have a deterrent effect on the behavior of potential offenders. The prior research is limited in its ability to directly test the effect of police on risk perceptions. The current study directly explores the relationship between police presence and risk perceptions by exploiting a police intervention (Safe Streets) that occurred during data collection of the Pathways study. This study examines offenders' self-reported perceptions of risk and self-reported offending in the period prior to the implementation of Safe Streets and the periods during and after the surge in police presence. The current study is the first to explore the impact of changing police strategies at micro units on individual self-reported perceptions of arrest risk over time.

The current study's focus on a sample of previously adjudicated adolescents, many whom continued to engage in crime, (i.e., those for which deterrent messages are designed) is superior to the existing research[8] on the relationship between punishment threats and perceptions which has focused on community samples. Additionally, the use of a within-person longitudinal analysis is superior to the between-person analyses previously discussed. Examining the same individuals over time allows for tests of whether or not risk perceptions are based on some objective level of risk (Manski, 2004) and if changes are positively correlated with objective risk (Apel, 2013). It also addresses the issue of 'coherent arbitrariness' that arises from examining risk perceptions between persons (Thomas et al., 2018). Finally, and perhaps most importantly, the current study allows for the assessment of individuals' perceptions of their *own likelihood of arrest*, rather than individuals' perceptions of the arrest rate for an area.

Sample Restrictions

The analytic sample begins with the 700 adolescent respondents living in Philadelphia at baseline. For all models, all available cases are included for those in the community greater than 10% of the time since the last interview. [9] Item non-response does result in to varying sample sizes across models. These restrictions result in a final analytic sample of 3,142 and 3,148 person-waves (n=654) for the main analyses for perceptions of arrest risk for one's self and others, respectively. For analyses which examine arrest rates as a predictor, models only include

[8] This point is contested. Some have critiqued the use of an offender-only sample (Pickett & Roche, 2016), but most deterrence scholars have noted the importance of examining the effects of police on *offenders'* risks perceptions and classified it as a necessary next step for deterrence research (Apel, 2013; Apel & Nagin, 2011; Nagin, 2013).

[9] Comparing the analyses when changes were created before dropping cases, after dropping cases, or on the full sample by interacting Safe Streets by street time, the results yield substantively consistent findings. For example, the coefficients for Safe Streets for Model 1 is 0.625, 0.629, and 0.624 when creating change measures before dropping cases, after dropping cases, or with the full sample and an interaction between Safe Street and street time, respectively.

those who were actively offending at each wave, therefore the sample decreases to 870 person-waves (n=362).

Measures

Dependent Variable

Perceptions of arrest risk is a measure of respondents' perceived risk of arrest. Respondents are asked "How likely is it that you would be caught and arrested for the following crimes?" including fighting, armed robbery, stabbing someone, breaking and entering, stealing, vandalism and motor vehicle theft. Respondents report on a 10-point scale ranging from no chance (0) to absolutely certain (10) that they would be caught and arrested. The average response for these seven items is used as the measure of perceptions of arrest risk. Alphas range from 0.89 to 0.93 across waves.

Perceptions of arrest risk of others is nearly identical to the above measure but instead asks respondents how likely is it that neighborhood kids would be caught and arrested for the same seven crimes. Again, each item ranges from no chance (0) to absolutely certain (10) that they would be caught and arrested, and the average is used. Alphas range from 0.82 to 0.89 across waves.

Independent Variable

The focal predictor is a dichotomous variable indicating that the interview took place after the start of *Safe Streets*. Respondents are considered 'treated' if they are interviewed anytime beginning one month after Safe Streets was implemented (i.e., on or after June 1st, 2002). The one-month lag is to allow time for respondents to adjust their perceptions.[10] Once respondents experience exposure to Safe Streets, they are coded 1 for all remaining interviews, even if these interviews fall beyond the intervention period.

Time-Varying Controls

This study controls for arrest, offending behavior and time in the community at each wave. *Arrest* is measured as a dichotomous indicator for arrest (1) or no arrest (0) since the last interview. This measure is based on official arrest data collected from juvenile and adult records by the Pathways to Desistance study. Offending behavior is conceptualized as a *variety score*, indicating the percentage of different crimes that were endorsed since the last interview. The majority of respondents are asked about 24 unique crimes, but in waves that did not ask about all 24, the denominator is adjusted accordingly. All respondents are coded as having been arrested (1) and having had offended (1) at baseline as all respondents were previously arrested and adjudicated as a requirement for the study. Crime variety scores at baseline refer to all behavior in the last six months prior to entry into the study as opposed to the time since the last interview as it is measured at all later waves.

A respondent's *arrest rate* is measured as the number of arrests in a given recall period over the number of offenses committed, for those who have offended at least once. The arrest rate ranges from 0, for those who have offended but have not been arrested, to 1 for those who have been arrested as many times as the number of offenses committed. In regression models, the rate is multiplied by 10 so that the coefficient can be more easily interpreted so each unit

[10] Scholars have previously noted that interventions may have a delayed effect on risk perceptions as it takes time for policy messages to be spread, for vicarious deterrence (e.g., news about peers' arrests), and for personal experiences with law enforcement to take place (Lochner, 2004; Raj Sah, 1991).

increase corresponds to a 10-percentage point increase in the arrest rate. It is important to note that these models are only based on the sample who had offended since the last interview.

Street time is a measure of the percentage of days in the recall period in which respondents are in the community (i.e., not incarcerated, in a hospital or other secure setting which restricts community exposure). In order to retain respondents, it is assumed that all respondents have had some exposure to the community prior to baseline and are coded as 1, regardless of whether they were incarcerated or not at the initial time of the intervention.

Moderator

Neighborhood drug disorder is reported by respondents regarding how often on a four-point scale ranging from never (1) to often (4) the following conditions are present in their neighborhood: needles or syringes; people smoking marijuana; people smoking crack; and people using needles or syringes to take drugs. *Low drug disorder* is categorized by those that scored in the bottom quartile of drug disorder (scores on the neighborhood drug disorder scale less than or equal to 1.75). *Moderate to high drug disorder* is defined by those in the highest quartile (scores on the neighborhood drug disorder scale greater than or equal to 2.5).

Analytic Strategy

To estimate the effect of Safe Streets on perceptions of arrest risk, the current study proceeds in several steps. First, descriptive statistics are presented for key predictors and outcomes for waves prior to, during, and after Safe Streets. Next, aggregate changes in perceptions for the waves prior to Safe Streets are compared to changes which occurred during the transition to Safe Streets, as well as to changes over later periods after the intervention had ended. These changes are then presented conditional on offending, arrest, street time, and levels of neighborhood drug disorder. Following the presentation of bivariate statistics, first-difference linear regression models are presented which predict changes in individuals' perceptions over time to isolate the effect of Safe Streets.

The following equations represent the main first-difference models that are estimated. In each equation, p represents perceptions of arrest risk and *ß1* signifies the effect of Safe Streets on the change in perceptions.

$$\Delta p_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta AGE_{it} + \Delta\varepsilon_i \tag{1}$$

$$\Delta p_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta VARIETY_{it} + ß4\Delta STREETTIME_{it} + ß5\Delta AGE_{it} + \Delta\varepsilon_i \tag{2}$$

Models are then presented which examine the interaction between Safe Streets and arrest to examine if the effect of Safe Streets is different for those who have and have not been arrested in the period since the last interview.

$$\Delta p_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta\ VARIETY_{it} + ß4\Delta ARREST^{*}\ VARIETY_{it} + ß5\Delta STREETTIME_{it} + ß6\Delta AGE_{it} + \Delta\varepsilon_i \tag{3}$$

Next, models examine one's arrest rate rather than a dichotomous indicator of arrest. These models only include those for whom crime was committed.

$$\Delta p_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta(A_{it}/C_{it}) + ß3\Delta VARIETY_{it} + ß4\Delta STREETTIME_{it} + ß5\Delta AGE_{it} + \Delta\varepsilon_i \tag{4}$$

$$\Delta p_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta(A_{it}/C_{it}) + ß3\Delta VARIETY_{it} + ß4\Delta(A_{it}/C_{it})^{*}\ VARIETY_{it} + ß5\Delta STREETTIME_{it} + ß6\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (5)$$

Equations 1 through 5 are estimated for both the outcomes of perceptions of arrest risk for one's self and perceptions of arrest risk for others. Necessarily, equations 4 and 5 cannot include those who have not recently offended in the estimation. These results for these equations must be interpreted as the effects of Safe Streets for those who have offended only.

Results

Bivariate Results

Table 4.1 presents descriptive statistics on perceptions of arrest risk for one's self and others at periods prior to, during and following Safe Streets.[11] Information on covariates in relation to Safe Streets is available in Table 3.4. In the bivariate, it appears that Safe Streets may positively impact the two perceptional outcomes of interest. Before Safe Streets, perceptions were declining from 5.23 to 4.79 for one's self and from 5.53 to 5.18 for perceptions of others arrest risk. Once Safe Streets began, perceptions for one's self increased to 5.42 and perceptions of others increased to 5.48. These perceptions remained fairly consistent hovering at about 5.3 for one's self and 5.45 for others in the periods after Safe Streets.

Comparisons of the average perceptions between waves offers some support that Safe Streets had an effect on perceptions, but a comparison of average changes across waves is more informative. Table 4.2 presents aggregate changes between waves in perceptions of arrest risk for one's self (Panel A) and for others (Panel B). In each panel, the top row denotes changes for

[11] For the outcomes from this Study, as well as Studies 2 and 3, by Wave instead of by relation to Operation Safe Streets, see Appendix Table A.2.

all respondents while the following rows present changes conditional on arrest, offending, a combination of arrest and offending, and street time all measured since the last interview, as well as conditional on neighborhood drug disorder categorized at baseline. This allows for further examination of the effects of Safe Streets given recent offending behavior and arrest experiences.

Significant stars in Table 4.2 reflect significant differences between changes after the implementation of Safe Streets (column t – t-1) compared to all other untreated changes, based on rank sum tests. Panel A shows that, on average, respondents' perceptions of arrest certainty were declining in the periods prior to Safe Streets, and this pattern holds for all conditional averages. Arrest certainty declined the most for those who had offended but were not arrested (-0.49 and -0.65 for t-2 – t-3 and t-1 – t-2, respectively) and decreased the least over time for those who did not offend (-0.37 and -0.06, respectively). Upon the implementation of Safe Streets, the trend in perceptions of arrest risk reverses. Perceptions of arrest certainty increased on average by 0.51 once Safe Streets was implemented. This pattern is consistent conditional on all combinations of covariates. These average increases are greatest for those who had been arrested and are the weakest for those who did not offend and were also not arrested in the recall period. These significant differences generally hold for those who were not arrested, who offended, who were in the community greater than 50% of the time, and who lived in locations with moderate to high neighborhood drug disorder at baseline. Notably, these significant differences in changes over time are not evident when comparing periods of first treatment exposure to later periods when Safe Streets was ongoing or had ended.

Panel B examines perceptions of arrest risk for others. The patterns are largely consistent with those for perceptions of arrest risk for one's self. For the whole sample, perceptions were declining but then increased substantially after Safe Streets began. These increases tapered off to minimal levels after the first treated wave. Comparing the first treated wave to the most recent untreated wave, for all categories, changes in the perceptions of arrest risk for others increased contrary to the decrease that happened in the prior wave. These differences in changes are significant for all categories except when defined as those who were arrested, those who were arrested and offended and those from low drug disorder neighborhoods.

To further explore this relationship, I examine perceptions of arrest risk only for those who were interviewed in the months immediately prior to and following the start of Safe Streets. Because the intervention was implemented on one specific day, the first of May 2002, but interviews were being conducted on a rolling basis, I exploit this to assess the average perceptions of arrest risk at each month of interview. Figure 4.1 presents average perceptions of arrest risk for self (blue line) and others (dotted gray line) based on respondents interview month.[12] For example, in April, 120 respondents were interviewed, and for both self and others, they reported perceptions of arrest risk of about 4.8 on a ten-point scale. In May, these perceptions remained stable for others and declined slightly for self (n=93). However, by June, one month after Safe Streets began, average perceptions of arrest risk for one's self and others increased to about 5.8 (n=82). These patterns are again supportive of a perceptual deterrent effect of Safe Streets.

[12] Theoretically it is possible that selection effects took place and explain these differences. Because the interview timing was seemingly random as respondents did not select their interview date, there is minimal concern about selection into interview timing. We would yield a slightly different conclusion if by June, the prevalence of respondents who were incarcerated was greater than the prevalence who were incarcerated in pre-treatment waves. In waves prior to Safe Streets, the prevalence who were interviewed in a secure setting was 64%, 52%, and 55% respectively. In May, 47% of interviewed respondents were in a secure setting at the time of the interview, and in June through October, the prevalence interviewed in a secure setting ranged between a high of 37% (June) to a low of 23% (October). These rates of secure confinement are higher prior to Safe Streets which helps to minimize concerns over selection and the concern that post treatment risk perceptions were increased because respondents were more likely to have been incarcerated. It also further bolsters the point that Safe Streets was not designed to increase enforcement (i.e., arrests and incarceration).

Main Results

These bivariate statistics suggest perceptions of arrest risk were declining but then began increasing after Safe Streets began. To test for the impact of the intervention on individuals' changes in perceptions, Table 4.3 presents the results of first-difference models for arrest risk. All models control for age at last birthday to account for general trends in perceptions over time (age estimates are not shown for brevity).

Model 1 of Panel A shows that experiencing Safe Streets is related to a 0.63 increase on a 0-10 scale, in one's perception of arrest certainty. When compared to the average arrest risk perceptions prior to the intervention (4.79), this translates to an approximately 13.2% increase in perceptions of arrest risk. Model 2 adds controls for arrest, variety score and street time. The effect of Safe Streets remains significant and consistent at 0.62, now holding experiences with offending and arrest, as well as street time, in the recall period constant. Those who endorsed all 24 possible crime types reported significantly lower perceptions of arrest risk (b = -2.22) than those who did not offend in the recall period. The coefficient on street time shows that perceptions of arrest risk are greater for those spent a greater percentage of their time in the community (i.e., street time) (b = 0.56). In order to assess if the effect of Safe Streets is dependent on arrest, Model 3 examines the interaction between Safe Streets and arrest. For those who were not arrested in the preceding period, Safe Streets is related to a 0.72 increase in arrest certainty while arrest is related to a 0.75 increase in arrest perceptions. The interaction shows that for those who were arrested, Safe Streets results in an additional 0.23 increase in arrest risk perceptions. This interaction term is marginally significant at $p<0.01$.

Models 4 and 5 examine the effects of one's arrest rate rather than a dichotomous indicator of arrest in the recall period. As such these models only include respondents who offended in the recall period. When controlling for the arrest rate, Safe Streets is related to a 0.84 increase in perceptions of arrest risk. Each 10-percentage point increase in the arrest rate[13] is related to a 0.07 increase in perceptions of arrest risk. The interaction between arrest rate and Safe Streets in Model 5 is consistent with the interaction between arrest and Safe Streets in Model 3; Safe Streets is related to a 0.79 increase in risk perceptions for those who were not arrested at all. For periods with Safe Streets, each additional 10 percentage point increase in one's arrest rate is related to an additional 0.03 increase in one's perception of arrest risk. In more concrete terms, a person who experienced Safe Streets and was arrested for all of their crimes would experience an increase in their perceptions of arrest risk of about 1.76, though the Safe Streets by arrest rate interaction does not reach statistical significance.

Panel B examines parallel models but explores respondents' reports of perceptions of arrest risk for others. The effect of Safe Streets is smaller (0.48) but remains significant in the simplest model (Model 1) and when controls are included for arrest, offending and street time (Model 2). The interaction in Model 3 demonstrates that Safe Streets increased perceptions more for those who were not arrested. In Models 4 and 5 which examine the effect of Safe Streets just for those who offended in the recall period controlling for arrest rate, Safe Streets yields slightly larger and still significant effects on perceptions. The arrest rate, and its interaction, appear to have little effect on perceptions of arrest risk of others.

Placebo Tests

The models presented above suggest an increase in individuals' perceptions of arrest risk that coincides with the implementation of Safe Streets. In the case of perceptions of arrest risk, it

[13] Because the arrest rate was rescaled to range from -10 to 10, every unit increase refers to an additional 10 percentage point increase on one's arrest rate.

remains possible that these models are picking up a gradual change in risk perceptions over time or some unmeasured change in Philadelphia around the treatment period. To further rule out the possibilities that an exogenous shock took place surrounding the time of Safe Streets that impacted perceptions, and to rule out the concern that preexisting trends in perceptions of arrest risk were conflated with the intervention, in-time placebo (or falsification) tests are examined to further support the identification strategy. In-time placebos are used to examine if changes in the outcome also took place during times when the intervention had not yet been implemented (Abadie, Diamond, & Hainmueller, 2015; Heckman & Hotz, 1989). Models re estimated which assume Safe Streets began 12 and 6 months earlier than it did, respectively.

Additional falsification tests are used to show that, first, alternative outcomes which should not be impacted by Safe Streets did in fact not change at the hands of the intervention and, second, that similar samples which did not experience the treatment do not experience a similar change in perceptions. The first test is done by examining the effect of Safe Streets on the social costs of punishment. Theory would not expect a hot spots policing intervention to impact the subsequent social costs of punishment as the punishment is theoretically the same before and after the intervention, though the *likelihood* of receiving punishment has potentially been altered. Respondents report how likely it is on a scale ranging from very unlikely (1) to very likely (5), that they would be suspended from school, lose respect from close friends, family, neighbors or a significant other[14], and make it harder to find a job if police caught them breaking the law. The average of these six items is used to measure the social costs of punishment (alpha = 0.68 at baseline and greater than 0.74 at all follow-up waves).

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[14] Respondents only report on losing respect from a boyfriend or girlfriend if they previously reported having a partner.

The second test examines if the timing of the intervention had any impact on perceptions of arrest risk for the remaining 654 respondents from Maricopa County, Phoenix. These respondents were similar to the Philadelphia sample, but are more likely to be Hispanic. They were sampled in a similar fashion at the same time as the Philadelphia sample and answered identical questions. This makes them a useful sample to rule out the possibility that the results of the current study are due to survey design effects or some unmeasured change which impacted this age group around the time of Safe Streets.

Models 1 and 2 in Table 4.4 present the effects of a placebo intervention based on start dates 1 year prior, and 6 months prior, to the true start date of the intervention. Model 1 offers strong support that the main findings are not the result of preexisting trends. Moreover, the large negative effect of a placebo intervention, defined as having begun 6 months prior to Safe Streets, further demonstrates that perceptions of arrest risk were actually *declining* prior to the true start of Safe Streets. This finding, as well as the aggregate downward trend in perceptions in the periods prior to Safe Streets shown in Table 4.2, provides further support that Safe Streets increased perceptions of arrest risk. Model 3 examines the social costs of crime. As expected, Safe Streets did not significantly impact perceptions of the social costs to crime. Finally, in Model 4, the results suggest that Safe Streets had no impact on perceptions of arrest risk for respondents in Phoenix, once again, offering strong support for the conclusion that Safe Streets increased perceptions of arrest risk for adolescent respondents in Philadelphia.

Sensitivity Analyses

The results of the main analyses and placebo tests suggest a significant increase in one’s perception of arrest risk of themselves and others after experiencing a change to hot spots policing. In order to ensure that the results are not determined by the model selection, the constraints on the model, or the various decisions in coding the variables, sensitivity analyses are conducted to provide further confidence in the above findings. More specifically, the decisions which warrant further attention include the decision to lag the intervention by one month, the decision to include a variety score of offending rather than a dichotomous indicator or count of offenses, and finally, the decision to use first-difference models rather than fixed-effects models. The results from these analyses are presented in Tables 4.5 and 4.6, with Panel A representing models for perceptions of arrest risk for one's self and Panel B showing identical models which examine the outcome of perceptions of arrest risk for others.

A priori, I chose to examine the effect of Safe Streets starting one month after its inception as theory suggests that the effects of the intervention may have a delayed effect on perceptions. It is not clear how long of a time lag to expect for deterrent messages to be perceived from a policy shift (Greenberg & Kessler, 1982), however, it has been hypothesized that it takes time for policy messages to be spread and for vicarious deterrence (e.g., news about peers' arrests) to take effect. In order to test if the intervention was related to a change in risk perceptions beginning on its first day, identical models to the main analyses were estimated, but without this lag in treatment (i.e., Safe Streets was coded 1 for all interviews after May 1, 2020). In Model 1 of Table 4.5, the results suggest that the intervention had a smaller impact on risk perceptions on average (0.37 and 0.28 for self and others, respectively). The results remain significant for perceptions of arrest risk of one's self but are not significant for perceptions of others' risk. This evidence suggests that it likely does take time for the intervention to alter one's perceptions of arrest certainty.

It is also possible that a variety score of offending does not capture the underlying latent concept related to perceptions of arrest risk as well as a dichotomous indicator or count of offending. These analyses are presented in Models 2 and 3 of Table 4.5. When examining risk for self (Panel A) and others (Panel B), the results are nearly identical to models which control for one's variety score of offending (Model 2 in Table 4.3). These results are consistent for perceptions of both one's self and others.

