THE EFFECT OF INTEGRATED CCTV CAMERA SYSTEMS ON CRIME IN PUBLIC PLACES: AN EVALUATION OF DETROIT "GREEN LIGHT"

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ABSTRACT

THE EFFECT OF INTEGRATED CCTV CAMERA SYSTEMS ON CRIME IN PUBLIC PLACES: AN EVALUATION OF DETROIT "GREEN LIGHT"

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The rapid growth of the "place and crime" literature has demonstrated the need for proactive police strategies in crime hotspots. Research consistently finds that most crime is concentrated at relatively few addresses and that these places tend to remain "hot" consistently over time (Sherman, Gartin, & Buerger, 1989; Weisburd, et. al., 2004). Today, some of the discussion has shifted to determining what strategies can best accommodate crime problems at these locations. With the advent of new technologies, researchers have begun examining whether closed-circuit television (CCTV) cameras exert a significant deterrent effect at crime hot spots. In 2016, Detroit began the "Green Light" initiative by outfitting businesses with CCTV cameras connected live to their computer-aided dispatch system. Utilizing the start of the Detroit Green Light initiative in 2016, this study examines 86 business that joined the Green Light program between January 1, 2016 and December 31, 2016, compared to a matched sample of businesses that did not. Using hierarchical linear models and Bayesian inference, this study assesses the impact of the Green Light program on violent crime, property crime, disorder crime, and calls for service in and around the immediate vicinity of businesses. A cost-benefit analysis of the program determines whether the program is a cost-effective method of crime reduction.

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INTRODUCTION

Following its decline as a center of manufacturing and industry, Detroit has experienced staggering levels of poverty and violence. While nation-wide violent crime rates have followed a decreasing trend over the past decade, Detroit's violent crime rate remains five times higher than the national average (Crime in the United States, 2017). In response to sustained high rates of homicides, non-fatal shootings, and violent victimization, the Detroit Police Department (DPD) utilized several offender-focused strategies that had been proven effective in other similar cities such as Boston and Kansas City (Braga, Hureau, & Papachristos, 2014; McGarrell, et. al., 2006). In 2012, the city of Detroit leveraged Federal partnerships through the Project Safe Neighborhoods initiative to prosecute violent gun offenders and gangs on the city's West side. Subsequently, in 2013, the city began project Ceasefire on the city's East side, that focused intelligence and enforcement actions on known gun offenders and groups. As these offender-based strategies evolved, an understanding began to emerge that certain features of the city disproportionately contributed to crime opportunities. In Detroit, many of the gas stations, liquor stores, and convenience stores generated large numbers of calls for service and were often observed as the nexus of street disputes, drug sales, armed robberies, and gang activity. An official DPD report found that about 25% of violent crimes reported between 10 PM and 8 AM occurred within 500 feet of a gas station (City of Detroit, 2016). The level of violence at many of these places prompted the city to begin a new, place-based initiative under the name "Detroit Green Light."

Beginning in January 2016 with eight initial gas stations, Detroit Green Light leveraged partnerships between local business owners and DPD. By December 31, 2016 there were 86 businesses participating in the Green Light program – most of them gas stations, liquor establishments, and convenience stores, while over 1,000 other similar businesses were not participating. Project Green Light represented a multi-faceted approach to improving safety at businesses in Detroit. As part of the program businesses purchased high-definition surveillance cameras and signage that identified them as a participant in the program (see Appendix A for an example of Green Light signage and camera mounts). These businesses were outfitted with improved outdoor lighting, high-speed internet and were connected to a live-stream feed monitored at DPD's real-time crime center. An important part of Green Light was its attempt to improve police responsiveness to calls for service at participating businesses. As part of the program all calls for service, regardless of severity, were considered "priority 1." This meant calls for service from Green Light businesses were prioritized, and officers were dispatched immediately. Following each call, community policing officers were required to meet with business owners and address any residual complaints or concerns. Officers were also encouraged to increase the number of proactive stops and patrols around Green Light businesses.

Participants in Green Light were required to meet a series of requirements, however. Businesses in the Green Light program had their locations inspected to ensure they remained in compliance. This meant businesses were responsible for maintaining their camera systems, ensuring adequate lighting, and keeping their signage visible. Additionally, business owners were expected to fund the installation of cameras and the maintenance fees, that reduced direct costs to the city of Detroit (Project Green Light Detroit, 2017). On average, the cost to each business was approximately $4,000-$6,000 for installation and $150 per-month for maintenance. This initiative was intended to supplement other violence-reduction programs already occurring in the city, such as Ceasefire and COMPSTAT. These programs, which focus resources on known chronic offenders, were expected to utilize information from Green Light businesses and leverage partnerships with business owners to develop a comprehensive solution to public-area violent crime. Along with this, the Green Light program led to the construction of a $9 million real-time crime center (RTCC), that allowed crime analysts to evaluate surveillance footage in real time.

The anticipated effect of the Green Light program was twofold: first, increased surveillance at locations and real-time connections to the police department may lead to faster police response, improved identification of offenders, and better cooperation between business owners and police. Second, the presence of increased lighting, visible cameras, and Green Light signage may produce a deterrent effect against potential offenders and lead to fewer reported crimes, calls for service, or public disorder incidents. An evaluation of the Detroit Green Light program was commissioned as part of the federal funding provided under the Smart Policing Initiative (SPI). In particular, the goals of the evaluation were to determine whether the Green Light program produced a measurable decrease in on-premise violent crime, property crime, disorder crime, and calls for service. Second, the evaluation included a cost-benefit analysis component to determine whether the costs incurred by the city of Detroit were offset by the reduction in criminal activity.

In the chapter 'Review of Literature', I provide a brief review of the extant research relevant to place-based criminology. I focus my attention on the current understanding of how place management, CCTV cameras, and citizen-police partnerships affect crime in public places. I consider the interrelationship between how offenders make decisions, and how designing public places can change the mental calculus in the decision to commit a crime. I discuss how routine activities theory and the place-based crime literature are especially relevant to understanding the impact of interventions at crime hot spots. I also consider the relevance of crime concentration as the basis for using Green Light as a deterrent strategy. In this discussion of the literature I integrate examples that are relevant to the Green Light initiative, and the wider use of integrated CCTV camera systems in general. In the chapter ‘Methodology’ I provide an outline of the Green Light program in Detroit, the units of analysis, and the steps taken to provide a causal interpretation of the results. I also review the use of Bayesian statistics as a method of statistical inference and its links to the cost-benefit analysis. In ‘Analysis’ and ‘Results’ I describe the study, the use of the propensity score to develop a comparison sample, and then provide an in-depth review of the results. The following chapter ‘Supplementary Analyses’ includes a set of sensitivity analyses regarding the chosen buffer size and also includes the cost-benefit analysis. Finally, I discuss how the results from this study have both practical and theoretical relevance to research on CCTV camera systems in the chapter ‘Conclusion and Discussion.’

REVIEW OF LITERATURE

CCTV Cameras and Routine Activities Theory

CCTV cameras have been used as both a deterrent against crime and as an investigative tool for at least several decades (Clark, 1997). Utilizing CCTV cameras as a deterrent has a rational basis, and also has some support in criminological literature. Routine activities theory states that the intersection of suitable victims and motivated offenders in the absence of guardians increases the likelihood of crime occurring (Cohen & Felson, 1979). Under routine activities the context in which victims and offenders meet determines the risk of a crime. Therefore, changing either the characteristics or context of places can reduce deviant activity and crime. For instance, Felson (1995) posited that guardianship plays a greater role in crime prevention than the presence of motivated offenders. He suggested a revision of the original routine activities tenet to include the importance of "effective guardians", "motivated offenders without an effective handler", and "facilitating places[s] without an attentive manager." It is this idea of guardianship and lack of attentive managers that is especially salient to crime reduction strategies aimed at public businesses.

Several studies indicate that individuals taking some degree of responsibility for the safety on locations is substantially important (Clarke, 1995; Felson, 1995; Mazerolle, 1998). These studies suggest that place managers, such as officers on patrol, watchful employees, or concerned citizens discourage crime by monitoring on-site activities. Personal or assigned responsibility for the safety of places (so-called "handlers"), such as doormen at a bar who deny entry to intoxicated individuals, or store owners who stop customers from shoplifting, prevent crime by directly deterring motivated offenders (Clarke, 1995). Semi-public places like gas stations, convenience stores, or restaurants are at higher risk for crime when they are poorly managed or policed by ineffective handlers (Mazerolle, 1998). Motivated offenders may be more likely to commit crimes in places where they feel no one is taking responsibility for safety and believe the risk of apprehension, identification, or sanction is low. On the other hand, when place managers engage in collective crime control activities, crime and disorder often decreases (Weisburd & Green, 1995; Mazerolle, 1998; Welsh, Mudge, & Farrington, 2010)

In the criminological literature, CCTV cameras may play a guardianship role within the routine activities framework through their role as a formal surveillance mechanism (Clarke, 1995). CCTV cameras enhance the ability of guardians at the location to detect crime and may deter offenders who believe the risk of detection is credible. Therefore, the sole presence of a CCTV camera may even serve as a guardian in and of itself by recording activities at the location and serving to identify offenders at a later point. Real-time integrated CCTV cameras (such as those in the Detroit Green Light initiative) can directly connect surveillance footage at the location with law enforcement, who may deploy officers when illegal activity is detected. Because some places lack effective handlers willing to intervene when a crime is about to occur or is occurring, integrated CCTV may hasten the arrival of "peacemakers" who can deal with these problems (Felson, 1995). For instance, bystanders or employees may be hesitant to break up a drug deal occurring on the premises, while police officers would likely intervene. However, these benefits are contingent on security personnel monitoring the cameras (Clarke, 1997; Piza, et. al., 2016).

Situational Crime Prevention and CPTED

Beyond individual guardianship at places, much of the literature regarding crime and place focuses on the physical features of locations. Beginning in the early 1970s, a greater understanding of how the design of buildings, streets, and cities could affect public safety emerged (Clarke, 1997). Environmental criminology and situational crime prevention consider how opportunities and local contexts facilitate crime. Most simply, these theories recognize that crime requires a target location (such as a business or parking lot) a victim (either an individual or property) and, possibly, a facilitator (crowbars, guns, or disinhibitors like drugs and alcohol) (Clarke, 1997). Situational crime prevention consists of identifying contributing factors and altering their features or their contexts to deter crime. When considering public areas, the tenets of CPTED – or Crime Prevention Through Environmental Design – are particularly salient. The principles of CPTED state that crime, and fear of crime, can be reduced by designing public features that reduce opportunity and increase the cost of committing crime. These consist of six broad approaches that include: territoriality, surveillance, access control, activity support, image management, and target hardening (Cozens, Saville, & Hiller, 2005). Of these factors, surveillance, image management, and target hardening appear most naturally related to the Detroit Green Light initiative.

Surveillance is one of the most relevant CPTED feature for Green Light. Place-based surveillance represents watching or monitoring an area for suspicious activity. This can consist of both informal and formal methods with varying levels of activity. More passive, or natural methods of surveillance focus on improving visibility of the location, such as increasing lighting, removing trees or shrubs that block windows, and improving line of sight (Cozens, Saville, & Hiller, 2005). Locations with fewer areas for concealment increase the likelihood of an offender being detected by either formal or informal guardians. More formal methods of surveillance utilize shopkeepers, employees, or security guards to watch vulnerable areas – what Felson (1995) might call "place managers." There is some research indicating that crime is deterred in businesses where security guards are employed, but the evidence is somewhat mixed (Cozens, Saville, & Hiller, 2005). CCTV cameras occupy another obvious facet of the surveillance category. Cameras may enhance the ability of place managers to observe potential offenders at the location if they are used actively - that is, if the feed is watched continuously (Ekblom, 2011). Offenders who believe they are being watched may change their behavior – forcing them to less valuable areas out of sight or preventing them from completing a crime at all. While passive approaches like improved lighting have had mixed effects (Cozens, et. al., 2003), there appears to be somewhat more positive support for active surveillance methods like security guards (Cozens, Saville, & Hiller, 2005), or CCTV cameras (Farrington & Welsh, 2002). A study by Piza and others (2014) found that CCTV cameras combined with police response increased the police response time to incidents and arrest rates. This effect decreased, however, when more cameras were added than staff to monitor them.

Improving the image and built environment of a place represents another important feature for CPTED. The relationship between observed disorder, fear of crime, and actual crime is well documented in the criminological literature. Wilson and Kelling's (1982) "Broken Windows" study suggested that individuals use the physical environment as indicators of social cohesion and informal social control in the neighborhood. In places with high levels of visible disorder and decay, offenders may use this as a visual cue that deviant activity will, at the very least, not be impeded (Taylor, 1991). In some cases, simple graffiti may highlight a location as a gang hangout or drug-selling market (Ekblom, 2011). Locations that are not well managed, such as vacant homes or dilapidated businesses can serve as sites for drug dealing or prostitution. Additionally, there is evidence that physical disorder increases fear of crime, which then leads to reduced levels of informal social control – what Skogan (1990) calls a "spiral of decay." Adequately maintaining and improving the physical attributes of a location can provide decreases in fear of crime and victimization (Felson, et. al., 1996; Eck, 2002). Similarly, removing graffiti, cleaning up trash, and ensuring the premises are properly looked-after may reduce incivilities and crime (Ekblom, 2011). Indeed, there is evidence that the mere perception that a location is being maintained may reduce disorder and fear of crime (Painter & Farrington, 1997). The reduction in fear of crime at locations may increase the use of public spaces and, therefore, increase the level of informal place management (Tilley, 1997; Felson, 1995). Some tentative evidence exists that the presence of CCTV cameras may decrease fear of crime under some circumstances (Cho & Park, 2017). Under the Green Light model, businesses are expected to maintain their properties in line with city guidelines – that include maintaining lighting, signage, and keeping premises clean.

Target hardening is, perhaps, one of the most visually obvious facets of CPTED. While other factors focus on improving the image of the location and reducing crime opportunities through better surveillance, target hardening increases the effort that offenders must expend in order to commit a crime (Cozens, Saville, & Hiller, 2005). These methods may include installing new locks, reinforcing doors or windows, or placing employees behind bullet-proof glass windows. Most generally, target hardening is used to protect targets in and of themselves – such as preventing damage to the property. Target hardening strategies also reduce the ability for offenders to move freely in and around locations by constraining their movement. Locking doors, placing surveillance cameras in alleyways, and keeping valuables in locked containers limits the ability of offenders to freely operate (Ekblom, 2011). CCTV cameras may play a role in target hardening strategies as they may be used as part of a greater security system that limits access to unknown individuals. CCTV cameras may reduce the ability for offenders to successfully escape from the location after committing a crime, or it may prevent entry by summoning security guards or police when they are detected. There has been some research indicating that target hardening strategies resulted in reductions in burglaries in a number of locales (Tseloni, et. al., 2004).

The Role of Deterrence and Rational Choice Theory

The underpinning theory behind routine activities and CPTED assumes that criminal actors are both rational and calculating. Primarily, these theories focus on how offenders perceive the risks and benefits of committing a criminal act. This perspective, as originally ascribed to Beccaria and Bentham, theorizes that individual seek the greatest amount of pleasure while minimizing their personal cost (Piliavin, et. al., 1986). This idea lends itself to an easily understood and logical theory of crime prevention. By simply increasing the costs of committing crime, potential offenders will be deterred from committing a further act. While a somewhat simplistic model, based in the classical school of criminology, it gave way to contemporary theories of deterrence and rational choice theory (RCT) (Pratt, et. al., 2006).

More current theories posit that offenders, who have some underlying criminal propensity, make a series of limited cost-benefit decisions prior to committing a crime. They weigh the risk of being detected or apprehended, their likelihood of being able to complete the crime, and the potential benefits after the crime is committed (Pease, 2006). Tests of this theory have found some support for the RCT perspective. Nagin and Paternoster (1993) found college students' self-reported likelihood of drunk driving, theft, or sexual assault was related to their perceived risk of discovery and consequences. Among a survey youth in New Zealand, those who reported a higher fear of being caught committing a criminal act reported lower levels of offending - even among those with high criminal propensities (Wright, Caspi, & Moffit, 2004). In a longitudinal survey of youth, Matsueda, Kreager and Huizinga (2006) found that increased perceived risk decreased the incidence of subsequent thefts and burglaries - however these effects were small. While offenders may weigh risks and benefits before committing a crime, the decision-making processes prior to the crime are likely more limited than fully-rational (Cornish & Clarke, 1987). Therefore, while rational choice likely has some impact on the likelihood of an offender committing a crime, its effect may be marginal relative to other factors (such as self-control).

Situational crime prevention relies on these theories, that suggest increasing the risk of an offender being detected, or lowering the probability of success, will deter deviant and criminal behavior (Hayward, 2007). Similarly, in a routine activities perspective, motivated offenders will be deterred when guardians are present in places with suitable victims (Cohen & Felson, 1979). Both these theories assume that criminal propensity is given (Clarke & Felson, 1993), although Hirschi (1986) maintains that self-control remains the underlying motivation. Given these assumptions, situational crime prevention involves designing locations to minimize the likelihood of a crime succeeding and increasing the probability of an offender being detected or apprehended. In the case of CCTV cameras deterring crime, the probable causal mechanism would function through potential offenders determining that the increased risk of detection or apprehension outweighs the benefits of their crime. Here, both deterrence theory (Pratt & Cullen, 2005) and rational choice theory (Cornish & Clarke, 2014) support the major claims behind routine activities theory. Under rational choice theory, offenders are motivated to commit crime in order to satisfy their own personal needs, such as money, status, sex, and excitement (Clarke, 1995). Offenders make a series of simple decisions about the place and method in which to obtain these needs, weighing the potential risks versus the possible rewards (Cornish & Clarke, 1987). While deterrence based on the threat of severe punishments may not be especially effective, the increased risk of apprehension may likely be a more effective crime deterrent (Nagin, 2013).

A potential burglar, then, might be less likely to break into a business with bright lighting and a private security guard versus a dark, unsecured business. Considering many offenders are often motivated by short-term rewards or are in desperate need of money, the presence of guardianship at a location may be enough to outweigh any potential benefits of crime (Hirschi, 1986; Wright & Decker, 1994). Increasing the costs or effort of committing a crime may deter those who are motivated by low self-control or a need for immediate gratification, however some evidence suggests that harsher deterrent measures do not reliably decrease crime (Pratt & Cullen, 2005). Secondary factors, such as intense anger, alcohol, or drug use, may impair an individual's decision-making processes, and increase their likelihood of committing a crime despite overwhelmingly negative consequences (Exum, 2002).

Despite the seemingly credible threat of CCTV cameras, there is evidence that offenders often disregard the increased risk of apprehension. Some studies have shown that offenders are willing to commit crimes within view of surveillance cameras (Butler, 1994; Ditton & Short, 1998). Offenders may feel that simply being observed by cameras does not necessarily increase police response time or later investigative success (Ditton & Short, 1998). The way offenders manage risk perception suggests that many may underestimate their likelihood of apprehension, that increases after successful crimes (Horney & Marshall, 1992; Wright, Caspi & Moffitt, 2004). Indeed, it appears that offenders often are concerned more with the presence of police, rather than being identified (La Vigne et al., 2011). Therefore, CCTV camera programs that have integrated police response may provide a more credible threat of apprehension (La Vigne et al., 2011; Piza, et. al., 2016). However, deploying officers based solely on cameras operators detecting crime is likely infeasible (Piza, 2014).

Crime Concentration, Hot Spots, and Place-Based Initiatives

Consistent with literature on routine activities, a growing body of study has examined the places and contexts in which crime occurs. From among the earliest studies of Chicago neighborhoods, researchers have found that crime and disorder occur disproportionately in a small number of places (Sutherland, 1973). Sherman, Gartin, and Buerger (1989), found that 50% of calls for service came from only 3% of addresses in Seattle. Further replications have illustrated similar levels of concentration in other locales. For instance, 75% of gun crime in Boston was concentrated at about 5% of street blocks, while 50% of commercial robberies occurred at only 1% of street blocks (Braga, Papachristos, & Hureau, 2010; Braga, Hureau, & Papachristos, 2011). The sum total of this literature spurred Weisburd (2015) to coin the term, "the law of crime concentration" – suggesting that crime almost always disproportionately affects certain individuals or places. A side-effect of this shift to place-based research is the increasing use of "micro-places" as the unit of analysis. While early research relied on relatively large geographical units – such as the tract or block-group level, micro-place research utilizes individual addresses or street blocks as the ideal unit of analysis. Supplementary studies have found that much of the variation in crime occurs at a very small spatial resolution (Groff, Weisburd, & Morris, 2009; Groff, Weisburd, & Yang, 2010). Street blocks, street segments, or spatial grids have been deemed more effective at predicting future criminal events (Rosser et. al., 2017; Ratcliffe & McCullagh, 1999).

While many studies have documented the importance of studying the concentration of crime, these findings are not especially new. Beginning with the use of physical pin maps, police agencies have long identified crime “hot spots” in their jurisdictions. However, an important development in placed-based criminology regards the remarkable temporal stability of crime hot spots. Utilizing group-based trajectory models, Weisburd et. al. (2004), illustrated that most places with little or no crime remained free of crime. On the other hand, a small number of high-crime street blocks experienced somewhat volatile trends, but generally remained high and stable. Wheeler, Worden, and McLean (2016) found very similar time-stable trajectories in their analysis of crime hot spots in Albany, New York. An important addition to their analysis found that higher crime street blocks tended to cluster near one another – what they suggested might be due to a diffusion effect of crime. A study of disaggregated crime types at street blocks in Vancouver, Canada found that all crime types (assaults, burglaries, robberies, thefts) were concentrated at a similar number of street blocks, and exhibited similar time-stable trajectories (Andresen, Curman, & Linning, 2016). Both replications of Weisburd et. al.’s (2004) original study confirmed that the majority of places see little crime, and rarely change over time.

Given evidence that crime is largely time-stable at relatively few places, a number of practical implications have developed from this literature. Hot spot policing, while contentious, has considerable support among evaluation studies. Several high-quality randomized experiments have shown that high crime locations that receive directed police attention observe decreases in crime (Rosenfeld, Deckard, & Blackburn, 2014; Braga, Papachristos & Hureau, 2014; Braga & Bond, 2008). While directed patrol at crime hot spots appears to have some impact, there is evidence as well that the specific activities that police perform at crime hotspots have considerably more importance. Problem oriented policing, a strategy where police identify specific problems and develop a response, may also play a role in reducing crime at hot spots (Goldstein, 1979; Weisburd, Telep, & Eck, 2010). Studies examining problem-oriented policing have illustrated the wide variety of circumstances in which it can be applied. Braga et. al. (1999), and Taylor, Koper, and Woods (2011) examined the impact of directed police activity on violent crime hot spots, finding that these strategies reduced the incidence of violent crime. Similarly, Weisburd and Green (1995) and Braga and Bond (2008) found that problem-oriented policing reduced drug selling and physical and social disorder. Both directed police patrol and problem-oriented policing have been shown to generate a "diffusion of benefits", where areas just outside the targeted area see similar decreases in crime (Clarke & Weisburd, 1994; Weisburd, et., al, 2006).

Theoretical Implications for CCTV Cameras and Detroit Green Light

The relevant literature in criminology generally supports the use of CCTV cameras as a method of reducing crime in public places. Routine activities theory predicts that in places where guardianship is present, motivated offenders will be deterred. In this case, CCTV cameras operate as a measure of guardianship. The presence itself of a camera may deter offenders because they fear being detected. On the other hand, CCTV cameras may increase the swiftness and certainty of punishment. If offenders believe they are more likely to be identified by the police and caught, they may be deterred from locations with cameras. Indeed, a central part of the Green Light initiative was designed to increase the visibility of security at businesses. The signature flashing green light at participating businesses provides a deterrent message to would-be offenders that the location is being actively protected and monitored by the police. Similarly, signage adds additional credibility to the message that the location is under protection.

The place-based criminological literature finds that a disproportionately large amount of crime occurs in a small number of places, largely in agreement with the underlying theory of routine activities. Locations that have many potential victims and are lacking credible guardians are at an increased risk for victimization. These places with disproportionately high numbers of crimes consistently year to year likely have features that are criminogenic in nature. For instance, they may represent a block of bars with unruly patrons, or a neighborhood of vacant homes used for drug selling. In the Detroit context, violent victimization at businesses (such as gas stations or convenience stores) is often due to a high volume of customers coming and going in places where on-premise security is low and police responses are often slow (Crichlow & McGarrell, 2016). In addition, these places often operate as open-air drug markets, gang hang-outs, and the nexus for ongoing street disputes. Consistent with the theoretical backing stated above, reducing the disproportionate levels of crime at these places might logically begin with increasing guardianship. The literature on routine activities, deterrence, and rational choice theory seem to agree that integrating CCTV cameras with computer-aided-dispatch might provide a meaningful way to reduce crime at chronically problematic businesses.