Finally, it is possible that the effects of Safe Streets were not driven by the increase in police presence but rather by an increase in arrest as a result of the intervention. Safe Streets was designed to focus on increased police presence without increasing arrest, supplemental analyses (see Appendix A.1) provide evidence that Safe Streets significantly decreased the prevalence and number of arrests for respondents in the sample. The focal analyses accounted for a dichotomous indicator of arrest, but it is possible that the number of arrests is more impactful for one's perception of arrest risk than the rate for which they successfully avoided arrest. The final columns in Table 4.5 present these results; for personal arrest risk and others' arrest risk, nearly identical effects of Safe Streets were found when accounting for the number of arrests (0.64 and 0.48) in each period rather than a dichotomous indicator of arrest (0.63 and 0.48 from Table 4.3), respectively.

Table 4.6 presents the main models again, but instead presents results estimated using a fixed-effects estimator. For one's self and for others' arrest risk, the results follow a pattern substantively similar to the main results in Table 3.3, however, the effect sizes are somewhat smaller. This conclusion is unsurprising given that in these models, the estimates show the increase in arrest risk from each respondents mean report of arrest risk in the study period, rather than relative to the interview right before Safe Streets was enacted. Though the original model specification is preferable to examine the immediate effect on perceptions, these models further demonstrate that these changes were large enough to be detected by deviations from the mean and likely persisted long enough to be detected by these models.

As a final check on the model specification, I estimated event study models, using the Stata 15 package “eventdd” (Clarke & Schythe, 2020). An event study design is useful in that this method accounts for the pre-existing trend in perceptions prior to the intervention. Typically, an event study design is used to assess the parallel trends assumption in difference-in-differences studies (i.e., to assess if the treatment and control group were on comparable trajectories prior to the treatment). With a single group, an event study design uses the pre-trend as the counterfactual. Figures 4.2 and 4.3 present these results with 95% confidence intervals. The vertical line denotes the last period before treatment. As you can see in both Figures, there are 4 pre-treatment waves and no consistent pattern in perceptions of arrest risk before treatment. However, once Safe Streets began, there is a small statistically significant increase in perceptions of arrest risk of one’s self and a marginally significant increase in perceptions of arrest risk of others. These analyses again align with the results of the previously reported first-difference models.

Robustness Checks

Robustness checks are presented to examine if the main results were robust to specific populations within the sample, and to various conceptualizations of perceptions of arrest risk. First, I explore if the effects of Safe Streets are similar for those who had high or low levels of neighborhood drug disorder at baseline as these respondents are more and less likely to have lived close to Safe Streets sites, respectively. Unfortunately, information on the exact home addresses of respondents are not available, so direct tests of the differences between a treatment and control group who did not experience the treatment are not possible. However, reports of neighborhood drug disorder can be used to disaggregate respondents into those who were more or less likely to live near a targeted drug hot spot based on reports of neighborhood drug problems.

In Panel A of Table 4.7, Model 1 shows the results for those from perceived low drug disorder neighborhoods at baseline while Model 2 presents the results for those from perceived moderate to high drug disorder at baseline. As expected, Safe Streets is related to a larger increase in perceptions of arrest risk for those living in moderate to high drug disorder neighborhoods (b = 1.06) compared to those from low drug disorder neighborhoods (b = 0.45). A similar pattern is seen in Panel B for perceptions of others' risk, with larger effects for those living in moderate to high drug disorder neighborhoods at baseline (0.70) compared to those from low drug disorder neighborhoods (0.42). Despite the difference in the magnitude of the coefficients, z-tests of the estimates (Paternoster, Brame, Mazerolle, & Piquero, 1998) reveal that these effects are not significantly different from each other (critical value = -0.662).

Next, I assess impacts for income versus aggressive crimes to assess if Safe Streets operated differently for these types of crimes. Models 3 and 4 in Panels A and B demonstrate that the estimates are fairly consistent across aggressive and income crimes for both perceptions of one's own arrest risk and perceptions of others' arrest risk. The small differences are not significantly different based on z-tests.

Finally, I examined models which tested perceptions of each of the seven crimes separately in Table 4.8. In Panel A, the effects for perceptions of one's self are largely consistent across crime types; the effect of Safe Streets is largest for perception of arrest risk for breaking into a store or home (0.76) and smallest for auto theft (0.47). This latter effect is the only crime for which the effect of Safe Streets does not reach statistical significance. These effects across crimes are not statistically significant from each other at $p<0.05$ based on z-tests of the equality of estimates. What is most notable about these analyses is that the effect is largely consistent, suggesting that Safe Streets may have had similar effects for each of the seven crime types.

In Panel B which examines perceptions of arrest risk of others, the pattern across crimes is less consistent. The effect of Safe Streets ranges from a low of 0.08 for perceptions of others' arrest risk for fighting to a high of 0.70 for perceptions of others' arrest risk for robbery with a gun. These effects, despite being quite different in magnitude, are not statistically different from each other. Nonetheless, the difference in effect sizes for fighting, as well as breaking into a store or home, compared to the other crimes suggests that Safe Streets may have operated differently across different crimes.

Discussion

The current study is the first to use a within-person longitudinal design to examine the impact of a hot spots policing intervention on individual perceptions of arrest risk. This study found that Safe Streets was related to an increase in perceptions of arrest risk for one's self and for perceptions of arrest risk of others. This makes this the first to offer evidence that criminal justice interventions do in fact have an impact on perceptions of arrest risk, and one of the first to show that subjective perceptions are in fact correlated with objective levels of punishment in the real world. These conclusions are based on several different model specifications, a host of theoretically relevant sensitivity analyses, and are supported by null results from placebo tests.

The results suggest that Safe Streets had larger effects on one's personal perceptions of arrest risk, though it still increased individuals' perceptions of others' risk. This result is important because it lends some credence to the argument that all measures of perceptions of arrest risk are not equal. When asking respondents to report on the average arrest rate for a community, or on perceptions for others, we may not be capturing a true measure of deterrence. Furthermore, in this prior work, we may have been underestimating the effects of police on perceptions of arrest risk and not assessing the impact on perceptions of arrest risk of one's self which is ultimately what we know impacts offending behavior.

Additionally, the results suggest that Safe Streets impacted perceptions of arrest risk, for those who were and were not arrested. Controlling for prior arrest experiences and examining an interaction between Safe Streets and arrest made it possible to assess if the effect of the intervention on perceptions of risk was only due to changes in arrest during the treated period. Because the results showed that even after controlling for arrest, and when examining only those who were arrested, there is *still* a positive effect of Safe Streets, the results are promising for the deterrent effect of the threat of punishment without punishment itself.

The results of the current study also suggest that the effects of Safe Streets likely took some time to manifest. When comparing analyses with and without the one-month lag in treatment, the results show that larger effects occurred when including this lag. Though this only altered the treatment variable for 8 respondents, it provides preliminary results to suggest that it may take some time for hot spots policing to impact individuals' perceptions of arrest risk. This is somewhat at odds with the hot spots policing literature which finds that reductions in crime are almost immediate. These distinctions should be examined in future research.

Finally, the similar effects of Safe Streets on income and aggressive crimes, as well as across the seven individual crime types, suggests that Safe Streets did impact perceptions of arrest risk for a host of crimes in similar ways. Unfortunately, the current data does not allow for analyses of perceptions of arrest risk for crimes which typically occur indoors rather than outdoors. Future research should examine the impact of police presence on perceptions of arrest risk for crime types disaggregated by those which typically occur outdoors (and theoretically are more likely to be impacted by police presence) compared to those which occur indoors. These differences across crime types are important areas for future research.

It is worth noting that the use of this particular sample may have resulted in conservative estimates of the deterrent effect of police for two reasons. All baseline interviews were conducted after youth had been adjudicated. This may make their baseline estimates of risk higher than ‘normal’. The sample is limited to respondents who have theoretically already ‘updated’ their risk perceptions to account for their contact with law enforcement and the criminal justice system, as each respondent had been previously arrested and adjudicated. On the other hand, because all respondents in the sample had been arrested and adjudicated, theoretically this study examined the deterrent effect of Safe Streets on a sample for whom deterrence already once failed. It remains unclear whether deterrent effects should be weaker for serious offenders relative to less serious or non-offenders (Hagan & McCarthy, 1997; Zimring & Hawkins, 1973), or, if deterrence is more likely for those who have more room to increase their perceptions and are otherwise not deterred by morals or informal social control. The estimates of the effect of Safe Streets may also be conservative because this study examines the effect on average for all 700 study participants. Because the effects were averaged across 700 respondents, it is possible that not all were treated or had high dosages of exposure to Safe Streets, which weakened the effects. This possibility was highlighted when the results demonstrated larger treatment effects for those from high drug disorder neighborhoods.

The current study is not without limitations. Information on respondents' home addresses was not available, nor was the full set of targeted Safe Streets, so it was impossible to create a nontreated control group. There also was no available survey information which asked potential offenders if they were aware of the Operation Safe Streets intervention and accompanying increase in police presence. But media accounts of the intervention suggest it was a widely known change in policing (Moran et al., 2002). For example, just a few weeks into the intervention, one community member was quoted saying "There is clearly, absolutely a visible difference in police presence across the city" (Smith, 2002). The large increase in uniformed foot patrol officers has also been described as arguably the most salient and visible sign of deterrence (Kleck & Sever, 2018). Nevertheless, this study is designed with the assumption that respondents knew about Safe Streets and the results support this.

Additional information which was not captured in this study such as knowledge regarding the apprehension (or failure to apprehension) for those who victimized the respondent, and the knowledge about these incidents passed on from one's immediate friends and family, may also play a role in forming perceptions of arrest risk. While the current study did not account for these possibilities and time-varying experiences which may impact perceptions of arrest risk, the research design does eliminate all time-stable factors which would be related to perceptions of arrest risk.

The current study examined specifically the role of police on the risk of arrest and did not examine how other criminal justice factors may impact one's certainty of punishment. The threat of punishment can come from any of the three branches of criminal justice (i.e., police, courts and corrections), but from a policy standpoint, the most proximate factor for potential offenders is the certainty of arrest, studied in this chapter. Assessing how the certainty of punishment is impacted by these other criminal justice factors, such as the certainty of conviction (Kleck et al., 2005), is an important area for future research.

Overall, this study has addressed many of the theoretical and methodological limitations of existing research and is now the first study to show the effect of changes in punishment threats on perceptions of arrest risk for the same individuals over time. The results suggest that perceptions are malleable and can be positively impacted by changes in policing even when individuals themselves are not arrested. This evidence is promising as developing ways to increase perceptions of arrest risk without direct punishment experience and the subsequent negative consequences of criminal justice contact is a crucial next step for criminal justice policy.

Tables and Figures

Table 4.1. Perceptions of Arrest Risk by Safe Streets Timing

	Baseline	*t-3*	*t-2*	*t-1*	***t***	***t1***	***t2***	***t3***	*t4*
Arrest Risk (Self)	5.04	5.23	5.06	4.79 [a]	**5.42**	**5.25**	**5.34**	**5.36**	5.32
Arrest Risk (Others)	5.36	5.53	5.44	5.18	**5.48**	**5.49**	**5.47**	**5.42**	5.41
n	700	149	355	612	**471**	**473**	**459**	**99**	514

Notes. Periods during Safe Streets are noted in bold. Sample sizes are reported for the maximum number of interviews for each time period.

[a] Significant differences (p <0.05) from means at time t

Table 4.2. Changes in Perceptions of Arrest Risk by Safe Streets Timing

	Average Within Person Change					
	t-2 - t-3	*t-1 - t-2*	***t- t-1***	*t1 - t*	*t2 - t1*	*t3 - t2*
Panel A - Self	-0.43 **	-0.36 **	**0.51**	-0.01 *	0.09	-0.07
Arrested	-0.39	-0.35	**0.73**	-0.17	0.30	0.02
Not arrested	-0.44 *	-0.37 **	**0.45**	0.04	0.03	-0.10
Offended	-0.48	-0.62 **	**0.35**	-0.11	-0.18	-0.23
Did not offend	-0.37	-0.06	**0.67**	0.08	0.28	0.05
Street time > 50%	-0.06	-0.52 **	**0.45**	0.01	0.13	0.02
Street time <= 50%	-0.77 *	-0.22	**0.68**	-0.09	-0.16	-
Arrested & offended	-	-0.49	**0.69**	-0.11	-0.15	-
Not Arrested & offended	-0.49	-0.65	**0.18**	-0.11	-0.19	-0.31
Low drug disorder (baseline)	-0.66 *	-0.10	**0.44**	-0.05 *	-0.04	-0.10
Moderate/high drug disorder (baseline)	-0.14	-0.73 **	**0.87**	-0.07	0.45	-0.47
n	148	340	**365**	459	452	98
Panel B - Others	-0.32 *	-0.30 ***	**0.39**	0.05	0.01 **	-0.19
Arrested	-0.60	-0.58	**0.10**	-0.23	0.42	-0.01 *
Not arrested	-0.27 *	-0.26 ***	**0.48**	0.13	-0.10 ***	-0.24
Offended	-0.40 *	-0.35 **	**0.25**	-0.03	-0.10	-0.49
Did not offend	-0.21	-0.24 *	**0.53**	0.12	0.09 *	0.03
Street time > 50%	-0.17	-0.43 **	**0.40**	0.03	0.00	-0.16
Street time <= 50%	-0.46 *	-0.18 *	**0.38**	0.15	0.09 *	-0.67
Arrested & offended	-	-0.42	**0.13**	-0.26	0.27	-0.57
Not Arrested & offended	-0.43 *	-0.33 *	**0.31**	0.09	-0.27 *	-0.47
Low drug disorder (baseline)	-0.41	0.01	**0.35**	-0.23	0.11	0.04
Moderate/high drug disorder (baseline)	-0.49 *	-0.49 *	**0.60**	0.25	-0.06	-0.79 *
	148	340	**365**	459	454	98

Notes. Significance stars denote comparisons to changes from *t* to *t*-1 based on Rank Sum tests; Cells with n<20 not reported.
**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 4.3. First-Difference Longitudinal Linear Regressions Predicting Perceptions of Arrest Risk

	Model 1		Model 2		Model 3		Model 4		Model 5	
Panel A - Self	b	SE	b	SE	b	SE	b	SE	b	SE
Safe Streets	0.63 **	0.19	0.62 **	0.18	0.72 ***	0.20	0.84 **	0.32	0.79 *	0.32
Arrest			0.38 **	0.11	0.75 **	0.24				
Safe Streets * arrest					-0.49	0.27				
Arrest rate							0.07	0.04	0.03	0.05
Safe Streets * arrest rate									0.67	0.66
Variety score			-2.22 ***	0.56	-2.50 ***	0.58	-0.90	0.67	-0.87	0.67
Street time			0.56 **	0.17	0.46 *	0.18	0.60 *	0.28	0.62 *	0.28
n = persons(person-waves)	654(3,145)		654(3,142)				362(870)			
Panel B - Others										
Safe Streets	0.48 **	0.15	0.48 **	0.15	0.50 **	0.16	0.54 *	0.22	0.54 *	0.22
Arrest			0.13	0.10	0.19	0.17				
Safe Streets * arrest					-0.09	0.20				
Arrest rate							-0.01	0.03	-0.01	0.04
Safe Streets * arrest rate									0.01	0.54
Variety score			-0.59	0.45	-0.64	0.46	-0.23	0.57	-0.23	0.57
Street time			0.27	0.14	0.26	0.15	0.36	0.22	0.36	0.22
n = persons(person-waves)	654(3,151)		654(3,148)				362(872)			

Notes. SE = robust standard error

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Table 4.4. First-Difference Longitudinal Linear Regressions - Placebo Tests

	1 Year Early			*6 Months Early*			*Social Costs*			*Phoenix*		
	Model 1			Model 2			Model 3			Model 4		
	b		SE	b		SE	b		SE	b		SE
Safe Streets	0.01		0.27	-0.37		0.22	0.05		0.06	-0.08		0.18
Arrest	0.38	**	0.13	0.36	**	0.13	-0.02		0.04	0.19		0.10
Variety score	-2.28	***	0.57	-2.39	***	0.56	-0.31		0.20	-2.14	***	0.41
Street time	0.57	**	0.17	0.53	**	0.17	0.09		0.06	0.40	*	0.17
n = persons(person-waves)	654(3,142)						654(3,169)			607(2,991)		

Notes. SE = robust standard error

*p< 0.05; **p <0.01; ***p< 0.001

Table 4.5. First-Difference Longitudinal Linear Regressions - Sensitivity Tests

	No Treatment Lag			*Dichotomous Offending*			*Frequency of Offending*			*Number of Arrests*		
	Model 1			Model 2			Model 3			Model 4		
Panel A - Self	b		SE	b		SE	b		SE	b		SE
Safe Streets	0.37	*	0.18	0.63	**	0.19	0.65	**	0.19	0.64	**	0.19
Arrest	0.38	**	0.13	0.31	*	0.13	0.24		0.13	0.15	*	0.06
Offending	-2.21	***	0.56	-0.36	**	0.13	0.00		0.00	-2.31	***	0.61
Street time	0.57	***	0.17	0.37	*	0.16	0.35	*	0.16	0.52	**	0.17
n = persons(person-waves)	654(3,142)											
Panel B - Others												
Safe Streets	0.28		0.15	0.48	**	0.15	0.49	**	0.15	0.48	**	0.15
Arrest	0.13		0.10	0.13		0.09	0.11		0.09	0.02		0.04
Offending	-0.58		0.45	-0.19		0.10	0.00		0.00	-0.49		0.47
Street time	0.28	*	0.14	0.23		0.13	0.22		0.13	0.28	*	0.14
n = persons(person-waves)	654(3,148)											

Notes. SE = robust standard error

*p < 0.05; **p <0.01; ***p < 0.001

Table 4.6. Fixed-Effects Longitudinal Linear Regressions - Sensitivity Tests

	Model 1		Model 2		Model 3		Model 4		Model 5	
	b	SE	b	SE	b	SE	b	SE	b	SE
Panel A - Self	0.49 **	0.14	0.32 *	0.15	0.61 **	0.18	0.41 *	0.20	0.45 *	0.21
Safe Streets			0.19	0.11	0.68 **	0.21				
Arrest					-0.65 **	0.22				
Safe Streets * arrest							0.05 *	0.02	0.06	0.04
Arrest rate									-0.03	0.05
Safe Streets * arrest rate										
Variety score			-2.15 ***	0.42	-2.45 ***	0.43	-1.69 ***	0.47	-1.69 ***	0.47
Street time			0.69 ***	0.17	0.51 **	0.19	0.60 **	0.22	0.59 **	0.23
Constant	5.91 ***	0.24	5.59 ***	0.30	5.38 ***	0.31	5.33 ***	0.42	5.30 ***	0.42
n = persons(person-waves)	699(3,961)		699(3,958)				657(2,004)			
Panel B - Others										
Safe Streets	0.34 **	0.11	0.27 *	0.12	0.38 **	0.14	0.21	0.16	0.20	0.16
Arrest			0.07	0.08	0.25	0.16				
Safe Streets * arrest					-0.25	0.18				
Arrest rate							0.00	0.02	-0.01	0.03
Safe Streets * arrest rate									0.01	0.04
Variety score			-0.81 *	0.35	-0.92 *	0.36	-0.43	0.41	-0.43	0.41
Street time			0.33 *	0.14	0.26	0.15	0.30	0.18	0.30	0.18
Constant	5.99 ***	0.21	5.81 ***	0.25	5.73 ***	0.25	5.54 ***	0.35	5.55 ***	0.35
n = persons(person-waves)	699(3,964)		699(3,961)				657(2,005)			

Notes. SE = robust standard errors

**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 4.7. First-Difference Longitudinal Linear Regressions - Robustness Checks

	Low Drug Disorder		*Moderate/High Drug Disorder*		*Aggressive Crimes*		*Income Crimes*	
	Model 1		Model 2		Model 3		Model 4	
Panel A - Self	b	SE	b	SE	b	SE	b	SE
Safe Streets	0.45	0.27	1.06 **	0.36	0.65 **	0.20	0.61 **	0.21
Arrest	0.36	0.21	0.44 *	0.21	0.35 **	0.13	0.40 **	0.14
Variety score	-2.38 *	1.02	-1.41	0.81	-2.11 ***	0.57	-2.32 ***	0.60
Street time	0.57 *	0.26	0.41	0.30	0.45 **	0.17	0.66 ***	0.18
n = persons(person-waves)	161(1,410)		151(1,040)		654(3,142)		654(3,140)	
Panel B - Others								
Safe Streets	0.42	0.23	0.70 *	0.27	0.44 **	0.16	0.55 **	0.18
Arrest	-0.04	0.17	0.34 *	0.15	0.15	0.10	0.13	0.11
Variety score	-0.02	0.76	-0.48	0.69	-0.85	0.50	-0.68	0.49
Street time	0.41 *	0.21	-0.02	0.24	0.35 *	0.15	0.25	0.15
n = persons(person-waves)	284(1,414)		228(1,040)		654(3,155)		654(3,151)	

Notes. SE = robust standard errors

**p* < 0.05; ***p* <0.01; ****p* < 0.001

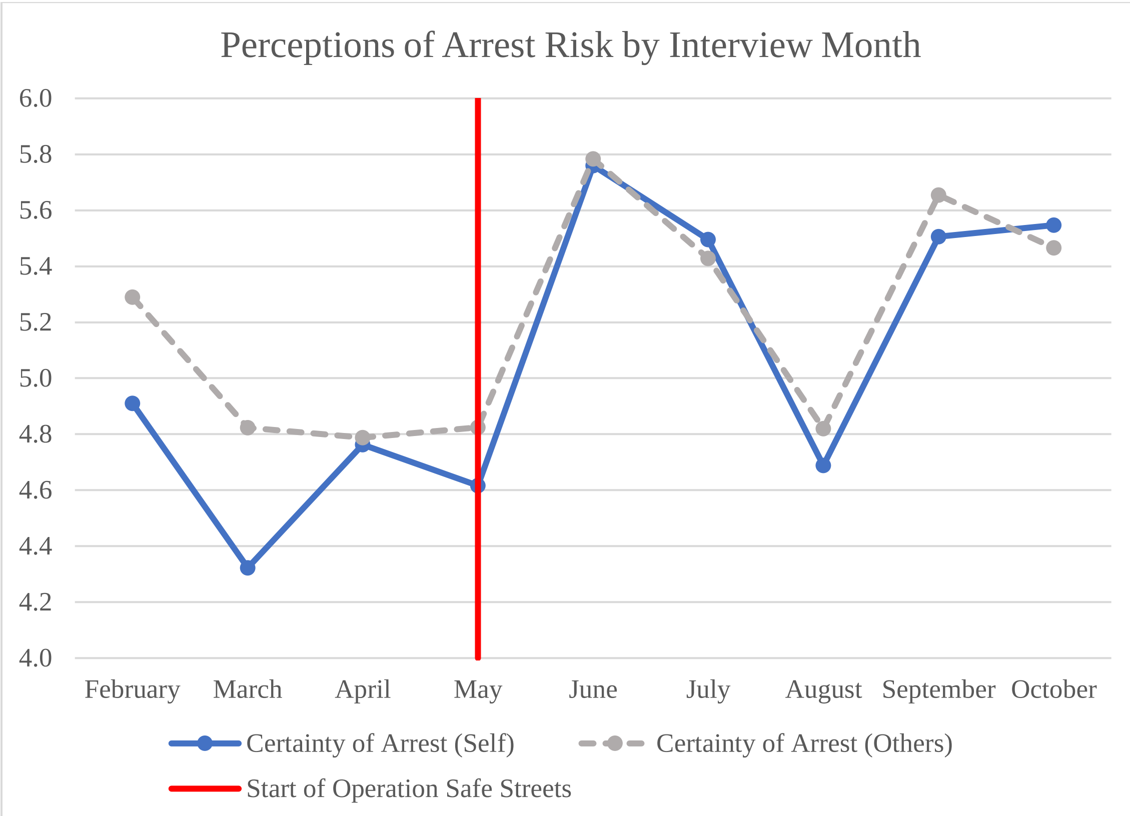
Table 4.8. First-Difference Longitudinal Linear Regressions - Robustness Checks - Crime Specific

	Fighting		*Robbery with a Gun*		*Stabbing Someone*		*Breaking into a Store/Home*		*Stealing Clothes from a Store*		*Vandalism*		*Auto Theft*	
	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7	
Panel A - Self	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE
Safe Streets	0.67 **	0.24	0.67 **	0.25	0.68 **	0.25	0.76 **	0.25	0.53 *	0.26	0.65 *	0.28	0.47	0.28
Arrest	0.31 *	0.14	0.61 ***	0.17	0.32	0.18	0.29	0.18	0.37 *	0.17	0.41 *	0.18	0.33	0.18
Variety score	-2.37 ***	0.61	-3.59 ***	0.77	-1.74 *	0.74	-2.42 **	0.73	-1.62 *	0.73	-2.06 **	0.73	-1.58	0.82
Street time	0.37	0.19	0.78 ***	0.21	0.59 **	0.22	0.56 **	0.21	0.59 **	0.22	0.45	0.23	0.71 **	0.23
n = persons(person-waves)	654(3,140)		654(3,134)		654(3,131)		654(3,138)		654(3,131)		654(3,138)		654(3,138)	
Panel B - Others														
Safe Streets	0.08	0.23	0.70 **	0.23	0.56 *	0.23	0.34	0.25	0.50 *	0.24	0.68 **	0.24	0.61 **	0.23
Arrest	0.09	0.14	0.22	0.14	0.30	0.15	0.13	0.14	0.04	0.15	0.11	0.15	-0.01	0.15
Variety score	-0.49	0.66	-0.47	0.61	-0.70	0.63	-0.68	0.64	-0.54	0.67	-1.05	0.70	-0.13	0.67
Street time	0.32	0.18	0.22	0.18	0.46 *	0.20	0.26	0.20	0.49 *	0.19	0.25	0.20	0.11	0.20
n = persons(person-waves)	654(3,144)		654(3,140)		654(3,137)		654(3,137)		654(3,128)		654(3,139		654(3,132)	

Notes. SE = robust standard errors

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Figure 4.1. Perceptions of Arrest Risk by Interview Month



Note. Sample sizes range from a low of 63 in September to a high of 120 in April.

Figure 4.2. Event Study Graph – Perceptions of Arrest Risk (Self)

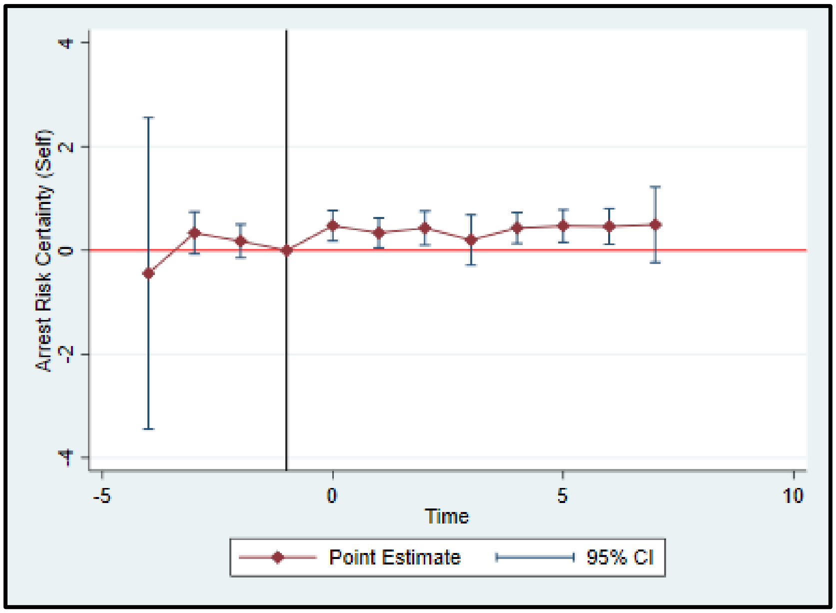
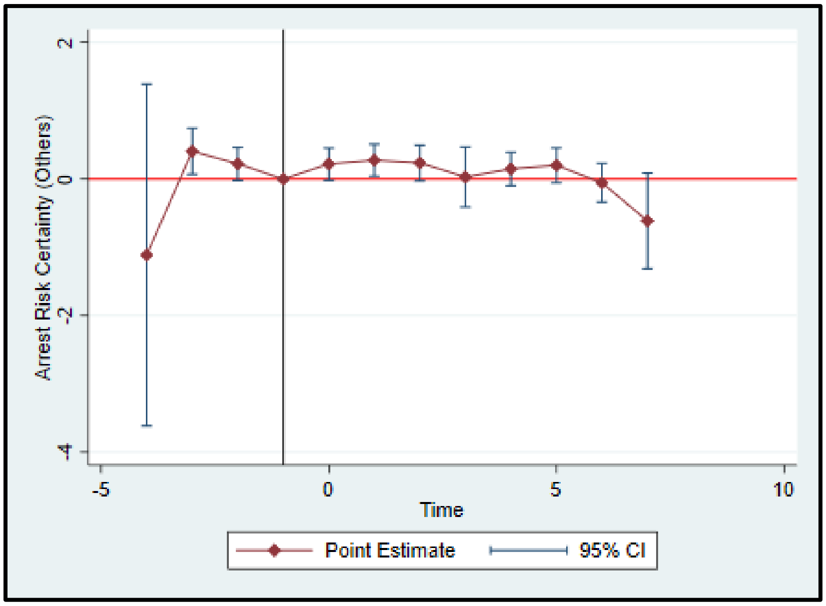


Figure 4.3. Event Study Graph – Perceptions of Arrest Risk (Others)



Chapter 5. STUDY 2

The police do not prevent crime. This is one of the best kept secrets of modern life. Experts know it, the police know it, but the public does not know it…

David Bayley, 1994

In 1994, David Bayley made this assertion that police do not prevent crime. A quarter of a century later, there is evidence to support his claim; many studies on the relationship between police and official crime data have yielded null or in some cases, even positive effects (i.e., police increase crime). Yet evaluations of specific police interventions like hot spots policing have shown that this specific police strategy is effective at reducing crime. The divergence between studies is due in large part to methodological differences and limitations of these areas of research, which were discussed in more detail in Chapter 2.

The most important limitation of the research on police and crime is the focus on official crime data or calls for service rather than individual self-reported offending. It is difficult to assess the relationship between police and crime due to the endogeneity between the outcome and predictor. Police presence may reduce crime but also increase detection of crime. Police presence may also increase residents' willingness to report crime resulting in increased official crime and calls for service. Additionally, the focus on official crime makes it impossible to examine how police presence impacts individual offending behavior. It is unknown if individuals actually decrease their offending, or simply alter their offending in some way as a response to changing police presence (e.g., changing the location or specific types of crimes they commit), which causes their offenses to go unrecorded in official crime rates for the jurisdiction of interest. Research is needed which examines changes in policing practices on individuals' reports of offending and which focuses not only on the prevalence of offending, but also the frequency and types of offending.

Now that it has been established that Safe Streets had a positive impact on perceptions of arrest risk, the next logical question is whether or not Safe Streets had an impact on offending. Deterrence theory and Rational Choice theory state that crime can be prevented or deterred if one's perception of arrest risk can be increased, and specifically, increased to levels high enough which make the risks unacceptable, or higher than the expected rewards. Higher levels of perceptions of punishment certainty, such as perceptions of the likelihood of arrest following the commission of a crime, have been shown to have an inverse relationship with offending for serious crimes (Matsueda et al., 2006), as well as less serious crimes such as shoplifting or petty theft (Teevan, 1976a), or substance use (i.e., alcohol and marijuana) (Matthews & Agnew, 2008; Teevan, 1976b), and even white-collar crimes, such as tax evasion (Klepper & Nagin, 1989). Hot spots policing is known to reduce crime and now has been shown to increase perceptions of arrest see (see Study 1) but no study has examined self-reported offending as an outcome in these evaluations.

Theoretical Expectations for Operation Safe Streets

Given the significant positive effect of Safe Streets on perceptions of arrest risk for adolescents in this sample, the existing research suggesting that hot spots policing is effective at reducing crime, and the previously published analysis which demonstrated that Safe Streets was related to a significant reduction in drug-related violence (Lawton et al., 2005), it is expected that Safe Streets will have a significant negative effect on self-reported offending. Furthermore, this is expected because the majority of recent research has suggested that hot spots policing reduces crime without spillover effects to adjacent communities (National Academies of Sciences, Engineering, and Medicine, 2018). Finally, because the intervention began with a large shock in the presence of police, and because theory predicts that individuals' are more responsive to changes in police presence than stable rates (Ariely et al., 2003; Kahneman & Tversky, 1979; Pogarsky & Loughran, 2016), it is expected that Safe Streets will reduce offending.

Hypotheses

H5.1: Operation Safe Streets will lead to a decrease in the prevalence of offending in the period (wave or month) immediately following its implementation.

H5.2: Operation Safe Streets will lead to a decrease in the frequency of offending in the period (wave) immediately following its implementation.

H5.3: Operation Safe Streets will lead to a decrease in the types of offending in the period (wave or month) immediately following its implementation.

Study Contributions

The current study is the first to examine the impact of hot spots policing on individual self-reported offending. More specifically, the current study explores the effect of Safe Streets on the prevalence of offending, the frequency of offending, and the number of unique crime types that are endorsed by adolescents before and after Operation Safe Streets. This study examines these key outcomes by interview wave as well as by examining more granular month-level data. Additional related outcomes, such as the ability to purchase a firearm, the cost of firearms, peer offending, victimization and exposure to others' victimization, as well as the prevalence of illegal work and the monetary gains from illegal work are also explored.

The current study contributes to the literature in four key ways. First, the focus on a single city rather than between county differences allows for a smaller unit of analysis, at a level where offenders are more likely to be aware of changes in police (Nagin, 2013). Second, the use of individual offending data reduces known concerns over endogeneity and reciprocal relationships in the policing literature. Third, self-reported offending data captures offenses unknown to police. Fourth, analyses of self-reported offending data mean that the current study can assess if in fact offenders are deterred (i.e., stop engaging in crime), or if they continue to offend in adjacent locations, or transition to different forms of crime.

Sample Restrictions

This analysis utilizes data from all available interviews except those for when respondents are in the community for less than 10% of days during the recall period for the wave-level analyses. For month-level analyses, months for which respondents are in secure settings, meaning they are unable to offend, are dropped from the analyses. This results in a final analytic sample of 642 persons and 2,994 person-waves for wave-level analyses and 396 persons and between 12,513 and 12,525 person-months for month-level analyses.

Measures

Dependent Variables (Wave)

Self-reported offending is measured in three ways at each interview.[15] First, *offending* is conceptualized as a dichotomy coded 1 for those who reported that they offended and 0 for those

[15] Offending at baseline is available for: 1) if the respondent *ever* committed each of 24 crimes, 2) the number of times in the last year for each crime, and 3) the total offending variety score for the past 6 months. Therefore, to calculate offending in the last 6 months at baseline, a dichotomous indicator was created from any score over 0 on

who reported that they did not offend in each recall period or in the 6 months prior to baseline. *Crime variety* is a variety score of offending which indicates the percentage of different crimes that were endorsed since the last interview. The possible 24 crime types include destruction of property, arson, breaking into a location, shoplifting, receiving stolen property, check/cred card fraud, stealing a car or motorcycle, selling marijuana, selling other drugs, carjacking, driving while intoxicated, prostitution, rape, murder, shooting and hitting someone, shooting someone, assault with a weapon, assault with no weapon, fighting and causing injury, fighting, gang beatings, carrying a gun, breaking into a car, and joyriding. The majority of respondents are asked about all 24 crimes, but in waves that did not ask about all 24, the denominator is adjusted accordingly, as well as in periods where respondents did not provide answers to all questions (i.e., item non-response). *Frequency of offending* is a count which denotes the sum of the number of crimes committed for each of the 24 crime types. Due to instances where individuals report extremely high frequencies of offenses for each crime type, this measure is truncated (recoded) at the 99th percentile, following prior research (Anwar & Loughran, 2011).

Dependent Variables (Month)

In addition to examining aggregate reports of offending since the last interview. The current study examines individuals' retrospective reports of offending for each month since the last interview. Respondents mapped their offenses to each month using Life-History Calendar methods, (explained in more detail on page 74). Self-reported offending is measured in two ways for each month. First, *offending* is conceptualized as a dichotomy coded 1 for those who offended and 0 for those who did not offend in each month. Second, *crime variety* is a variety

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the variety score for the last 6 months. Frequency of offending was calculated by dividing the number of offenses in the past year by 2.

score of offending which indicates the percentage of different crimes that were endorsed during each month since the last interview.

Independent Variable

The focal predictor is conceptually identical to the measure of *exposure to Safe Streets* in Study 1 (for a more in-depth description, see pages 99-100), however, it differs somewhat in measurement for both the wave-level and month-level analyses given the retrospective nature of offending. At each interview, respondents reported on their behavior since the last interview. This is counter to the measures of perceptions of arrest risk which were concerned with the risk at that point in time. In this study, the treatment indicator needs to relate to periods for which the retrospective period was treated. For most respondents, this equates to examining the effect of Safe Streets in a future period on current reports of offending. This simply means that in order to assess the effect of Safe Streets on offending, I must go back one period to examine the offending that actually occurred once Safe Streets began.

Given the rolling nature of the interviews, there is some nuance in the way this measure is created. If respondents were interviewed within the first few months of Safe Streets (e.g., June, July, August) then they are not considered treated until the next period. If, however, they were interviewed later into Safe Streets (e.g., October, November, December), Safe Streets is coded as 1 because a large majority of their retrospective months were ‘treated.’ Ultimately, Safe Streets is coded as 1 for any respondent who’s interview occurred at a time where 66% or more of the days in their retrospective recall period occurred after Safe Streets had been in effect for at least one month, (since June 1, 2002). For those who had less than 66% of their time since the last interview after Safe Streets began, they were not considered treated until the next interview wave. The earliest date of a treated interview is September 11, 2002, where over 84% of the respondents' days since recall occurred after May 31, 2002. One limitation to this coding scheme is that in some instances, respondents had not been interviewed for up to 14 months. Therefore, they had periods where they had experienced Safe Streets for several months, but still did not surpass the threshold of having had 66% of the days since the last interview after Safe Streets began.[16]

Month-level analyses are more straightforward. In month-level analyses, respondents report on the number of offenses in each month, retrospectively. As such, all respondents are considered treated on June 1, 2002, which allows for the one month of time for respondents to perceive the Safe Streets intervention.

Time-Varying Controls

For wave-level analyses, controls are included for *street time* only. Street time is measured in the same way as previously discussed in Study 1 (see pages 99-100 for a description). Controls are not included for prior offending as it is necessarily included in the model as the outcome is the change from prior offending levels. For month-level analyses, there is no indicator for street time as only months in which the respondent spent the majority of their time in the community are included in the analyses. In both sets of models, contrary to models in Study 1, there are no controls for arrest. Arrest is only theoretically relevant in this study as it would impact one's ability to offend if the arrest resulted in incapacitation and this is already

[16] Several different conceptualizations of this measure were examined, and the results were largely consistent based on 75% of days since June 1, 2002 or based on 66% of days since May 1, 2002 (the true start date of Safe Streets).

controlled for in wave-level analyses with measures for street time and in month-level analyses by restricting the sample to only those in the community for the majority of each month.

Moderator

Neighborhood drug disorder is identical to the measure in Study 1 (see page 101). Low drug disorder is coded 1 for those that scored in the bottom quartile of drug disorder while moderate to high drug disorder is defined by those in the highest quartile of drug disorder at baseline.

Analytic Strategy

First, descriptive statistics are presented on the outcomes by interview wave in relation to Safe Streets. In addition to exploring differences by interview wave, reports of monthly-level offending in the months immediately preceding and following the start of Safe Streets are presented. As a final descriptive exercise before examining multivariable models, changes in the outcomes across periods without experiencing Safe Streets to periods where Safe Streets first begins are presented. This allows for a comparison in the changes that may have been expected without Safe Streets to the changes that actually occurred in the first period when Safe Streets began.

First-difference models are presented to examine the effect of Safe Streets on offending at the wave level. Then I present similar models which examine offending at the month level.[17] The following equations represent the focal analyses. Equations 6-8 refer to wave-level analyses

[17] It is important to note the power of these month-level models. Though each respondent has up to 47 months of data, the power is not equal to a model with 32,900 person-time periods because months were reported retrospectively at each wave interview. This correlation within waves and within person is adjusted for by clustering on the individual.

and equations 9-10 represent month-level analyses. Note that equations 6 and 9 are logistic regressions as they examine the effect of Safe Streets on a dichotomous indicator for offending, and equations 7, 8 and 10 are ordinary least squares regressions.

$$\log(\Delta o_{it} / 1 - \Delta o_{it}) = ß0 + ß1\Delta TREAT_{it+1} + ß2\Delta STREETTIME_{it} + ß3\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (6)$$

$$\Delta variety_{it} = ß0 + ß1\Delta TREAT_{it+1} + ß2\Delta STREETTIME_{it} + ß3\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (7)$$

$$\Delta\lambda_{it} = ß0 + ß1\Delta TREAT_{it+1} + ß2\Delta STREETTIME_{it} + ß3\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (8)$$

$$\log(\Delta o_{it} / 1 - \Delta o_{it}) = ß0 + ß1\Delta TREAT_{it+1} + ß2\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (9)$$

$$\Delta\lambda_{it} = ß0 + ß1\Delta TREAT_{it+1} + ß2\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (10)$$

Results

Bivariate Results

Panel A of Table 5.1 presents the prevalence, variety scores and frequency of offending at baseline, and in the interviews prior to and after the start of Safe Streets. At baseline, 88% of the sample had engaged in some form of offending in the past year, with an average of 92 offenses per person in that year. On average, respondents engaged in 18% of the 24 possible types of offenses, which translates to just over 4 different types of crimes per person. In periods *t*-3 and *t*-2, prior to the start of Safe Streets, approximately 77-78% of respondents engaged in offending, committing on average 15% of the possible types of offenses (4 crime types on average) and committed around 65-75 offenses. In the period most immediately before Safe Streets began, there is a 20% drop in the prevalence of offending and a nearly 50% reduction in the types of crimes committed per person. The frequency of offending stays rather consistent at around 65 crimes. In the first treated period, where respondents had experienced Safe Streets for at least 66% of the days in the recall period, the prevalence and frequency of offending, as well as the average variety score per person is nearly identical to the preceding period, suggesting little change when Safe Streets began. In periods following Safe Streets, crime prevalence declines further ranging from 37-43% of individuals, and the number of distinct crime types decreases. The frequency of offending does not follow such stark patterns as it fluctuates from a low of 47 offenses at time $t1$ to a high of 80 offenses at $t2$.