Prior Evaluations of CCTV's Impact on Crime

A substantial portion of research on CCTV cameras' impact on crime has come from the United Kingdom (UK), where the utilization of cameras in public places has increased precipitously. A 2013 report stated that the UK had employed between 4.2 and 5.9 million CCTV cameras (Norris & McCahill, 2005). While not nearly as prolific, the United States has begun a similar regimen of increasing surveillance in public places. However, this increasing reliance on CCTV as a tool to reduce crime is not yet fully supported in the evaluation literature. While there exists a logical theoretical basis for the deterrent effect of CCTVs on crime, the actual measurable effect is still in dispute. Several studies and meta-analyses have systematically examined the effect of CCTVs on a number of outcomes, revealing somewhat mixed results (Farrington, et. al., 2007; Welsh & Farrington, 2009).

Evaluations in the UK found that the installation of CCTV cameras at car parks was associated with a decrease in break-ins and thefts from vehicles (Tilley, 1993), while a Scottish town center study found conflicting results (Ditton & Short, 1999). Other studies found small decreases in property crimes, burglary, and theft from vehicles in town centers (Armitage, 2002). A subsequent UK evolution of 14 camera systems across a number of different locales indicated that cameras in car parks and train stations showed the greatest deterrent effect on property crime, but relatively little effect on crime in public or residential areas (Gill & Spriggs, 2005). Most of the observed crime decrease was related to reductions in vehicle theft, but not other types of crimes (Farrington, et. al., 2007). Bridging both UK and US evaluations of CCTV cameras, Welsh, and Farrington (2009) performed a meta-analysis on 41 studies evaluating the effectiveness of CCTV cameras on crime in a number of settings (primarily public housing, public transport areas, and car parks). Their analysis utilized only studies where CCTV was the focus of the intervention, where crime was a measurable outcome, where an experimental or quasi-experimental design was used, and where statistical power was plausibly high enough to detect an effect. The results of their meta-analysis suggested the installation of CCTVs in treatment areas decreased crime by roughly 16% relative to control areas. However, they found most of the positive, significant results were limited to British studies that occurred in car parks. A subsequent study that limited analysis to CCTV programs that were evaluated under randomized or natural experiments found similar, positive results (Alexandrie, 2017). On average, treated locations observed a decrease of 24 to 28% in property and disorder crimes. There was little evidence of crime displacement or diffusion of benefits. However, much of the benefit was observed in public street settings and subway stations, but not in parking facilities.

Recent studies carried out in the US have found a similar, modestly positive crime reduction effect of CCTV cameras. More recent work has evaluated the impact of a few cameras in small areas, or single cameras at individual street intersections. Many previous studies have only examined the effectiveness of CCTV cameras as an aggregate effect (i.e.: many cameras at a single business or location, or within an entire neighborhood); however, research suggests that much of the variation in crime occurs at a relatively small scale (Weisburd, 2015; Steenbeek & Weisburd, 2016). Ratcliffe, Taniguchi and Taylor (2009) evaluated the installation of CCTVs at street blocks in Philadelphia. They found locations with CCTVs saw a 16% reduction in disorder crimes and a 13% reduction in all crimes. They were unable to determine whether the presence of CCTVs had any effect on violent crimes, which they attributed to low baseline counts. An important finding in their study was that, while crime decreased by 13% overall, a supplementary analysis revealed that half of locations that installed CCTV cameras saw *no* significant effect on crime. Caplan, Kennedy, and Petrossian (2011) utilized individual viewsheds of CCTV cameras at street blocks in Newark, NJ – finding a measurable decrease in crime 12 months after the installation of cameras. They observed decreases in both shootings and auto thefts, with no evidence of displacement, and weak evidence of diffusion of benefits. However, they only utilized a simple pre-post design, that did not account for month-to-month variation in crimes during the study period. Similarly, McLean, Worden, and Kim (2013) examined 150-foot viewsheds around 12 CCTV cameras in Schenectady, New York. They found pole-mounted cameras were responsible for a modest decrease in all crime, with the most substantial decrease in disorder crimes and calls for service. Importantly, they noted the wide variation in effects between cameras, which they noted as strength of their study design. More recently, Lim and Wilcox (2017) found the effect of CCTV cameras installed in Cincinnati was extremely modest and constrained to a limited number of locations and times. Thus, the current literature regarding the deterrent effect of CCTV cameras on crime indicates a somewhat positive, but highly variable and conditional effect. In the next section I will discuss the implications of these mixed findings on CCTV cameras.

Implications of Prior Research on CCTV Cameras

Evaluations of CCTV cameras' effect on crime indicates a generally positive effect (it seems to deter crime, especially property crime and minor offenses), but this effect seems to vary considerably – both between and within individual studies. One suggested reason for the significant variation in reported effects may relate to how the camera systems are implemented, maintained, and used. Some studies have found that the effectiveness of CCTV cameras was correlated with the degree of coverage, which is maximized in car parks (Farrington, et. al., 2007). If crime occurs outside the viewshed of the camera, then the possible deterrent effect may be considerably lessened (Caplan, Kennedy, & Petrossian, 2011). Therefore, the placement of cameras may be a more important factor in its effectiveness. Furthermore, many of the CCTV interventions were also accompanied by increased lighting and security guards (Welsh & Farrington, 2009). Consistent with the principles of defensible space and CPTED, a combination of solutions may be more effective than a single solution (Cozens, Saville, & Hiller, 2005). Finally, many of the studies did not indicate whether or not the cameras were being actively monitored. While the mere presence of a camera may have some deterrent effect of its own (Gill & Spriggs, 2005), cameras that are actively monitored may be more likely to detect, and prevent, criminal activity (Caplan, Kennedy, & Petrossian, 2011). The implications of these mixed effects suggest that the cost of installing CCTV cameras may not outperform cheaper and similarly effective solutions – such as improved street lighting (Lawson, Rogerson, & Barnacle, 2017).

The most comprehensive meta-analysis evaluating the effectiveness of CCTV cameras as a crime prevention tool indicated that most of positive effect of cameras came specifically from studies in the UK and which were conducted in car parks (Welsh and Farrington, 2009). Within the US evaluations of CCTV camera systems have observed similar positive effects - however those that examined individual cameras (rather than groupings of cameras) found the effect varied from camera to camera (Ratcliffe, Taniguchi and Taylor, 2009; McLean, Worden, and Kim, 2013). Failing to account for within-study variation may produce results that "average over" the differences between locales. A profound weakness with some of the existing literature is mistaking the lack of a statistically significant effect for *no* effect – something McLean, Worden, and Kim (2013) point out in their own study. Many prior evaluations of CCTV cameras have utilized samples that are quite small by conventional standards and have minimal power to detect a statistically significant effect. As Gelman and Carlin (2014) point out, unpowered studies have both an increased risk of type II errors (incorrectly retaining the null hypothesis), and of statistically significant effects with inflated effect sizes or with the wrong sign. Given the size of estimated effects and the significant variation between locations, there is a need to develop a methodology that can directly model uncertainty in parameter estimates and variation between units. Ratcliffe, Taniguchi, and Taylor (2009) identify the importance of utilizing hierarchical linear models (HLM) that can control for time trends and combine information about individual and group effects. More current studies have adopted methods that address this problem, primarily using hierarchical growth-curve models (see Lim & Wilcox, 2017).

METHODOLOGY

Site Description

In the past three decades the city of Detroit has experienced among the highest levels of concentrated disadvantage, poverty, and violent crime. As one of the many rust belt cities that was negatively impacted by the downfall of the auto manufacturing industry, Detroit's population declined from a peak of 1.8 million in 1950, to roughly 670,000 in 2016 (United States Census Bureau, 2017). By 2013, the city had nearly 80,000 vacant homes and a joblessness rate twice the national average (Crime in the United States, 2017). This precipitous outmigration of population brought about a decline in neighborhood quality and a paucity of funds to maintain public places – especially business avenues and neighborhood corridors. Increases in violent crime plagued the city, which peaked in 1994 at a rate of nearly 2,700 per 100,000 residents – placing the city among the highest in the nation (Crime in the United States, 2017). However, from the late 2000's through 2017, crime in the city had decreased. In general, both violent and non-violent crimes exhibited a downward trend since 2010, decreasing at a rate of about 5-10% per year (see Figure 1). By 2017 reported property crimes fell about 35% from 2010, while violent crimes decreased by 33% and disorder crimes by 26%. While crime in Detroit has decreased substantially in the past decade, following nationwide trends, the rate of violent crime remains well above national averages (UCR, 2017).

Figure 1. Detroit City-Wide Crime Incidents (2010 – 2017)

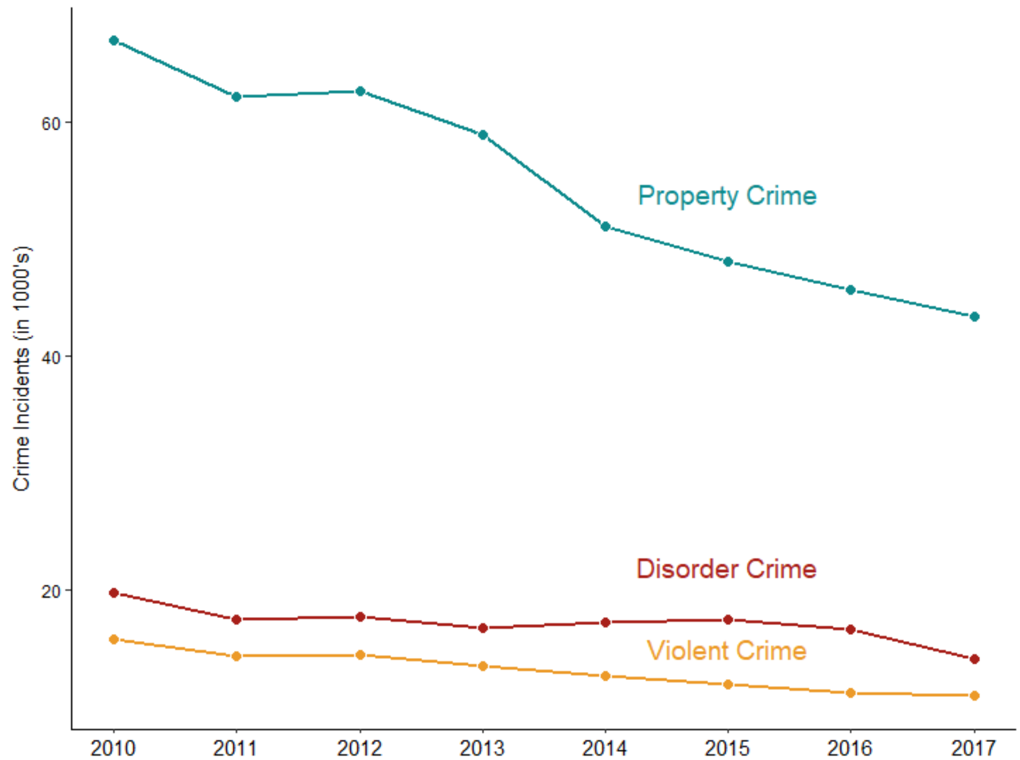

Figure 1 shows the city-wide number of disorder crimes, property crimes, and violent crimes by year. In general, property crimes and violent crimes decreased at a rate of about 5% to 10% per year after 2012, while disorder-related crimes saw more sporadic decreases. In 2015, one year before Green Light was implemented, city-wide property and violent crime reports were about 7% lower than 2014, while disorder crimes were relatively similar.

Detroit is divided into twelve separate police precincts, corresponding to historical neighborhood boundaries. Violent crime is highly concentrated in a handful of neighborhoods on the East Side (comprising the 5th and 9th precincts) and the West Side (the 6th and 8th precincts). The central business district and Midtown are comprised of the 1st, 3rd and 7th precincts, and represent an area of economic improvement and gentrification. While Detroit's downtown districts have experienced a recent revitalization, many of the outlying neighborhoods remain impoverished and lack amenities. Violent crime concentrated in and around neighborhood businesses has been noted by the Detroit Police Department as a significant problem. In an effort to reduce violent activity at businesses, the city of Detroit began work on its Green Light initiative. Figure 2 displays a map of the city of Detroit, with the 86 Green Light businesses implemented in 2016 highlighted in Green.

Figure 2. Map of Detroit and Green Light Businesses (2016)

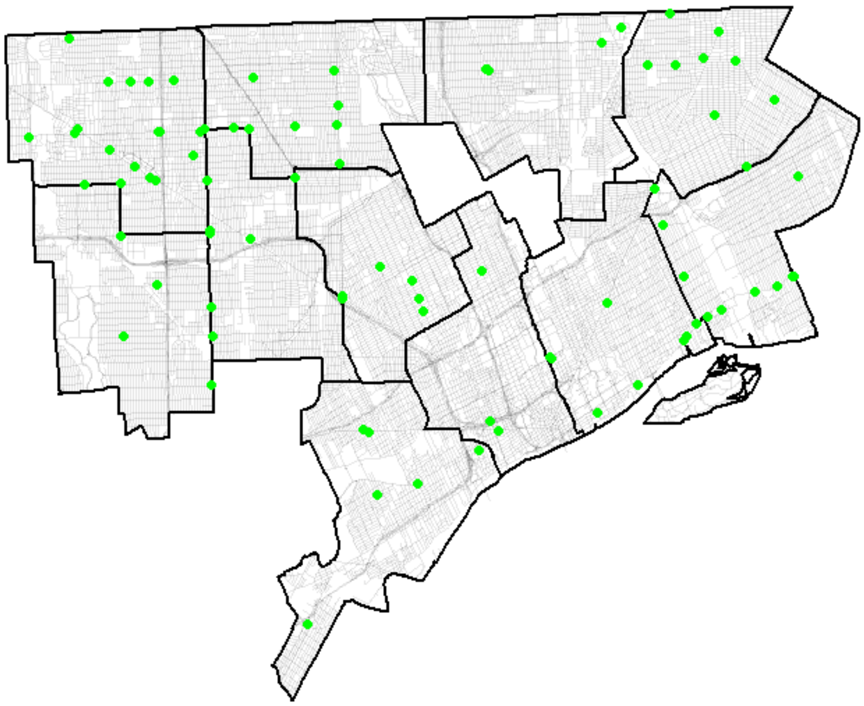

Figure 2 displays the city of Detroit and the 11 police precincts (shown in black lines). The green dots correspond to the 86 Green Light businesses that joined the program in 2016. Note that the distribution of businesses is roughly distributed across the city, but a large proportion are concentrated in the north-western portion of Detroit (police precinct 8).

Participating Businesses

Implementation of the Green Light program began in late 2015 and formally started on January 1, 2016 with 8 gas stations. While the initial plan was intended to include only businesses open past 10 PM (such as gas stations and liquor stores), emerging interest from other retail establishments and restaurants led to an expansion of the program. Businesses were continually added to the program throughout 2016, with about half the sample (n = 44) on-line by October 1, 2016. The speed of implementation increased during the last three months of 2016, with the remaining 43 businesses connected by January 1, 2017 (See Figure 3). By early 2018 the city of Detroit had added approximately 280 businesses to the Green Light program – including the Greektown corridor. In addition, DPD's new Real Time Crime Center (RTCC) was formally opened in November of 2017 – adding greater monitoring and dispatching capabilities.

Figure 3. Proportion of Businesses Added to Green Light (1/1/2016 – 12/31/2016)

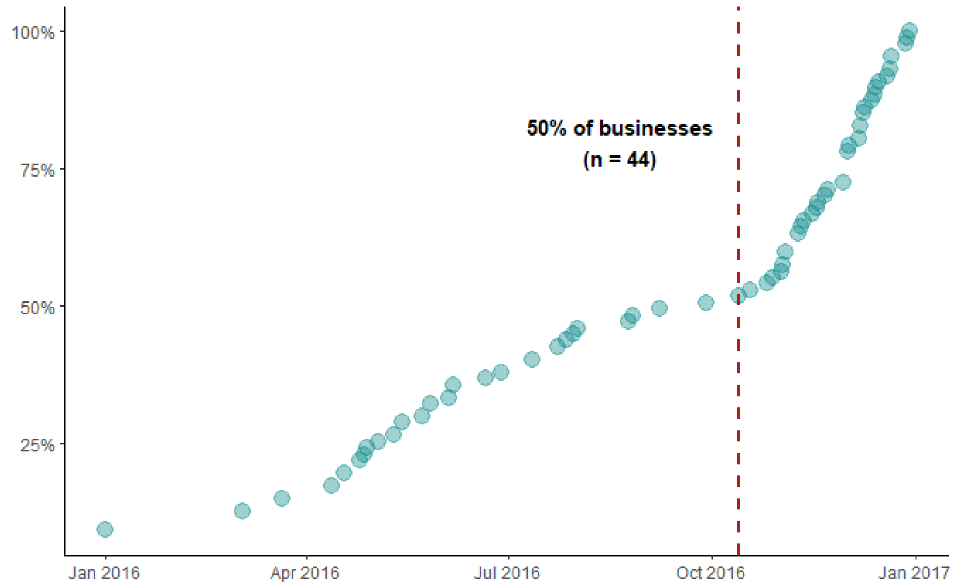

Figure 3 displays the implementation phase of Green Light for 2016. The y-axis reports the cumulative proportion of businesses connected to DPD's Green Light system. Eight initial businesses began on Jan 1, 2016. Implementation to other businesses began slowly but increased rapidly after October of 2016. The final 50% of businesses added went on line between October and December.

For the purposes of this study, the analysis was limited to the first phase of Green Light which occurred between 2016 and 2017. Therefore, the final sample consisted of 86 businesses that joined Green Light during 2016. While most of these businesses were gas stations (n = 31), they included a mix of commercial establishments. Twenty-one locations were fast-food or dining locations, 13 were convenience stores, 13 were liquor (or so-called "party stores"), and 3 were bars or adult entertainment businesses where liquor is served on premise. Five other locations did not fall within any of these categories and were labeled "other" – these included two cell phone stores, a women's center, a coin laundry, and a dance studio (See Appendix B for all study business types and live dates).[1] In the section below, I discuss my primary research questions and elaborate on the study selection and measurement criteria. I then provide a discussion elaborating on the methodological choices taken and how they are relevant to this study, and CCTV camera studies in general.

Research Questions and Selection Criteria [2]

The city of Detroit implemented the Green Light program primarily in hopes of increasing safety at businesses by deterring crime and increasing the rate of apprehension (City of Detroit, 2017). In order to evaluate the effectiveness of this program, I address three specific research questions:

1. Is the installation of Green Light cameras associated with a subsequent decline in property crime, violent crime, disorder crime, or calls for service at businesses?
2. Does the installation of Green Light cameras cause crime to be displaced to other nearby locations, or does it cause a "diffusion of benefits" by decreasing crime in nearby locations?
3. Given the cost of installation, maintenance, and enforcement, is the Green Light program cost-effective or cost-neutral for the city of Detroit?

[1] The city maintains a running list of partnering businesses, that is open and freely viewable. http://www.greenlightdetroit.org/participating-businesses/

[2] To date, very few criminological studies have utilized Bayesian methods, that are quickly being recognized as an alternative to frequentist statistics – most particularly NHST. Traditionally, studies report *a priori* power analyses based on their anticipated sample size and effect size. Power analyses are often required in publicly funded work as a method of evaluating the strength of the proposed research. In frequentist studies, the power of a study represents the probability of committing a type II error (that is, incorrectly retaining the null hypothesis). By convention most researchers aim for a power of .8, that equates to accepting a 20% chance of committing a type II error – however Gelman and Carlin (2014) note some problems and misconceptions with this premise. In Bayesian analysis, there is no direct comparison to the frequentist power analysis because the primary interest is not in testing a null hypothesis, but rather estimating a range of credible values for a parameter of interest. This does not imply that Bayesian analyses are immune from sample size requirements – indeed, small samples will yield very poor inferences relative to larger samples.

This study utilizes a quasi-experimental design in order to answer these questions. Because treatment was not randomly assigned, “treated” Green Light businesses were compared to a matched sample of “untreated” comparison businesses using propensity score matching. The pool of untreated comparison units was drawn from a list of about 1,300 businesses registered in Detroit as either a gas station or having obtained a liquor selling license. The study focuses on the time period prior to the beginning of the initiative (Jan 1, 2015 to Dec 31, 2015), during the staggered implementation period (Jan 1, 2016 to Dec 31, 2016), and one year following implementation (Jan 1, 2017 to Dec 31, 2017). Control units that joined Green Light during any portion of the study were eliminated from the comparison unit pool.

The unit of analysis represented a 200-foot circular buffer drawn around each business (the so-called, “catchment area”). This 200-foot buffer represented roughly ½ the length of the average street block in Detroit. Other studies have utilized similarly-sized units of analysis (i.e.: Caplan, Kennedy, & Petrossian, 2011). In order to test for crime displacement or a potential diffusion of benefits, an additional 100-foot buffer was utilized that excluded all incidents within the catchment area (the “displacement zone”). Therefore, any changes within the displacement zone would indicate either diffusion of benefits or crime displacement. One potential issue in causal inference is the assumption that the treatment assignment of one unit does not affect the outcome for another unit – also known as the stable unit treatment value assumption (SUTVA). For instance, one business may install Green Light cameras and reduce crime at its premise, as well at as the adjacent business, biasing the results toward zero. This type of interference violates the SUTVA assumption (Imbens & Rubin, 2015). Therefore, locations were chosen such that their catchment and displacement zones did not intersect.

Crime data was retrieved from the Detroit Police Department's SunGard records management system. All crime data between 2015 and 2017 were extracted, while all calls for service data between 2016 and 2017 was extracted[3]. This data contained information about the crime type, the time and date the incident occurred, the incident address, and the exact x-y coordinates. In order to answer the research questions, crimes were recoded into three distinct types: 1.) violent crimes, 2.) property crimes, 3.) disorder crimes. Outcomes were measured using a longitudinal design, where the monthly number of crimes were compared prior to Green Light cameras being implemented, and then following the intervention. The study utilized a within-subjects design, wherein repeated measures were performed on individual businesses. The within-subjects design was chosen due to its distinct advantages for this study. First, within-subjects studies are well suited for use in hierarchical linear models (HLMs). Rather than treating the Green Light initiative as a single intervention across an averaged count of businesses, HLMs allow the treatment effect to vary by locations. Therefore, the average treatment effect can be more precisely estimated by partially pooling the results from individual observational units as they began Green Light. This has several benefits: (1) variation among and between observational units is explicitly modeled, (2) extreme observations are shrunk toward the group-level mean, (3) varying intercepts and slopes can be modeled – providing a better fit to the data (Gelman & Hill, 2006). Furthermore, several recent studies of CCTV cameras (Caplan, Kennedy, & Petrossian, 2011) have examined experimental units and treatment units only in the aggregate, which ignores variation *between* observational units. Specifically, in the case of studies where multiple observations are made on individual units longitudinally, HLMs allow the slope of covariates to vary by each unit over time. For instance, the effect of Green Light

[3] Due to data constraints, calls for service data were not available for earlier than 2016, which is a noted limitation of this study.

cameras might logically be expected to differ in intensity at each business over time. Allowing the effect of treatment to vary by time, by each unit, creates individual "growth curves" that can uncover trends in treatment that would not be observable if the effect of Green Light was fixed. Braga, Papachristos, and Hureau (2009) performed a similar analysis evaluating the concentration of gun violence at Boston street blocks using growth curve regression models.

For the cost-benefit analysis portion of the study, the negative impacts (costs) were weighed against the positive impacts (benefits) of Green Light. Because the Green Light program is operated and maintained by the City of Detroit, they were considered the population of interest. Costs were operationalized as the monetary value of: 1.) police dispatch, patrol, and follow-up work, 2.) maintaining and monitoring the Green Light camera systems. Operational costs relating to camera monitoring, police dispatch, increased patrols, and follow-up work was estimated based on data from the Detroit Police Department. Benefits were operationalized as the monetary value of: 1.) reductions in crime, disorder, and calls for service in and around Green Light businesses. Conceptually, a reduction in crime and calls for service would have societal benefits (less victimization) as well as operational benefits for the City of Detroit (fewer officers deployed, fewer cases prosecuted, fewer offenders held in jail). Therefore, the cost-benefit analysis considers whether the Green Light program produces a reduction in crime or calls for service – the cost of which offsets the costs of operating and maintaining cameras and deploying officers to Green Light businesses.

Below, I elaborate on the use of these methods, and justify their importance for the current inquiry. In addition, I discuss the appropriateness of Bayesian statistics as the primary method of parameter estimation and inference.