Panel B presents the prevalence of offending and variety scores of offending in the months immediately preceding and following the start of Safe Streets.[18] Period *m* refers to June 2002, and period *m*-1 refers to May 2002, when Safe Streets actually was first enacted. The prevalence of offending and the variety scores of offending are largely consistent across all of the months before and after Safe Streets began, ranging from 21-26% of individuals engaged in offending each month and variety scores ranging from 2% to 3%, which translate to about fewer than one type of crime per person. The results do not show much change in offending behaviors in the months prior to or following the start of Safe Streets. There are no significant differences in any months compared to June (*m*) or compared to May (*m*-1). However, it is worth noting that because monthly reports are retrospective, it is possible that the treatment itself impacted one's retrospective reports of offending, a problem that persists across all analyses in Study 2. As such,

[18] Descriptively, based on possible confounders such as age, race, sex, incarceration status and number of prior arrest (some offending covariate), the respondents who reported on months just before and just after the start date of Safe Streets do not vary significantly.

I also examined monthly frequencies and the prevalence of offending for those who were surveyed in the month immediately preceding Safe Streets and the months right after it to avoid this problem with retrospective data. The results (not reported) are substantively similar but suggest a steeper decline from approximately 32% to 20% in the prevalence of offending beginning in May and persisting in June, but these percentages are based on sample sizes of 10 and 93, respectively.

In Panel A of Table 5.2, I present the changes over time in relation to Safe Streets. Time *t - t-1* in bold represents changes from respondents' most recent interview before Safe Streets to respondents' first treated wave. Changes in this period were not significantly different than changes in the two preceding periods for offending and frequency of offending, nor for those in the community more or less than 50% of the time since the last interview. However, for respondents on average, changes in one's variety score were significantly lower in the period *before* Safe Streets compared to the period it first began. In other words, in the bivariate, there are no significant declines in offending once Safe Streets began and the only one significant different suggests that individuals' variety scores were declining more between the previous two *untreated waves* than the change once treated.

Despite not reaching statistical significance, which is perhaps due to the high variation in frequencies of offending at each period, the first treated change does represent a large decline in the frequency of offending for all respondents and for those in the community greater than 50% of the time. Prior to Safe Streets, individuals' frequencies of offending were increasing by 3.76 crimes and 8.52 crimes between waves for all respondents on average and by 6.62 and 20.65 for those in the community greater than 50% of the time. When Safe Streets began, crimes declined by approximately 30 and 35 crimes for all respondents on average and for those in the community greater than 50% of the time, respectively.

Comparing the first treated wave to later waves does show that reductions in offending, variety scores and frequency of offending declined less in later periods than when Safe Streets began. This likely occurred because offending had already declined substantially and was only reduced slightly across these later waves. It may also be due to general patterns of reductions in offending over time as respondents age.

In Panel B, changes between months before, during and after Safe Streets took place are presented. Period *m* – *m*-1 represents the change from May 2002 to June 2002. Across all changes beginning with differences from March to April and ending with changes from August to September, there are no significant differences from the changes that occurred from May to June. Though I decided *a priori* to examine offending in periods once Safe Streets had been enacted for at least one month, significance tests between all periods and the period from April to May also yielded no significant differences.

Main Results

The main results are presented in Tables 5.3 and 5.4. Table 5.3 shows the results of the wave-level analyses. In Model 1, Safe Streets is related to a 0.15 increase in the log odds of offending, which translates to a 16% increase in the odds of offending, controlling for street time and age trends, however, this result is nonsignificant, In Model 2, it appears that Safe Streets is related to a reduction in the frequency of offending. After Safe Streets began, individuals committed approximately 35 less crimes since the last interview (i.e., in the time since the last 6 months). This translates to about 1.4 fewer crimes per week. In agreement with this, Model 3 demonstrates that individuals engaged in fewer types of crime after Safe Streets began. The reduction of 0.04 translates to about 1 fewer type of crime since the last interview.

In Table 5.4, the results for the prevalence of offending and one's variety score measured at each month are presented. Again, it appears Safe Streets is not significantly related to offending. Experiencing Safe Streets is related to about a 0.29 nonsignificant increase in the log odds of engaging in crime, which is an increase in the odds of engaging in crime of about 28%. Safe Streets is, however, related to a 0.02 reduction in one's variety score. This translates to a reduction of about 1 crime type in the month when Safe Streets first began.

Sensitivity Analyses

The results of the main analyses suggest no change in the prevalence of offending after experiencing Safe Streets. However, the rate of offending and the diversity of crime types might have been negatively impacted after Safe Streets. several sensitivity analyses are conducted to test if these patterns are consistent when using a different empirical strategy. As the number of time periods increases, fixed-effects models are more efficient than the first-difference estimator. Fixed effects models are estimated on both wave-level and month-level analyses. These models are more common than first-difference models in the criminological literature, especially when the number of time periods is large. Month-level analyses include over 12,000 observations periods across up to 47 months.

In Table 5.5, Model 1 shows that Safe Streets appears to be negatively related to offending, where Safe Streets translates to a 0.79 decrease in the log odds (55% reduction in the odds) of engaging in offending. Models 2 shows that the frequency of offending is also reduced, though this effect does not reach statistical significance. Finally, in Model 3, it appears Safe

Streets is related to a 0.04 reduction in one's variety score. In Table 5.6, Model 1 suggests that Safe Streets is related to a reduction in offending though this estimate also does not reach conventional levels of statistical significance. Model 2 suggests that Safe Streets had no effect on one's frequency of offending at each month. Taken together, these results provide further suggestive evidence that Safe Streets may have reduced offending in some ways. The small effects from the month-level analyses and the larger effects from the fixed-effects models suggest perhaps that offending prevalence did decrease but the effect occurred over a longer period than just in the first wave (i.e., first few months) or first month. These results also suggest that there are likely declines in offending over time as respondents age.

Robustness Checks

Given the variation in results across measurement periods (waves or months) and variation when examining first-difference or fixed-effects models, models are estimated to first examine the effects moderated by neighborhood drug disorder, as in Study 1, to next examine models which focus explicitly on drug selling and crimes which are typically associated with drug-market related violence, and finally to assess several conceptually related outcomes to help paint a better picture of the effects of Safe Streets.

In Table 5.7, models are estimated which examine the focal outcomes of the prevalence of offending, frequency of offending and variety scores by neighborhood drug disorder at baseline. Panel A examines the effect of Safe Streets for those living in low drug disorder communities at baseline; these respondents are assumed to be less likely to experience treatment or less likely to experience high dosages of treatment. The results are similar to the main analyses, with no significant effect on the prevalence of offending. The impact on the frequency of offending is reduced in magnitude compared to the main analyses and now does not reach statistical significance. The results for one's variety score suggest similar reductions in the variety score of offending for respondents from low drug disorder neighborhoods at baseline compared to the entire sample. These estimates for all three outcomes are not significantly different from the main estimates (Table 5.3) based on z-tests.

Panel B reports the same analyses for respondents who lived in moderate to high drug disorder communities at baseline, meaning those who were more likely to have been treated or treated at higher dosages. The effects of Safe Streets on the prevalence of offending and one's variety score are similar to the main results and the results for those who lived in low drug disorder at baseline. However, the effect of Safe Streets on the frequency of offending appears to be greater for those from moderate to high drug disorder neighborhoods, but this effect is not significantly distinguishable from the main results (Table 5.3) nor from the effect for those from low drug disorder neighborhoods at baseline.

Table 5.8 examines the effect of Safe Streets on the prevalence and frequency of drug selling and possible drug-market related violence. *Drug selling* is measured as a dichotomous indicator denoting sales of either marijuana or other drugs, while *drug selling frequency* is the number of times either of these were sold in the recall period. *Drug market-related violence* includes all forms of violence which may possibly be related to drug selling including murder, shootings, robberies with and without weapons, causing serious injury, fights, gang fights and carrying a gun. *Drug market-related violence frequency* is the combined number of times that any of these crimes were committed in the recall period. For both frequency measures, frequencies at baseline were divided by two as these frequencies refer to the prior year and not the prior six months.

Safe Streets appears to significantly increase the prevalence of drug selling and drug market-related violence, contrary to expectations. The odds of selling drugs and committing violence are increased by 57% and 39%, respectively. On the other hand, it appears that Safe Streets reduced the frequency of drug selling and violence by 46 and 14 incidents, respectively. These patterns are consistent with the main analyses, suggesting that the likelihood of engaging in any crime is increased or unaffected, while the frequency with which one commits crime is decreased.

Tables 5.9 and 5.10 examine outcomes related to offending. Theoretically, if Safe Streets was designed to reduce crime and disorder, then in addition to impacting offending in the current sample, it is possible that it could also impact one's ability to buy a gun, the cost of that gun, reports of peer offending, victimization and witnessing other's victimizations, as well as the prevalence of illegal work and illegal earnings.

At the wave-level, I examine outcomes related to gun purchasing, peer offending, and exposure to violence. *Difficulty of gun buying* is based on responses to the item, "If a young person in this neighborhood wants to buy a gun, he/she can." Respondents answer based on a 5-point Likert scale ranging from strongly agree (0) to strongly disagree (5), making higher scores indicative of greater difficulty buying a gun. Gun price is the monetary amount in US dollars that respondents report it would cost to buy a 9mm and .38, respectively. *Peer offending* is a measure of the prevalence of offenders in one's friend group, assessed by asking respondents, "How many of your friends have: purposely damaged or destroyed property that did not belong to them, hit or threatened to hit someone, sold drugs, gotten drunk once in a while, carried a knife, carried a gun, owned a gun, gotten into a physical fight, stolen something worth more than $100, taken a motor vehicle or stolen a car and gone in or tried to go into a building to steal something"? Responses include (1) None of them, (2) Very few of them, (3) Some of them, (4) Most of them, and (5) All of them. The average score on these 11 items is included as peer offending, ranging from 1 to 5. *Exposure to violence (self)* is a measure of the count of how many of the following 6 questions were endorsed; "Have you been chased where you thought you might be seriously hurt?, Have you been beaten up, mugged, or seriously threatened by another person?, Have you been raped, had someone attempt to rape you or been sexually attacked in some other way?, Have you been attacked with a weapon, like a knife, box cutter, or bat?, Have you been shot at?, and, Have you been shot?" *Exposure to violence (witnessed)* similarly measures the count of the following 7 items; "Have you seen anyone get chased where you thought they could be seriously hurt?, Have you seen anyone else get beaten up, mugged, or seriously threatened by another person?, Have you seen someone else being raped, an attempt made to rape someone, or any other type of sexual attack?, Have you seen someone else get attacked with a weapon, like a knife, box cutter, bat, chain, or broken bottle?, Have you seen someone else get shot at? , Have you seen someone else get shot?, Have you seen someone else get killed as a result of violence, like being shot, stabbed, or beaten to death?"

The results presented in Table 5.9 suggest that Safe Streets may not have made it more difficult to find a gun to purchase, but it may have increased the price. Safe Streets is related to a significant increase of about $35 and $31 for a 9-millimeter and 38 gun, respectively. Furthermore, Safe Streets is related to a 0.15 decrease in friends who engage in offending measured on a 5-point scale. Finally, Safe Streets appears to be negatively related to being victimized and personally witnessing the victimizations of others. Safe Streets is related to reductions of 0.26 and 0.76, respectively.

At the month-level, the prevalence of illegal work and the income earned from illegal work is examined. *Illegal work* is a dichotomous indicator specifying whether or not the respondent engaged in any form of illegal work during that month. Respondents are asked "Have you made money other ways, including from activities that are illegal?" If respondents answered yes, they are coded 1 for that month and 0 if they answered no. *Illegal earnings* is the sum of money earned illegally from selling stolen property, stealing property, selling drugs, gambling, prostitution or any other form of illegal work. Illegal earnings are measured in two ways; first by including all respondents where those who do not work illegally are included as earning $0, second by examining only those who report any illegal work.

In Table 5.10, the results suggest that Safe Streets was negatively, but not significantly, associated with decreased engagement in illegal work (Model 1) and decreased earnings (Model 2). Safe Streets is related to a 0.75 reduction in the log odds of working illegally and a reduction of about $24 in earnings. Model 2 examines earnings for all respondents, meaning those who were working illegally but then stopped would be included as they now earned $0. In Model 3, I examined just those who reported working illegally in the current period. For those who are currently working, illegal earnings were reduced by approximately $1620 once Safe Streets began. However, these results do not reach statistical significance. Unfortunately, the sample is reduced substantially to only 157 individuals so there is limited power to detect significant differences in earnings.

Placebo Tests

The results of the focal analyses, sensitivity tests and robustness checks amount to somewhat ambiguous evidence on the role of Safe Streets on offending. Given the results thus far, it appears that Safe Streets did not deter individuals from engaging in crime, but it may have reduced the frequency of offending, and caused individuals to engage in slightly fewer crime types. These conclusions are bolstered by the significant reductions in peer offending, victimization and violence exposure, and significant increases in gun costs. Nonetheless, the non-significant effects for difficulty purchasing a gun, and frequency of offending, illegal work and illegal earnings at the month-level, raise questions.

To assess if the significant relationships between Safe Streets and frequency of offending and variety scores are true effects of Safe Streets or if they suggest pre-existing patterns of declining offending two forms of falsification tests are examined. First, models are re-estimated assuming that Safe Streets began 6 months prior to its start. In these models, respondents are considered treated in the first wave after December 1, 2001 (wave-analyses) or in the month of December 2001 (month-analyses). Next, models are presented examining the effect of Safe Streets on the sample of respondents from Phoenix County. As discussed in Study 2, these respondents were demographically similar in age and offending behavior and answered identical surveys to the focal respondents from Philadelphia. Wave analyses and month analyses are presented in Models 1-3 and Models 4-5 of Table 5.9, respectively, while Panel A refers to in-time placebo models assuming Safe Streets began earlier than it did and Panel B examines the effect of Safe Streets on the respondents from Phoenix County, who did not experience the treatment. The results of these models would raise concerns about the prior findings if the same or similar patterns of reductions in frequency of offending and variety scores were found in periods prior to Safe Streets and in the sample of adolescents who did not experience Safe Streets.

Panel A suggests that offending prevalence was not changing substantially before Safe Streets, and that the frequency of offending was increasing while variety scores were declining significantly. At the month level, it looks like the prevalence of offending was increasing 6 months before Safe Streets, but again, variety scores were declining. The results do not refute the main findings and in some ways are supportive of a true deterrent effect of Safe Streets because the frequency of offending was increasing before Safe Streets and offending prevalence at the month level was also increasing.

The results of Panel B are somewhat more concerning. The results are remarkably similar to the main analyses, suggesting that the effects of Safe Streets on respondents from Philadelphia are likely not true effects but rather artifacts of the trends in offending that seem to be happening for all respondents in the Pathways study. However, the reduction in the frequency of offending by 23 offenses per period for respondents from Phoenix is significantly smaller than the reduction in the frequency of offending for respondents from Philadelphia (35 crimes). Unfortunately, it is difficult to directly compare these results given vast differences in the contexts for Philadelphia and Phoenix and differences that may exist between respondents in each site. If these locations were more comparable, a difference in difference design would have been deployed initially to directly test if the changes in Philadelphia were different than the changes over the same period in Philadelphia. As such, these analyses should be viewed as supplemental to support the main findings and not necessarily as tests of the effects on a true untreated control group..

Taken together, these placebo models offer some support of a possible negative effect of Safe Streets on the frequency of offending and variety scores of offending, but the results also raise concerns that the original analyses detected the effect of pre-existing declines in offending prevalence, frequency and variety scores over time. The in-time placebo results in Model 2 which denote increases in the frequency of offending for the focal sample before Safe Streets began, as well as the significantly smaller decline in the frequency of offending for the Phoenix sample do offer some support that the results from the main analyses are detecting a true effect of Safe Streets. The remaining models do also suggest, in line with the focal analyses, that Safe Streets did not impact the prevalence of offending.

Discussion

The results of the current study demonstrate some evidence that Safe Streets may have been related to a small decline in the frequency of offending, but did not impact the prevalence of offending nor did it likely impact one’s variety score of offending. This conclusion is reached based on various model specifications, measures of the prevalence and variety score of offending as aggregates reports since the last interview and retrospectively monthly reports, an explicit focus on crimes which were most likely to have been impacted by Safe Streets and several falsification tests. The effects for related outcomes lend further support that Safe Streets likely did impact crime and offending behavior in some way. Gun costs were increased, peer offending decreased, victimizations decreased, and witnessing violence decreased after Safe Streets began.

Nevertheless, the results taken together do raise some questions about the analysis and the ability to detect effects in this dataset. First, is it possible that the results are picking up a ‘regression to the mean’ because rates of offending were so high upon entrance to the study? The prevalence of offending was not quite 100% at baseline as the question referred to offending in the past year, but nonetheless, individuals may have already decided to desist from crime for a host of reasons, one being that their adjudication for the offense which made them eligible for the study exuded some deterrent effect. Scholars now tend to believe that deterrence is a process that extends over time (Loughran, Nagin, & Nguyen, 2017), so the empirical findings from this study may be detecting deterrence processes that were already put into place.

Second and relatedly, are the results driven by ongoing declines in offending during this period in the life course? The adolescents in this sample were on average 17 years of age when Safe Streets began. According to Hirschi & Gottfredson (1983), in both self-report and official data, crime increases until around age 18 and then sharply declines. Since their writing on the relationship between age and crime, many others have demonstrated similar patterns in offending by age. Some have shown that arrest-rates peak at age 17 for robbery and burglary, but around age 21 for aggravated assault (Blumstein, Cohen, & Farrington, 1988). Others have noted that the age of the onset of offending peaks at age 16 before declining sharply at 17 (Monahan, Steinberg, & Cauffman, 2009; Wolfgang et al., 1972). Given this evidence, it is possible that on average, crime was already declining for this sample before Safe Streets began or that a decline would have occurred due to aging regardless of Safe Streets. These possibilities made it impossible to fully identify the effect of the intervention without a comparable control group.

Third, are the significant reductions in variety scores found from in-time placebo models the results of anticipation effects? These anticipation effects would occur if respondents began reducing their offending if they knew a surge in policing were about to take place. This is unlikely for the sole reason that Operation Safe Streets was designed just one month prior to its inception. In April of 2002, the current Police Commissioner Sylvester Johnson and current Mayor John F. Street held a series of meetings to design the strategy (Philadelphia Police Department, 2002). Instead, it seems more likely that the results of the placebo model are evidence that offending was already declining for these adolescents and this was simply occurring as Safe Streets was taking effect.

The discrepancies in the results across models and the results of the falsification tests argue against a significant effect of Safe Streets, but the consistent findings for reductions in the frequency of offending and the significant impact on related outcomes like gun costs, peer offending and violence exposure suggest that Safe Streets likely had *some* impact on offending. The effect may, unfortunately, not be easily identified in this dataset but it also may be that the true effects are really just as nuanced and complex as the results suggest. Safe Streets may very well have resulted in reductions in the frequency of offending, but had little impact one decreasing one's variety score or likelihood of desisting from crime entirely in the period when it first began.

It is helpful to view these findings alongside the significant reductions in crime that were found after Safe Streets occurred. The findings of the current study do not negate or refute previous findings which showed that Safe Streets was related to declines in crime (Lawton et al., 2005). It is possible that many more potential offenders in Philadelphia were deterred beyond the 700 respondents from the Pathways study. However, the results of the current study do raise some important points. Is it possible that Safe Streets diverted offending to adjacent communities, but did little to slow involvement in crime? Or is it possible that Safe Streets just did not fully deter offenders, as evidenced by the nonsignificant effects on offending prevalence, but rather resulted in some 'restrictive' deterrence, where individuals began engaging in fewer crimes after Safe Streets? Unfortunately, the current study was unable to answer these questions more comprehensively.

The current study also examined the effect of Safe Streets on specific crimes which were targeted by Safe Streets such as drug selling and drug-related violence. The measures of drug selling were limited to items which asked about marijuana and all other crimes. Unfortunately, it was impossible to further disaggregate into narcotics specifically which was the intended target of Safe Streets. Furthermore, the assumption was made that particular forms of violence, such as robbery and shootings were related to drug markets. Unfortunately, in the data, it is impossible to distinguish violence which was directly related to drug-markets, such as violence which resulted may have resulted from feuds over drug selling territory (e.g.. gang shootings) or violence which occurred in an effort to acquire money for drugs (e.g., robberies). Future evaluations of such targeted interventions should explicitly ask questions regarding the motives for violence in order to assess if drug-market related violence is actually directly impacted. It may also prove useful to examine crime specific models can be examined to assess if the effects of Safe Streets vary by certain types of crime as there is reason to suspect that deterrence varies by crime type (Paternoster, 1986).

These possibilities should be explored in future research which is explicitly designed to examine the effects of hot spots policing on individual offending behavior. The current study was an important step toward examining these outcomes, and demonstrates the potential importance of understanding the nuance between crime rates and individual-level offending, but the study does suffer from some limitations. This includes the retrospective reports of crime that were recorded in some cases, around a year after the crimes were initially committed. The current study also suffers from the inability to examine a true control group which is desirable to examine for example, differences in differences in offending across persons over time. Finally, the small sample size meant it was not possible to examine variation in treatment effects by age or individual-level traits. It is possible that these exist given research on the age-crime curve and variation in the ‘deterrability’ of individuals. Future research should prioritize addressing these limitations to better understand the effect of hot spots policing on individual-level offending behavior.

Designing an experimental or quasi experimental study with the intention of examining individual offending experiences is perhaps a best next step. Furthermore, incorporating questions which explicitly ask individuals whether they perceive changes in policing and whether or not they believe these changes altered their offending are important components to study in order to fully understand how police interventions like Safe Streets impact individuals’ behavior. This nuance is an important next step for criminological theory as well because different theoretical frameworks would propose differential impacts of the prevalence versus frequency of offending. Further understanding if police interventions can be effective by resulting in full desistance from crime versus restrictive deterrence (where one offends less frequently) is an important area for future research.

Tables

Table 5.1. Offending by Safe Streets Timing

Panel A - Wave	*Baseline*	*t-3*	*t-2*	*t-1*	***t***	*t1*	*t2*	*t3*	*t4*
Offending	88.14%	0.78 ***	0.77 ***	0.57	**0.54**	0.43 ***	0.40 ***	0.37 ***	0.38 ***
Variety Score	0.18	0.15 ***	0.15 ***	0.08	**0.08**	0.05 ***	0.05 ***	0.04 ***	0.05 ***
Frequency of Offending	91.78	75.20	64.81	64.84	**64.01**	46.59	80.20	67.25	77.04
n	700	232	394	450	**449**	461	469	467	409
Panel B - Month	*m-4*	*m-3*	*m-2*	*m-1*	***m***	*m1*	*m2*	*m3*	*m4*
Offending	-	25%	26%	23%	**24%**	21%	23%	25%	25%
Variety Score	-	0.03	0.02	0.02	**0.02**	0.02	0.03	0.03	0.03
n		159	174	182	**192**	207	218	224	232

Notes. The period when Safe Streets first began is noted in bold. Sample sizes are reported for the maximum number of interviews for each time period.

[a] Significant differences (p <0.05) from means at time t based on t-tests

Table 5.2. Changes in Offending by Safe Streets Timing

	Average Within Person Change											
Panel A - Waves	*t-2 - t-3*		*t-1 - t-2*		***t- t-1***		*t1 - t*		*t2 - t1*		*t3 - t2*	
Offended	-0.18		-0.20		**-0.16**		-0.13		-0.08	*	-0.05	*
Street time > 50%	-0.17		-0.14		**-0.10**		-0.10		-0.07		-0.05	
Street time <= 50%	-0.21		-0.35		**-0.38**		-0.29		-0.10	*	-0.10	*
Variety Score	-0.03		-0.06	*	**-0.05**		-0.04		-0.02	***	-0.01	***
Street time > 50%	-0.02		-0.04		**-0.04**		-0.03		-0.01	*	0.00	***
Street time <= 50%	-0.09		-0.11		**-0.09**		-0.06		-0.06		-0.03	*
Frequency of Offending	3.76		8.52		**-30.42**		-5.59		28.82	*	-2.11	***
Street time > 50%	6.62		20.65		**-35.23**		2.92		31.07	*	11.88	**
Street time <= 50%	-5.12		-22.46		**-9.97**		-51.92		14.99		-98.89	
n	155		315		**447**		461		469		465	
Panel B - Months	*m-2 - m-3*		*m-1 - m-2*		***m - m-1***		*m1 - m*		*m2 - m1*		*m3 - m2*	
Offended	-0.04		-0.08		**-0.08**		-0.06		-0.03		-0.02	
Variety Score	-0.06		-0.02		**-0.05**		-0.01		0.02		-0.02	
n	182		192		**207**		217		224		232	

Notes. The period when Safe Streets first began is noted in bold. Significance stars denote comparisons to changes from *t* to *t1* based on Rank Sum tests.

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Table 5.3. First-Difference Longitudinal Regressions Predicting Offending (Wave)

	Offended		*Frequency of Offending*		*Variety Score*	
	Model 1		Model 2		Model 3	
	b	SE	b	SE	b	SE
Safe Streets	0.15	0.10	-35.36 *	13.66	-0.04 ***	0.01
Street time	0.22 *	0.11	-19.56	14.80	-0.02 *	0.01
n = persons(person-waves)			642(2,994)			

Notes. SE = robust standard error

*p< 0.05; **p <0.01; ***p< 0.001

Table 5.4. First-Difference Longitudinal Regressions Predicting Offending (Month)

	Offended		*Variety Score*	
	Model 1		Model 2	
	b	SE	b	SE
Safe Streets	0.29	0.18	-0.02 *	0.01
n = persons(person-waves)	396(12,513)		396(12,525)	

Notes. SE = robust standard error
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Table 5.5. Fixed-Effects Longitudinal Regressions Predicting Offending (Wave) - Sensitivity Tests

	Offended			*Frequency of Offending*		*Variety Score*		
	Model 1			Model 2		Model 3		
	b		SE	b	SE	b		SE
Safe Streets	-0.79	***	0.15	-16.91	9.73	-0.04	***	0.01
Street time	0.84	***	0.19	24.40	14.04	0.06	***	0.01
Constant				31.02	20.06	0.12	***	0.02
n = persons(person-waves)	508(3,093)			700(3,792)				

Notes. SE = robust standard error
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Table 5.6. Fixed-Effects Longitudinal Regressions Predicting Offending (Month) - Sensitivity Tests

	Offended		*Variety Score*	
	Model 1		Model 2	
	b	SE	b	SE
Safe Streets	-0.16	0.11	0.00	0.00
Constant			0.03 ***	0.01
n = persons(person-waves)	294(9,422)		397(12,747)	

Notes. SE = robust standard error

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Table 5.7. First-Difference Longitudinal Regressions Predicting Offending - Robustness Checks (Wave) - Moderated by Neighborhood Drug Disorder

	Offended		*Frequency of Offending*		*Variety Score*	
	Model 1		Model 2		Model 3	
Panel A - Low Drug Disorder	b	SE	b	SE	b	SE
Safe Streets	0.19	0.15	-16.68	18.47	-0.04 ***	0.01
Street time	-0.01	0.16	-29.17	21.54	-0.02	0.01
n = persons(person-waves)	279(1,356)		279(1,354)			
Panel B - Moderate/High Drug Disorder	b	SE	b	SE	b	SE
Safe Streets	0.05	0.18	-59.16 *	26.67	-0.05 ***	0.01
Street time	0.33	0.18	-8.60	26.24	-0.01	0.01
n = persons(person-waves)			223(985)			

Notes. SE = robust standard error
**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 5.8. First-Difference Longitudinal Linear Regressions Predicting Drug Outcomes - Robustness Checks (Wave)

	Drug Selling			*Drug Selling Frequency*			*Drug Market-Related Violence*			*Drug Market-Related Violence*		
	Model 1			Model 2			Model 3			Model 4		
	b		SE	b		SE	b		SE	b		SE
Safe Streets	0.45	***	0.11	-46.46	*	20.41	0.33	**	0.11	-14.15	***	3.34
Street time	-0.06		0.13	-22.15		20.14	0.19		0.11	4.38		4.83
n = persons(person-waves)	642(2,998)											

Notes. SE = robust standard error

*p< 0.05; **p <0.01; ***p< 0.001

Table 5.9. First-Difference Longitudinal Linear Regressions Predicting Related Outcomes - Robustness Checks (Wave)

	Difficulty of Gun Buying		*9mm Gun Cost*		*38 Gun Cost*		*Peers Variety Score*		*Personal Violence*		*Witnessed Violence*	
	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE
Safe Streets	-0.09	0.07	34.90 *	15.93	30.77 *	14.09	-0.15 ***	0.04	-0.26 ***	0.06	-0.76 ***	0.12
Street time	0.25 ***	0.07	-16.11	18.28	-31.66	16.46	-0.10 *	0.05	-0.01	0.06	0.05	0.13
n = persons(person-waves)	631(2,780)		408(1,274)		390(1,162)		639(2,960)		642(2,994)		642(2,994)	

Notes. SE = robust standard error

**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 5.10. First-Difference Longitudinal Regressions Predicting Related Outcomes - Robustness Checks (Month)

	Illegal Work		*Illegal Earnings*		*Illegal Earnings of Illegal Workers*	
	Model 1		Model 2		Model 2	
	b	SE	b	SE	b	SE
Safe Streets	-0.75	0.47	-23.87	43.50	-1620.10	1093.21
n = persons(person-waves)	395(12,267)		395(12,263)		157(237)	

Notes. SE = robust standard error

**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 5.11. First-Difference Longitudinal Regressions - Placebo Tests

	Offended			*Frequency of Offending*			*Variety Score*			*Offended*			*Variety Score*		
	Model 1			Model 2			Model 3			Model 4			Model 5		
Panel A - In-Time Placebo	b		SE	b		SE	b		SE	b		SE	b		SE
Safe Streets	-0.02		0.15	8.41		11.45	-0.02	*	0.01	1.34	***	0.15	-0.11	***	0.01
Street time	0.25	*	0.10	-28.36	*	14.26	-0.03	***	0.01						
n = persons(person-waves)	642(2,996)			642(2,994)			642(2,994)			397(12,731)			397(12,743)		
Panel B - Phoenix Sample															
Safe Streets	0.06		0.11	-23.49	**	8.61	-0.03	***	0.01	0.37	*	0.17	0.00		0.01
Street time	0.51	***	0.14	0.18		12.58	-0.05	***	0.01						
n = persons(person-waves)	605(2,953)			605(2,952)			605(2,952)			433(14,165)			433(14,184)		

Notes. SE = robust standard error, Models 1-3 are wave-level analyses and Models 4-5 are month-level analyses

*p< 0.05; **p <0.01; ***p< 0.001

Chapter 6. STUDY 3

"I'm all for getting rid of drugs, but I should be able to go into the corner store without police asking, 'What are you doing?' or telling you to get out of the store"

Philadelphia Resident when asked about Operation Safe Streets, 2002 (Moran et al., 2002)

When evaluating policing strategies and specific policing interventions, it is of course important to think about the possible benefits, but also the potential costs. Weighing these costs and benefits can be difficult as there are many factors to consider. It is important to reduce the negative effects of these interventions as much as possible, and maximize the gains. In order to make an intervention as effective as possible, it is also important to ensure that the negative effects themselves do not counteract the gains in order. For example, if reductions in crime occur immediately, but perceptions of police are harmed in such a way that crime ends up returning at higher rates in later months, this would not be an effective strategy.

It has been accepted that police will be the most successful at reducing crime when they can positively impact perceptions of arrest risk. Though the impact on perceptions of arrest risk was confirmed in Study 1 and some reductions in offending were confirmed in Study 2, additional factors may impact how effective Safe Streets was on crime and offending. For example, policing strategies such as hot spots policing, are expected to be the most effective at reducing crime when they do not negatively impact perceptions of procedural justice and police legitimacy and when they do not increase feelings of legal cynicism. Additionally, these strategies' effectiveness are believed to be maximized when neighborhood disorder is also reduced. Finally, if these policing strategies have iatrogenic effects by increasing the thrill or social rewards derived from crime, any gains in perceptions of arrest risk (which reduce offending) may be offset.

The concerns over the negative impacts of hot spots policing specifically are rooted in the idea that heightened levels of police presence (i.e., hot spots policing) may coincide with both objective and subjective increases in contact with citizens and unfair treatment (Ratcliffe et al., 2015). These potential 'backfire effects' (e.g., negative consequences) (Weisburd, 2016; Weisburd, Hinkle, Famega, & Ready, 2011) are important to consider when evaluating the success of hot spots policing interventions (Kochel, 2011; Rosenbaum, 2006), especially when considering the likelihood of sustained, long-term crime reductions (Kochel & Weisburd, 2017). Recent research has concluded that hot spots policing typically does not result in these deleterious backfire effects for community residents (National Academies of Sciences, Engineering, and Medicine, 2018). However, these studies more often than not examine samples of law-abiding citizens who are likely to be older, female, or more educated than the average community member (Ratcliffe et al., 2015). In other words, the effect of hot spots policing on these outcomes for those who are engaged in criminal activity and likely to experience police contact because of the hot spots policing intervention is unclear (Weisburd & Telep, 2014). Furthermore, the lack of attention on outcomes such as the social or personal rewards to crime have made it impossible to assess all the factors that offenders may include in their choice calculus when deciding whether or not to offend.

Theoretical Expectations for Operation Safe Streets

In regard to the theoretical expectations of Operation Safe Streets on perceptions of police and legal cynicism, it is important to distinguish that Operation Safe Streets was not designed specifically to be a "procedurally just" intervention, as many policing interventions have been, nor was it designed to include aspects of procedural justice as several hot spots policing interventions have done (see Higginson & Mazerolle (2014) for a review of procedurally just hot spots policing interventions). However, “Safe Streets Officers”, those officers assigned to patrol Safe Streets sites, were required to watch a two-part training video which described a typical street-level narcotics operation, and addressed the legal principles and policies related to drug enforcement. These officers were also provided a written document describing these trainings and tips on how to handle similar situations. Community input was also sought throughout the intervention through the media, neighborhood, business and civic associations, town watch groups and police district advisory councils (Philadelphia Police Department, 2002).

Because of this, as well as the focus on police presence as the mechanism to reduce crime as opposed to increased arrests and enforcement, the framework of procedural justice, police legitimacy and legal cynicism would not necessarily predict a beneficial effect for these outcomes. But these frameworks also would not predict large deleterious impacts as efforts were taken to attempt to prevent any deleterious consequences. Nonetheless, some anecdotal evidence, such as the quote by one resident which opened this chapter, suggests that it is possible that Safe Streets was too intrusive and negatively impacted citizens lives. Given this and the limited prior literature which has explored perceptions of procedural justice, police legitimacy and legal cynicism for those likely to notice and experience differences in policing given heightened police presence, theory and prior literature do not offer a clear expectation about how any increase in police presence would be perceived by a sample of previously adjudicated respondents, nor adolescent respondents.

In regard to disorder, theory would predict that an increase in police presence, particularly police aimed at decreasing disorder or drug disorder as Safe Streets Officers were told to do, would result in a decrease in disorder. It is worth noting that according to Broken Windows theory, Kelling & Wilson (1982) highlight the need to target those communities on the margins for crime and disorder, rather than those which are already severely impacted by crime. Safe Streets, however, targeted all of those communities with the highest crime so it is possible that reductions in disorder either did not occur, or did not result in increased citizens engagement in the community, informal social control, or reduced fear of crime and disorder.

Finally, given existing theoretical predictions and prior research, it is expected that Safe Streets would result in an increase in the personal and social rewards to crime. Because Safe Streets was designed to make drugs harder to come by, and to increase perceptions of arrest risk, which was confirmed in Study 1, it is expected that crime would become more thrilling as it is now more difficult. Crime may also have become more socially rewarding as a result, especially if certain crimes, such as drug selling, became less common as it became harder to acquire drugs. Media reports which included quotes from active drug users confirmed this point, as did information from the Philadelphia Police Department. The Philadelphia Police Department noted that informants commented on the difficulty in purchasing drugs in areas which were previously easy to acquire drugs and at least one active drug user was quoted, saying “They got two cops on pretty much every drug corner in Kensington, you got to pretty much hunt around for hours [to find drugs]” (Moran et al., 2002). Given these expectations, I have formulated several hypotheses.

Hypotheses

H6.1: Operation Safe Streets will have no effect on perceptions of procedural justice in the period (wave) immediately following its implementation.

H6.2: Operation Safe Streets will have no effect on perceptions of police legitimacy in the period (wave) immediately following its implementation.

H6.3: Operation Safe Streets will have no effect on legal cynicism in the period (wave) immediately following its implementation.

H6.4: Operation Safe Streets will lead to a decrease in neighborhood disorder in the period (wave) immediately following its implementation.

H6.5: Operation Safe Streets will lead to an increase in the personal rewards to crime in the period (wave) immediately following its implementation.

H6.6: Operation Safe Streets will lead to an increase in the social rewards to crime in the period (wave) immediately following its implementation.

Study Contributions

The current study is one of the first to examine a host of theoretically motivated outcomes which are believed to be impacted by hot spots policing or increases in officer presence on a sample of those likely to offend or actually experience the change in policing. The focus on a sample of previously adjudicated adolescents is ideal as it focuses on those likely to experience police contact as a result of the intervention and for those most likely to be engaged in crime, given prior offending experiences and age. This is also one of, if not the first study, to examine the impact of hot spots policing and subsequent changes in objective apprehension risk, on the social and personal rewards to crime.

Sample Restrictions

The current study restricts the sample similarly to Study 1. Those who were not in the community for at least 10% of the days since the last interview were dropped. To maximize power, all possible cases are retained for each outcome, so sample sizes vary across outcomes. These restrictions result in a final analytic sample of over 650 respondents and over 3,000 person-waves for all outcomes except for neighborhood disorder, which has a sample size of 609 and 2,485 person-waves. For fixed-effects models, more cases are preserved, resulting in 699-700 persons and over 3,700 person-waves for each model.

Measures

Dependent Variables

Perceptions of procedural justice is measured using the mean of scores on the Procedural Justice Inventory. This scale includes items regarding procedural justice based on personal experiences with police[19] (alpha 0.74 at baseline; alphas ranging from 0.73 to 0.75 for waves 1-6). For the majority of respondents, this measure is based on responses to 14 items, 12 of which refer to recent contacts or experiences with police. For those respondents who were not picked up or accused by police in the recall period, this measure is based on responses to 2 questions only (i.e., “Even after the police make a decision about arresting me, there is nothing I can do to appeal it; Even after the police make a decision about arresting me, someone in higher authority can listen to my case, and even in some cases, change the decision.”) The majority of items are measured on a Likert scale ranging from strongly disagree (1) to strongly agree (5). Negative items are reverse coded. The final scale ranges from 1 to 5, with higher scores denoting more

[19] Summary scores of Procedural Justice, which include both judges and police are commonly used in studies which rely on the Pathways to Desistance data (see Fagan & Piquero, 2007; Piquero, Fagan, Mulvey, Steinberg, & Odgers, 2005), however, the current study focuses specifically on questions related to perceptions of police.

positive perceptions of procedural justice. For further description of the list of questions, see Table 3.1 and for more explanation of the coding scheme see the Pathways to Desistance website at www.pathwaysstudy.pitt.edu.

Perceptions of police legitimacy is a composed of the average of 6 items[20] which ask respondents to rank how much they agree or disagree with each of the following on a four-point Likert scale: I have a great deal of respect for the police; Overall, the police are honest; I feel proud of the police; I feel people should support the police; The police should be allowed to hold a person suspected of a serious crime until they get enough evidence to charge them; and, The police should be allowed to stop people on the street and require them to identify themselves.

Legal cynicism is a scale composed of the average of 5 items which ask respondents to rank how much the agree or disagree with each of the following on a four-point Likert scale; Laws are meant to be broken; It is okay to do anything you want; There are no right or wrong ways to make money; If I have a fight with someone, it is no one else's business; A person has to live without thinking about the future.

Neighborhood disorder (self) is composed of 12 questions regarding the prevalence of physical disorder (i.e., cigarettes on the street or in the gutters; garbage in the streets or on the sidewalk; Empty beer bottles on the streets or sidewalks; boarded up windows on buildings; graffiti or tags; graffiti painted over; gang graffiti; abandoned cars; empty lots with garbage; condoms on sidewalk; needles or syringes; political messages in graffiti), and 9 questions regarding the prevalence of social disorder (i.e., gangs (or other teen groups) hanging out; Adults hanging out on the street; people drinking beer, wine or liquor; people drunk or passed out; adults fighting or arguing loudly; prostitutes on the streets; people smoking marijuana; people

[20] Five additional items regarding the legitimacy of the courts are available, however, these are theoretically distinct from perceptions of police, which is the focus of the current intervention and analysis.

smoking crack; people using needles or syringes to take drugs). For each item, respondents assess how often each of these types of disorder occurs in their neighborhood from (1) never to (4) often. At each period, respondents only report this measure for the neighborhood in which they lived for the longest period of time since the last interview. If they lived in two different places for the same amount of months in the recall period, they answer about the more recent location. If a respondent is detained for the entire recall period, these items are skipped.

Personal rewards to crime refers to the average of 7 items regarding “How much ‘thrill’ or ‘rush’ is it to do any of the following things?” including fighting, robbery with a gun, stabbing someone, breaking into a store or home, stealing clothes from a store, vandalism and auto theft. These are the same seven crimes for which perceptions of arrest risk are measured. For those who have never done these crimes, respondents are asked to report how much thrill or rush they think it would be if the committed these acts. The outcomes range from (0) no fun or kick at all to (10) a great deal of fun or kick.

Social rewards to crime are assessed for stealing, fighting and robbery. For each crime type, respondents are asked to assess their level of agreement on a 4-point Likert scale from (1) strongly disagree to (4) strongly agree with the following statements; If I take things; beat someone up; rob someone: Other people my age will respect me more; I'll get more respect from adults in my neighborhood; People my age will be afraid to mess with me; I'll impress my boyfriend (or girlfriend); and finally, I can get back at someone: who took things from me; who messes with me; if I take things from them; beat up him (or her) or someone close to him (or her); rob him (or her) or someone close to him (or her). The average score on these 15 items is used to assess the average social rewards to crime, ranging from 1 to 4, with higher scores denoting greater social rewards.

Independent Variable

The focal predictor is identical to the measure of *exposure to Safe Streets* in Study 1 (for a more in-depth description, see pages 99-101). Respondents are considered ‘treated’ if they are interviewed anytime beginning one month after Safe Streets was implemented.

Time-Varying Controls

This study controls for arrest, offending behavior and time in the community, as is done in Study 1 (see page 101 for a more detailed description). Arrest is measured as a dichotomous indicator for arrest since the last interview. Offending behavior is conceptualized as a variety score indicating the percentage of different crimes that were endorsed since the last interview. Street time is a measure of the percentage of days in the recall period in which respondents are in the community (i.e., not incarcerated, in a hospital or other secure setting).