Causal Inference and Quasi-Experimental Designs

Determining causal inference places a high burden on the researcher. One of the primary problems in causal inference in the inability to view any single observational unit in two states. For instance, if a treatment is administered to a unit, it is impossible to observe what would have happened to that unit in absence of treatment. Because the difference between the treated and untreated state $y_i^1 - y_i^0$ cannot be calculated for individuals, it is not possible to directly estimate individual-level causal effects (Morgan & Winship, 2007). Therefore, researchers generally consider the counterfactual model (also known as the "potential outcomes" framework), where y_i^1 is comprised of a treatment group compared to a control group y_i^0. This framework considers causal effects as comparisons of potential outcomes under alternative treatments (Rosenbaum, 2017). In a randomized experiment group assignment is performed without respect to pre-treatment covariates and the treatment indicator D is independent of the potential outcomes (Morgan & Winship, 2007), written as:

$$(Y^0, Y^1) \perp\!\!\!\perp D$$

Under a fully randomized experiment, the distribution of both observed and unobserved covariates are balanced in expectation (Imbens & Rubin, 2015). This minimizes threats to internal validity by ensuring that the treatment indicator is not correlated with observed variables. In addition, experimental designs significantly reduce threats to validity related to unobserved variables. If an unobserved variable u is related to treatment outcomes, u is balanced between treatment and control groups, and unbiased estimates of treatment effects can be calculated. Where the value of observed and unobserved variables does not affect the probability of treatment assignment, the effect of treatment assignment on the study's results are considered "strongly ignorable" (Rosenbaum, 2017). Under strong ignorability the average causal effect, or average treatment effect (ATE) can be estimated by the simple difference between the outcome of the treatment and control groups, as if the study arose from a completely randomized design. Departures from ignorability can be tested through the use of sensitivity analyses. As Rosenbaum (2017) notes, deviations from ignorability fall along a gradient, and the effect of an unmeasured confounder can be estimated. The precision of the causal effect can be improved through regression adjustment or stratification, which has the added benefit of controlling for residual differences between groups (Imbens & Rubin, 2015). While most researchers agree that fully randomized experiments represent the "gold standard" in causal inference, in reality they are often difficult or impossible to carry out practically (Shadish & Cook, 2002). Ethical concerns, budget limitations, and research partner cooperation often make randomized experiments untenable in criminological research (Weisburd, 2000). In cases where treatment and control cannot be randomly assigned to units, researchers may make use of quasi-experimental designs, which can approximate inferences drawn from a fully randomized study (Shadish & Cook, 2002; Imbens & Rubin, 2015).

Quasi-experimental designs (also known as observational studies) provide an alternative to randomized experiments. While the probability of group assignment is known in a randomized experiment, there is no such knowledge in a quasi-experiment. In many cases, the probability of treatment assignment is correlated with one or more observed or unobserved variables (Shadish & Cook, 2002). In the absence of random assignment, the researcher is unable to determine whether a treatment effect exists, or the observed differences are due to bias in the formation of groups (Rosenbaum, 2017). In Detroit, many businesses were already participating in Green Light, which was slowly implemented over the course of a year. Because the intervention had already occurred, the evaluation of Green Light occurs effectively *post-facto.* In this case, there is no assumption that Green Light cameras were installed randomly. Rather, places that received Green Light were likely chosen specifically because of features unique to their location – such as high numbers of calls for service, violent crime, and observed disorder. Unlike in circumstances where treated and control units are randomly assigned, quasi-experiments can only approximate a randomized study by reducing the dependence of the treatment indicator on pre-treatment variables.

In quasi-experiments there exists many methods to develop an adequate comparison unit to the treated unit, such as using instrumental variables (Morgan & Winship, 2007), regression discontinuity designs (Morgan & Winship, 2007), synthetic controls (Abadie, Diamond, & Hainmueller, 2010), and matching, stratifying, or weighting on the propensity score (Imbens & Rubin, 2015). While all these methods have strengths and weaknesses, the use of the propensity score is, perhaps, one of the most widely used quasi-experimental techniques. In the next section I will discuss the value of the propensity score as a tool to approximate a fully randomized experiment. In addition, I will illustrate methods in specifying the propensity score model and its uses as an adjustment technique. Finally, I will briefly discuss the use of Bayesian inference and its use in this study's methodology.

The Propensity Score

The propensity score represents a method to model the treatment assignment mechanism. In a randomized study the treatment assignment mechanism is known – generally all units have an equal probability of receiving the treatment. Under these circumstances, the treatment and control groups are assumed to be balanced in regard to all observed and unobserved covariates. This allows the researcher to conclude that systematic changes in the treatment group is due solely to the treatment, and not due to other confounding factors (Rosenbaum, 2017). When the probability of receiving treatment is *not* known, finding suitable control units that would have been equally likely to receive treatment can reduce the risk of treatment confounding. The relationship between covariates and treatment assignment can be combined into a single scalar value known as the propensity score. The propensity score indicates the probability that a specific unit would be observed in the treatment group (Morgan & Winship, 2007). Because the *true* propensity score is not known to the researcher (although, in a randomized experiment the propensity score for any unit is known to be 0.5), methods such as logistic regression, random forests and others can be used to estimate it (Ho, et. al., 2011).

In a quasi-experimental study, the researcher can rely on pre-treatment covariates to estimate the propensity score (a matrix of variables, denoted S). If S is fully observed, then the researcher has all information determining treatment assignment and selection. Complete observation allows the researcher to determine that treatment assignment is ignorable and that the remaining variation in the treatment indicator D is random (Morgan & Winship, 2007). Under these circumstances, a weaker version of conditional independence holds, where the treatment indicator is conditionally independent, given the propensity score:

$$(Y^0, Y^1) \perp\!\!\!\perp D \mid S$$

The treatment assignment mechanism is considered ignorable when the outcomes are independent of the treatment variable, given the covariates in S (Morgan & Winship, 2007). The ignorability or unconfoundedness assumption allows the researcher to proceed as if the study arose from a randomized experiment (Rosenbaum & Rubin, 1983; Imbens & Rubin, 2015). However, this assumption only holds if the propensity score is correctly specified (that is, all relevant variables are accounted for). A noted weakness of quasi-experiments is their inability to control for potentially unobserved variables (denoted u). The sensitivity of results to u can be tested through robustness checks. For instance, one can calculate the magnitude of effect that u would have to take on in order to substantively change the study results. Other robustness checks may possibly include introducing non-equivalent controls as a third, counterpart group (Rosenbaum, 2017).

There exists some disagreement on the best method to select variables for the propensity score. The so-called “kitchen sink” model includes as many variables and higher-order terms as possible, in hopes of properly specifying the model (Imbens & Rubin, 2015). A “theoretically informed model” utilizes only a small subset of variables that are deemed most important to predicting treatment (Apel & Sweeten, 2010). Regularization techniques, such as the LASSO or ridge regression can aid model selection as well, and reduce the negative impact of overfitting (Franklin, et. al., 2015). While no consensus exists on the best method to specify the propensity score, nearly all sources suggest that the sensitivity of model results be tested against various propensity score models (Imbens & Rubin, 2015). Once the propensity score is estimated, it can be utilized in a number of methods. I will consider the most widely-used option for this study: matching on the propensity score.

Matching represents an easily understood and logical framework. Given a set of treated units and a pool of untreated units, propensity score matching selects one or more untreated units as a comparison for a treated unit (Apel & Sweeten, 2010). The goal of matching is to reduce the imbalance between treated and untreated groups and, therefore, reduce the degree of model dependence when estimating causal effects (King & Nielsen, 2016). Matching on the propensity score ensures that the matched groups are similar on average across all covariate values – but not necessarily between each matched pair or pairs (Gelman & Hill, 2007). While regression techniques can reduce the dependence of the causal effect on extraneous variables, it introduces the risk of extrapolating when no comparable control unit exists, violating the overlap assumption (Gelman & Hill, 2007). Matching methods often discard some units to create a subset of treated and untreated units such that their covariate distributions overlap (Morgan & Winship, 2007). Conceivably, this mirrors the assumption in a randomized study that all units have an equal probability of being selected for either treatment or control (King & Nielsen, 2016). Because matching methods discard control units for which there is not a comparable treatment unit, they sacrifice some degree of external validity for a more precise estimate of the causal effect, improving internal validity (Imbens & Rubin, 2015).

Several matching algorithms have been suggested, although there is little consensus on which is best (Morgan & Winship, 2007). The simplest form of matching assigns each treated unit to the next most similar control unit – which is referred to as "greedy" pair matching (King & Nielsen, 2016). Treatment units may also be matched to two or more control units, either with or without replacement (Imbens & Rubin, 2015). A weakness of the pair matching approach is that matches are made without concern for subsequent matches, such that more optimal matches may not be available because they were used in previous matches. In general, pair matching is most effective when the number of control units is much larger than the number of treatment units (Hansen, 2006). When the number of control units is limited, using a fixed number of pairs forces sub-optimal matches in cases where few good control units remain (Hansen, 2006). Optimal full matching represents an alternative method to pair matching, which instead seeks to minimize the global distance between all matched pairs. Matching methods can be combined – such as exact matching on one or more important variables, and then nearest-neighbor matching on the propensity score (King & Nielsen, 2016). Alone, blocking or stratifying on some variables can considerably reduce imbalance (Imbens & Rubin, 2015).

In every case, matching methods result in some discarding of information. Control units with very low propensity scores, and treatment units with very high propensity scores are unlikely to find good matches. Pruning is often utilized when there are treatment cases for which a comparable control unit cannot be found in order to improve covariate overlap (Imbens & Rubin, 2015). A common method of pruning is to remove all treatment cases whose propensity score is larger than the largest propensity score in the control group, and all control cases whose propensity score is lower than the lowest propensity score in the treatment group (Imbens & Rubin, 2015).

Bayesian Inference

Bayesian inference stems from setting up a full probability model, consisting of all observable and unobservable quantities in a problem, and conditioning this model on observed data (Gelman, et. al., 2014). Estimates for a quantity of interest (for instance, the effect of CCTV cameras on crime) can be expressed as a range of plausible values with associated common-sense probabilities. The results from a Bayesian analysis can directly feed into a decision analysis – such as whether increased funding to CCTV cameras is warranted, given the probability that the associated crime decreases are within a given range. Modern Bayesian data analysis relies on the philosophy represented in Bayes' theorem – specifically[4]:

$$P(A|B) = \frac{P(B|A)\,P(A)}{P(B)}$$

Where the posterior probability of the data (A given B), is equal to the product of the likelihood of the data (B) and the prior (A), divided by the likelihood (McElreath, 2015). In its simplest

[4] While the modern use of Bayesian inference relies on the framework of Bayes' theorem, the actual computation of estimates is quite different. Rather than least-squares or maximum likelihood, estimates are derived using some form of Markov Chain Monte Carlo (MCMC), where parameter estimates are obtained via sampling from the posterior distribution (Gelman et. al., 2014). A complete explanation of this method is complex, and outside the scope of this study.

form, Bayes' theorem indicates that, given some observed data, one can estimate the probability of that data conditional on some prior beliefs. Therefore, the posterior probability represents a compromise between the observed data, and what is already known about that subject (McElreath, 2015). In practical terms, this allows researchers to specify prior information about the values of model parameters. This prior information can come from research papers, meta-analyses, or general scientific consensus (Gelman et. al., 2014). Priors have the benefit of improving inferences by performing regularization of parameters and providing more stable estimates – especially if data are particularly sparse (McElreath, 2015; Gelman, 2000).

Prior Distributions

A prior is generally represented as a specific probability distribution – for instance, Welsh and Farrington's (2009) meta-analysis indicated a positive, but rather modest, effect of CCTV cameras on crime reduction. In their study, they reported an overall effect of roughly 1.19, equating to a 16% reduction in crime. Subsequent studies have identified effects ranging between 13% and 30% (Ratcliffe, Taniguchi, & Taylor, 2009; Caplan, Kennedy, & Petrossian, 2011). Based on this information, an optimistic prior for the average log odds effect of CCTV cameras on crime θ might be modeled as a normal distribution: $\theta \sim N(\mu, \sigma)$, where the mean effect size μ is centered on -.17, and the standard deviation σ is set to .5. This would indicate a prior belief that a reasonable effect size might fall between -.07 and -.27 (equivalent to CCTVs causing between a 7% to 24% reduction in crime). Similarly, this distribution would indicate a belief that CCTV cameras exhibit at least a small effect on crime and might plausibly decrease crime by as much as 24% - very close to the largest pooled effect in Welsh and Farrington's (2009) meta-analysis.

When the prior knowledge about the state of the model parameters is low, or if the researcher wishes to generate a prior that plays a minimal role in the resulting posterior distribution, a non-informative prior distribution may be used. A true "non-informative" prior provides absolutely no information about the model parameters, which makes a uniform distribution with a minimum and maximum of negative and positive infinity a logical choice. However, in most cases a uniform prior distribution is inappropriate because it allows for estimates well beyond plausible values, and generally increases computational time – especially in complex models (Gelman et. al., 2014). However, there are other methods to generate priors that retain relative ignorance about the model parameters.

This can be accomplished by specifying a "diffuse" or "weakly-informative" prior (Gelman et. al., 2014). Under this specification the prior is allowed to provide a small amount of useful information about the model parameters – such as constraining them to within reasonable bound. However, the prior is weak enough that the data are allowed to "speak for themselves" (McElreath, 2015). In this case, a researcher may wish to specify a distribution with equal probability density both above and below zero – such as a normal distribution with a mean of zero and a standard deviation of 5 $\theta \sim N(0, 5)$. A weakly informative prior has the benefit of regularizing parameter estimates, while allowing providing very little information about the posterior distribution *a priori* (McElreath, 2015; Gelman et. al., 2014). Prior distributions can also accommodate effects that may reasonably fall far from the mean. The Student's t or Cauchy represent a class of long-tailed distributions that can usefully model extreme outcomes (Gelman et. al., 2014).

Values outside the range of the prior would be assigned lower credibility than those within it. In situations with small amounts of data, the prior has more effect on the posterior distribution. When large amounts of data are available, the effect of the prior is minimized (McElreath, 2015). The practical advantages of Bayesian data analysis are the ability to incorporate what is already known about the effect of CCTV cameras on crime with new data gathered from this study – effectively weighing the evidence about the size and range of the effect. In any case, using informative priors requires the researcher to explicitly state his or her beliefs about the range of plausible values. Sensitivity analyses using different prior distributions can strengthen inferences from the data. Given that frequentist methods do not assign any prior information about model parameters, the Bayesian equivalent would essentially assign a uniform distribution with lower and upper bounds at negative and positive infinity: $\theta \sim U(-\infty, \infty)$, where all observed data values would be viewed as equally credible within the model, and the prior would play no role in the posterior distribution.

Bayesian Estimation

Rather than reporting a single point estimate, Bayesian analyses generally summarize the posterior distribution using a number of methods. Commonly used summaries of location might include the mode (which would correspond with the 'most likely' value, given the data) or the mean (the average value). Summaries of the intervals might utilize quantiles or highest-density intervals (Gelman et. al., 2014). For instance, one might report the posterior mode as 10, with an 80% highest posterior density region of 7 to 13. This would indicate the most likely value is 10, with 80% of the probability mass contained within 7 and 13. This is similar, but not equivalent to a frequentist point estimate and confidence interval.

Cost-Benefit Analysis

While quantifying the exact cost of crime is difficult, there exists some prior literature providing rough estimates. Attempts to estimate the cost of a single crime relies on tangible and intangible costs. Tangible costs represent loss of property, loss of future earnings, incurred hospital or medical bills, and costs to the criminal justice system (McCollister, French, & Fang, 2010). More difficult to quantify are intangible costs, such as pain and suffering, reduced quality of life, and psychological problems. Some research has utilized a “willingness to pay” model (WTP) that asks survey respondents how much they would be willing to pay in order to prevent a certain number of crimes. Cohen et. al. (2004) surveyed a nationally representative sample about how much they would pay for a hypothetical program that would reduce specific crimes in their neighborhood by 10%. Estimates from their study were higher than those that used accounting methods to calculate the cost of crime, but were comparable to other subsequent scales (Heaton, 2010). Piza et. al (2016) performed a cost-benefit analysis of a CCTV camera system merged with directed police patrol. Costs in their study were estimated against each of the separate criminal justice systems (policing, courts, corrections). Costs in their study considered the installation of the CCTV system, maintaining, and operating the system, and the added cost of deploying officers to locations using estimates derived by LaVigne et al. (2011). The cost of crime was calculated separately for each actor in the criminal justice system.

Given that money for most criminal justice interventions comes primarily through public funds, evaluating whether the results of the program justify the costs presents an important and tangible question. The emphasis towards “evidence based” programs places an even greater importance on the balance between costs and outcomes (Zedlewski, 2009). Limited federal and state budgets make cost-benefit analyses (CBA) an important part of evaluating criminal justice interventions (Braga, Downey, & Roman, 2004). CBAs evaluate the benefits of a program (quality of life, time saved, crime reductions), against the estimated costs of implementation.

CBAs are well suited to answer multiple questions, such as the impact of a program on a wide range of outcomes (Braga, Downey, & Roman, 2004).

Setting up a CBA involves several steps. First, researchers must state the population against which the costs will be measured - known as standing (Braga, Downey, & Roman, 2014). Determining who has standing in the CBA affects what the costs and benefits will be calculated against (Marsh, Chalfin, & Roman, 2008). In some cases, the population can be individual victims or offenders, or broader categories such as municipalities or governments – as in the "public payer" perspective. Defining who has standing can significantly affect the results of the CBA. Costs to society, versus costs to organizations may vary considerably (Roman, 2012). During this stage, how costs are operationalized has significant effects on the CBA's results.

Costs for criminal justice programs are often divided into categories of: 1.) project expenditures, 2.) the value of public resources used by the program, 3.) the cost of services that are free or discounted due to the program, and 4.) the change in use of program resources due to the intervention (Roman, 2004). The first three categories reflect the direct costs of designing and implementing a program and are often simple to calculate. The fourth category recognizes that some programs may encourage increased use of some resources relative to what would have happened without the program (for instance, installation of CCTV cameras resulting in more 911 calls for service). While costs are often quantified as money, they may also manifest as non-monetary costs such as loss of time or declining satisfaction with police (Braga, Downey, & Roman, 2014). For police agencies, interventions that utilize officer time represent both monetary costs (in the form of salary) and opportunity costs (police services that might be used elsewhere). The decision to use direct costs (equipment, materials money) or indirect costs (loss of time, reduced quality of life) significantly impacts the final calculation.

Similarly, potential benefits can be estimated much in the same manner. First, there are the public costs that are reduced if the intervention is successful – such as the direct benefit of a reduction in enforcement-related costs or the indirect effect of an increase in revenue (Roman, 2004). For instance, a program that reduces crime in a business district may produce savings through fewer police hours spent patrolling and increased tax revenue due to improvements in business. More difficult to quantify are the private benefits, such as reduced victimization or improved health outcomes (Roman, 2004). In this case, cost savings can be estimated against health care costs, lost wages, and negative outcomes for victims. However, there is still considerable disagreement on the intangible, indirect costs of crime (McCollister, French, & Feng, 2010). The choice of costs should be based on the population of interest, and the relevant questions (Marsh, Chalfin, & Roman, 2008). Because these decisions have such a large effect on the final calculation, CBAs must explicitly state how costs and benefits will be estimated and make transparent all parts of the analysis (Braga, Downey, & Roman, 2014).

Estimating the cost-benefit ratio of the program involves aggregating the net benefits and comparing them to net costs (Roman, 2004). Obtaining an estimate of the cost-benefit ratio requires an impact evaluation to estimate the effect of the program above and beyond what would have happened in absence of the program. Generally, this manifests as a rigorous impact evaluation using experimental or quasi-experimental designs (Marsh, Chalfin, & Roman, 2008). Traditionally, if the impact evaluation suggests that changes in the treatment group are not likely to have occurred simply by chance alone, the monetary effect of the program can be estimated by translating statistical changes to dollars (Roman, 2004). If the benefits of the program are larger than negative outcomes, the program is considered cost beneficial. However, some research indicates an inconsistent relationship between the estimated effect size of the outcome analysis and the cost-benefit ratio (Marsh, Chalfin, & Roman, 2008).

The current method of conducting CBAs is not without caveats, however. Results from a CBA is highly sensitive to the outcome of the impact evaluation. Threats to validity, such as small samples, selection effects, and improper analyses can produce misleading estimates (Roman, 2004). In cases where the effects of the program are likely small and highly variable, an uncritical estimate of the average effect is likely significantly inflated (Gelman & Carlin, 2014). Ignoring uncertainty in effect estimates limits the validity of the study. Similarly, rare events with high costs (such as homicide) have the potential to significantly skew results in one direction. Finally, omitted variables in the impact evaluation can underestimate costs to the program, or unintended consequences not controlled for (Roman, 2004).

Because inferences from Bayesian models are inherently probabilistic, they can logically extend to a CBA. Using estimates derived from a model (for instance, a model estimating the average effect of CCTV cameras on crime), a researcher can conduct a decision analysis by determining the optimal conditions, given the data. Working backwards, estimates from the model can be used to calculate how much benefit is needed in order to offset the costs (Gelman et. al., 2014). In the case of Detroit Green Light, estimates from the model might indicate that certain businesses with at least two crimes per month would see a favorable cost-benefit tradeoff. The ability to use model results to generate predictions of what would happen under varying circumstances avoids one of the pitfalls that Roman (2004) identifies: that is, the sensitivity of the CBA to model results. A CBA using a Bayesian framework incorporates everything that is known about the parameters (via the use of prior information, and the data from the evaluation), and explicitly models uncertainty about these parameters. This uncertainty can then be propagated to the decision analysis portion of the CBA. The use of prior information is especially useful when studies are carried out with small sample sizes or are analyzing rare events (Gelman et. al, 2014; Roman, 2012).

ANALYSIS

Dependent and Independent Variables

Two sets of independent variables were chosen for this study. First, criminal activity at businesses was separated into three distinct categories: violent crime, property crime, and disorder crime. Utilizing the coding system from the Detroit Police Department's records management system, these categories were generated based on existing categorizations. Violent crime was operationalized to consist of serious violent crime including: aggravated assault, armed and unarmed robbery, and felony homicide. Property crime included burglary, larceny, theft from a vehicle, motor vehicle theft, retail fraud, possession of stolen property, and damage to property. Disorder crimes consisted of misdemeanor assault, possession of drugs, open liquor citations, illegal gambling, public drunkenness, and disorderly conduct. The second set of dependent variables represented calls for service from in and around the businesses. These consisted of data on 911 calls made to the Detroit Police Department and information about the officer's follow-up on the call. The primary category analyzed here was the total number of calls for service that officers responded to.

Creation of the Observational Units

In early 2015 members of the research team were given access to an electronic list of all businesses currently participating in the Green Light program, along with businesses identified as future locations. Addresses on this list were geocoded and merged with a shapefile listing the locations of businesses holding a liquor license. This shapefile consisted of approximately 1,000 gas stations, liquor stores, grocery and convenience stores, and numerous other business types. The final merged file contained the addresses of nearly 1,600 businesses – of which 86 were operational Green Light businesses by December 31, 2016 (see Appendix B). A set of pre-treatment covariates were merged to businesses, that represented theoretically-relevant correlates of crime (see Table 1). Constructing the observational units for analysis involved merging crimes to businesses, which was performed in several steps:

1. Circular catchment buffers (200 feet) and diffusion buffers (300 feet) were drawn around each business.
2. Data representing all crimes and calls for service for 2015 to 2017 were geocoded and mapped to the street centerline.
3. All crimes falling within a buffer were retained, while all crimes *outside* the buffers were eliminated.
4. Crimes were merged to the nearest business within each buffer.

This procedure successfully merged all crimes within the study period to the nearest business within a 200-foot buffer, or within a 300-foot buffer (but *not* a 200-foot buffer). Importantly, this method ensured that no double-counting occurred (because each crime was merged only once to the nearest business if it fell within overlapping buffers). This contrasts with other CCTV camera studies, that have either merged overlapping units into a single observational unit (see: Piza et. al., 2015) or have divided the number of incidents by the number of overlapping units (Lim & Wilcox, 2017).

The spatial merging process generated an $n \; x \; k$ matrix consisting of *n* rows and *k* columns.[5] This matrix was split into three dataframes (one for each crime type), consisting of information about each crime (time, date, location, crime type, and category) as well as the business it occurred near. To utilize this data in a regression model the dataframe was converted

[5] Here, I refer to matrices of data as a "dataframe", which is the name given to matrixes of mixed data types in the R statistical environment (Wickham, 2014).

into a long-form format, where each row represented the number of crime incidents at business i at time t, along with all static and time-varying covariates.

Estimation of the Propensity Score

Estimating the propensity score followed a multi-step process. First, a logistic regression model was fit to estimate the probability of a given business receiving Green Light, conditional on a set of pre-treatment covariates. The covariates utilized in the final propensity score model included: the quarterly number of violent crimes, property crimes, and disorder crimes reported at the business, the block group-level violent and property crime rate per 1,000, and a set of census block group variables.[6] Table 1 displays the descriptive statistics on the variables utilized. After the propensity score model was generated, estimates from the model were transformed into a linearized propensity score that reflects the log odds of the propensity score.

$$\log\left(\frac{e(x)}{1-e(x)}\right)$$

Working with the linearized propensity score provides a convenient distance measure, because the distance between small values of the propensity score are minimized, relative to larger values. For instance, the distance between .001 and .01 is much larger than .1 and .109 (Imbens & Rubin, 2015). After the propensity score was properly estimated and transformed, the values were used to match treated Green Light businesses with untreated control businesses.

Matching Procedure

Matching was performed using pair matching via the ‘optmatch’ statistical package (Hansen, 2006). Matches were constrained such that businesses were exactly matched to the same business type (i.e. treated gas stations to control gas stations). Next, treated Green Light

[6] The specific census-level variables were drawn from the American Community Survey 2015, 5 Year Estimates.

businesses were matched to the two nearest control businesses based on the distance of the linearized propensity score. After matching, the effective sample size was 116. A set of supplementary analyses were performed to determine whether the matching procedure was effective in reducing covariate distance between the treated and control units. Table 1 displays the standardized value of each covariate for Green Light businesses, all control units, and the subset of control units selected via matching. The pair matching procedure reduced the absolute mean error covariate distance by approximately 44%. A visual inspection of the overlap of propensity scores indicated that the matched sample contained units with a highly similar distribution of propensity scores (see Figure 4). Similarly, the distribution of covariates indicated that a substantial amount of bias was reduced through the pair-matching procedure. Together, these diagnostic tests indicated that the propensity score matching was successful.

Table 1. Covariates Pre-Match and Post-Match

Variable	Green Light	Pre-Match	Post-Match
Disorder Crimes Q1 2015	0.38	0.34	0.41
Property Crimes Q1 2015	1.07	0.93	0.94
Violent Crimes Q1 2015	0.37	0.33	0.39
Disorder Crimes Q2 2015	0.47	0.38	0.39
Property Crimes Q2 2015	1.00	1.08	1.13
Violent Crimes Q2 2015	0.49	0.43	0.47
Disorder Crimes Q3 2015	0.45	0.40	0.41
Property Crimes Q3 2015	1.06	1.09	1.05
Violent Crimes Q3 2015	0.53	0.44	0.42
Disorder Crimes Q4 2015	0.53	0.37	0.40
Property Crimes Q4 2015	1.17	1.13	1.17
Violent Crimes Q4 2015	0.40	0.35	0.37
% Male	0.48	0.48	0.47
% Black	0.83	0.79	0.86
% No HS Diploma	0.21	0.22	0.21
% HH in Poverty	0.25	0.24	0.24
% HH Income < $30k	0.38	0.38	0.37
% HH Rent > 30% of Income	0.21	0.21	0.20
% HH on Food Stamps	0.29	0.28	0.28
% Female Headed HH	0.23	0.21	0.23

Table 1. (cont'd)

% Unemployed	0.14	0.13	0.13
% Vacant HH	0.28	0.29	0.29
% HH Renting	0.35	0.37	0.34
Population Density	0.03	0.00	-0.01
Violent Crime Rate	-0.04	0.01	0.02
Mean Absolute Difference		0.69	0.38

Figure 4. Distribution of Propensity Scores, Pre and Post-Match

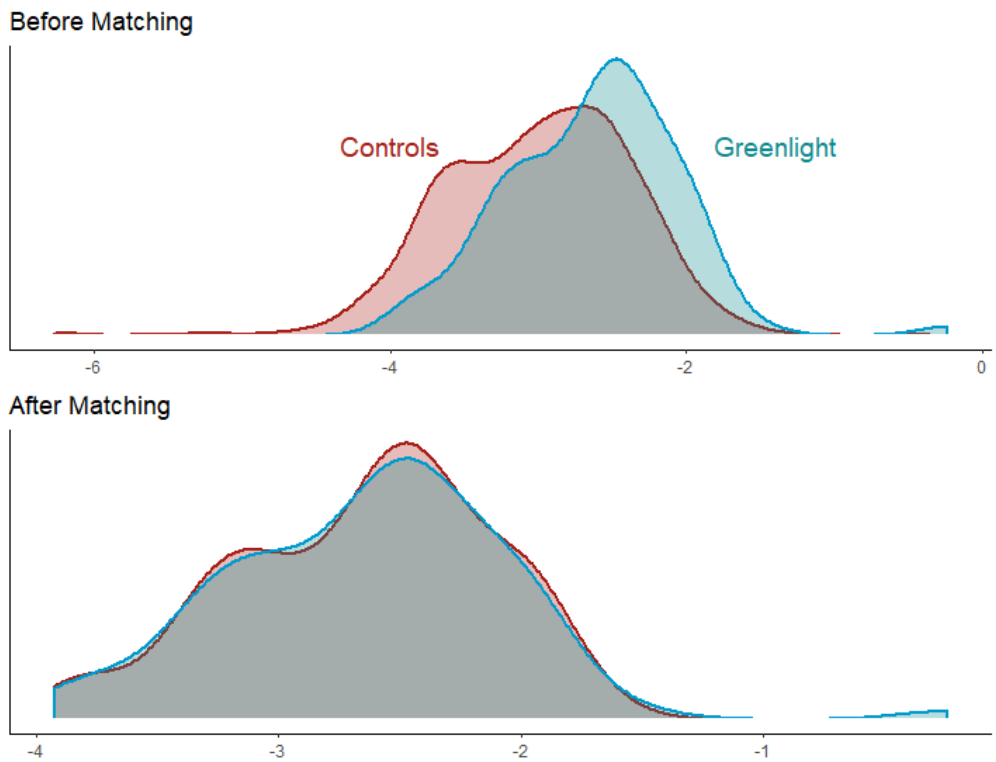

Figure 4 displays the distribution of linearized propensity scores for the control businesses (in red) and Green Light businesses (in blue). The top panel reveals significant departures in the distribution of propensity scores before matching. The bottom panel displays almost perfect overlap for the propensity scores for the Green Light and control businesses.

Statistical Modeling

Following the specification of the matched control group, a set of statistical models were fit. Analyses were performed utilizing the 'brms' package in R (Buerkner, 2016) that allows for the specification of multilevel Bayesian regression models using Stan. Stan is a free and open-source software package for performing fully Bayesian statistical analysis, which can be integrated with the R statistical environment (Gelman, Lee, & Guo, 2015). First, an appropriate model parameterization was chosen based on the form of the dependent variable. Because the dependent variable represented a discrete number of crime incidents per month, a multilevel Poisson regression was initially considered. A Poisson regression models variation in a count process as a function of linear predictors where:

$$y_i \sim Poisson(\theta_i)$$

$$\theta_i = exp(X_i\beta)$$

Under this form, the rate parameter for the Poisson distribution θ is estimated on the logarithmic scale. Poisson regressions do not have an independent variance parameter, rather they assume the mean and variance are equivalent – an assumption that is often violated (Gelman & Hill, 2007). In most realistic cases the data have greater variation that what would be expected under a Poisson distribution, known as "overdispersion" (Gelman & Hill, 2007). An alternative parameterization to the Poisson is an overdispersed negative-binomial model. The negative-binomial distribution provides a convenient alternative to the Poisson because it relaxes the assumption of a constant mean and variance. Instead, the variance in a negative binomial regression model is estimated separately in the form of an inverse scale parameter. Therefore, the variance of a negative-binomial distribution is given as:

$$var(\theta) = \frac{\alpha}{\beta^2}(\beta + 1)$$

Where α is the shape, or rate, parameter and β is the inverse scale parameter (Gelman et. al., 2014). Figure 5 displays the distribution of incidents per-month by incident type. As shown, the observed values (in red) show modest departures from what would be expected under a

Poisson distribution (in blue). In all three cases, the ratio of the variance to the mean was in excess of 1. Therefore, the dependent variable was modeled as negative-binomial distribution.[7]

Figure 5. Distribution of Monthly Crime Incidents – Theoretical versus Actual

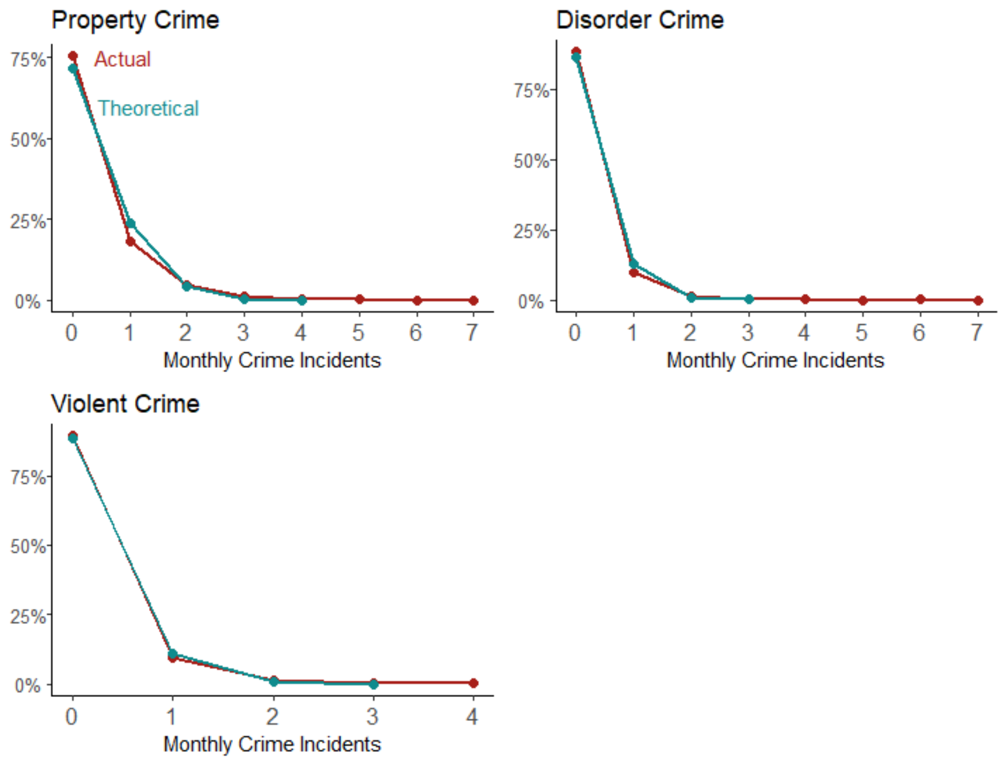

Figure 5 shows the distribution of monthly crime counts (in red) and the expected number under a Poisson distribution (blue) as a proportion of total monthly counts. The most common observation (75 – 80%) was no crime incidents. Note that the actual crime counts display more variation at higher values than would be expected under a Poisson distribution.

Growth-Curve Model

For this study, the primary outcome of interest was the change in monthly crime incidents. Because the dependent variable represented monthly counts nested within individual businesses, a hierarchical growth-curve modeling strategy was utilized. Growth curve models

[7] While the number of zeroes in the data was large, but not excessive, an alternative to the negative-binomial distribution was tested to account for the possible effect of excess zeroes. Generally, zero-inflated models can account for a separate process generating the excess zeroes by estimating a mixture of a logit model predicting zero-inflation and a Poisson or negative binomial model predicting the count outcomes. Using model comparison techniques via leave-one-out cross validation (LOO-CV), the zero-inflated model did not provide any improvement over the negative binomial model. Therefore, the negative binomial model was chosen for the final analysis.

represent a popular method for modeling longitudinal data in criminology and are well suited to outcomes that occur over more than two time periods (Bryk & Raudenbush, 1987). They represent a flexible method to measure within-individual and between-individual variation, as well as average change over time (Kreuter & Muthen, 2008). Most simply, growth curve models characterize the observed data as a function of time, with variation partitioned via varying effects across observational units (Kreuter & Muthen, 2008). Conceptually, this method is similar to a repeated-measures ANOVA or MANOVA, however the flexibility of growth curve models allows subjects to be measured at different time points and across unbalanced datasets (Bryk & Raudenbush, 1987).

In a growth curve model, the outcomes of interest generally are the trajectories of change over time - referred to as the "growth slopes" or "latent growth curves" (Bryk & Raudenbush, 1987). These trajectories might be flat (representing no change over time) or might be increasing or decreasing with either a linear or curvilinear form (Curran, Obeidat, & Losardo, 2010). Therefore, by specifying fixed and varying effects, researchers can examine the difference in conditional means (via varying intercepts) and conditional slopes (via varying slopes). Estimating growth curve models allow both static and time-varying variables to be utilized in the regression equation. (Curran, Obeidat, & Losardo, 2010).

The growth curve model is generally conceptualized in two stages: an individual-level model concerning the within-subject effects, and a group-level model concerning the between-subject effects (Bryk & Raudenbush, 1987). The level-1 model concerns the estimated value of an observational unit i at time t as a function of a systematic growth trajectory plus an error term (Bryk & Raudenbush, 1987). A common formulation contains an individual intercept π_{0i}, a linear time trend $\pi_{1i}\alpha_{it}$, a quadratic time trend $\pi_{2i}\alpha^2{}_{it}$, and a normally distributed random error term ε_{it} , for i = 1…n subjects where α_{it} represents the "age" of subject i at time t (Bryk & Raudenbush, 1987; Gelman et. al., 2014). Shown below, this formulates the within-subjects portion of the model:

$$y_{it} = \pi_{0i} + \pi_{1i}\alpha_{it} + \pi_{2i}\alpha^2{}_{it} + \ldots + \pi_{K-1i}\alpha_{it}^{K-1} + \varepsilon_{it}$$

The within-subjects model assumes that the growth parameters π_{ki} vary across individuals, which necessitates a between-subjects model to represent this variation (Bryk & Raudenbush, 1987). Importantly, the between-subjects model allows the individual growth parameters to vary as a function of measured variables at the individual level (such an individual business) or at the group level (such as treatment groups or census blocks). The following formula represents the between-subjects model:

$$\pi_{ki} = \beta_{k0} + \beta_{ki}X_{k1i} + \beta_{k2}X_{k2i} + \ldots + U_{ki}$$

Bayesian Prior Specification

A key facet of Bayesian inference is the ability to specify a prior distribution on model parameters (here, denoted θ). Making probability statements about θ in the Bayesian framework requires a prior distribution to be specified before conditioning on observed data y. The resulting posterior distribution p(y| θ) represents a comprise between the prior and the likelihood (Gelman et. al., 2014). Priors reflect a subjective belief about the distribution of the model parameters before viewing the data. Information about the prior can come from outside substantive knowledge about the value (for instance, results from a body of research or meta-analyses), from predictions via other models, or can reflect the relative ignorance about the model parameters (Gelman et. al., 2014). For this study, I will focus attention on two specific types of priors: a "weakly-informative" and an "informative" prior.

For the initial set of models, a weakly-informative prior distribution was specified for all parameters. Consistent with suggestions by Gelman et. al. (2014) a distribution was chosen to minimize the impact of the prior on the final posterior distribution but constrain estimates to within a reasonable bound. Because all values were rescaled to a unit scale of 0, the prior distributions represented the predicted standard deviation increase. A normal (Gaussian) distribution with a mean of 0, and a standard deviation of 1 was specified for all the beta coefficients and the standard deviations. Under this specification, 95% of the probability mass falls between ± 2.96, which roughly corresponds to an increased 6.8 times greater likelihood or 7 times decreased likelihood. This very broad and flat distribution provides almost no prior information about the parameters, allowing the likelihood to control most of the posterior distribution. The prior distributions for the remaining parameters followed the same logic – that is, they restricted values to reasonable bounds without providing substantial prior information. The prior for the negative binomial shape parameter was specified as a Cauchy distribution with a location of 0 and a scale factor or 2. An LKJ prior (Lewandowski, Kurowicka, & Joe, 2009) was specified for the correlation matrix of the group effects, consistent with default suggestions for Bayesian hierarchical linear modeling (Gelman et. al., 2014). Below are the priors for the weakly-informative model:

$$\beta \sim Normal(0,1)$$

$$\sigma \sim Student\ t(3,0,1)$$

$$r \sim Cauchy(0,2)$$

$$cor \sim LKJ(2)$$

A second set of models was fit using an informative prior distribution for the immediate impact of Green Light (β_{GL}) and the effect of Green Light during the post-intervention period

(β_{post_GL}). Rather than providing virtually no prior information about whether CCTVs reduce crime (via the weakly-informative prior), the informative prior utilized information based on the current state of literature. This information was based on a series of contemporary reviews, both experimental and quasi-experimental (Farrington et. al., 2007). The chosen prior, therefore, was modeled as a normal distribution with a mean of -.17 with a standard deviation of .5. This corresponded to a prior belief that CCTV cameras likely contribute between a 7% and a 24% reduction in crime, with the average centered around 16%. The remainder of the beta coefficients were assigned a normal distribution with a mean of 0 and standard deviation of 1, which is mostly uninformative, but constrains estimates farther from tail-end probabilities – reflecting a prior belief that most effects were unlikely to be larger than 2 standard deviations from 0. The standard deviations, shape parameter, and correlation matrix for the group effects were kept the same as in the weakly-informative model. Below are the priors for the-informative model:

$$\beta_{GL} \sim Normal(-0.17, 0.5)$$

$$\beta_{post\_GL} \sim Normal(-0.17, 0.5)$$

$$\beta \sim Normal(0,1)$$

$$\sigma \sim Student\ t(3,0,1)$$

$$r \sim Cauchy(0,2)$$

$$cor \sim LKJ(2)$$

A Brief Aside: Bayesian Computation

The computation of a Bayesian analysis differs significantly from typical frequentist methods in several ways – a few of which deserve a short mention here. Modern Bayesian data analysis is generally accomplished by iteratively drawing samples from the posterior distribution via Markov Chain Monte Carlo (MCMC) and then performing summaries of the draws – such as quantiles (Gelman et. al., 2014). MCMC represents a method separate from the typical least-squares or maximum likelihood methods utilized in frequentist statistics (McElreath, 2015). The program Stan is an open-source software package for performing a variant of MCMC (Hamiltonian Monte Carlo, or HMC) simulations with an interface for the R statistical environment. In short, HMC draws samples from the posterior distribution according to their relative probabilities, such that the most likely values are drawn more often while the least likely values are drawn less often (McElreath, 2015). To reduce the influence of the starting values, the first half of the sequence is generally discarded, with the second half retained for analysis - known as "warm-up" or "burn-in" iterations. Determining whether the number of posterior simulations is sufficient for analysis is generally assed by examining a plot of the iterations and determining whether the chains have converged to a single value (Gelman et. al., 2014). An estimate of whether the chains have converged, $\hat{R}$, determines whether further simulations would improve inference about the target distribution - where an $\hat{R}$ near 1 is considered optimal[8] (Gelman et. al. 2014). Once the number of draws has stabilized, relevant analyses can be carried out on the posterior simulations.

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[8] $\hat{R}$ represents the potential scale reduction factor, that is estimated as: $\hat{R} = \sqrt{\frac{\widehat{var}\,(\varphi|y)}{W}}$ which is the square root of the variance times the marginal posterior variance of the estimand divided by the within-sequence variance (W). A much more comprehensive discussion is available in Gelman et. al., 2014, Chapter 11.

RESULTS

Interpreting the results of this study presents the reader with several possible conclusions – all of which have some credibility. Consistent with Gelman & Loken (2013), I do not consider any single analysis as definitive evidence for an effect, or an absence of an effect. Rather, I interpret the results in contrast to the size of the measured effect and the relative uncertainty around the effect, as well as reporting all relevant analyses. In addition, I recognize that substantive decisions during the design phase may have some outcome on the final models (for instance, how large of a buffer should be drawn around each business). To account for the sensitivity of the results to this decision, additional analyses were carried out using iteratively smaller buffer sizes and compared to the primary analysis.

Comparison of Crime at Green Light Businesses and Matched Sample

Because the matching procedure was successful in balancing covariate distance between the treated Green Light businesses and the matched controls, an analysis could be carried out as if the results were obtained from a randomized experiment (Imbens & Rubin, 2015). As a descriptive step, Table 2 displays the difference in average yearly crime incidents at treated Green Light businesses and matched control businesses for each of the crime categories. Utilizing collected data on both treated and untreated businesses, pre-and-post intervention, an analysis could be made within and between groups – known as a “difference-in-differences” estimand (Gelman & Hill, 2007). Crime in the city had already been decreasing at a rate of about 4-5% per year since 2010 (see Figure 1). Therefore, this comparison accounts for the background change in crime rates and estimates the effect of Green Light independent of the overall city crime decrease. The inferences from this analysis are made more credible because confounding sources of variation had been reduced via the matching procedure. Figure 6 shows that Green

Light businesses generally experienced an increase in the number of reported crimes during the implementation period in 2016, relative to the matched controls.[9] Property crimes were about 17% higher, while violent crime and disorder crime were about 8% and 26% higher than the matched controls, respectively. In 2017 the mean number of incidents decreased within the treated Green Light businesses relative to the matched controls, such that property crimes were about 13% lower and disorder crimes were about 37% lower – equating to about 40 and 37 fewer crimes, respectively. However, violent crimes were roughly equivalent to the matched control businesses. The preliminary results here indicate that businesses that joined the Green Light program experienced a temporary increase in the incidence (or detection) of crimes, which later decreased compared to the matched control group in 2017.

Table 2. Difference in Mean Yearly Crime Counts – Green Light versus Matched Control

	Property Crime			Violent Crime			Disorder Crime		
	Treat	Control	% Diff	Treat	Control	% Diff	Treat	Control	% Diff
2015	376.9	369.6	2%	156.60	149.3	5%	159.73	143.0	1%
2016	399.9	331.0	17%	139.90	128.4	8%	190.01	139.9	26%
2017	317.4	358.1	-13%	103.36	103.4	0%	99.18	135.7	-37%

A simple comparison in this method ignores several sources of uncertainty. First, it over-simplifies a complex intervention that was staggered over the course of a year – ignoring month-to-month variation, and variation within-businesses. Second, it neglects uncertainty in the distribution of incidents. A fully-Bayesian analysis can provide direct probability statements about the likelihood that the observed differences between treatment and control are not a result of random variation. This modeling strategy can then determine *how likely* these differences would be observed under a given data-generating process (for instance, a Poisson or negative-binomial). Finally, because matching rarely removes all sources of bias, more precise estimates

[9] Because 2:1 matching was used, the estimated number of incidents in the control group was calculated by multiplying the annual incident rate by the number of units in the treatment group.

of the treatment effect can be estimated when the residual covariate differences between treatment and control businesses are controlled (Imbens & Rubin, 2015).

Figure 6. Estimated Number of Incidents by Treatment Group (2015 – 2017)

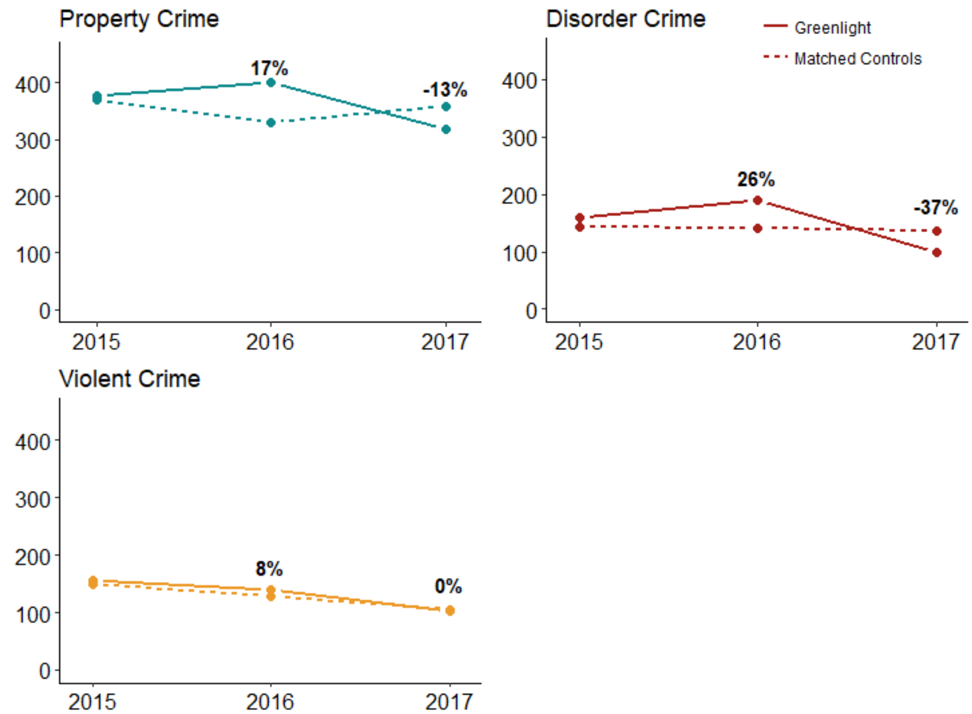

Figure 6 shows the number of incidents in treated Green Light businesses (solid line) and matched control businesses (dashed line). Among Green Light businesses, the mean number of incidents increased in 2016 during the implementation period for all crime types. These businesses saw decreases in the 2017 post-treatment period, with the most substantial decreases observed in disorder-related crimes (-37%) and property crimes (-13%).