Analytic Strategy

The analytic strategy for Study 3 is identical to the strategy employed in Study 1. First, means on the main outcomes are presented over time in relation to Safe Streets. Then changes across untreated and treated periods are compared. Finally, first-difference models are estimated to examine the effect of Safe Streets on various collateral outcomes. The following equations represent these main analyses.

$$\Delta pj_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta VARIETY_{it} + ß4\Delta STREETTIME_{it} + ß5\Delta AGE_{it} + \Delta\varepsilon_i \quad (11)$$

$$\Delta pj_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta\ VARIETY_{it} + ß4\Delta ARREST^{*}\ VARIETY_{it} + ß5\Delta STREETTIME_{it} + ß6\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (12)$$

$$\Delta legit_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta VARIETY_{it} + ß4\Delta STREETTIME_{it} + ß5\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (13)$$

$$\Delta legit_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta\ VARIETY_{it} + ß4\Delta ARREST^{*}\ VARIETY_{it} + ß5\Delta STREETTIME_{it} + ß6\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (14)$$

$$\Delta cynicism_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta VARIETY_{it} + ß4\Delta STREETTIME_{it} + ß5\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (15)$$

$$\Delta cynicism_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta\ VARIETY_{it} + ß4\Delta ARREST^{*}\ VARIETY_{it} + ß5\Delta STREETTIME_{it} + ß6\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (16)$$

Equations 11-16 represent the three outcomes regarding perceptions of police and the law. The odd numbered equations represent models which test main effects, and the even numbered equations include interaction effects of Safe Streets and arrest as theoretical expectations suggest that incorporating the effect by arrest is important for perceptions of police.

$$\Delta disorder_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta VARIETY_{it} + ß4\Delta STREETTIME_{it} + ß5\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (17)$$

$$\Delta personalrewards_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta VARIETY_{it} + ß4\Delta STREETTIME_{it} + ß5\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (18)$$

$$\Delta socialrewards_{it} = ß0 + ß1\Delta TREAT_{it} + ß2\Delta ARREST_{it} + ß3\Delta VARIETY_{it} + ß4\Delta STREETTIME_{it} + ß5\Delta AGE_{it} + \Delta\varepsilon_{i} \quad (19)$$

Equations 17-19 represent additional theoretical mechanisms to help explain the relationship between Safe Streets and offending, as well as possible collateral consequences. These analyses do not examine interactions with arrest because there is little theoretical reason to believe this is important for outcomes of disorder or rewards to crime.

Results

Bivariate Results

Table 6.1 presents the mean for each outcome across time. Recall that throughout the analyses, time *t* represents the first period after Safe Streets was implemented. In the bivariate, it appears that Safe Streets may positively impact perceptions of procedural justice. Perceptions of procedural justice are significantly greater after the implementation of Safe Streets; however, there appears to be a stable increase over time that began before Safe Streets. In the bivariate, Safe Streets does not appear to have any relationship with police legitimacy, legal cynicism, neighborhood disorder, or personal or social rewards to crime; there are no significantly greater means when Safe Streets first began compared to the period immediately prior.

Next a comparison of average changes across waves to assess if the changes are different from before to during Safe Streets is presented. Table 6.2 presents aggregate changes between waves on each outcome. For the first three outcomes (i.e., perceptions of procedural justice, police legitimacy and legal cynicism), the categories are further disaggregated to present averages conditional on those who were and were not arrested in the recall period as prior literature suggests arrest may impact these behaviors. Significant differences based on rank sum tests between the changes in periods prior to the intervention (*t-2* – *t-3* and *t-1* – *t-2*) and after (*t*1-*t*, *t*2-*t*1 and *t*3-*t*2) to changes that occur once the intervention begins (*t* – *t-1*) are noted with stars.

Perceptions of procedural justice increased on average between waves prior to Safe Streets. For all respondents and for those who were not arrested, perceptions of procedural justice were increasing over time prior to Safe Streets. Once Safe Streets was implemented, changes in perceptions of procedural justice remain positive, but these increases are smaller than the change over time across two untreated periods. For example, on average, perceptions of procedural justice increased by 0.29 in the prior period, but only increased by 0.09 from before to during Safe Streets. This change is significantly different, as is the comparison of changes for those conditioned by having not been arrested.

Changes in perceptions of procedural justice are the only significant or notable changes once Safe Streets began. There are no differences in changes in any of the five other outcomes in pre-Safe Streets untreated periods compared to the change experiences after the first treated period. After Safe Streets began, comparing this first treated period to later periods, there is some notable declines in perceptions of procedural justice for those who were arrested, as well as in neighborhood disorder and social rewards to crime, but these do not follow any pattern consistent with expectations of the intervention.

Main Results

The main results are presented in Table 6.3. Nine models are estimated which include one model for each of the six outcomes which controls for arrest, one's variety score of offending and street time, and for procedural justice, police legitimacy and legal cynicism, models are estimated which also interact Safe Streets by arrest.

Despite some evidence of changes in perceptions of procedural justice at the hands of Safe Streets in the bivariate, Model 1 suggests that there is no positive effect on perceptions of procedural justice. In fact, this precisely estimated zero suggests that there is no relationship at all. As expected, an arrest experience decreases perceptions (0.26), as does greater diversity in offending, and more time in the street, suggesting fewer punishment experiences are related to an increase in perceptions of procedural justice. These patterns persists when an interaction between Safe Streets and arrest is included (Model 2). The non-significant estimate of nearly 0 demonstrates that even for those who were arrested during Safe Streets, the intervention did not play any role in altering perceptions of procedural justice. The main effect of arrest is still negatively associated with perceptions of procedural justice.

Models 3 and 4 examine the effect of Safe Streets on perceptions of police legitimacy, with and without an interaction between Safe Streets and arrest. Similar to the findings for procedural justice, Safe Streets is not significantly associated with police legitimacy as a main effect, nor are there effects dependent on being arrested or not. Arrest has no effect on legitimacy but one’s variety score is negatively associated, and street time is positively associated, with legitimacy. Models 5 and 6 examine the effect of Safe Streets on legal cynicism, and again, yield no effect of Safe Streets on this outcome. One’s variety score of offending is positively related, demonstrating that when one engages in more types of crimes, they report more cynicism toward the law.

Model 7 demonstrates that Safe Streets positively but non-significantly impacted perceived neighborhood disorder. It is worth noting that these models have a significantly decreased sample size (n=609) because neighborhood disorder is only asked of those living in the community at the time of the interview and there is a large amount of missingness on this measure. Though the relationship is nonsignificant the positive relationship is unexpected.

Model 8 and Model 9 examine the effect of Safe Streets on one’s perceived personal rewards to crime and one’s perceived social rewards to crime, respectively. Once again, Safe Streets is not significantly associated with either outcome. Arrest nor street time are significantly associated with the rewards to crime, but one’s criminal variety score is positively associated with perceived personal and social rewards to crime.

Sensitivity Analyses

Similar to Study 1, identical models to the main analyses are estimated without a lag in treatment to see if the effects were more immediate. The results, shown in Table 6.4, are largely consistent for perceptions of procedural justice, legal cynicism and personal rewards to crime. However, the effect of Safe Streets now reaches statistical significance in several of the new models. Safe Streets now is a significant negative predictor of perceptions of police legitimacy, as well as a significant positive predictor of neighborhood disorder and social rewards to crime.

These differences are notable because in the main models (see Table 6.3) the coefficients for Safe Streets were not even close to marginally significant. Therefore, the substantive conclusions do change when examining Safe Streets without lagging the treatment by one month. These results suggest that Safe Streets may have an immediate negative effect on perceptions of police legitimacy, and a positive effect on neighborhood disorder and social rewards to crime.

Next, and again similar to Study 1, fixed-effects models are estimated to assess if the results are sensitive to the model selection. Table 5.5 demonstrates that when using a fixed-effects model, the results are fairly consistent, denoting no relationship between Safe Streets and procedural justice, police legitimacy, legal cynicism, or the rewards to crime. However, Safe Streets does appear to significantly increase perceived neighborhood disorder. This pattern is consistent with the results in the comparable first-difference model (Model 4 of Table 6.3), but the coefficient is slightly larger and reaches statistical significance. This difference may be explained by the increased sample size and increased efficiency of fixed-effects models over first-difference models.

Robustness Tests

Finally, several robustness checks were conducted to assess if Safe Streets did have an effect on these six outcomes when examining a different measure of perceived procedural justice, when examining collateral reports of similar measures, or finally, when the sample is split based on the likelihood of treatment. Table 6.6 presents first-difference models which examine the outcome of perceptions of procedural justice for others, with and without an interaction with arrest. *Perceptions of procedural justice for others* is measured using the additional 5 items regarding perceptions of procedural justice as it relates to others' experiences.

This includes the following items: "Of the people you know who have had a contact with the police (in terms of crime accusation), how much of their story did the police let them tell?; Police treat males and females differently; Police treat people differently depending how old they are; Police treat people differently depending on their race/ethnic group; and Police treat people differently depending on the neighborhoods they are from." Models 1 and 2 in Table 6.6 demonstrate that Safe Streets did not a play a role on perceptions of procedural justice when defined as how one perceives others are treated by police.

Next, instead of relying on one's self reports of neighborhood disorder, models are estimated which examine if Safe Streets had a significant impact on neighborhood disorder as reported by a collateral respondents. Collateral respondents are typically parents or other relatives. Seventy-eight percent of collateral respondents at baseline were parents, 15% were female relatives including grandmothers, aunts, nieces and cousins, and the remaining 7% were friends, siblings and male relatives. *Neighborhood disorder (collateral)* is identical to the measures of disorder described previously (see page 101), but is reported by collateral respondents at baseline and at waves 1-3. At baseline, collateral respondents reported on neighborhood conditions if they lived with the focal adolescent respondent at the time of the interview or at some point in the last 6 months. At follow-up waves, the collateral respondent only reports on these items if they live in the same neighborhood as the focal respondent or have spent some time in that neighborhood. Unlike the focal respondents' self-report of neighborhood disorder, which is skipped if the respondent is incarcerated, collateral respondents report on the adolescent respondents' most recent neighborhood even if they are currently incarcerated. Similar to the results for neighborhood disorder as reported by the focal respondent, Safe Streets did not significantly impact collateral respondents' reports of neighborhood disorder.

Finally, to assess if the effects of the treatment vary by the likelihood of having received the treatment, models are estimated by neighborhood disorder. Table 6.7 examines the effect of Safe Streets on the six main outcomes for those from neighborhoods with low perceived drug disorder at baseline (Panel A) and for those who were living in neighborhoods with moderate to high perceived drug disorder at baseline (Panel B). The results are consistent across varying levels of neighborhood drug disorder for the police outcomes (i.e., procedural justice, legitimacy and cynicism). However, the effects vary for the other three outcomes. Safe Streets negatively, though not significantly, decreased perceived drug disorder for those originally from high drug communities, but positively and significantly increased drug disorder for those originally from low drug disorder communities. It is important to note that these results cannot clearly state that disorder itself increased in these communities because it is possible that respondents moved over time to different locations. However, on average, it seems that those from low perceived drug disorder communities experienced increases in their perceived neighborhood drug disorder. These differences are significant, noting distinct effects across groups based on perceived drug disorder at baseline. This finding, if interpreted as impacting communities, would be consistent with a spatial spillover of crime as a result of Safe Streets.

The effects on rewards to crime are all non-significant but suggest some interesting patterns. For those who reported living in low drug disorder communities at baseline, Safe Streets is related to an increase in personal and social rewards to crime, while it is negatively associated with these rewards to crime for those who reported living in moderate to high drug disorder communities at baseline. In addition to the main effects, these differences are not significantly distinct from each other based on Z-tests of the equality of the estimates.

Discussion

The current study explored a host of theoretically driven outcomes that may have been impacted by an increase in police presence in adolescents' communities. Theory and prior literature predict that hot spots policing interventions may negatively impact perceptions of police and legal cynicism (meaning improved confidence in police), and decrease levels of disorder. The theoretical expectations for the personal and social rewards were rather uncharted territory, but it was hypothesized that an increase in apprehension risk may have made crimes more thrilling and more socially rewarding.

Contrary to these theoretical expectations, Safe Streets did not appear to effect any of the six outcomes for a sample of previously adjudicated adolescents examined in Study 3. There were no significant effects for procedural justice measured either as perceived procedural justice for one's self or perceived procedural justice regarding how others are treated. There were also no effects for legitimacy of legal cynicism. There were no significant effects in either first-difference models or fixed effects models, nor were there significant effects when examining only those from moderate to high drug disorder communities at baseline. The results of bivariate relationships do offer some evidence that Safe Streets may have slowed the increase in perceptions of procedural justice that appeared to be occurring over time. For the periods before Safe Streets began, perceptions of procedural justice were increasing by .23 and .29, respectively, but declined to increases of only 0.09 when Safe Streets began. If this is the case, the null effects are still somewhat discouraging. Taken together, this is suggestive evidence that Safe Streets stunted the expected increase in perceptions of procedural justice over time. Additionally, the evidence from models which did not include a one-month lag in the effects of Safe Streets on perceived police legitimacy suggests that there may have been some immediate negative impacts on this outcome. The effect is small and only appears in this one model specification, but it is worth further exploration when assessing the impacts of other hot spots policing interventions in future research. Overall, these findings are in line with prior research which has not found negative impacts of hot spots policing on these outcomes for samples of community residents (National Academies of Sciences, Engineering, and Medicine, 2018). However, this evidence should be viewed as preliminary with the cautions noted above.

Beyond perceptions of police, the analyses provide some limited evidence that Safe Streets may have had a small impact on the personal rewards to crime (i.e., the thrill of crime) and the social rewards, but the evidence is weak. These patterns would align with theory, where as crime becomes more difficult and apprehension risk increases (see Study 1) it may become more thrilling and more socially rewarding. Again, the results are inconsistent across models but the marginally significant increase in personal rewards in the main model and the significant increase in social rewards in the model with no treatment lag do offer some support that Safe Streets may have had a positive effect. Future research should explore these outcomes.

Finally, the results regarding the impact of Safe Streets on neighborhood disorder are the most complex. The bivariate comparisons and the main first-difference models provide no evidence that Safe Streets altered neighborhood disorder. The robustness checks which examines the collateral reports of neighborhood disorder are also in line with the main analyses, suggesting no effect. However, in the analyses which use the fixed-effects estimator there is a significant positive effect on disorder, as well as in models which do not lag the timing of Safe Streets. Furthermore, for those living in high drug disorder communities at baseline, meaning those whom I would expect their homes to be the closest geographically to targeted Safe Streets sites, it appears that levels of neighborhood disorder decreased after Safe Streets began. This would fall in line with the aims of the intervention. On the other hand, those who reported lower levels of neighborhood drug disorder at baseline, reported higher neighborhood disorder after Safe Streets began. This finding aligns with the idea that crime and disorder may have been displaced to the communities that were not targeted by Safe Streets. Unfortunately, given limitations of the data, there is no further test that can be done to confirm or deny this hypothesis. Furthermore, this finding is complicated by the fact that perceptions of neighborhood disorder correspond with where the focal respondent lived at the time of the interview, and as such, may reflect that the respondents moved to new homes or places with differing levels of disorder. These results raise questions regarding whether or not Safe Streets increased disorder. This is problematic of course, as it goes against the aims of Safe Streets, and may also offset any gains in perceptions of arrest risk (see Study 1) as a mechanism for reducing crime. Future research should prioritize perceived neighborhood disorder as an outcome in hot spots policing evaluations, and should focus on the perceived disorder for those most likely to spend time outdoors in these higher disorder areas.

Future research should continue to explore these perceptual outcomes (National Academies of Sciences, Engineering, and Medicine, 2018; Sargeant, Wickes, & Mazerolle, 2013) and additional outcomes and crime-reducing mechanisms which may be impacted by hot spots policing. The current study did not examine fear or crime, informal social control, or collective efficacy, which are all both important focal outcomes and mechanisms which theory predicts should be impacted by an increase in police presence. Specifically, Safe Streets was designed with the concepts of Broken Windows policing, and with intentions to increase community engagement and decrease fear of crime. Examining these outcomes both from the perspective of the focal adolescent respondents, as well as fellow community members, is an important area for future research. Additionally, future work should test additional rewards to crime, such as monetary rewards. Though the results of Study 2 suggest that Safe Streets did not impact illegal earnings, the sample size was small leading to limited power to detect these differences. Future research which measures the effects of police on social, personal and monetary rewards may help to understand the impact of police on potential offenders' entire choice calculus when thinking about engaging in crime.

Overall, the results of the current study suggest that there may be some, albeit small, negative effects of Safe Streets on these collateral consequences. In the same breath, the results are promising that a hot spots policing intervention with such high levels of police presence did not negatively impact perceptions of police for those likely to have noticed the change in policing or had an interaction with police because of Safe Streets. It is important to remember that Safe Streets was not coupled with high levels of arrest or enforcement, which is not always the case in hot spots policing interventions, so this caveat must be included when discussing the lack of negative effects of Safe Streets. It is also important to highlight that this sample only included adolescent respondents. It remains unclear if Safe Streets caused negative effects for other groups.

Tables

Table 6.1. Other Explanations & Collateral Consequences by Safe Streets Timing

	Baseline	*t-3*	*t-2*	*t-1*	***t***	***t1***	***t2***	***t3***	*t4*
Perceptions of Procedural Justice	2.73	2.73 ***	2.80 ***	2.95 *	**3.04**	**3.13 ***	**3.18 ****	**3.26** **	3.22 **
Perceptions of Police Legitimacy	2.21	2.21	2.18	2.22	**2.23**	**2.27**	**2.34 ****	**2.27**	2.27
Legal Cynicism	1.99	2.01	2.04	2.01	**1.97**	**2.00**	**2.05**	**2.07**	2.00
Neighborhood disorder	2.60	2.46 ***	2.56 **	2.64	**2.74**	**2.69**	**2.70**	**2.77**	2.71
Personal rewards to crime	1.63	1.81 **	1.58	1.45	**1.31**	**1.40**	**1.25**	**1.13**	1.02 *
Social rewards to crime	2.01	1.91	1.98	1.99	**1.97**	**1.86 ****	**1.87 ****	**1.88**	1.87 **
n	700	149	354	609	**470**	**473**	**457**	**99**	514

Notes. Periods during Safe Streets are noted in bold. Sample sizes are reported for the maximum number of interviews for each time period.

*p<0.05; **p<0.01; ***p<0.001 represent significant differences (p <0.05) from means at time *t*

Table 6.2. Changes in Other Explanations & Collateral Consequences by Safe Streets Timing

	Average Within Person Change					
	t-2 - t-3	*t-1 - t-2*	***t- t-1***	*t1 - t*	*t2 - t1*	*t3 - t2*
Perceptions of Procedural Justice	0.23	0.29 **	**0.09**	0.03	-0.01	-0.03
Arrested	0.07	-0.07	**0.01**	-0.38 **	-0.23 *	-0.30
Not arrested	0.26	0.34 **	**0.11**	0.16	0.05	0.04
Perceptions of Police Legitimacy	-0.04	0.04	**-0.04**	0.02	0.04	-0.07
Arrested	-0.12	-0.08	**-0.08**	-0.08	-0.05	-0.16
Not arrested	-0.02	0.06	**-0.03**	0.05	0.06	-0.05
Legal Cynicism	0.06	-0.01	**-0.01**	0.01	0.05	0.00
Arrested	0.02	0.13	**-0.08**	0.02	-0.02	-0.12
Not arrested	0.07	-0.04	**0.01**	0.00	0.08	0.04
Neighborhood Disorder	0.07	0.10	**0.07**	-0.06 **	-0.02	-0.11 **
Personal Rewards to Crime	-0.33	-0.24	**-0.07**	0.05	-0.20	-0.15
Social Rewards to Crime	-0.01	-0.03	**0.02**	-0.14 ***	0.00	-0.03
n	354	607	**461**	468	451	97

Notes. Significance stars denote comparisons to changes from *t* to *t*-1 based on Rank Sum tests. Sample sizes are reported for the maximum number of interviews for each time period.