Estimate of Treatment Effect with Regression Adjustment

Regression adjustment represents a method to control for residual differences between treated and matched control units to obtain more precise estimates of the average treatment effect, often known as a "doubly-robust" method (Imbens & Rubin, 2015). A special case of the difference-in-differences model occurs when the same set of units are measured at multiple time points, with indicators for treatment groups and time periods. Estimates of the causal effect can be estimated by regressing on the outcome variable with an indicator for the treatment group, an indicator for the time period, and an interaction between the two (Gelman & Hill 2007). In this method, the coefficient on the interaction represents the average treatment effect (Gelman & Hill, 2007). Using this framework, the growth-curve modeling strategy described above was utilized to model the effect of the Green Light intervention on crime. Because crime incidents were measured at individual businesses, within a set of distinct time periods, within separate precincts of the city, a 3-level model was utilized. The final model specification was specified as:

$$y_{itk} = \pi_{0tk} + \pi_{1i}\alpha_{itk} + \pi_{2i}\alpha^2{}_{itk} + X_{itk} + \pi_{itk} * GL + \pi_{itk} * post\_GL + \pi_{K-1i}\alpha_{itk}^{K-1} + \varepsilon_{itk}$$

$$\pi_{0tk} = \beta_{00k} + \pi_{10}\alpha_{00k} + \pi_{2i}\alpha^2{}_{00k} + r_0$$

$$\beta_{00k} = \gamma_{000} + \mu_{00k}$$

Where y_{itk} represents the estimated number of crime incidents at business *i* at time *t* within precinct *k*. This is modeled as a function of: π_{0tk} representing the varying intercepts, $\pi_{1i}\alpha_{itk}$ + $\pi_{2itk}\alpha^2{}_{itk}$ the linear and quadradic time trends, and X_{itk} representing a matrix of fixed pre-treatment covariates. The pre-treatment covariates included all variables in the propensity score model, indicator variables controlling for month-of-year effects (relative to January), and business-type effects (relative to gas stations). $\pi_{itk} * GL$ is an indicator variable that takes on the value of 0 before a business began Green Light, and 1 thereafter. $\pi_{itk} * post\_GL$ is a continuous variable measuring the time after the Green Light intervention, that begins at 0 and increments by 1 per month. The static indicator variable $\pi_{itk} * GL$ accounts for the mean change in reported crimes immediately after the intervention began, while the continuous $\pi_{itk} * post\_GL$ accounts for the gradual change in slope following the intervention. This formulation accounts for baseline level trends, and changes in crime reporting post-intervention (Wagner, Sourmerai, & Ross-Degnan, 2002; Taljaard et. al., 2014).

As a hierarchical linear model, varying (aka "random") intercepts and slopes were specified for individual businesses and by precincts - thereby allocating the variance into three levels. Varying slopes were specified for the time trends, the treatment indicator, and the treatment-by-time trend interaction at level 2 (the within-business level). Varying intercepts were fit for each of the 11 police precincts in the city of Detroit, accounting for within-precinct mean crime levels at level 3. Specifying the model in this fashion partially shrinks the model coefficients toward the group-level mean - which generally improves predictive inferences (Gelman et. al., 2014). In addition, the within-business and within-precinct correlations are appropriately modeled. Using the 'brms' package, 500 samples were generated across 4 chains - resulting in 1000 posterior samples after discarding the initial warm-up iterations. All models converged with $\hat{R}$ less than 1.1 and no divergent transitions.

Three dependent variables were considered for the crime models: property crime, violent crime, and disorder crimes, that were modeled under two different specifications. Model 1 included all pre-treatment covariates, a linear and quadradic time trend variable, and treatment indicators, utilizing the weakly-informative prior distribution. Model 2 was specified identical to Model 1; however, the informative prior distribution was used in place of the weakly-informative one - placing a tighter, and mostly negative posterior mass on the treatment indicator and treatment time variables. Model 2, thus, was fit under the prior belief that CCTV cameras exert at least a modest negative effect on crime that is unlikely to vary far beyond -7% to -24%. In contrast, Model 1 was fit with very little prior information about the how CCTV cameras might affect crime. For analysis, both models were presented to determine the effect of the prior specification on the coefficients of interest.

To facilitate efficient computation, continuous variables were rescaled so that they were centered around zero. Rescaling variables in a Bayesian analysis introduces both computational simplicity and improves predictive inferences from hierarchical linear models (Gelman, 2004).

The continuous time variable (ranging from 1 to 36) was zero-centered at t = 13 (corresponding to January 2016 – the first month the Green Light intervention began) and was divided by 12. Therefore, the time variable was transformed such that each time point represented a fraction of one year (i.e. 1 month = .0833), with one year was equivalent to 1. The time variable ranged from -1 (January 2015) to 1.917 (December 2017). For conciseness, only the treatment indicator variables are shown in Table 3, while the control variables are omitted. The full models containing the control variables are shown in Appendix D through K. Results were summarized using the posterior mean and quantiles (lower and upper 95% and 50%).

Effect of Green Light on Reported Crime

Table 3 displays the results for the crime models, utilizing both prior distributions (weakly-informative and informative). These models estimate the average effect of the covariates on an individual business, rather than a population average effect. Because the treatment indicators $\pi_{itk} * GL$ and $\pi_{itk} * post\_GL$ did not occur at all businesses immediately, the coefficients in the model represent the estimated change in crime reports relative to the matched controls *and* other Green Light businesses in the treatment group that had not yet implemented their own cameras. Therefore, the following models and analyses consider two *related but distinct* quantities. First, the model coefficients estimate the change in the number of crimes over time at individual businesses, comparing those who received Green Light to those who did not (including both businesses in the Green Light treatment pool who had not yet received cameras and businesses in the matched control pool). Second, utilizing estimates from the models allows a focused comparison of *only* Green Light businesses relative to the matched controls. This second analysis provides counterfactual estimates of what would have happened, in the aggregate, to the 86 Green Light businesses had the program not been implemented. To begin, I will first discuss the significance of the model coefficients, as they relate to the behavior of individual businesses. The following analyses focuses on the results in Model 2, utilizing the informative prior distribution.

Table 3. Estimated Effect of Green Light and Green Light Post-Intervention Time – 200 Foot Buffer

	Property Crime		Violent Crime		Disorder Crime	
Variable	mean	est. error	mean	est. error	mean	est. error
Model 1: Weakly Informative Prior						
Green Light	0.37	0.12	0.15	0.19	0.22	0.21
Green Light Post-Intervention	-0.26	0.17	-0.03	0.24	-0.25	0.25
Model 2: Informative Prior						
Green Light	0.34	0.12	0.15	0.17	0.17	0.19
Green Light Post-Intervention	-0.23	0.16	-0.05	0.2	-0.22	0.23

The immediate impact of Green Light $\pi_{itk} * GL$ was associated with an estimated 40% increase in reported property crimes the month the business began the program (β = .34; 95% CI = .09, .56). Thereafter, the estimated number of reported property crimes decreased at a rate of about -1.9% per-month, or about -23% after one year following the intervention (β = -.26; 95% CI = -.56, .09). The change in post-intervention slope varied considerably and could have plausibly varied by between -46% and 9%. Relative to property crimes, the number of reported disorder crimes increased at Green Light businesses by a smaller, and more variable amount immediately following the intervention – approximately 13% (β = .17; 95% CI = -.22, .54), while the change in slope post-intervention was about -1.7% per-month, or -20% one year after (β = -.22; 95% CI = -0.67, .24), with relatively wide credible intervals. In contrast to property crimes and disorder crimes, the number of reported violent crimes was not substantially or consistently affected by the immediate impact of Green Light or after it had been implemented for at least a year. The initial implementation was associated with a highly variable violent crime increase of 16% (β = .15; 95% CI = -.17, .48), while the effect a year post-intervention was an estimated -5% decrease ($\beta = -.05$, 95% CI = -.43, .34). The considerable uncertainty around this estimate suggests that the change in slope associated with post-intervention time could have been responsible for as much as a 35% decrease or a 40% *increase* in reported violent crimes – providing very little evidence that Green Light affected violent crime reports consistently. For all three models, the choice of prior distribution (weakly informative versus informative) did not strongly affect the results in any of the model specifications. In most cases, the estimates were nearly identical, however the informative prior distribution constrained the variability of the estimates slightly (primarily due to the narrow standard deviations specified). The estimates in all three crime models exhibited a large amount of residual uncertainty – suggesting the impacts of Green Light may not have affected all participating businesses equally. This makes the estimation of an average effect difficult and increases the errors of the estimates. In a conventional (frequentist) framework, most of these estimates would not reach the standard statistical significance threshold of .95 used in other similar research.

Utilizing predictions from the fitted model allows the estimation of a counterfactual estimand - that is, an estimate of what would have happened in the absence of Green Light. Figure 7 displays the estimated number of crime incidents at Green Light businesses and at the matched controls, while Figure 8 shows the estimated mean difference between the two (estimated number of Green Light crimes minus the estimated number of matched control crimes). These figures highlight the month-to-month change in reported crime incidents during the implementation and post-implementation phases of Green Light. By subtracting the estimates from the matched controls from the Green Light businesses, Figure 8 illustrates the estimated increase in reported property throughout 2016 and the gradual decrease mid-way through 2017.

The changes in disorder crimes were similar, but less evident, while changes in violent crimes appeared driven primarily by underlying trends and monthly seasonal fluctuations.

Figure 7. Predicted Number of Crimes, Green Light vs. Matched Controls

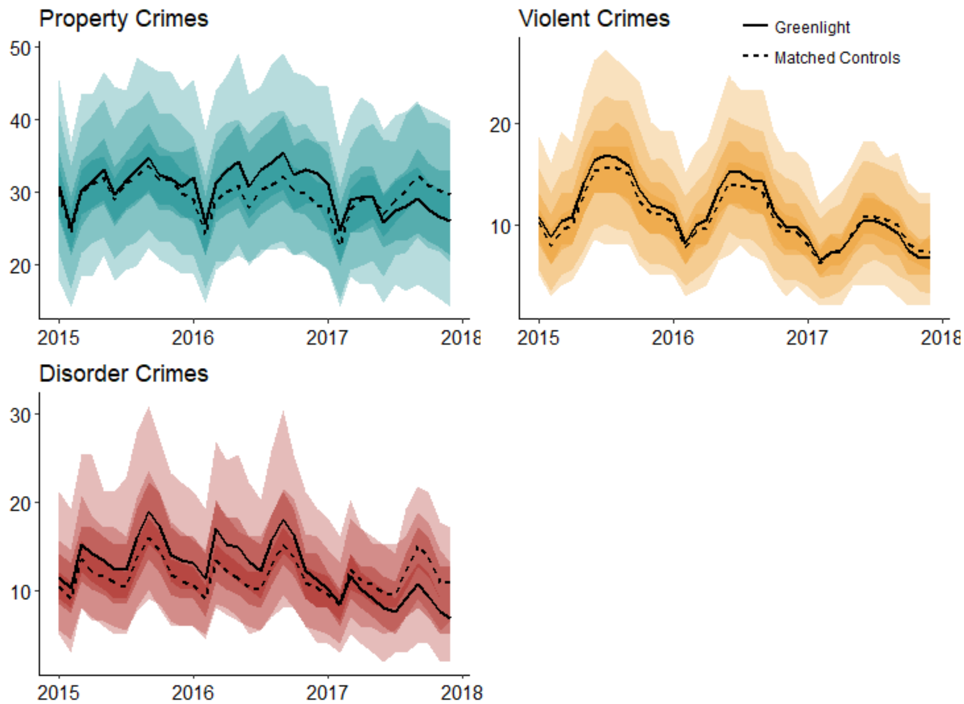

Figure 7 displays the predicted number of monthly crime incidents, with the 50% and 95% posterior mean intervals highlighted. The estimated number of property crimes and disorder crimes were both higher than the matched controls during the implementation phase in 2016. During the latter portion of 2017, these incidents decreased relative to the controls. Violent crimes were not substantially different than the matched control businesses.

Figure 8. Difference in Predicted Number of Crimes, Green Light vs. Matched Controls

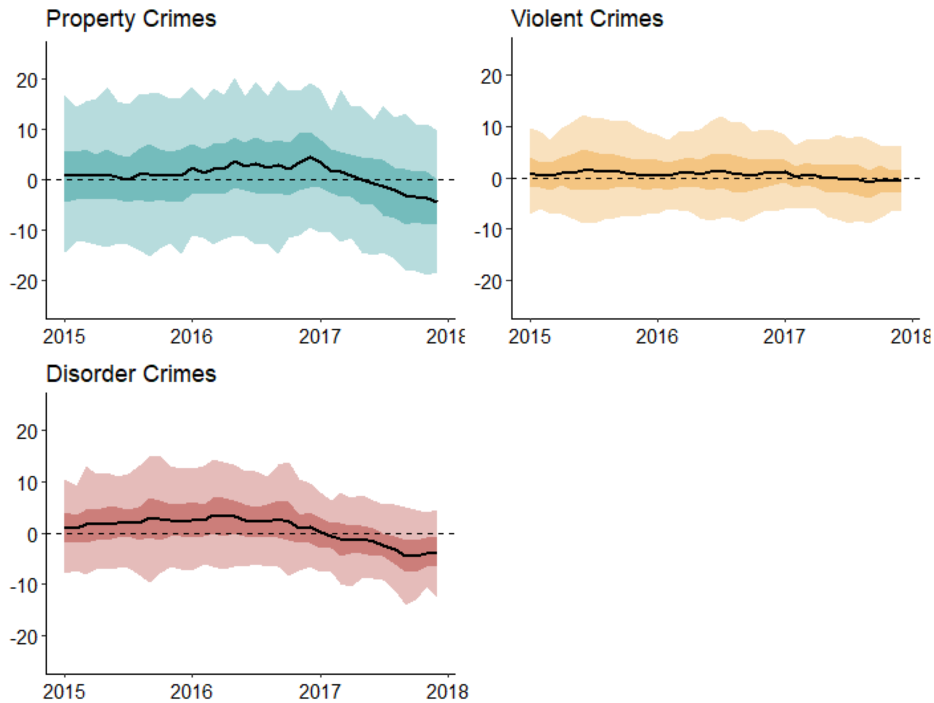

Figure 8 displays the difference in predicted number of monthly crime incidents, with the 50% and 95% posterior mean intervals highlighted. This figure is functional similar to Figure 7 but highlights the difference between the matched controls and the Green Light businesses by subtracting the number of crimes in the matched control group to the Green Light group. Note that by late 2016 to early 2017 the predicted number of crimes began to decrease relative to the matched controls.

Table 4 displays the predicted number of crime incidents at Green Light and the matched control businesses by quarter, while Table 5 displays the percentage change. During 2016, as Green Light was being implemented, the average number of property crime incidents increased by an estimated 12% relative to the matched controls, while disorder crimes increased by about 27%, and violent crimes by about 14%, with a considerable amount of residual error. During the post-implementation phase the most substantial decreases were estimated for the third and fourth quarters of 2017. Property crimes were estimated -6% lower in Q3 2017 and a further -11% in the Q4 2017 relative to the matched controls – equating to approximately 7 and 10 fewer incidents, respectively. The number of disorder crimes was estimated to have decreased at a more rapid rate: -20% in Q3 2017 and -31% in Q4 2017, relative to the matched controls –

equaling 9 and 12 fewer incidents. Violent crimes were mostly unaffected throughout the implementation and post-implementation phase. Figure 9 shows the quarterly percentage difference between Green Light and the matched controls, with the associated 95% and 50% credible intervals. Consistent with the results from the fitted model, the residual uncertainty about the effect of Green Light remained very high, making it difficult to estimate with certainty the effect on crime incidents. However, the most general pattern (derived from the mean estimates) indicated that property crimes and disorder crimes may have decreased at least marginally relative to the control businesses during the last half of 2017. In absolute numbers, the estimated number of crimes prevented because of Green Light were relatively modest and estimated with a high degree of uncertainty. In 2017 as a whole, the estimated number of property crimes decreased by -11.8 (95% CI: -182.2, 164.4), while the number of violent crimes decreased by -2.9 (95% CI: -89.5, 90.3), and the number of disorder crimes by -25.6 (95% CI: -121.1, 73.9).

Table 4. Estimated Mean Quarterly Difference in Crime Incidents – Green Light vs. Matched Control

	Property Crime					Violent Crime					Disorder Crime				
Time	2.50%	25%	mean	75%	97.5%	2.50%	25%	mean	75%	97.5%	2.50%	25%	mean	75%	97.5%
Q1 2016	-35.9	-7.1	6.9	22.3	49.6	-19.8	-6.6	2.0	9.6	26.6	-19.2	-1.0	8.3	18.2	37.2
Q2 2016	-33.4	-7.6	9.2	24.8	54.4	-23.3	-6.7	3.2	12.6	31.4	-17.5	-0.1	9.4	18.7	40.2
Q3 2016	-34.9	-6.6	9.1	25.3	53.4	-24.8	-7.1	3.3	13.3	31.6	-22.5	-2.5	7.7	17.7	38.9
Q4 2016	-31.1	-5.6	9.8	24.3	52.4	-21.5	-6.1	1.9	10.1	27.3	-23.3	-5.1	4.8	13.8	34.2
Q1 2017	-32.4	-8.1	6.7	20.7	48.3	-18.7	-6.1	0.9	7.6	21.5	-24.0	-8.6	-0.1	7.6	24.8
Q2 2017	-44.5	-15.7	-0.9	13.7	44.0	-23.3	-8.1	-0.5	7.6	24.3	-27.8	-12.1	-4.3	3.5	20.3
Q3 2017	-50.1	-21.8	-6.9	8.1	38.7	-26.8	-10.1	-1.7	7.6	24.8	-33.1	-18.2	-9.3	-0.5	15.4
Q4 2017	-55.2	-26.8	-10.7	5.6	33.4	-20.7	-9.1	-1.6	4.7	19.7	-36.2	-20.2	-11.9	-4.0	13.4

Table 5. Estimated Percentage Change in Crime Incidents – Green Light vs. Matched Control

	Property Crime					Violent Crime					Disorder Crime				
Time	2.50%	25%	mean	75%	97.5%	2.50%	25%	mean	75%	97.5%	2.50%	25%	mean	75%	97.5%
Q1 2016	-36%	-10%	10%	29%	74%	-58%	4%	16%	42%	142%	-44%	-5%	33%	60%	157%
Q2 2016	-35%	-5%	14%	30%	71%	-52%	5%	16%	40%	120%	-43%	-4%	32%	60%	152%
Q3 2016	-35%	-6%	12%	27%	77%	-50%	5%	13%	37%	105%	-46%	-6%	27%	53%	137%
Q4 2016	-33%	-6%	13%	29%	70%	-59%	4%	16%	40%	143%	-54%	-14%	19%	43%	132%
Q1 2017	-38%	-9%	12%	29%	75%	-68%	3%	16%	43%	158%	-65%	-29%	3%	28%	114%
Q2 2017	-41%	-16%	3%	18%	68%	-66%	3%	7%	30%	131%	-68%	-37%	-8%	12%	96%
Q3 2017	-45%	-23%	-5%	10%	51%	-65%	3%	-2%	24%	109%	-73%	-45%	-20%	-2%	64%
Q4 2017	-51%	-29%	-11%	3%	45%	-75%	2%	-2%	27%	150%	-81%	-54%	-30%	-13%	53%

Figure 9. Percent Change in Predicted Number of Crimes, by Year-Quarter, Green Light vs. Matched Controls

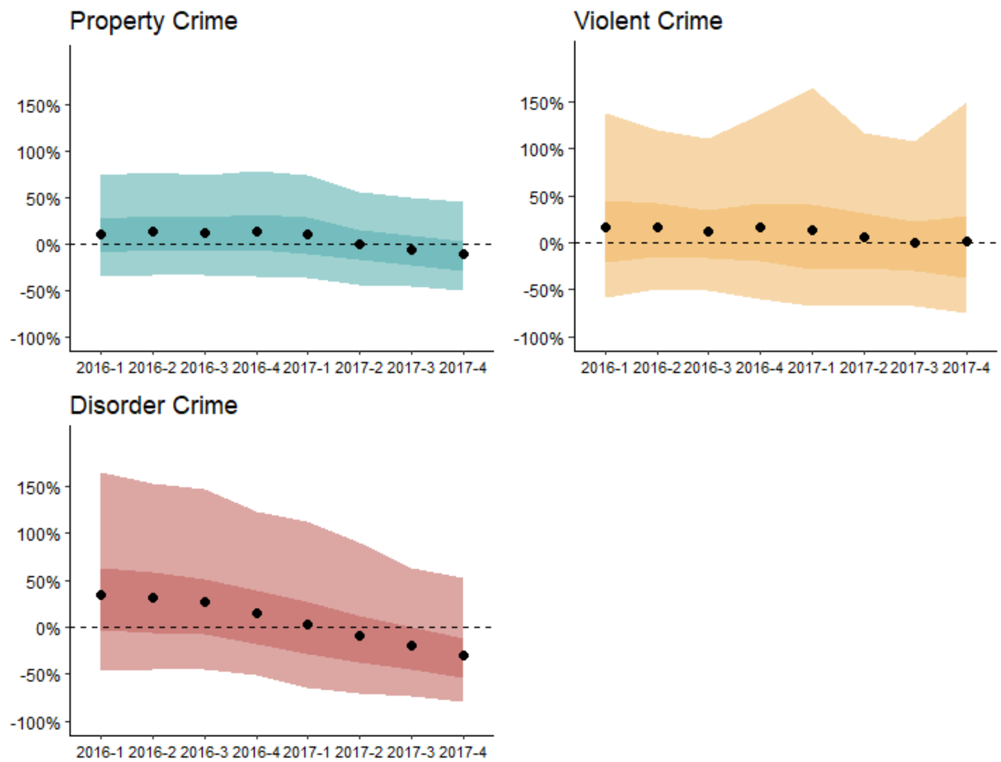

Figure 9 shows the estimated percent change in number of crimes by year-quarter. Note that Q3 and Q4 2017 observed modest decreases relative to the matched controls for property and disorder crimes, while the estimated percentage change among violent crimes was small and highly volatile.

Figure 10 shows the predicted number of crime incidents, post-intervention, for each of the business categories and crime types, illustrating the mean change in crime incidents reflected in the model coefficients. Nearly all business types observed an immediate and abrupt increase in the number of reported property crimes post-intervention. This increase varied by between 50% for gas stations to 28% for liquor stores. Consistent with the model results, the number of reported incidents decreased monthly, post-intervention, at a modest rate. As a whole, these estimates provide a mixed picture about the effect of Green Light in the aggregate. The uncertainty and variability in these estimates complicate the understanding of Green Light's *average* effect across the entire pool of treatment units. In cases where treatment effects may vary considerably based on context, it is often important to consider the variability of treatment in different circumstances.

Figure 10. Predicted Number of Crimes, Post-Green Light, by Business Type

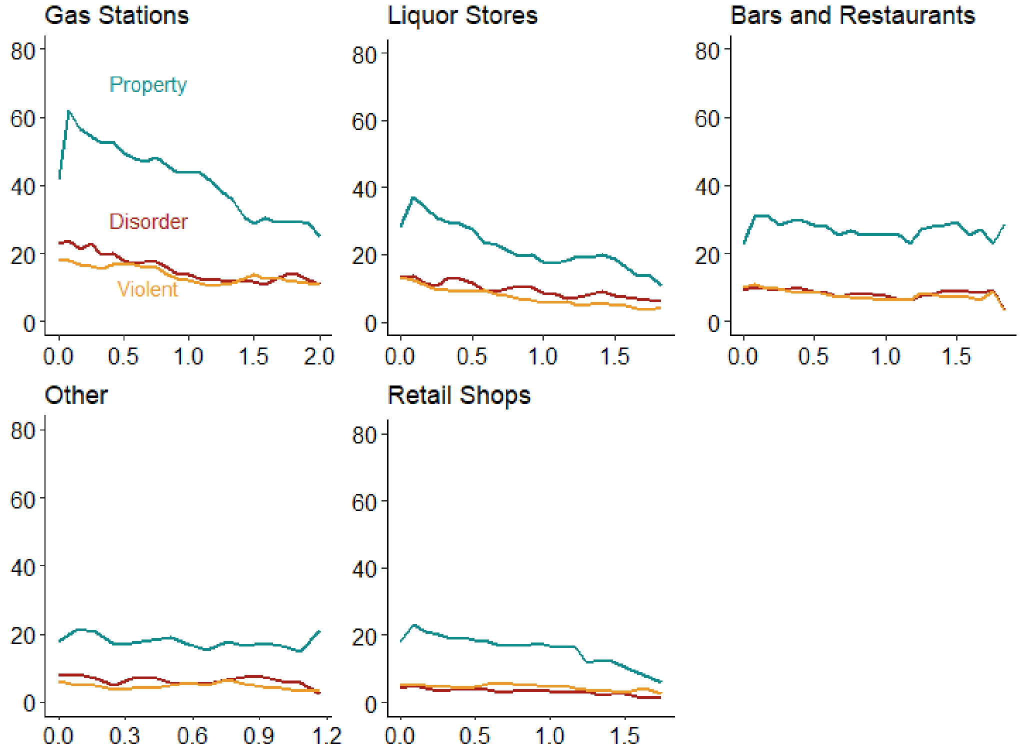

Figure 10 presents the predicted average monthly number of crimes for Green Light businesses post-implementation, where 0 represents the month *prior* to the intervention. The x-axis represents the number of year, post-Green Light (i.e. .5 = 6 months, 1 = 1 year). While most businesses saw at least a modest increase in the number of reported property crimes post-Green Light gas stations and liquor stores observed the largest immediate increase in reported incidents. After about a year post-intervention, the number of property crime reports had declined to near their pre-intervention levels.

Variation of Green Light's Effect on Reported Crime

Variation in the treatment effect of Green Light was explicitly modeled by specifying varying slopes for each individual business – allowing an examination of the variability of these results. Figure 11 shows the estimated standard deviations of the varying effects from each of the fitted models. These plots show that the individual intercepts for each business (Intercept * id), exhibited the greatest amount of variation among all the model coefficients. This means much of the difference in between-business variation was due to base-rate crime trends. The time trends

(Time) and immediate impact of the intervention (Green Light) varied relatively little between businesses – indicating the overall time trends were stable, and the immediate impact of the intervention was relatively consistent between-businesses (that is, the change in crime reports immediately after Green Light affected most locations similarly). The standard deviations for the change in slope, post-intervention (Green Light * Post Intervention), appeared moderately large and was estimated with less precision than some of the other varying effects (indicating the change in slope, post-intervention varied more than the immediate impact). This suggests that the effect of Green Light after the first month cameras were installed varied more between businesses.

Figure 11. Estimated Standard Deviations of Varying Effects

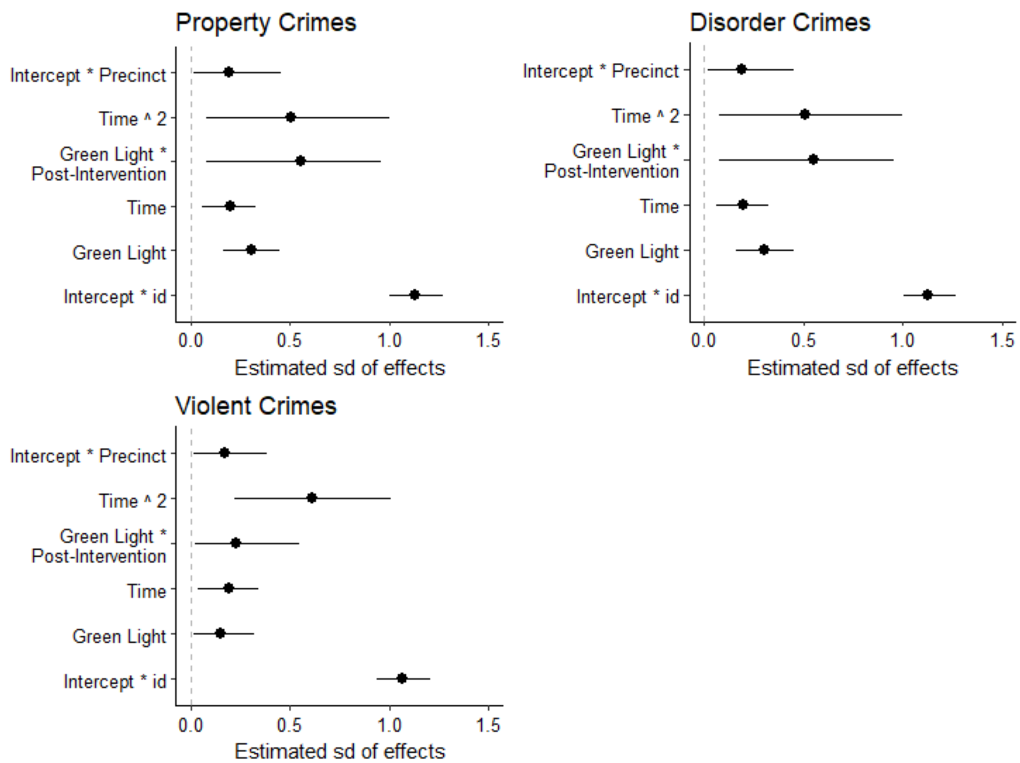

Figure 11 displays the estimated standard deviations of the varying effects from each of the crime models, with the posterior means and 95% intervals shown. For all three models, the greatest variation was observed in the varying intercepts by business id. The initial impact of Green Light varied relatively little, compared to the change in post-intervention time.

Understanding the variation in individual businesses' intercepts is more apparent when viewing the substantial difference in average monthly incident counts by business. Appendix L, M, and N show the actual number of reported crime incidents per month, with the estimated 95% and 50% intervals from the fitted models highlighted. Many businesses observed *no* crime incidents throughout the entire duration of the study period. About 5% of businesses reported no property crime, 14% no violent crime, and 18% no disorder crime between 2015 and 2017. On the other hand, a small number of businesses experienced a much higher and consistent number of incidents. Indeed, much of the variation in crime was concentrated at a handful of businesses. Among the 86 Green Light businesses studied, about 4% (5 properties) of accounted for 21% of property crime reports, 25% of violent crime reports, and 34% of disorder crime reports between 2016 and 2017. Furthermore, 50% of property crime incidents occurred at 17 businesses, 50% of violent crimes at 13 businesses, and 50% of disorder crimes at 10 businesses (see Figure 12). This illustrates that the majority of crime incidents among the Green Light businesses was disproportionately concentrated among very few businesses, which was especially apparent among violent and disorder crimes. While it is difficult to estimate a treatment effect for only this small subset, these businesses observed an 5% decrease in property crimes, a 38% decrease in violent crimes, and a 48% decrease in disorder crime from 2015 to 2017. This high amount of variation in between-business incident counts is reflected in the wide uncertainty intervals in the fitted models.

Figure 12. Proportion of Crime Incidents by Number of Businesses

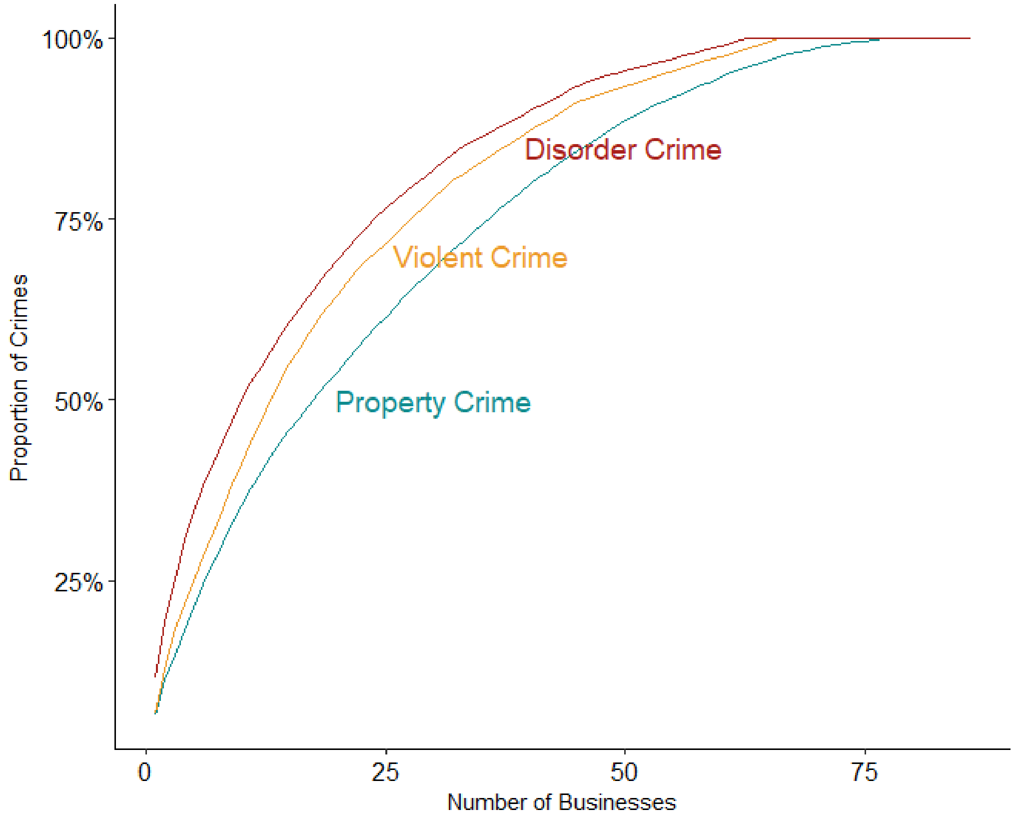

Figure 11 shows the proportion of crime incidents by the number of businesses. Disorder and violent crimes were more highly concentrated at few businesses than property crimes.

Impact of Green Light on Diffusion or Displacement of Crime

A persistent concern regarding place-based crime prevention strategies is whether crime simply "moves around the corner" (Weisburd et. al., 2006). On the other hand, there is some evidence indicating that these crime prevention strategies may produce a diffusion of benefits to the surrounding area by reducing crime there as well. Testing for these effects in regard to the Green Light intervention involved performing the same analysis for the crime outcome models with the unit of analysis changed to the 100-foot displacement zone around each business. This analysis, therefore, determined whether there was a change in the number of crimes just outside the catchment zone that could be attributed to the Green Light intervention. These models were specified using the same priors and model parameters as the in the main analysis.

Table 6 displays the results from the three displacement zone models. As indicated, there was little evidence that reported crime changed substantially in the area immediately outside of the catchment area, with most of the coefficients of interest centered near zero. Figure 13 shows the estimated difference between crimes in the displacement zone for Green Light and the matched controls, where no substantial change was evident. This analysis found no consistent evidence that property, violent, or disorder crime was displaced to areas outside of Green Light businesses nor was there an apparent residual benefit. Given the main effect of the Green Light intervention was modest and highly variable, it is unsurprising that additional effects would be observed outside the business as well. However, it does lend some credence to the finding that, under the most optimistic settings (i.e. the decrease in disorder-related incidents) Green Light did *not* simply displace these crimes to nearby areas. Rather, these modest decreases were likely concentrated within the immediate vicinity of the business.

Table 6. Estimated Effect of Green Light and Green Light Post-Intervention Time on Displacement Zone

	Property Crime		Violent Crime		Disorder Crime	
Variable	mean	est. error	mean	est. error	mean	est. error
Model 1: Weakly Informative Prior						
Green Light	-0.08	.21	-0.01	.28	-0.02	.30
Green Light Post-Intervention	-0.08	.25	-0.02	.31	-0.34	.35
Model 2: Informative Prior						
Green Light	-0.09	.21	-0.02	.25	-0.07	.26
Green Light Post-Intervention	-0.08	.25	-0.03	.27	-0.28	.30

Figure 13. Difference in Predicted Number of Crimes, Green Light vs. Matched Controls within 100-foot Diffusion Zone.

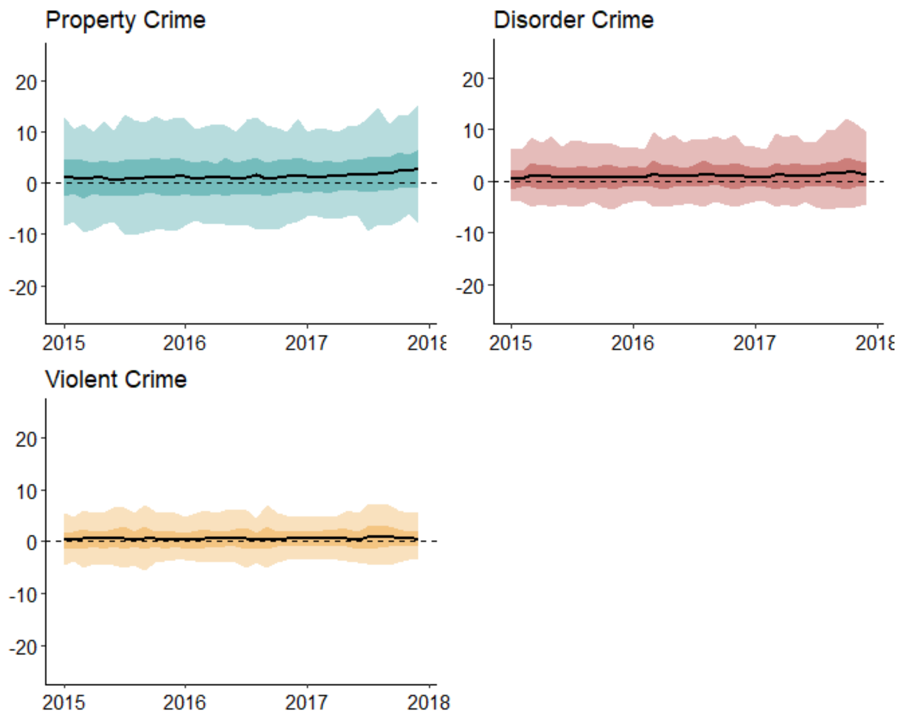

Figure 13 displays the difference in predicted number of monthly crime incidents, with the 50% and 95% posterior mean intervals highlighted, for the 100-foot diffusion model.

Effect of Green Light on Calls for Service

Calls for service, a category of incidents distinct from crime reports, were also analyzed. These represented all 911 calls to the DPD crime reporting system and excluded officer-initiated calls for service (i.e.: preventative patrols, stop and frisks, and traffic stops). Table 7 displays the results for the calls for service models. As stated previously, due to data limitations, the analysis was constrained to the period between 2016 and 2017, while pre-Green Light data was not available for 2015. Therefore, these models were analyzed separately from the crime models, although the model formulation remained identical. In contrast to the crime models, the calls for service models were estimated with a higher amount of precision – primarily because the monthly number of calls for service was less variable than the monthly number of crime incidents. In Model 2, the estimated immediate effect of Green Light was a 23% increase in the number of calls for service (β = .21; 95% CI = .02, .37). The number of calls for service increased modestly thereafter – by an additional 7% for the year after implementation (β = .07; 95% CI = -.17, .31), that varied by between -16% and 36%. Even though, on average, businesses experienced an increase in the number of calls for service, this effect varied substantially between businesses (see Appendix N). Some locations with very few or no calls for service observed little change in their calls for service post-Green Light, while others observed an abrupt and consistent increase. The estimated standard deviations of the varying effects reflect this variability as well. Much of the variation in the varying effects was observed in the difference in individual businesses' intercepts and the immediate effect of Green Light. This reflects the large differences in the baseline number of calls for service at businesses (i.e. many businesses were high-rate locations, while others had virtually no calls for service) and the variable impact of Green Light (i.e. many businesses observed a large immediate increase, while others observed virtually no change). This is in contrast to the models for property, violent, and disorder crimes, which generally showed a fairly consistent effect of Green Light the month it began. This may reflect the reporting behavior of the business owners, rather than the change in criminal behavior at the business. Table 8 displays the estimated number and percentage quarterly change in crimes for service.

Table 7. Estimated Effect of Green Light and Green Light Post-Intervention Time – 200 Foot Buffer

	Calls for Service	
Variable	mean	est. error
Model 1: Weakly Informative Prior		
Green Light	.21	.09
Green Light Post-Intervention	.07	.13
Model 2: Informative Prior		
Green Light	.20	.09
Green Light Post-Intervention	.07	.12

Table 8. Estimated Number and Percentage Quarterly Change in Calls for Service

	Mean Difference					Percentage Difference				
Time	2.50%	25%	mean	75%	97.5%	2.50%	25%	mean	75%	97.5%
Q1 2016	-198.7	-43.1	34.4	111.9	266.0	-22%	-5%	5%	14%	36%
Q2 2016	-169.8	-32.5	47.8	129.6	271.0	-17%	-4%	6%	15%	33%
Q3 2016	-152.8	-24.8	54.8	125.6	282.3	-16%	-3%	7%	15%	34%
Q4 2016	-120.9	4.55	77.5	146.7	278.5	-15%	1%	11%	20%	40%
Q1 2017	-88.2	20.7	83.1	142.1	260.4	-13%	3%	13%	22%	42%
Q2 2017	-96.4	41.5	113.1	184.2	330.1	-11%	5%	14%	23%	44%
Q3 2017	-73.9	63.5	143.3	216.1	381.9	-8%	7%	17%	26%	46%
Q4 2017	-50.1	83.2	162.9	236.7	392.9	-6%	10%	22%	32%	54%

Figure 14. Predicted Quantities for Calls for Service Models

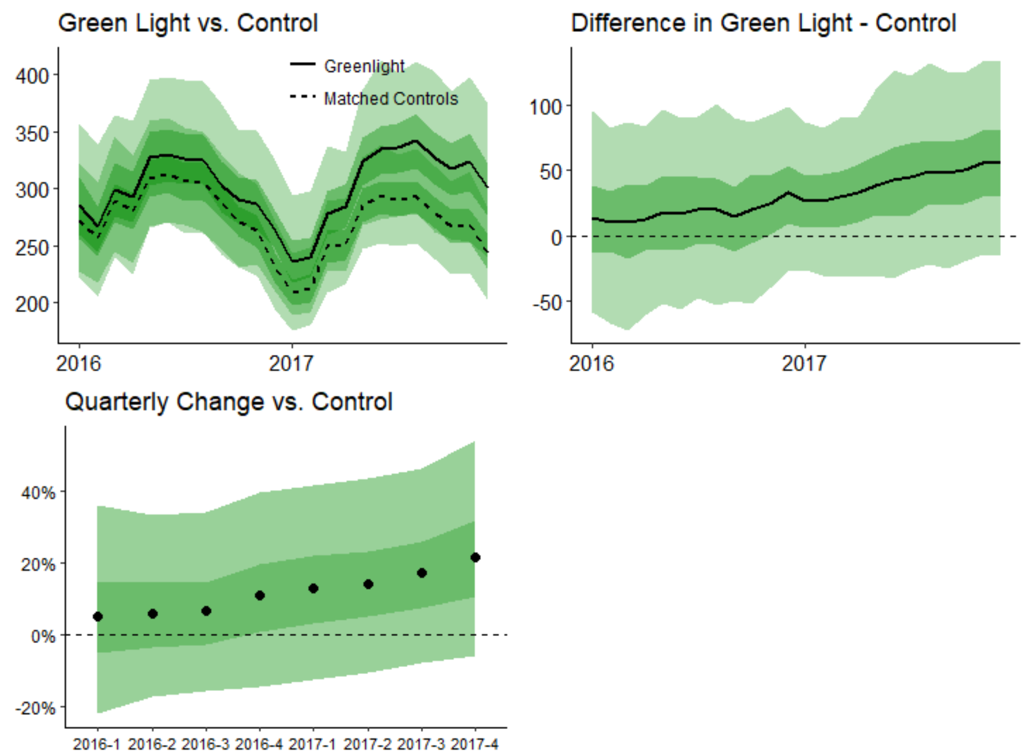

Figure 14 shows the predicted quantities from the calls for service models. The first panel shows the predicted number of calls for service in the Green Light group versus the matched controls per month. The second panel shows the difference (Green Light minus matched control), and the third panel shows the average quarterly percent difference between the two groups.

Figure 15. Estimated Standard Deviations of Varying Effects – Calls for Service

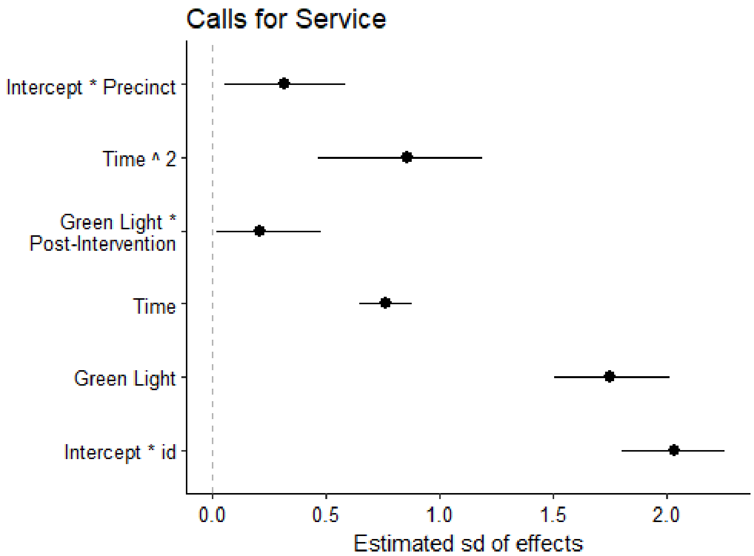

Figure 15 displays the estimated standard deviations of the varying effects for the calls for service model, with the posterior means and 95% intervals shown. Most of the variation is related to the underlying calls for service and the initial effect of Green Light.

SUPPLEMENTARY ANALYSES

Sensitivity Analysis for Buffer Size

While the presented analysis indicates a set of models that fit the data reasonably well, it can be argued the results would have turned out different if an alternative set of models had been fitted. There is compelling evidence that so-called "researcher degrees of freedom" can significantly impact a study's outcome – due to selective reporting and arbitrary design decisions (Gelman & Loken, 2013). Sensitivity analyses present a method to determine whether the substantive conclusions of the analysis are robust to changes in model specification or design choices (Gelman et. al., 2014). For this study, I focus my attention on a single major decision point: the chosen size of the buffer around each business.

For the primary model, the chosen buffer size was conceptualized as a 200-foot buffer around the business (equating to approximately a one-half block length). This buffer size was consistent with other similar studies utilizing CCTV camera systems as their unit of analysis – that have generally varied between 1/4 to 1/2 of an average block length[10] (Caplan, Kennedy & Petrossian, 2011; Ratcliffe, Taniguchi, & Taylor, 2009; Waples, Gill, & Fisher). This is also consistent with preliminary analyses carried out by DPD which considered a 250-foot area around each business. The analysis was constrained to the area generally around the address of the business because most of the Green Light cameras were internal-facing or captured incidents just outside the business (i.e. on nearby intersections or roads). However, several arguments can be made that the size of the buffer could be conceptualized as significantly *smaller* than 200 or 250 feet. Constraining the buffer to within 150 or 100 feet would limit the analysis to crimes that occurred more directly on the premises, rather than in the immediate area around the business.

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[10] Caplan, Kennedy, and Petrossian utilized an average 582-foot viewshed; Ratcliffe, Taniguchi, and Taylor a 500-foot buffer; Waples, Gill, and Fisher a 300-foot buffer.

This would assume that the effects of Green Light would be limited to incidents directly on the premises of the business. To test the effect of shrinking the buffer size, all analyses were performed again by re-estimating the propensity score and outcome models for all crimes and calls for service, using a 150-foot, and 100-foot buffer. Table 9 displays the results from this re-estimated sample, compared to the main analysis utilizing the 200-foot buffer.

Table 9. Estimated Effect of Green Light and Green Light Post-Intervention Time, Results by Buffer Size

		Property Crime		Violent Crime		Disorder Crime	
Buffer	Variable	mean	est. error	mean	est. error	mean	est. error
	Model 1: Weakly Informative Prior						
100	Green Light	0.66	0.19	0.08	0.34	-0.01	0.35
150	Green Light	0.35	0.14	0.04	0.23	0.14	0.24
200	Green Light	0.37	0.12	0.15	0.19	0.22	0.21
100	Green Light Post-Intervention	-1.16	0.29	-0.59	0.46	-0.74	0.47
150	Green Light Post-Intervention	-0.28	0.2	-0.2	0.27	-0.34	0.3
200	Green Light Post-Intervention	-0.26	0.17	-0.03	0.24	-0.25	0.25
	Model 2: Informative Prior						
100	Green Light	0.51	0.18	-0.05	0.29	-0.12	0.29
150	Green Light	0.31	0.14	0.01	0.21	0.09	0.23
200	Green Light	0.34	0.12	0.15	0.17	0.17	0.19
100	Green Light Post-Intervention	-0.89	0.25	-0.41	0.36	-0.5	0.33
150	Green Light Post-Intervention	-0.25	0.19	-0.16	0.26	-0.3	0.25
200	Green Light Post-Intervention	-0.23	0.16	-0.05	0.2	-0.22	0.23

Comparing the results from the three models, there is evidence that the results are moderately sensitive to the size of the buffer - especially when the analysis is constrained to within 100 feet of a business. While there were no substantial differences between the 200-foot buffer and the 150-foot buffer (their model coefficients were nearly identical), a 100-foot buffer yielded more optimistic estimates of the effect of Green Light on the crime outcomes, that was especially pronounced among property crimes. Consistent with the primary analysis, businesses reported an increased number of property crimes the month they began Green Light – by about

66% (β = .51; 95% CI = .15, .85), that subsequently decreased by -59% one year following the intervention (β = -.89; 95% CI = -1.39, -.39). Violent crimes and disorder crimes were not estimated to have increased immediately after the implementation of Green Light, and their small main effects relative to the estimated error indicated a null effect. Post-intervention, violent crimes and disorder crimes were estimated to have decreased by about -34% (β = .41; 95% CI =-1.12, .28 and -39% (β = -.50; 95% CI = -.1.18, .11) respectively. However, the estimated error of these coefficients was very high, making it difficult to determine whether the decrease was different from zero. For the calls for service models, there were not substantial differences between the different buffer sizes, however the initial impact of Green Light was much lower for the 100-foot models, while the post-intervention change remained relatively similar (see Table 10).