$^*p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$

Table 6.3. First-Difference Longitudinal Linear Regressions Predicting Other Explanations & Collateral Consequences

	Perceptions of Procedural Justice				*Perceptions of Police Legitimacy*				*Legal Cynicism*				*Neighborhood Disorder*		*Personal Rewards to Crime*		*Social Rewards to Crime*	
	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6		Model 7		Model 8		Model 9	
	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE
Safe Streets	-0.01	0.05	-0.01	0.05	-0.05	0.03	-0.03	0.04	-0.02	0.04	-0.02	0.04	0.07	0.05	0.10	0.13	0.05	0.03
Arrest	-0.26 ***	0.04	-0.26 ***	0.06	-0.02	0.02	0.02	0.04	-0.04	0.03	-0.05	0.04	-0.03	0.03	-0.09	0.08	0.00	0.02
Safe Streets * arrest			0.00	0.07			-0.06	0.04			0.01	0.05						
Variety score	-0.65 ***	0.16	-0.65 ***	0.16	-0.36 **	0.11	-0.40 ***	0.11	0.55 ***	0.13	0.56 ***	0.14	0.07	0.12	1.84 ***	0.37	0.50 ***	0.09
Street time	0.14 **	0.04	0.14 **	0.04	0.11 ***	0.03	0.10 **	0.03	-0.04	0.03	-0.03	0.04	-0.12 *	0.05	-0.02	0.10	-0.05	0.02
n = persons(person-waves)	652(3,095)				654(3,170)				654(3,170)				609(2,485)		654(3,167)		654(3,168)	

Notes. SE = robust standard error

**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 6.4. First-Difference Longitudinal Linear Regressions Predicting Other Explanations & Collateral Consequences - Sensitivity Tests - No Treatment Lag

	Perceptions of Procedural Justice			*Perceptions of Police Legitimacy*			*Legal Cynicism*			*Neighborhood Disorder*			*Personal Rewards to Crime*			*Social Rewards to Crime*		
	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
	b		SE	b		SE	b		SE	b		SE	b		SE	b		SE
Safe Streets	0.07		0.05	-0.07	*	0.03	-0.05		0.04	0.10	*	0.05	0.04		0.13	0.06	*	0.03
Arrest	-0.25	***	0.04	-0.02		0.02	-0.04		0.03	-0.03		0.03	-0.09		0.08	0.00		0.02
Variety score	-0.64	***	0.16	-0.37	**	0.11	0.55	***	0.13	0.09		0.12	1.84	***	0.37	0.50	***	0.09
Street time	0.14	**	0.04	0.11	***	0.03	-0.04		0.03	-0.12	*	0.05	-0.02		0.10	-0.05	*	0.02
n = persons(person-waves)	652(3,095)			654(3,170)			654(3,170)			609(2,485)			654(3,167)			654(3,168)		

Notes. SE = robust standard error

**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 6.5. Fixed-Effects Longitudinal Linear Regressions Predicting Other Explanations & Collateral Consequences - Sensitivity Tests

	Perceptions of Procedural Justice		*Perceptions of Police Legitimacy*		*Legal Cynicism*		*Neighborhood Disorder*		*Personal Rewards to Crime*		*Social Rewards to Crime*	
	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE
Safe Streets	0.02	0.04	0.01	0.03	0.02	0.03	0.09 *	0.04	0.03	0.03	0.01	0.02
Arrest	-0.32 ***	0.03	-0.04 *	0.02	-0.04	0.02	-0.06 **	0.02	-0.01	0.02	-0.01	0.02
Variety score	-0.65 ***	0.11	-0.45 ***	0.08	0.35 ***	0.10	0.17	0.10	0.55 ***	0.08	0.57 ***	0.07
Street time	0.17 ***	0.04	0.18 ***	0.03	-0.04	0.03	-0.13 **	0.05	-0.07 *	0.03	-0.05	0.03
Constant	3.13 ***	0.08	2.29 ***	0.06	1.91	0.07	2.67 ***	0.08	1.79 ***	0.06	1.87	0.05
n = persons(person-waves)	699(3,925)		699(3,970)		699(3,970)		700(3,705)		699(3,968)		699(3,968)	

Notes. SE = robust standard error

**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 6.6. First-Difference Longitudinal Linear Regressions Predicting Other Explanations & Collateral Consequences - Robustness Checks

	Procedural Justice (Others)		*Procedural Justice (Others)*		*Neighborhood Disorder (Collateral)*	
	Model 1		Model 2		Model 3	
	b	SE	b	SE	b	SE
Safe Streets	0.00	0.04	0.01	0.04	0.03	0.07
Arrest	0.01	0.03	0.02	0.05	-0.23 **	0.07
Safe Streets * arrest			-0.01	0.06		
Variety score	-0.40 **	0.14	-0.41 **	0.14	-0.07	0.25
Street time	0.05	0.04	0.04	0.04	0.08	0.08
n = persons(person-waves)	654(3,169)				455(863)	

Notes. SE = robust standard error

**p*< 0.05; ***p* <0.01; ****p*< 0.001

Table 6.7. First-Difference Longitudinal Linear Regressions Predicting Other Explanations & Collateral Consequences - Robustness Checks - Moderated by Neighborhood Drug Disorder

	Perceptions of Procedural Justice		*Perceptions of Police Legitimacy*		*Legal Cynicism*		*Neighborhood Disorder*		*Personal Rewards to Crime*		*Social Rewards to Crime*	
	Model 1		Model 2		Model 3		Model 4		Model 5		Model 6	
Panel A - Low Drug Disorder	b	SE	b	SE	b	SE	b	SE	b	SE	b	SE
Safe Streets	0.00	0.08	-0.05	0.05	-0.01	0.06	0.18 **	0.07	0.23	0.19	0.08	0.04
Arrest	-0.29 ***	0.05	-0.03	0.03	-0.06	0.04	-0.03	0.04	-0.17	0.13	0.00	0.03
Variety score	-0.63 *	0.25	-0.24	0.18	0.80 ***	0.22	-0.41 *	0.20	2.15 ***	0.54	0.52 **	0.16
Street time	0.12	0.06	0.10 *	0.05	0.00	0.05	-0.19 *	0.08	0.07	0.15	-0.04	0.04
n = persons(person-waves)	283(1,398)		284(1,426)		284(1,426)		265(1,127)		284(1,426)		284(1,426)	
Panel B - Moderate/High Drug Disorder												
Safe Streets	-0.04	0.08	0.02	0.06	-0.01	0.07	-0.17	0.10	-0.18	0.24	-0.01	0.05
Arrest	-0.23 ***	0.06	-0.06	0.04	-0.05	0.04	0.00	0.04	-0.08	0.13	-0.02	0.03
Variety score	-0.75 **	0.23	-0.45 *	0.18	0.58 **	0.19	0.25	0.19	1.64 **	0.61	0.63 ***	0.13
Street time	0.13	0.07	0.13 **	0.05	-0.10	0.06	-0.07	0.08	-0.15	0.15	-0.09 *	0.04
n = persons(person-waves)	228(1,020)		228(1,045)		228(1,045)		207(808)		228(1,045)		228(1,044)	

Notes. SE = robust standard error

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$

Chapter 7. DISCUSSION, POLICY IMPLICATIONS & CONCLUSION

The current dissertation sought to examine the impact of a hot spots policing intervention on perceptions of arrest risk, self-reported offending, perceptions of police, disorder and rewards to crime. In the following sections, a summary of the findings from the three empirical studies is presented. Next, the generalizability of the findings are discussed, as well as study limitations and implications for policy and future research. Finally, this is followed by some concluding remarks regarding the future of crime prevention strategies.

Summary of Findings

Study 1 offered strong evidence that Safe Streets did significantly increase perceptions of arrest risk amongst male and female adolescents. After Safe Streets began, perceptions of arrest increased by about 0.6 on a ten-point scale, an effect size similar to that of experiencing arrest found in the current study and in prior research (Anwar & Loughran, 2011). The results were robust to various measurements and model specifications, and were supported by several falsification tests. These findings, coupled with null effects from multiple falsification tests, are strong evidence that Safe Streets likely did increase perceptions of arrest risk in the sample, and in fact, reversed the existing downward trend in perceptions of arrest risk that occurred before Safe Streets. The results of Study 1 support the stated hypotheses which are summarized in Table 7.1.

The results of Study 2 were not as straightforward. There is some evidence that Safe Streets reduced individuals' frequency of offending and perhaps had a weak effect on one's variety score of offending. Furthermore, in support of this, Safe Streets did negatively impact peer offending and violence exposure, and positively impact the cost of guns. There is no indication that Safe Streets had any effect on the prevalence of offending. Overall, absent a control group, it is difficult if not impossible, to discern if these effects were driven by a true effect of Safe Streets or by declines in offending behavior over time as respondents aged. These results from Study 2 made it difficult to draw firm conclusions regarding the stated hypotheses and as such, all results should be thought of as falling somewhere on a continuum ranging from supported or refuted with the majority of the results being inconclusive. In Study 2, the significant effect on frequency of offending is the strongest result in the current study and is the only result which can be viewed as firmly in support of a crime-reducing effect of Safe Streets.

Finally, Study 3 showed that in general Safe Streets did not have deleterious consequences for perceptions of police, the law, neighborhood disorder or the rewards to crime. Results were almost always nonsignificant, and for outcomes like procedural justice, legal cynicism and personal rewards to crime, no association was found. There was limited evidence of an immediate positive (and substantively negative) effect on neighborhood disorder and the social rewards to crime, especially for those who lived in neighborhoods with moderate to high drug disorder at baseline. Study 3 hypotheses regarding perceptions of police and the law are supported but the effects for disorder and rewards to crime are not clear. To review all of hypotheses from this dissertation and their placement on a continuum from supported to refuted, see Table 7.1.

Contributions

This dissertation has addressed several gaps in our understanding of the relationship between police and crime and the effects of hot spots policing on various perceptual and behavioral outcomes. First, few studies have tested the assumption that police interventions can actually manipulate perceptions of arrest risk. This first-order problem meant that criminologists and policymakers could not fully understand the mechanisms for why police interventions, such as hot spots policing, are effective. The results from Study 1 showed that it is possible to manipulate offenders' perceptions of arrest risk by implementing hot spots policing. As such, Study 1 was the first to assess and find evidence of a significant effect of a change in policing on individuals' perceptions of arrest risk over time. Focusing on a sample of previously adjudicated adolescents also helped to contribute to the literature by assessing the relationship between police and perceptions of arrest risk for those most likely to be in the market for crime (Apel, 2013).

Study 2 was the first study to examine individual-level self-reported offending as the outcome of interest in an assessment of hot spots policing. The finding that Safe Streets likely reduced one's frequency of offending but not ones likelihood of engaging in crime suggests that the effect of hot spots policing on crime is much more nuanced that the existing research using official crime data has led us to believe. Though the effects on the various offending outcomes did not offer a clear pattern of the effect of Safe Streets, the results demonstrate important findings regarding the capacity for hot spots policing to reduce offending rather than displace crime.

Study 3 improved our understanding of the potential backfire effects of hot spots policing, as well as contributed to our understanding of the potential mechanisms through which police may reduce crime. The effect of Safe Streets on perceptions of police, while consistent with prior literature, also provided insight on these effects for adolescents and for those likely to have actually experienced a change in policing as a result of hot spots policing. The small or null positive effects of Safe Streets on perceived rewards to crime also offers some of the first evidence to suggest that hot spots policing likely does not ‘backfire’ by making crime more rewarding.

Generalizability

Several aspects of Safe Streets and the Pathways study are worth discussing in more detail to address the generalizability of the findings. Safe Streets was a hot spots policing intervention which was not characterized by high levels of enforcement. This fact may have attributed to the perceptual deterrent effect which persisted above and beyond the impact of arrest. It may also be a reason for why Safe Streets did not appear to negatively impact perceptions of police.

Safe Streets was not defined as a procedurally just intervention. Study 3 was intended to assess if a ‘non-procedurally just’ hot spots policing intervention impacted perceptions of police. As such, it was not expected that Safe Streets have a significant positive effect on perceptions of police. Given the lack of prior research which has found that procedurally just policing can positively impact perceptions of police, as well as the stated intention of Safe Streets to not arrest individuals, it was unlikely that Safe Streets would result in positive or negative impacts of perceptions of procedural justice or related perceptions of police and the law. Nonetheless, Safe Streets did have some key attributes, such as community input, and low enforcement, which may have attributed to its lack of negative effects. Furthermore, Safe Streets was implemented ‘under a microscope’ so to speak because it was a hotly contested strategy due to its high costs for the city and was viewed by many as a publicity stunt by the mayor to help win re-election. This may have resulted in increased scrutiny of officer behavior and it is possible that this also contributed to a lack of negative impacts.

Relatedly, the current study does not assess the effect of Safe Streets on criminal justice contact. In supplemental analyses, I examined the effect of Safe Streets on arrest and found no indication that Safe Streets increased the number or prevalence of arrests for adolescents in the sample. However, it is possible that Safe Streets increased less severe or punitive police contact. This could include stops, questioning, formal 'stop, question and frisks', or possible harassment of citizens by police. The current study did not explore if these types of contacts increased, and subsequently, if these contacts are the mechanism between Safe Streets and increased perceptions of arrest risk or decreased offending. Future research can examine the impact of Safe Streets on stops by police. Furthermore, future studies should prioritize self-reported offending, as well as self-reported and official police stops that occur as a result of hot spots policing. The current study cannot make claims regarding what exactly police were doing at hot spots during Safe Streets, but examining police behavior on individuals' behavior is an important next step for future research (Famega, Hinkle, & Weisburd, 2017).

It is important to note the timing of Safe Streets as it relates to weather and crime. Prior research has demonstrated that crime fluctuates with temperature (Cruz, D'Alessio, & Stolzenberg, 2020). Safe Streets was enacted when the weather began to get warmer, and when adolescents were approaching time of from school for the summer. It is possible that some of the impact of Safe Streets on perceptions or behavior was due to this seasonality. However, in Study 1, a placebo test demonstrated that there was no impact of 'Safe Streets' on perceptions when Safe Streets was measured as having begun a year prior to its true start date. Nevertheless, it remains possible that increasing temperatures resulted in these adolescents increased time outside and that the summer months resulted in increased free time or unstructured socializing, both of which may have resulted in increased visibility and acknowledgement of uniformed police officers. This possibility should be explored in future research, perhaps by examining the impact of a hot spots intervention rolled out over a series of months across different communities.

It is also important to remember that Safe Streets was enacted over a longer period of time than most previously evaluated hot spots policing intervention. Safe Streets was a much more intense policing strategy than those previously examined. For example, Ratcliffe and colleagues (2015) found no effect of hot spots policing on procedural justice when examining an intervention that increased police presence at hot spots to 8 hours a day 5 days a week for 3 months. The current study finds no effect for an intervention that increased police presence *24-hours a day for over a year*. In some ways the lack of a negative effect is therefore especially surprising and perhaps promising, but it is important to note that the analytic strategy employed examined immediate effects. It is possible that over time, Safe Streets did in fact have harmful effects on perceptions of police.

Safe Streets was implemented at more targeted hot spots than most of the existing hot spots policing evaluations and was implemented city-wide. Most hot spots policing evaluations select a proportion of crime hot spots to treat, leaving a host of serious hot spots as controls. Safe Streets was not designed as a randomized controlled trial. Rather, it was designed to target all of the most problematic narcotics corners in the city. This may be one reason it was so effective at increasing perceptions of arrest risk. Unfortunately it is impossible to assess this implementation may have played a role in the effects on risk or the effects on crime (Lawton et al., 2005), but it is certainly important to consider when comparing the effect of Safe Streets to other research.

It is possible that Safe Streets was effective at increasing perceptions of arrest risk in part because of its high visibility and widespread implementation. Safe Streets was a highly publicized and highly visible intervention. As previously discussed (see Chapter 3), Safe Streets was rolled out alongside ‘Safe Streets block parties’ and ‘rallies’ which, in addition to garnering buy in from community residents, may have served as ‘threat communication’ to potential offenders. The strategy itself of heightened officer presence via primarily foot patrol was designed explicitly to be noticed by potential offenders. Uniformed foot-patrol officers are described as one of the most salient and visible signs of deterrence (Kleck & Sever, 2018). At most times during Safe Streets between 400-600 uniformed foot patrol officers were on the street. Anecdotal evidence from media accounts of the intervention suggest there was a widely recognized change in policing; mere weeks into the intervention, one community member was quoted saying “There is clearly, absolutely a visible difference in police presence across the city” (Smith, 2002:2). Future research should think critically about how the increased visibility and salience of uniformed officers and media attention may have impacted the effectiveness of the intervention (Nagin & Sampson, 2019).

The focus on a high-risk sample of adolescent offenders made it possible to speak more to the effect of a hot spots policing intervention on those who have offended, or in other words, for those for whom deterrence had already once failed (Durlauf & Nagin, 2011). As such, the results cannot be extrapolated to apply to those who have yet to engage in crime. It is possible that the deterrent effect for potential offenders who have yet to engage in crime are stronger than those found here as all respondents in the current sample have hypothetically already ‘updated’ their risk (Anwar & Loughran, 2011). Alternatively, the effects on a non-offender sample could be smaller or non-existent as nonoffenders are already deterred by morals or informal sanctions

(Cullen & Pratt, 2016; Pogarsky, 2002; Wright et al., 2004). The results here suggest a promising deterrent effect for those who are the most likely to partake in criminal activity, but future research should explore effects for other lower risk groups as well.

The results of all three studies are not generalizable to all adolescents or even all adjudicated adolescents in Philadelphia. The Pathways Study is not a representative sample of adjudicated adolescents in Philadelphia so the estimates from this dissertation must be qualified as such. Moreover, it is possible that Safe Streets impacted the various outcomes of study for other Philadelphia residents in differing ways than it did for those in the Pathways study. The estimates must be viewed as estimates for a sample of adolescents rather than the effect of Safe Streets for those who experienced Safe Streets. While there are many reasons to conclude that Safe Streets was noticed by the majority of Philadelphia residents (see Chapter 3 for further discussion), the estimates from the current study include those who received varying levels of dosage of the treatment. Furthermore, the current study explored the effects of the intervention on adolescents residing in Safe Streets neighborhoods, but it did not include *all* individuals impacted by the intervention.

The Philadelphia sample from the Pathways study is a sample of predominantly Black male adolescents. Given the importance of racial dynamics in policing, the results of the current study should not be generalized to all adolescents as the results may be driven by the fact that the majority of the sample is Black, male, and approximately 17 at the time that Safe Streets was implemented. Furthermore, it is possible and perhaps likely that the effects of Safe Streets varied by sex and race (and perhaps neighborhood). Approximately 86% of the sample is male, therefore it was not possible to examine differences by sex because there are only 95 females in the sample. Seventy-two percent of the sample is Black, 10% White, 15% Hispanic and 3% of other races. Again, these small sample sizes made it difficult to assess differences by race. Furthermore, the aims of the current dissertation were to assess causal estimates of the effects of Safe Streets on various outcomes. Models which disaggregated by race, age or sex, would have required a different set of assumptions and different conclusions beyond the scope of this study. Nonetheless, these effects should be explored in future studies. And the results of the current study should not be viewed as generalizable to all adolescents.

Finally, the focal data for this study were collected beginning two decades ago and the intervention itself occurred 19 years ago. In that time, many things have changed. For example, adolescent offending and crime in general is lower today than it was when Safe Streets was implemented. Furthermore, the nature of drug use and drug market-related violence has changed. At the time of Safe Streets, the concerns were over narcotics and violence related to narcotics markets. Today, these concerns have shifted more specifically to opioids and methamphetamine. Drug markets likely operate differently now given changes in popular drug choice, the means of drug use and changes in technology. For example, drug use and sales may now be more likely to be indoors regardless of police presence given the prevalence of cell phones which make it easier to coordinate transactions in advance. If this is the case, policing strategies which flood public spaces may not be as effective today as they were in 2002.

Limitations

Overall, this study attempted to address several gaps in the existing literature on deterrence and hot spots policing, however, it is not without limitations. First, given data limitations, the current study could not explicitly test variation in outcomes for those exposed and those not exposed to Safe Streets absent an untreated control group. Across studies, the effects estimated can be thought of as the Average Treatment Effect on the Treated (ATT) because there is no counterfactual or control group. Further, lacking data on all hot stops targeted by Safe Streets over time and respondents' home addresses, it is unclear if all respondents in the sample were actually 'treated' or treated to large enough dosages to have an effect. Though there are many reasons to believe that Safe Streets was visible and known to most Philadelphia residents, it is possible that some adolescents in the sample never actually experienced Safe Streets because they relocated outside of the city or lived in the few areas which were untreated or far from treated hot spots. Because of this, it is likely the estimates found here are conservative or can be thought of as the lower bounds. Future research should prioritize studies which focus on the effects of hots spots policing on individual-level perceptions and behavior and include a control group, or better perhaps, respondents who receive varying levels of treatment dosage (i.e., they live at a hot spots versus several blocks away).

Without a true control group, this dissertation used a first-difference design, as opposed to a difference-in-differences design. Study designs like this suffer from the "missing counterfactual" problem (Bjerk, 2009). The missing counterfactual problem occurs typically when employing fixed-effects or first-difference models which use each respondents' 'counterfactual' as their levels of offending (or other outcomes) prior to treatment, as the counterfactual. Because of the general trend toward declining rates of offending as individuals age, using periods from when respondents were younger, and perhaps in their peak years of offending, may have lead to overestimates of the treatment effect. Because of the general trend toward desistance over time, the intervention necessarily occurs as crime is theoretically declining. Because of this, it is difficult to assess if the effects are true effects of the treatment or rather are artifacts of the age-crime curve.

The second limitation is that the current study cannot speak to the specific actions or lack of actions taken by police officers during Safe Streets. These actions may have made it more or less effective at increasing perceptual deterrence without negatively impacting perceptions of procedural justice. On its face, Safe Streets did not appear to have an explicit intention toward procedurally just policing. Because of this, it is important to note that the current results do not suggest that procedurally just policing is not needed or that hot spots policing does not effect potential adolescent offenders' perceptions of police. Future research must be conducted which focuses more explicitly on officer behavior and other facets of potential offenders' perceptions of police. There was some evidence of overall increases in neighborhood disorder, which may be a result of crime displacement, but without further exploration of exactly where respondents were living at each period in relation to Safe Streets, it is impossible to draw valid conclusions about the effects on disorder in the current study.

Due to the relatively small sample size, it was not possible to examine differential impacts of Safe Streets on adolescents by race or sex. It is possible that Safe Streets resulted in more frequent contacts with police or more exposure to police based on one's race or sex. For example, Safe Streets sites may have been more likely to be in predominantly Black or Hispanic neighborhoods. Furthermore, males may have been more likely to be exposed to Safe Streets because of their greater likelihood of spending time outdoors. The current study can only be generalized to adolescents in this sample and claims regarding the impact of Safe Streets by race or sex cannot be made given the relatively small number of non-Hispanic Whites and females in the sample. Future research should prioritize the racial differences in the effects of increased police presence or hot spots policing on the outcomes in this dissertation, as well as police contacts and formal criminal justice sanctions.