Figure 15 displays the quarterly mean difference between the Green Light businesses and the matched controls, for each of the buffer specifications. Comparing the results across the three different buffer specifications illustrates the moderate sensitivity to the choice of size. This sensitivity was most evident for property crimes, where the estimated error was substantially lower, and the estimated effect of Green Light was much more optimistic for the 100-foot buffer size. For instance, in Q4 2017, which represented the three-month period with the greatest decrease relative to the matched controls, the number of property crimes in Green Light businesses was estimated about -61% (95% CI: -95%, -4%) lower for the 100-foot buffer, while they were about -7% and -12% lower for the 150 and 200-foot buffers, respectively. Here, the models suggest that property crimes may have decreased directly on the business' property, with less optimistic estimates coming from models including areas immediately around the business.

Table 10. Estimated Effect of Green Light and Green Light Post-Intervention Time, Results by Buffer Size

		Calls for Service	
Buffer	Variable	mean	est. error
	Model 1: Weakly Informative Prior		
100	Green Light	-.38	.31
150	Green Light	.22	.14
200	Green Light	.21	.09
100	Green Light Post-Intervention	.11	.22
150	Green Light Post-Intervention	.01	.16
200	Green Light Post-Intervention	.07	.13
	Model 2: Informative Prior		
100	Green Light	-.29	.26
150	Green Light	.20	.15
200	Green Light	.20	.09
100	Green Light Post-Intervention	.01	.28
150	Green Light Post-Intervention	.01	.16
200	Green Light Post-Intervention	.07	.12

While these models present a much more optimistic view of Green Light, the smaller number of incidents considered substantially increased the variability of the estimates. The informative prior distribution constrained estimates of Green Light and Green Light post-intervention to values more consistent with the general knowledge about CCTV. However, because many businesses in both the Green Light and matched control groups experienced *no* incidents in some of the quarters, estimating the precise magnitude of the effect was made substantially more difficult, especially when comparisons were made on rare events (i.e. violent crimes). The general findings were not changed substantially by the varying the buffer size.

Figure 16. Difference in Predicted Number of Crimes, Green Light vs. Matched Controls, by Buffer Size

Figure 16 displays the quarterly mean difference between Green Light businesses and matched control businesses, with the 95% and 50% credible intervals. Estimates from the 100-foot, 150-foot, and 200-foot buffer models are highlighted in yellow, blue, and red respectively. The results across all three specifications remain relatively consistent, however the 100-foot model yields a more optimistic estimate of the effect on property crimes.

Cost-Benefit Analysis

Determining the optimal circumstances under which Green Light is cost-effective requires the analysis of the model predictions under a set of various assumptions. Cost-benefit analyses are highly sensitive to assumptions – therefore this analysis represents only one of many outcomes. For the purposes of this study, the city of Detroit (including the Detroit Police Department) was considered as the primary stakeholder. While costs and benefits could quite logically be measured against business owners (for instance, do Green Light cameras increase business traffic?) or against citizens (does the intangible harm of crime outweigh the costs of implementation?), this study focuses on those factors that are most easily observed and measurable. In addition, the cost-benefit analysis considers *only* the post-implementation period in 2017 when all businesses were enrolled. The first set of assumptions are the various costs incurred by the city of Detroit due to the Green Light program. In order to estimate the cost-benefit ratio of the Green Light program, I utilize estimates from the models to determine the costs (or savings) relative to the matched controls. Therefore, this amount represents the change in costs relative to what might have happened if Green Light cameras had not been installed. Based on prior research, a modified version of the LaVigne et al. (2011) scale was utilized to estimate approximate costs of crime (see Appendix C). Total estimated costs (tangible and intangible) were used in the calculation and were adjusted for inflation by 2017 US dollars. These represented the cost of arrest, pre-sentence and adjudication, and the cost of incarceration. These did not consider the potential averted social costs associated with crime.

One cost-saving benefit of the program is the deferral of CCTV camera installation and maintenance costs (roughly $4,000 to $6,000) to participating businesses, plus the installation of signs ($400-$650) and the installation of lighting ($450-500). Applied over the 86 businesses this was a deferred cost of between $348,000 to $522,000 for camera installation and maintenance, $34,800 to $56,550 for signage, and $39,150 to $43,500 for lighting (City of Detroit, Costs to Get Involved, 2017). Fixed costs related to Green Light represent the annual cost of maintaining the camera monitoring system at the Detroit Police Department headquarters, the salary of officers involved, and the monthly cloud storage of camera data. Staffing for Green Light consisted of both sworn officers and civilian analysts embedded in DPD’s crime intelligence unit. In 2016 the crime intelligence unit employed 15 civilian analysts, 12 police officers, 5 intelligence specialists, and 3 part-time police assistants. Command staff included one captain, one lieutenant, and 4 sergeants. In total, assuming 2080 working hours in a year, the annual salary cost of the crime intelligence unit cost DPD approximately $2.2 million. Approximately

60% of the crime intelligence unit's time was devoted to Green Light related activities – costing the department about $1.32 million in salary. However, by the end of 2017 there were 237 businesses actively enrolled in Green Light – of which only 86 (36%) were considered for this study. Therefore, assuming staff time was split evenly among all enrolled businesses, the portion of cost allocated for the 86 businesses in this study for 2017 was $475,200 ($1,320,000 * .36). Storage of CCTV camera footage was handled through a digital cloud storage service, that cost DPD approximately $1,000 per month. While the city constructed a $3 million real-time crime center, of which Green Light was a part, this analysis does not consider this as part of the costs. Furthermore, during the time under consideration (2015 to 2017) the real-time crime center was not yet fully operational.

Computing the cost of additional calls for service included estimating the cost of an officer's time, and the time spent for incidents. The average salary of a DPD officer ranges between $37,000 and $56,000 (City of Detroit, Officer Pay Ranges, 2017). Assuming a 40-hour workweek and 52 working weeks a year, this equates to an hourly salary of roughly $18 to $26. For the purposes of this study I assume the average annual income of a DPD officer is approximately $50,000, with an hourly rate of $24. This estimate is likely an underestimate of the true cost, as it does not consider non-salary costs such as medical leave, wear and tear to vehicles, and officer supplies involved in responding to calls for service. The fitted models estimated that Green Light increased the number of calls for service by about 23% (95% CI: 2%, 44%) the month the program began, and then by an additional 7% (95% CI: -16%, 36%) the year after. In practical terms this equated to 502 (95% CI: -308.5, 1365.3) additional calls for service in 2017. The average time officers spent on calls for service to Green Light businesses was 63 minutes per call, or 1.05 hours. Therefore, the average *additional* cost related to calls for service was an estimated $12,508 for 2017 (95% CI: -$7,626, $33,742). The most optimistic estimates suggest a cost-savings of $7,626, while the least optimistic estimate suggests a cost increase of $33,742. While there is some uncertainty around these estimates, they generally point to an increase in costs due to responding to calls for service.

The change in crime represented the largest potential cost savings as part of Green Light. Appendix C shows the estimated value of crimes, as they pertain to arrest, court, and corrections. For this analysis I consider the *median* cost for each of the crime categories (property crime, violent crime, and disorder crime). The median cost of a violent crime was estimated at $37,922, while the median cost of a property crime was $14,534, and disorder crimes were $8,282. Based on the models' mean estimates, property crimes were estimated to have decreased by -11.8, violent crimes by -2.8, and disorder crimes by -25.8 relative to the matched controls. Using these mean estimates equates to a cost savings of $171,466 for property crimes, $107,877 for violent crimes, and $212,591 for disorder crimes – or $491,934 total. Given the large uncertainty in the fitted models there was a corresponding wide range of credible estimates. There was a 95% probability that the change in crime due to Green Light was responsible for between a - $7 million cost savings, or a $6.4 million cost increase. There was a 50% probability that this cost ranged between -$2.8 million and $1.7 million.

Based on these costs and estimated savings Green Light *cost* the city an additional $21,149 with a 95% credible interval of -6.5 million to 6.9 million. This means Green Light may have been quite nearly cost-neutral, but there was roughly an equal probability that it could have cost or saved the city about ±$6 million. The installation of the camera systems at each business represented a large up-front cost that was passed on to business owners – avoiding a large fixed cost of between $348,000 to $522,000. As Piza et. al. (2016) found, most of the costs associated with CCTV camera systems were represented in the initial start-up and installation. Costs related to additional services made up a small proportion of the total cost. While there was an increase in the number of calls from Green Light businesses, the actual cost (in terms of officer hours) was modest – on average $12,508. The upper estimate was $33,742, while the lower limit was -$7,626. In any case, the estimated average increased costs due to calls from service would be relatively low. In fact, most of the costs incurred by the city were related to staffing the crime intelligence unit – which represented a substantial fixed cost due to the number of staffing involved. While there was concern among DPD and city staff that calls for service would increase, the actual cost of these increases were minimal, with most of the cost savings reflected in the change in crime. Because the costs of crime utilized in this study are likely an overestimate (primarily because the entire cost of crime is not fully absorbed by the city), the cost-benefit ratio likely represents a less optimistic scenario (Piza et. al., 2016; LaVigne et. al., 2011). Furthermore, this analysis does not consider benefits that might affect business owners or community members. Indeed, it is likely that costs incurred by the city might generate significant benefits to business owners, citizens, or other commercial entities. Another concern is the substantial costs that violent crim in particular inflicts upon victims – above and beyond the costs of arrest, trial, and incarceration. The costs of crime in this study focused solely on the tangible costs of handling and processing criminal cases – independent of the intangible costs of crime. Violent crime represents an immense cost to victims due to physical injuries, reduced quality of life, and (in the case of homicide) a lifetime of lost wages and productivity. Estimates of these intangible quantities have been placed near $450 billion annually in the United States (Miller, Cohen, & Wiersema, 1996). Therefore, if these intangibles were considered in the analysis the estimated cost savings would likely be significantly higher and might have offset the costs generated by the program. A similar analysis by LaVigne and others (2011) found that CCTV cameras without considering victimization costs were roughly cost-neutral, while they were cost-effective when victimization costs were considered. However, this total cost is beyond the limited scope of this study. While it was not possible to determine with much certainty the average amount saved by Green Light, the 95% credible intervals provide some estimate of the upper and lower bounds of cost savings that could be plausibly expected. A cost savings or cost increase of approximately \$6 million represents a small, but not insignificant portion of the city's \$400 million annual public safety budget (Budget in Brief – City of Detroit, 2018).

CONCLUSION AND DISCUSSION

Results

Interpreted as a whole, the results of this study provide mixed support that the Green Light program was responsible for a decrease in crime among the first 86 participating locations. Several items are obvious from a cursory review. First, crime *did* decrease among Green Light businesses between 2015 and 2017. However, after comparing these businesses to a matched sample of very similar businesses (who could have just as likely been enrolled in Green Light), these decreases were often not substantially different for violent crimes and varied considerably for property crimes and disorder crimes. While a year-over-year comparison indicated that disorder crimes were about 22% lower than the matched comparison group in 2017, this finding was not highly robust. Similarly, the estimated 12% decrease in property crimes among Green Light businesses was also tenuous. After adjusting for seasonal effects, time trends, and other sources of variability, the statistical models indicated that the change in crime between Green Light businesses and the matched controls were likely small, and highly variable. On a month-by-month basis, the greatest impact of Green Light was observed near the end of the study period. In Q4 2017, the three-month period with the largest estimated decrease in disorder crimes, there was a 50% probability that this decrease was between -54% and -13%, while there was a 95% probability that it was between -73% and 53%. Among property crimes, there was a 95% probability that the decrease was between -51% and 45% and a 50% probability that it was between -29% and 3%. In absolute numbers the models estimated about 12 fewer property crime incidents and 25 fewer disorder crime incidents relative to the matched controls in 2017. These results are consistent with a very similar study by McLean, Worden, and Kim (2013), that found a loose network of CCTV cameras had modest, but inconsistent effects on disorder crime – although they did not utilize a matched comparison group.

Estimates of Green Light's effects were moderately sensitive to the size of the buffer chosen around the business. While the results were relatively similar to the primary analysis utilizing the 150-foot buffer, when the analysis was constrained to within 100 feet of the business (essentially, incidents that occurred directly on the property), the estimated effect of Green Light was more optimistic. Property crimes were estimated to have decreased by -60%, post-Green Light, while disorder crimes decreased by -40%. When compared to the matched controls, this post-intervention decrease represented a -41% (95% CI: -86%, 31%) change in property crimes and -35% (95% CI: -100%, 121%) change in disorder crimes for 2017. However, even under these more optimistic estimates, the uncertainty intervals were still quite large when comparing Green Light businesses to the matched controls in the aggregate - especially among disorder crimes (where the uncertainty bounds essentially cover -100% to +121%). However, in none of these model specifications were violent crimes substantially or consistently affected by the implementation of Green Light.

The most consistent and stable estimates were related to the number of calls for service at Green Light businesses compared to the matched controls. The statistical model estimated an immediate *increase* in the number of reported calls for service the month Green Light began, and during the subsequent year after implementation. On average, businesses enrolled in Green Light increased their calls for service by about 23% immediately after enrollment, then by about an additional 7% the year after. In practical terms, this meant a business averaging 10 calls for service a month would have reported 12 incidents the month of implementation, and then averaged about 13 incidents a month the year following. In the aggregate this equated to about

502 (95% CI: -308.5, 1365.3) additional calls for service in 2017 as a whole. Therefore, the Green Light program may have encouraged businesses owners to bring to police attention minor crime incidents and other problems that would have not normally been formally identified. However, these estimates were somewhat variable, and included zero in the 95% credible interval.

In the most general terms, this study provides some evidence that the Green Light program was responsible for a modest decrease in property crimes and disorder crimes. These decreases were most consistently observed when the study was constrained to within 100 feet of businesses, and during the final three months of the study (Q4, 2017). There was stronger, more consistent evidence that Green Light increased utilization of police services through more reported property crimes the month of implementation, and a substantial increase in calls for service. This study was not able to find strong evidence that the Green Light program decreased the incidence of serious violent crime, however. The effect of the Green Light program was not consistent across all businesses either. Many businesses that reported few crimes or calls for service prior to implementation continued to observe relatively low counts. On the other hand, some establishments observed very large increases in calls for service, and others observed large decreases in reported property or disorder crimes. Translating these estimates into policy decisions is rendered very difficult due to the high variability of crime incidents within-businesses and between-businesses. This variability was evident in both the varying effects of the statistical model and by an examination of the distribution of crimes by business. Even among those businesses with the highest and most consistent number of crime incidents, the effect of the Green Light program was not consistent. Several businesses saw immediate and significant decreases post-Green Light, while others observed no change, or even slightly increased. The wide variation in this effect has some practical concerns when it comes to statistical estimation. Primarily, the credible intervals around the estimates of Green Light and Green Light post-intervention was large relative to the size of the effect, especially among disorder crimes and violent crimes – making it difficult to speak with a high degree of confidence about the extent of the change. However, this does not mean the Green Light program had *no* effect. Rather, given the variability of the change in crime, and the variability of crime incidents, estimating the actual effect with a low degree of uncertainty was not possible.

One question this study was not able to answer was whether the installation of Green Light cameras increased the detection and willingness of business owners to report minor crime incidents – and thereby masking a decrease in crime incidents. There exists considerable evidence crime is generally underreported, and often includes minor property crimes resulting in little financial loss (Skogan, 1977). For instance, a BJS report estimated that about 60% of property crime victimizations were not reported to police, while 50% of simple assaults and 40% of serious violent crime were not reported (Langton et al., 2012). If these patterns were expected to hold in Detroit, it is possible that business owners may have been more likely to report property, violent, and disorder crime incidents that would have not previously been officially recorded. At the very least, it is evident that business owners utilized DPD services at a rate higher than other similar businesses after joining the Green Light program, evidenced by the increase in calls for service. Given the deterrent effect of CCTV cameras on crime is estimated to be relatively modest (between -7% and -24%) and often contextual in nature, increases in reporting and improved detection may have "washed-out" any detectable crime decreases.

Practical Implications

The results of this study have some clear implications to the use of integrated CCTV camera systems as a crime-prevention tool. Perhaps most evident from this study, it is obvious that directly connecting business owners with the police is likely to increase service utilization – especially for less serious crimes and minor complaints. Among Green Light businesses the number of calls for service and reports of property and disorder crime incidents increased following the installation of cameras. In particular, gas stations and liquor stores accounted for much of this increase. Minor crime incidents (for instance, shoplifting or vandalism) that previously may not have been reported were more likely to come to the attention of police. Indeed, there is prior evidence that business owners in Detroit were often encouraged *not* to contact police for minor crime incidents (Crichlow & McGarrell, 2015). Equipped with surveillance cameras and a promise from the city to respond quickly to calls for service, business owners may have been emboldened and increased their utilization of police services. In this case, the city of Detroit may have been successful in its effort to improve police responsiveness to businesses and strengthen business-owner relationships with the police. While this study did not address issues such as citizen fear of crime or business-owner satisfaction with the police these remain highly relevant concerns for large, urban police agencies.

On the other hand, this study provides inconclusive evidence that the Green Light program reduced the number of violent crime incidents at participating businesses. After accounting for city-wide trends in violent crime and other confounding factors, businesses enrolled in Green Light observed, on average, no change relative to the matched controls. While the number of serious violent incidents did decrease during this period (by about -8.5%), these changes could have plausibly been attributed to ongoing reductions in city-wide violence.

Despite the city of Detroit anticipating Green Light as an efficacious method of reducing on-premise violent crime, there is little prior evidence that CCTV programs consistently affect the incidence of violent crimes (Welsh & Farrington, 2009; Piza et. al., 2015). From the standpoint that CCTV cameras might deter individuals who are motivated to commit a crime, there is not strong evidence that it would stop individuals motivated by short-term anger or aggression. Here, the results are mostly consistent with other evaluations of large-scale CCTV programs finding most of the positive effects on property crimes rather than violent crimes (Farrington et. al., 2007; Welsh & Farrington, 2009; Piza, 2016).

From a pragmatic standpoint, it is difficult to determine the cost-effectiveness of the Green Light program based on the results in this study. In the aggregate, the effect of Green Light was highly variable, and a simple calculation of cost-benefit indicated that, conditional on the specified costs, the program would have cost the city roughly an additional $8000. In practical terms the Green Light program may have been effectively cost-neutral, and likely would have, *at most,* saved or cost the city about $6 million. The relatively high degree of uncertainty in estimating the effect of Green Light on crime makes this estimation unstable because most of the cost-savings are derived from decreases in crime. Other factors, like additional calls for service, even in the most extreme estimates would have not substantially changed the cost-benefit ratio of the program. A more pertinent concern would be ensuring staffing levels to deal with the additional calls for service – rather than the marginal increased cost. At the very least this study finds that most of the cost of an integrated CCTV camera scheme like Green Light would be derived from start-up costs and staffing rather than responding to an increase in calls for service.

This study also found that the distribution of crime incidents was disproportionately concentrated at a small number of very high-rate businesses, while many other businesses experienced few or no crime incidents. This concentration was especially apparent among violent crimes and disorder crimes, where five Green Light businesses accounted for between 24% and 30% of incidents, respectively. From a cost-benefit perspective, this suggests that a more targeted focus on businesses experiencing disproportionately high numbers of violent crime incidents would be most efficacious. Prior research finds that targeted police interventions at crime hot spots can often produce large decreases in violent crime (Braga & Bond, 2008; Rosenfeld, Deckard, & Blackburn, 2014). A very broad approach to reducing violent crime (such as Green Light) is likely a less cost-effective method given the concentration of violent incidents at few locations. In addition, expanding Green Light to more than 400 businesses (City of Detroit, 2017) may make it cost-prohibitive to actively monitor the camera feeds. While Green Light may have moderately reduced property crimes within the immediate vicinity of the businesses, the estimated cost-savings of these minor crimes may have not been enough to balance the additional cost of subsequent officer calls for service. Vastly increasing the network of cameras may also present logistical issues related to crime detection. Piza, Caplan & Kennedy (2014) found that crime detections were reduced as the camera system grew in size. Therefore, if crime prevention and deterrence are the primary desired outcomes, a more parsimonious monitoring scheme may be more effective - especially if they are concentrated at active crime hot spots.

Detroit Green Light was developed as a police-community partnership, and in that respect, it may have succeeded in connecting business owners to the police and improving the reporting of crime in public places. Strengthening citizen-police relationships remain an important tool in managing crime, and these kinds of partnerships can improve collective efficacy (Nix et. al., 2015; Kochel, 2012). The voluntary nature of Green Light makes it of interest in practical terms as well. The program was compelling enough that participating businesses were willing to spend upwards of $5,000 to $7,000 of their own money to adopt the Green Light cameras and branding. No other program has ever been performed on this scale, which makes its implementation a unique case study. For other cities considering similar schemes, partnerships like these may provide a way to offset the significant costs of implementation.

Theoretical Implications

The findings from this study may hold some limited implications for the general body of research on CCTV cameras and its theoretical links to deterrence and rational choice theory. Prior research regarding deterrence theory indicated that offenders evaluate a limited set of choices before committing a crime - such as weighing the likelihood of being caught against the expected value of the crime (Matsueda, Kreager, & Huizinga, 2006). Consistent with deterrence theory, the presence of CCTV cameras may have been responsible for a modest decrease in property or of disorder crimes – however this effect varied considerably based on the buffer size, and differing implementation between businesses. A modest decrease in minor crimes would be consistent with a routine activities framework. Changing the context of risky places by improving guardianship (here, monitored CCTV cameras and improved lighting), motivated offenders may be deterred from committing crime or may have limited the ability of offenders to commit crimes. Improved lighting, camera monitoring, and police response to calls for service may have reduced the opportunities available to offenders and increased the perceived risks. Therefore, based on the results of this study – which also incorporate prior studies - CCTV cameras may marginally fill a role as a capable guardian. The extent to which CCTV cameras might function as capable guardians may be limited, however. Currently, there exists little evidence that CCTV cameras are an effective deterrent against serious violent crime. In this study the real (or perceived) risk of detection by Green Light cameras did not substantially deter offenders from serious violent offenses. In a theoretical perspective this is not entirely at odds with routine activities or deterrence theory.

While some studies have provided evidence that increased certainty of apprehension may decrease the self-reported incidence of violent offending (Wright, Caspi, & Moffitt, 2004), most interventions designed to deter violent activity are focused on individuals, rather than places (see: Braga et. al., 2001). Individual-based interventions are expected to increase the perceived risk of apprehension which likely is a more effective form of deterrence than the harshness of punishment (Nagin, 2013). This generally fits with the evaluation literature of CCTV cameras – finding that most programs produce small decreases in property offenses and other minor crimes, but generally do not affect serious violent crime. In short, CCTV camera evaluation studies find generally positive (but limited) effects on crime. The effectiveness of this deterrence is likely tied to the extent to which the perceived risk of apprehension is internalized by offenders. Even if the risk of apprehension is increased in reality, crime will only decrease if offenders believe it. At a minimum, this study provides a modicum of evidence toward the deterrence perspective.