These limitations are especially important when considering the impact of a large increase in police presence in minority neighborhoods on perceptions of police. Perceptions of police legitimacy, procedural justice and legal cynicism are concepts which are formed in complex ways and are difficult to study. These outcomes are known to be difficult to change and impacted by a host of factors including respondents race, prior police contact, vicarious police contact and exposure to other arms of the criminal justice system. Given the fact that Safe Streets was enacted over a relatively short period when assuming that these perceptions are formed over one’s entire life, it is perhaps unsurprising that there was not effect of perceptions of police. However, this does not mean that Safe Streets did not add new information to one’s portfolio of police experience which may impact their perceptions in the future, or the perceptions of their friends, family and future children. Future research should prioritize more frequent measurements of perceptions of police which take place over a longer time period and in a larger sample which can tease out the impact of hot spots policing broadly from direct and vicarious exposure to police.

Finally, Operation Safe Streets was primarily designed and implemented as a general deterrent strategy that did not focus on an increase in arrests. While aware of the intentions of the policy because of media reports and information from the Philadelphia Police Department, data on the rates of arrest prior to and during the intervention do not exist. It remains possible that an increase in police presence paired with an increase in arrests may have resulted in different results. Unfortunately, this study is limited in that I cannot account for the specific actions that police officers engaged in during the intervention, nor the type of police presence[21] (e.g., foot patrol or car patrols) implemented throughout the entirety of the intervention period.

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[21] Safe Streets began as a strictly foot-patrol strategy after 6 months included vehicle patrols and bicycle patrols.

Implications

Policy Implications

The results of these three studies can be viewed in their totality to offer some policy recommendations with the goal of reducing crime without collateral consequences for individuals. The results suggest that Safe Streets, a low-arrest strategy that increased police presence to high levels, was effective at manipulating offenders' perceptions of arrest risk. Importantly, Safe Streets increased adolescents' perceptions of arrest risk without deleterious impacts on perceptions of procedural justice, police legitimacy, legal cynicism or neighborhood disorder.

Given the persistent effect of Safe Streets on perceptual deterrence above and beyond arrest, these results suggest that perceptual deterrence may be possible without the use of arrest. This finding is promising given the noted benefits of reliance on the sentinel role of police over the apprehension agent role (Nagin et al., 2015).This finding is extremely important for policy as it is far more advantageous and cost effective to prevent crime through general deterrence than it is to prevent later crimes by specific deterrence or incapacitation (Nagin, 2013). Furthermore, the impact of Safe Streets net of arrest, and the remaining significant positive effect of Safe Streets for those who were arrested once an interaction was included, suggests an additional benefit of increased police presence for deterrence *even for those who experienced arrest*. It is promising to conclude from the results that there is a deterrent effect of increased police visibility without arrest, as well as the finding that this policy may have an additional benefit even for those which arrest may be necessary.

The current study did not find evidence that Safe Streets negatively impacted perceptions of police. These findings are contrary to theoretical expectations, though they are in line with existing evidence from hot spots policing studies which have examined perceptions of police. However, the findings regarding perceptions of procedural justice are important to interpret with caution. Though the majority of results suggest that Safe Streets did not impact perceptions of police, there did appear to be a slowing or stop in the trend toward increasing perceptions of procedural justice over time. Even if Safe Streets did not harm perceptions of police, if it slowed or stopped improvements in perceptions, this is still of great concern. Scholars have suggested that hot spots policing interventions which do not harm, and ideally bolster, procedural justice perceptions are important in order to create long-lasting crime reductions (Kochel & Weisburd, 2017; Weisburd et al., 2011).

The current study also did not make it possible to assess the effects of Safe Streets on those that experienced extremely high levels of policing in their communities. It remains possible that for these respondents, there were negative effects. As discussed, these results should be viewed as conservative estimates since they include respondents who experienced various dosages of treatment and perhaps include some who were never treated. Additionally, in unreported analyses, Safe Streets was found to have had a large marginally significant negative effect on perceptions of procedural justice (-0.18) for those from neighborhoods in the highest quartile for drug disorder at baseline, as reported by collateral respondents (drug disorder>2.25). Sensitivity tests which examined the effect of Safe Streets when there was no lag in treatment also suggest that safe Streets significantly and negatively (-0.07) impacted perceptions of police legitimacy. If the assumption that respondents from high drug disorder neighborhoods at baseline experienced higher dosages of treatment, meaning greater levels of police presence at the hands of Safe Streets, this finding is concerning. To reiterate comments by other criminologists, hot spots policing strategies which have a deterrent effect but do not simultaneously decrease perceptions of procedural justice should be the goal.

Some results from Study 2 and Study 3 suggest that Safe Streets may have resulted in the spatial spillover of disorder or crime as well as the relocation of crimes indoors. The findings in this dissertation align with previous critiques of Safe Streets, some anecdotal evidence, and findings from the Philadelphia Police Departments internal assessment of Safe Streets. For example, media accounts of the crime rate in Camden, New Jersey, a short distance from Philadelphia, suggest that crime there spiked after Safe Streets was implemented. Many community residents speculated that crime and drug sales did not stop or decline but rather just moved to other communities (Volk, 2003) or moved indoors (Moran, 2003, p. 200; Vaughan, 2003). According to the Philadelphia Police Department (2003), calls to the Operation Safe Streets Drug Hotline were substantially higher for complaints regarding indoor drug use compared to calls for outdoor drug use issues. Unfortunately, the Pathways study did not provide information on crimes which enabled their categorization into indoor versus outdoor crimes.

Much can be learned from the evaluation of this intervention but there are several points to keep in mind when thinking of designing a similar strategy. Implementing this intervention in additional locations would be difficult due to the cost and the manpower required. Safe Streets required many patrol officers resulting in a surge in overtime pay, and as such, was estimated to have cost over $100 million (crimesolutions.gov). Ultimately, Safe Streets was unsustainable as a long-term strategy and the cost makes it difficult to deploy similar strategies in other cities. The question remains, however, if similar effects could be achieved with fewer officers, or if this particular style of hot spots policing is more or less effective at altering offenders’ perceptions compared to hot spots policing which varies officer presence sporadically throughout the day. Varying officer presence at hot spots is cited as the most effective way to reduce crime, with specific guidance suggesting that the “Koper Curve” (i.e., patrolling each hot spot for 10-16 minutes intervals every two hours) maximizes crime reduction and deterrence (Koper, 1995). The results of this study do not refute this conclusion, and in fact, these prior findings suggest that even greater perceptual deterrent effects may be possible with less police resources and lower costs. This should be explored further in additional study locations.

Finally, and most importantly is that expectations of police and their role in communities has come under great scrutiny in the past two years. Highly tragic and publicized instances of police misconduct and police violence, such as the murder of George Floyd by Minneapolis Police last May, have dramatically changed the climate surrounding police and police community relations. The *Defund the Police* movement has drawn attention to the large amount of funding allocated to police agencies, and calls for reallocating these funds to other social services and government programs. It is hope that reallocating funds to address some of the causes and correlates of crime, such as homelessness, drug addiction and mental health issues, will help to reduce crime and reliance on law enforcement for non-violent crimes and small disturbances. This movement raises many important points and has hopefully begun to change the ways that Governors, Mayors and Police Chiefs think about the role of police. It is important to view the police as just one approach to reduce crime.

In addition to the *Defund the Police* movement, the *Black Lives Matter* movement has more specifically drawn attention to issues of police use of force for Black and other minority residents. Instances of police shootings of unarmed minority males and females such as Rayshard Brooks, Breonna Taylor, Atatiana Jefferson, Philando Castile, Alton Sterling, and most recently, Daunte Wright, have forced the country to rethink policing as it relates to minority group members and minority communities. Safe Streets increased police presence across high crime areas in Philadelphia, which included many communities which were predominantly Black or Hispanic. The current study did not examine if Safe Streets itself was tied to any instances of police misconduct or officer involved shootings, but nonetheless, programs such as Safe Streets and other high-intensity policing interventions should be designed with attention toward police-community relations and existing concerns over police presence and use of force. Programs such as Safe Streets should be designed with the intention of acknowledging and improving residents attitudes toward police, including mistrust in police. The percentage of Americans that report quite a lot or a great deal of confidence in the police is at 48%, the lowest it has been in the past 30 years (Gallup, 2020).

Because of these movements and the current political climate, it is unlikely that mayors or police chiefs in 2021 would call for or receive the approval or support for interventions like Safe Streets that increase police presence to such high levels in minority neighborhoods. Although an intervention such as Safe Streets may not be the best choice of policing strategy to implement today, there are several lessons learned from this intervention and from the results of this dissertation. Future work should prioritize evaluating police interventions which aim to reduce crime through routes other than punishment and which seek to include community buy-in and promote positive perceptions of police. In addition, these interventions should be coupled with increased funds toward programs which have been shown to decrease crime through alternatives to police.

Research Implications

In addition to policy implications, this dissertation has several implications for future research. Several long-standing theoretical and policy questions remain, and new questions were spurred from this dissertation. Most importantly, *would the adolescents respondents in this study have decreased their offending over time without this intervention?* This question has been answered to the extent that it can from the current study. Future research should examine a sample of adjudicated individuals and similarly aged individuals who were engaged in less serious offenses across a wider range of ages. Because individuals tend to desist or at least decrease offending with age, examining a wider range of ages and offender seriousness will help to assess whether individuals would have engaged in less crime over time even without police intervention (Cullen & Pratt, 2016).

Did Safe Streets result in treatment effect heterogeneity? It may be the case that all offenders reduce their frequency of offending, or it may be that some offenders reduce their offending substantially (or stop offending altogether) while others are not impacted. This could be a function of individual-level traits, such as age, race, sex, risk-aversion or self-control, or it could be a function of the type of offender, such as a property offender versus a violent offender. An additional benefit of individual level data is that it allows for the examination of person-specific attributes which may make one more or less susceptible to deterrent messages (Cohen, 1978). It is also possible that the effect of Safe Streets varied by the level of disorder in the targeted community. Broken Windows theory proposed that targeting disorder in communities which were not yet completely overburdened with crime and disorder would result in the most positive effects for community residents. Given the relatively small sample size, and data limitations, the current study design did not allow for the exploration of differences by individuals' traits or neighborhoods, but future research may benefit from exploring differences across individual characteristics such as race, sex, or age, as well as traits including low self-control or risk-aversion, or neighborhood factors like disorder.

Can police interventions like Safe Streets result in desistance from crime or are they mainly effective at reducing crime by decreasing one's frequency of offending? Within the criminological literature, most theories which discuss the deterrent effect of police focus on the idea that individuals will reduce their offending because particular crime opportunities are no longer worth the risk. Most of these theories do not explicitly state that policing and deterrence should result in full desistance from crime. Therefore, deterrence may be effective at reducing crime in the aggregate by decreasing the frequency of offending amongst a group of offenders. However, other criminological theories propose mechanisms which may result in one's full desistance of crime. These alternatives, which may include increasing prosocial opportunities, or bonds to education or employment may be more appropriate pathways to decrease the prevalence of offending in society. Future research should explore if police interventions or police contacts impact one's likelihood of desistance from crime and how this compares to other theories such as the age-graded theory of informal social control (Laub & Sampson, 1993).

Would differences in officer behavior or the implementation of Safe Streets have resulted in improvements to individuals perceptions of police? Future research should continue to explore ways in which perceptions of procedural justice, police legitimacy, and legal cynicism can be manipulated, with a focus on specific strategies at hot spots, such as procedurally just policing at hot spots or problem-oriented policing at hot spots. Recently scholars have suggested that research move away from broad evaluations of hots spots policing and instead focus on what police do at hotspots (Famega, Hinkle, & Weisburd, 2017). It is the hope that with more research, interventions can be designed which move beyond null effects for perceptions of police and instead, result in positive gains in attitudes toward the law and law enforcement. Recent research suggests that we need to move beyond examining the effectiveness of different strategies and instead home in on the individual officers' behavior in communities to assess why different programs are effective. For example, it is important to focus not on police presence but what police do at hotspots to determine what behaviors impact crime (National Academies of Sciences, Engineering, and Medicine, 2018).

Did the theorized mechanisms of perceptions of arrest risk and rewards to crime, perceptions of police and perceptions of disorder operate as mediators of the relationship between Safe Streets and offending? In the current study, formal mediation tests were not examined. The primary intent of this dissertation was to establish if main effects between Safe streets and perceptual outcomes, and Safe Streets and offending, were present. Future research which utilizes additional interventions and datasets with larger sample sizes should assess the potential mediating effects of perceptions of arrest risk, rewards to crime, perceptions of police, and disorder, as well as peer offending and informal social control. For example, formal mediation models can examine the effect of Safe Streets on specific crimes with crime-specific measures of arrest risk for certain crimes such as shoplifting, receiving stolen property, or stealing a car or motorcycle. These offenses do not align perfectly (e.g., motor vehicle theft would be measured with joyriding) but they have been examined together in prior research (see Anwar & Loughran, 2011). Models which formally assess mediation will be more straightforward tests of the theories of Deterrence and Rational Choice.

Did the significant effects of Safe Streets persist over time or did any null effects result in significant effects after more time passed? The existing hot spots policing literature has been critiqued for its focus on short-term outcomes. The need for a focus on longer-term outcomes, particularly in studies which examine perceptions of procedural justice and police legitimacy, has been noted by a group of criminologists in the National Academies of Sciences review of Proactive Policing (2018). This dissertation was primarily concerned with assessing if Safe Streets resulted in any immediate effects and unfortunately was designed in a way which made it possible to clearly examine these effects over time.

Finally, *did Safe Streets impact any additional unmeasured outcomes for either the adolescents in the sample or their caretakers?* It is possible that Safe Streets impacted outcomes that were not included in this study. For example, individual outcomes such as emotional reactivity, anxiety and mental health, neurological functioning, community involvement, and parental monitoring (only available for those living with parents) could be explored. Heightened police presence may have resulted in deleterious impacts on some of these psychological outcomes. Prior research has found that heightened law enforcement presence coupled with order-maintenance policing in New York City reduced test scores amongst African American adolescent boys (Legewie & Fagan, 2019). Safe Streets may also have resulted in changes to time spent in the community. For example, parents may have increased monitoring of their adolescents or decreased the time they allowed their adolescents to spend outside over concerns for their criminal justice contact once officer presence in their communities was increased. It is also possible that the adolescents themselves limited their time outdoors for this reason. Recent research suggests that individuals may respond to increased police presence by limiting their time outdoors (Fader, 2021). If police interventions reduce crime but at the expense of keeping individuals indoors and not engaged in their community, ironically one of the aims of Safe Streets and many hot spots policing interventions, than the benefits may not outweigh the costs.

Unfortunately, measures of parental monitoring were only asked of respondents who lived at home with their parents so there was not sufficient data on this outcome to assess this over time.

Conclusion

Deterrence scholars and practitioners have long made the claim that a large increase in police presence should reduce crime; "Very few people would violate the law if there were a policeman on every doorstep" (Andenaes, 1966; p. 960). More recently, scholars have suggested that such an invasive strategy would likely result in negative effects for community members. Given the evidence from police crackdowns and hot spots policing interventions, it seemed likely that a large increase in police presence could reduce crime but may result in negative consequences.

Until now, we did not know if police presence, and specifically hot spots policing, resulted in a perceptual deterrent effect, if it reduced offending behavior, or if it resulted in backfire effects ranging from increasing the rewards to crime, impacting neighborhood disorder, or harming perceptions of police for adolescents most likely to be impacted by hot spots policing.

Operation Safe Streets was not an intervention that placed police on every doorstep per se, but it was an intervention that led to a large surge in police presence. Between 200 and 200 hot spots were occupied by 2 patrol officers on a continuous basis for over 18 months. This resulted in an increase of 400-600 visible police officers in the city at all times. The current study demonstrated that this increase was enough to increase individuals' perceptions of arrest risk, but these perceptions may have not been increased enough to actually translate to reductions in offending. This is problematic given the vast amounts of time and money spent on increasing officer presence, however, the results of prior analysis on official crime data, suggest that Safe Streets did reduce crime. This paradox should be tested in additional studies to examine exactly how effective policing interventions are at impacting offending behavior. This may be useful in helping policy makers understand the limits of how much crime can be prevented by police alone.

Regardless of the results, it may now be time to think of ways to reduce crime through avenues other than punishment or punishment threats studied here. Safe Streets was not found to negatively impact perceptions of police, but it also appears not to have done anything to improve these perceptions. Furthermore, though Safe Streets increased perceptions of arrest risk, this did not translate into large reductions in offending. Given the lack of effects on offending prevalence, and lack of positive gains in perceptions of police, if the goal is to reduce the number of individuals engaged in crime and involved with the criminal justice system, and improve police-community relations, alternatives to policing should be prioritized.

More holistic approaches which incorporate additional social services to reduce reliance on officers and increase access to prosocial opportunities like education and employment should be prioritized. Approaches like this may be one potential strategy to reduce crime, improve perceptions of police and improve other outcomes for neighborhoods and their residents. These strategies may prove to be the most efficient way to create *Safe Streets* for everyone.

Tables

Table 7.1. Summary of Hypotheses & Findings

	Hypothesis	← Supported-	Inconclusive-		Refuted →	Conclusion
4.1	*Safe Streets will lead to an increase in perceptions of arrest risk of one's self in the period (wave) immediately following its implementation.*	✓				Safe Streets was significantly and positively associated with perceptions of arrest risk of one's self in all model specifications.
4.2	*Safe Streets will lead to an increase in perceptions of arrest risk of others in the period (wave) immediately following its implementation, but this effect will be smaller than the increase for arrest risk of oneself.*	✓				Safe Streets was significantly and positively associated with perceptions of arrest risk of others in all model specifications.
4.3	*Operation Safe Streets will have a larger effect on perceptions of arrest risk of one's self than perceptions of arrest risk of others.*	✓				The effect on perceptions of arrest risk of one's self are greater than the effects of the arrest risk of others.
5.1	*Safe Streets will lead to a decrease in the prevalence of offending in the period (wave or month) immediately following its implementation.*				✓	Safe Streets was not significantly associated with the prevalence of offending at the wave or month in any model specifications.
5.2	*Safe Streets will lead to a decrease in the frequency of offending in the period (wave) immediately following its implementation.*		✓			Safe Streets was significantly and negatively associated with the frequency of offending at the wave but not month-level.
5.3	*Safe Streets will lead to a decrease in the types of offending in the period (wave or month) immediately following its implementation.*		✓			Safe Streets was significantly and negatively associated with the variety score of offending at the wave and month-levels.
6.1	*Safe Streets will have no effect on perceptions of procedural justice in the period (wave) immediately following its implementation.*	✓				Safe Streets was not significantly associated with perceptions of procedural justice in any model specifications.
6.2	*Safe Streets will lhave no effect on perceptions of police legitimacy in the period (wave) immediately following its implementation.*		✓			Safe Streets was not significantly associated with perceptions of police legitimacy in main analyses but it was negatively associated in models without a lag in treatment.
6.3	*Safe Streets will have no effect on legal cynicism in the period (wave) immediately following its implementation.*	✓				Safe Streets was not significantly associated with perceptions of legal cynicism in any model specifications.
6.4	*Safe Streets will lead to a decrease in neighborhood disorder in the period (wave) immediately following its implementation.*			✓		Safe Streets was significantly and positively associated with disorder in models without a lag in treatment, fixed-effects models and for those from low drug disorder neighborhoods at baseline.
6.5	*Safe Streets will lead to an increase in the personal rewards to crime in the period (wave) immediately following its implementation.*		✓			Safe Streets was not significantly associated with personal rewards to crime in any analyses.
6.6	*Safe Streets will lead to an increase in the social rewards to crime in the period (wave) immediately following its implementation.*		✓			Safe Streets was only significantly and positively associated with social rewards in models without a lag in treatment.

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Appendix

Table A.1. First-Difference Longitudinal Linear Regressions Predicting Arrest

	Arrest			*Number of Arrests*		
	Model 1			Model 2		
	b		SE	b		SE
Safe Streets	-0.03		0.03	-0.20	*	0.08
Variety Score	1.58	***	0.10	4.47	***	0.29
Street Time	0.18	***	0.03	0.70	***	0.06
n = persons(person-waves)	654(3,170)					

Notes. SE = robust standard error

*p< 0.05; **p <0.01; ***p< 0.001

Table A.2. Outcomes by Wave

	Baseline	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6	Wave 7
Arrest Risk (Self)	5.04	4.71	5.00	5.13	5.26	5.28	5.24	5.33
Arrest Risk (Others)	5.36	5.23	5.33	5.44	5.38	5.33	5.29	5.31
Offending	88.14%	55.26%	47.92%	43.29%	42.58%	34.73%	34.48%	40.98%
Variety Score	18.05%	6.10%	5.51%	5.23%	4.87%	3.91%	3.75%	5.10%
Frequency of Offending	91.78	21.20	33.59	48.89	59.77	47.53	55.29	72.94
Perceptions of Procedural Justice	2.73	3.05	3.14	3.18	3.25	3.27	3.20	3.22
Perceptions of Police Legitimacy	2.21	2.18	2.24	2.29	2.26	2.30	2.26	2.24
Legal Cynicism	1.99	2.04	2.02	2.01	2.01	2.00	2.02	1.97
Neighborhood Disorder	2.60	2.72	2.70	2.73	2.70	2.69	2.68	2.64
Personal Rewards to Crime	1.63	1.37	1.27	1.32	1.22	0.98	1.03	0.88
Social Rewards to Crime	2.01	1.95	1.93	1.90	1.89	1.89	1.92	1.95
n	700	646	649	626	613	622	612	366

Notes. Sample sizes are reported for the maximum number of interviews for each time period.

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