Limitations and Future Inquiries

The scope of this study was narrowly focused on a few key questions. Primarily, this study was concerned with whether the initial implementation of Green Light had a consistent effect on the number of reported crime incidents and calls for service. However, what this study did *not* consider is also meaningful. While Green Light was initially advertised as a crime deterrent, it was later billed as a method of solving crimes, improving police response, and reducing citizen fear of crime. Whether or not the Green Light program positively changed attitudes toward police is one outcome highly relevant for the city of Detroit. Improving police response to calls for service and working directly with business owners might logically be expected to increase perceptions of police legitimacy (Skogan, 2005). Furthermore, working with the community might also be responsible for an increase in collective efficacy and a reduction in fear of crime (Circo, Melde, & McGarrell, 2018; Kochel, 2012). Second, this study did not consider whether crime incidents captured on camera were more likely to result in case clearance. This question remains especially important, as one of the largest potential benefits of Green Light lie in its surveillance capabilities. In Detroit, many victims and witnesses in shooting incidents are often reluctant to provide information to the police. An analysis of incidents by an MSU research team found about 40% of non-fatal shooting cases remained inactive because of lack of witnesses. Here, CCTV camera footage may provide additional investigative leads to cases where victims or witnesses are not forthcoming. Increased investigate abilities due to surveillance footage may actually increase the perceived risk of committing crime at Green Light businesses.

A future evaluation of the Green Light program should consider these important questions. Other cities have found that police working in concert with citizens at crime hot spots can effectively reduce crime and increase perceptions toward the police (Wells, Schafer, & Varano, 2006; Nix et. al., 2015). It is likely these findings might be generalizable to Detroit as well. Because targeted policing has been shown as an effective method of reducing crime at hot spots, pairing the Green Light camera infrastructure with a problem-oriented policing strategy may boost the deterrent effects. Currently, no studies have examined whether CCTV cameras combined with problem-oriented policing can reduce crime more effectively than just CCTV or problem-oriented policing alone. This question may be highly relevant as the adoption of widespread CCTV camera schemes increase throughout the nation.

There was substantial variation in the number of crime incidents between businesses, making estimation of treatment effects more difficult. Future research in this vein should consider more precise measures of crime and utilizing proxy variables for criminal activity at businesses. This may help account for simultaneous increases in crime detection and decreases due to deterrence. Because this study examined only the first phase of Green Light (the initial 2016 implementation of 86 businesses) future evaluations will need to consider these results as the number of businesses grows. Indeed, by the end of 2017 there were nearly 280 businesses enrolled in Green Light – with this number expected to increase to over 400 by 2019 (City of Detroit – Project Green Light Detroit, 2017). As the number of businesses enrolled continues to grow, more stable estimates will able to be estimated due to the increase sample size and amount of time passed. Further evaluations of the program will be able to determine whether the patterns observed in the first phase generalize to the wider implementation.

APPENDICES

APPENDICES

APPENDIX A: Green Light Signage and Camera Mount

APPENDIX B: Green Light Businesses, Business Types, and Live Date

Study Id	Business Type	Police Precinct	Live Date	Study Id	Business Type	Police Precinct	Live Date
1	GAS	2	01/01/16	45	GAS	8	10/13/16
2	GAS	5	01/01/16	46	GAS	8	10/18/16
3	GAS	6	01/01/16	47	CONV	8	10/26/16
4	GAS	7	01/01/16	48	LIQUOR	9	10/28/16
5	GAS	8	01/01/16	49	CONV	6	11/01/16
6	GAS	9	01/01/16	50	GAS	9	11/02/16
7	GAS	9	01/01/16	51	CONV	9	11/03/16
8	GAS	12	01/01/16	52	GAS	9	11/03/16
9	LIQUOR	5	03/03/16	53	BAR	4	11/09/16
10	LIQUOR	8	03/03/16	54	OTHER	4	11/09/16
11	FOOD	8	03/03/16	55	FOOD	4	11/09/16
12	GAS	4	03/21/16	56	GAS	2	11/10/16
13	FOOD	4	03/21/16	57	LIQUOR	10	11/11/16
14	GAS	9	04/12/16	58	FOOD	12	11/15/16
15	CONV	9	04/12/16	59	OTHER	5	11/17/16
16	FOOD	8	04/18/16	60	CONV	4	11/18/16
17	GAS	10	04/18/16	61	FOOD	12	11/21/16
18	GAS	7	04/25/16	62	GAS	7	11/22/16
19	GAS	8	04/25/16	63	GAS	8	11/29/16
20	GAS	5	04/27/16	64	LIQUOR	5	12/01/16
21	FOOD	7	04/28/16	65	FOOD	6	12/01/16
22	LIQUOR	5	05/03/16	66	CONV	11	12/01/16
23	LIQUOR	7	05/10/16	67	FOOD	11	12/01/16
24	BAR	6	05/14/16	68	LIQUOR	11	12/01/16
25	CONV	12	05/14/16	69	CONV	12	12/02/16
26	FOOD	3	05/23/16	70	BAR	12	12/06/16
27	LIQUOR	7	05/27/16	71	FOOD	2	12/07/16
28	FOOD	8	05/27/16	72	LIQUOR	2	12/07/16
29	FOOD	4	06/04/16	73	GAS	3	12/08/16
30	LIQUOR	2	06/06/16	74	GAS	10	12/08/16
31	FOOD	2	06/06/16	75	GAS	11	12/09/16
32	GAS	3	06/21/16	76	FOOD	11	12/12/16
33	FOOD	4	06/28/16	77	LIQUOR	8	12/14/16
34	FOOD	5	07/12/16	78	FOOD	12	12/15/16
35	FOOD	8	07/12/16	79	OTHER	8	12/19/16
36	FOOD	7	07/23/16	80	OTHER	9	12/20/16
37	CONV	8	07/23/16	81	CONV	5	12/21/16
38	LIQUOR	10	07/27/16	82	CONV	5	12/21/16
39	GAS	2	07/30/16	83	GAS	8	12/27/16
40	CONV	12	08/01/16	84	BAR	10	12/27/16
41	GAS	8	08/24/16	85	CONV	5	12/28/16
42	GAS	8	08/26/16	86	OTHER	8	12/29/16
43	GAS	7	09/07/16				
44	GAS	8	09/28/16				

APPENDIX C: Estimated Costs

Costs to the City of Detroit (2017 Dollars)

Cost Type	Total Cost	Marginal Cost		
Green Light				
Initial Start Up	-			
Maintenance	$1,000/mo			
Personnel	$475,200			
Calls for Service				
Officer on-scene hours	$24.00/hr			
		Arrest	Court	Corrections
Violent Crime				
Homicide	$70,842.3	$20,429	$18,389	$32,025.2
Aggravated Assault	$37,922.4	$20,429	$10,017	$7,477.3
Robbery	$38,766.5	$20,429	$10,017	$8,321.4
Property Crime				
Larceny	$8,968.85	$3,076	$4,186	$1,706.6
Retail Fraud	$8,968.85	$3,076	$4,186	$1,706.6
Stolen Property	$8,283.45	$3,076	$4,186	$1,021.2
Stolen Vehicle	$14,534.85	$3,076	$10,017	$1,442.1
Damage to Property	$8,283.45	$3,076	$4,186	$1,021.2
Burglary	$15,894.15	$4,171	$10,017	$1,706.6
Disorder Crime				
Dangerous Drugs	$14,240.45	$3,076	$10,017	$1,147.7
Liquor	$8,283.45	$3,076	$4,186	$1,021.2
Gambling	$8,283.45	$3,076	$4,186	$1,021.2
Drunkenness	$8,283.45	$3,076	$4,186	$1,021.2
Disorderly Conduct	$8, 283.45	$3, 076	$4,186	$1,021.2

APPENDIX D: Model 1 - Property Crime (Weakly Informative Prior)

Variable	estimate	est error	2.50%	97.50%
Intercept	-1.00	0.58	-1.91	-0.08
Green Light	0.37	0.12	0.17	0.56
Green Light * Post-Intervention	-0.26	0.17	-0.52	0.02
Time	-0.07	0.05	-0.16	0.01
Time ^ 2	-0.05	0.04	-0.12	0.02
Month-of-Year Effects				
February	-0.20	0.10	-0.36	-0.03
March	-0.01	0.10	-0.17	0.14
April	0.03	0.09	-0.12	0.19
May	0.07	0.10	-0.09	0.23
June	-0.03	0.09	-0.19	0.11
July	0.05	0.10	-0.11	0.20
August	0.07	0.09	-0.08	0.22
September	0.13	0.09	-0.01	0.29
October	0.07	0.09	-0.09	0.22
November	0.05	0.10	-0.11	0.22
December	0.01	0.10	-0.15	0.17
Control Variables				
% Male	-0.88	0.63	-1.93	0.10
% Black	0.11	0.39	-0.51	0.73
% No HS Degree	-0.51	0.51	-1.32	0.30
% HH Poverty	-0.29	0.73	-1.45	0.91
% HH Income < $30k	-0.66	0.62	-1.62	0.39
% HH Rent > 30% Income	0.25	0.72	-0.95	1.43
% HH on Food Stamps	0.76	0.68	-0.36	1.92
% Female-Headed HH	0.10	0.39	-0.52	0.74
% Unemployed	0.40	0.71	-0.74	1.53
% Vacant HH	-0.25	0.54	-1.09	0.67
% HH Renting	0.44	0.58	-0.52	1.43
Property Crime Rate per 1000	0.00	0.11	-0.18	0.18
Violent Crime Rate per 1000	0.02	0.10	-0.14	0.20
Drug Arrest Rate per 1000	-0.07	0.97	-1.62	1.52
Demolition Rate per 1000	0.02	0.96	-1.48	1.61
Blighted Property per 1000	-0.08	0.94	-1.63	1.47
Business Type				
Bar & Food Establishments	-0.17	0.17	-0.46	0.09
Liquor Stores	-0.33	0.19	-0.62	0.01
Retail Shops	-0.41	0.22	-0.77	-0.07
Other	-0.24	0.30	-0.74	0.27

APPENDIX E: Model 2 Property Crime (Informative Prior)

Variable	estimate	est error	2.50%	97.50%
Intercept	-1.03	0.54	-1.86	-0.09
Green Light	0.34	0.12	0.14	0.53
Green Light * Post-Intervention	-0.23	0.16	-0.50	0.04
Time	-0.07	0.05	-0.15	0.02
Time ^ 2	-0.05	0.04	-0.12	0.02
Month-of-Year Effects				
February	-0.19	0.09	-0.34	-0.04
March	0.00	0.09	-0.15	0.15
April	0.04	0.09	-0.12	0.19
May	0.07	0.09	-0.08	0.21
June	-0.03	0.10	-0.19	0.12
July	0.05	0.09	-0.09	0.20
August	0.08	0.09	-0.07	0.23
September	0.13	0.09	-0.02	0.29
October	0.07	0.09	-0.08	0.22
November	0.05	0.09	-0.10	0.21
December	0.01	0.09	-0.14	0.17
Control Variables				
% Male	-0.91	0.64	-1.97	0.13
% Black	0.12	0.35	-0.42	0.70
% No HS Degree	-0.43	0.52	-1.28	0.40
% HH Poverty	-0.28	0.68	-1.42	0.88
% HH Income < $30k	-0.64	0.61	-1.67	0.36
% HH Rent > 30% Income	0.32	0.73	-0.82	1.55
% HH on Food Stamps	0.77	0.63	-0.28	1.74
% Female-Headed HH	0.07	0.39	-0.59	0.70
% Unemployed	0.40	0.71	-0.79	1.51
% Vacant HH	-0.27	0.49	-1.09	0.54
% HH Renting	0.39	0.56	-0.53	1.29
Property Crime Rate per 1000	0.01	0.11	-0.16	0.18
Violent Crime Rate per 1000	0.03	0.10	-0.14	0.21
Drug Arrest Rate per 1000	-0.13	0.99	-1.73	1.50
Demolition Rate per 1000	0.01	0.91	-1.48	1.47
Blighted Property per 1000	-0.13	0.94	-1.70	1.43
Business Type				
Bar & Food Establishments	-0.14	0.16	-0.41	0.12
Liquor Stores	-0.33	0.19	-0.63	-0.03
Retail Shops	-0.40	0.21	-0.75	-0.05
Other	-0.27	0.29	-0.76	0.19

APPENDIX F: Model 1 Violent Crime (Weakly Informative Prior)

Variable	estimate	est error	2.50%	97.50%
Intercept	-2.62	0.62	-3.66	-1.60
Green Light	0.15	0.19	-0.18	0.47
Green Light * Post-Intervention	-0.03	0.24	-0.41	0.37
Time	-0.11	0.07	-0.22	0.02
Time ^ 2	-0.12	0.07	-0.24	-0.01
Month-of-Year Effects				
February	-0.26	0.16	-0.53	0.00
March	-0.09	0.17	-0.36	0.19
April	-0.02	0.15	-0.27	0.22
May	0.21	0.14	-0.01	0.45
June	0.39	0.14	0.15	0.63
July	0.39	0.14	0.16	0.63
August	0.39	0.14	0.16	0.62
September	0.38	0.15	0.15	0.61
October	0.17	0.15	-0.07	0.41
November	0.07	0.16	-0.19	0.33
December	0.06	0.16	-0.21	0.34
Control Variables				
% Male	-0.61	0.69	-1.71	0.45
% Black	0.54	0.38	-0.10	1.19
% No HS Degree	-0.04	0.57	-1.00	0.87
% HH Poverty	-0.31	0.73	-1.52	0.89
% HH Income < $30k	0.07	0.67	-0.96	1.17
% HH Rent > 30% Income	0.22	0.76	-1.01	1.42
% HH on Food Stamps	0.04	0.72	-1.14	1.20
% Female-Headed HH	-0.19	0.45	-0.91	0.56
% Unemployed	0.14	0.70	-0.98	1.32
% Vacant HH	-0.38	0.54	-1.28	0.48
% HH Renting	0.57	0.60	-0.44	1.52
Property Crime Rate per 1000	0.07	0.12	-0.13	0.26
Violent Crime Rate per 1000	0.04	0.11	-0.15	0.23
Drug Arrest Rate per 1000	-0.12	0.97	-1.70	1.49
Demolition Rate per 1000	0.07	1.03	-1.55	1.71
Blighted Property per 1000	-0.22	0.98	-1.87	1.36
Business Type				
Bar & Food Establishments	-0.34	0.18	-0.63	-0.04
Liquor Stores	-0.50	0.22	-0.89	-0.17
Retail Shops	-0.45	0.24	-0.84	-0.05
Other	-0.38	0.34	-0.90	0.19

APPENDIX G: Model 2 Violent Crime (Informative Prior)

Variable	estimate	est error	2.50%	97.50%
Intercept	-2.66	0.63	-3.64	-1.59
Green Light	0.15	0.17	-0.13	0.42
Green Light * Post-Intervention	-0.05	0.20	-0.37	0.29
Time	-0.10	0.07	-0.22	0.02
Time ^ 2	-0.12	0.07	-0.23	-0.01
Month-of-Year Effects				
February	-0.25	0.16	-0.52	0.02
March	-0.07	0.16	-0.33	0.19
April	-0.01	0.15	-0.28	0.22
May	0.23	0.15	0.00	0.48
June	0.41	0.14	0.18	0.64
July	0.41	0.14	0.19	0.65
August	0.41	0.14	0.18	0.64
September	0.39	0.14	0.16	0.62
October	0.19	0.15	-0.07	0.42
November	0.09	0.16	-0.18	0.34
December	0.08	0.16	-0.18	0.34
Control Variables				
% Male	-0.58	0.68	-1.70	0.56
% Black	0.52	0.39	-0.12	1.14
% No HS Degree	-0.04	0.57	-0.98	0.85
% HH Poverty	-0.35	0.75	-1.57	0.87
% HH Income < $30k	0.06	0.66	-1.00	1.16
% HH Rent > 30% Income	0.28	0.74	-1.03	1.47
% HH on Food Stamps	0.04	0.68	-1.02	1.12
% Female-Headed HH	-0.17	0.44	-0.91	0.54
% Unemployed	0.20	0.73	-1.01	1.44
% Vacant HH	-0.38	0.55	-1.29	0.53
% HH Renting	0.60	0.61	-0.35	1.60
Property Crime Rate per 1000	0.05	0.12	-0.14	0.24
Violent Crime Rate per 1000	0.06	0.11	-0.11	0.25
Drug Arrest Rate per 1000	-0.12	0.97	-1.73	1.47
Demolition Rate per 1000	0.06	0.96	-1.54	1.65
Blighted Property per 1000	-0.22	0.96	-1.89	1.34
Business Type				
Bar & Food Establishments	-0.36	0.20	-0.68	-0.02
Liquor Stores	-0.50	0.21	-0.84	-0.16
Retail Shops	-0.44	0.25	-0.85	-0.04
Other	-0.39	0.32	-0.89	0.14

APPENDIX H: Model 1 Disorder Crime (Weakly Informative Prior)

Variable	estimate	est error	2.50%	97.50%
Intercept	0.34	0.74	-0.86	1.54
Green Light	0.21	0.09	0.06	0.37
Green Light * Post-Intervention	0.07	0.13	-0.14	0.27
Time	0.33	0.17	0.04	0.60
Time ^ 2	-0.11	0.08	-0.24	0.02
Month-of-Year Effects				
February	0.02	0.05	-0.07	0.10
March	0.18	0.05	0.10	0.27
April	0.20	0.05	0.11	0.28
May	0.33	0.05	0.24	0.42
June	0.36	0.05	0.28	0.44
July	0.35	0.05	0.27	0.44
August	0.36	0.05	0.27	0.44
September	0.30	0.05	0.22	0.39
October	0.25	0.05	0.16	0.34
November	0.22	0.05	0.14	0.31
December	0.09	0.06	0.00	0.19
Control Variables				
% Male	-0.57	0.75	-1.80	0.66
% Black	0.28	0.49	-0.53	1.04
% No HS Degree	-0.66	0.68	-1.80	0.45
% HH Poverty	0.01	0.82	-1.35	1.29
% HH Income < $30k	-0.18	0.77	-1.49	1.06
% HH Rent > 30% Income	0.38	0.81	-0.93	1.68
% HH on Food Stamps	0.04	0.74	-1.13	1.18
% Female-Headed HH	-0.25	0.52	-1.08	0.63
% Unemployed	-0.53	0.76	-1.75	0.74
% Vacant HH	-0.13	0.62	-1.12	0.86
% HH Renting	-0.16	0.67	-1.24	0.91
Property Crime Rate per 1000	0.07	0.16	-0.19	0.34
Violent Crime Rate per 1000	0.00	0.15	-0.25	0.24
Drug Arrest Rate per 1000	-0.12	1.00	-1.77	1.49
Demolition Rate per 1000	0.08	1.04	-1.65	1.77
Blighted Property per 1000	0.19	0.96	-1.29	1.77
Business Type				
Bar & Food Establishments	-0.08	0.24	-0.45	0.33
Liquor Stores	-0.09	0.25	-0.50	0.33
Retail Shops	-0.25	0.38	-0.87	0.38
Other	-0.59	0.28	-1.03	-0.12

APPENDIX I: Model 2 Disorder Crime (Informative Prior)

Variable	estimate	est error	2.50%	97.50%
Intercept	-3.19	0.68	-4.32	-2.08
Green Light	0.17	0.19	-0.16	0.47
Green Light * Post-Intervention	-0.22	0.23	-0.58	0.18
Time	-0.02	0.08	-0.15	0.12
Time ^ 2	-0.12	0.07	-0.23	-0.01
<u>Month-of-Year Effects</u>				
February	-0.14	0.15	-0.38	0.11
March	0.25	0.13	0.03	0.46
April	0.17	0.14	-0.07	0.40
May	0.10	0.14	-0.12	0.33
June	0.03	0.14	-0.20	0.25
July	0.00	0.15	-0.25	0.25
August	0.25	0.14	0.02	0.48
September	0.42	0.13	0.20	0.64
October	0.33	0.14	0.09	0.55
November	0.11	0.14	-0.13	0.33
December	0.04	0.15	-0.21	0.29
<u>Control Variables</u>				
% Male	-0.29	0.74	-1.53	0.91
% Black	0.52	0.41	-0.15	1.19
% No HS Degree	-0.46	0.60	-1.44	0.57
% HH Poverty	-0.03	0.74	-1.25	1.15
% HH Income < $30k	-0.11	0.67	-1.19	0.98
% HH Rent > 30% Income	0.36	0.78	-0.92	1.62
% HH on Food Stamps	0.45	0.70	-0.70	1.63
% Female-Headed HH	-0.01	0.44	-0.74	0.67
% Unemployed	-0.18	0.72	-1.41	1.05
% Vacant HH	0.45	0.57	-0.49	1.43
% HH Renting	0.58	0.60	-0.39	1.57
Property Crime Rate per 1000	0.08	0.13	-0.12	0.29
Violent Crime Rate per 1000	-0.01	0.12	-0.20	0.18
Drug Arrest Rate per 1000	-0.02	1.03	-1.80	1.71
Demolition Rate per 1000	0.15	0.98	-1.52	1.71
Blighted Property per 1000	-0.19	0.95	-1.70	1.35
<u>Business Type</u>				
Bar & Food Establishments	-0.17	0.19	-0.51	0.15
Liquor Stores	-0.19	0.22	-0.56	0.17
Retail Shops	-0.55	0.27	-1.00	-0.09
Other	-0.39	0.36	-0.96	0.20

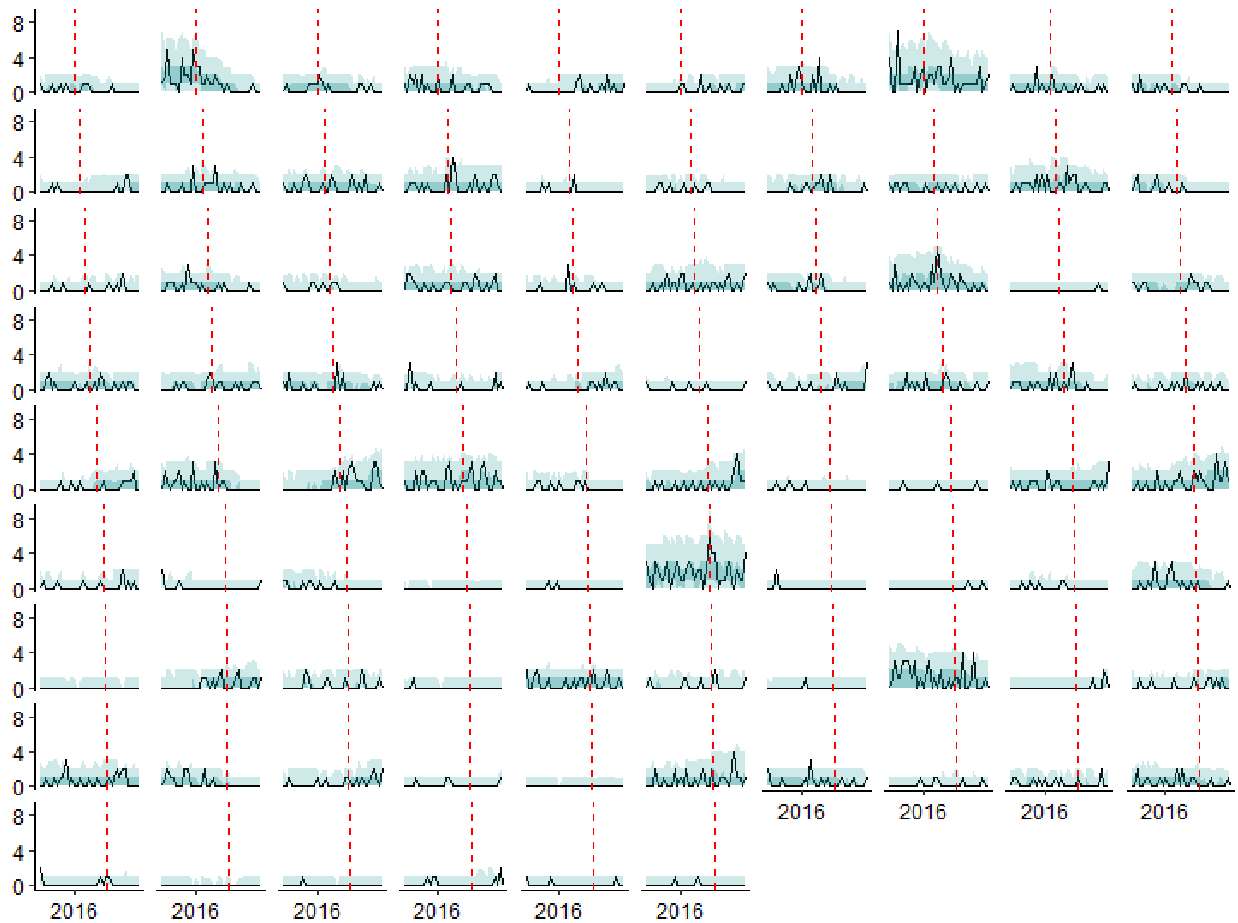
APPENDIX J: Model 1 Calls for Service (Weakly Informative Prior)

Variable	estimate	est error	2.50%	97.50%
Intercept	0.34	0.74	-0.86	1.54
Green Light	0.21	0.09	0.06	0.37
Green Light * Post-Intervention	0.07	0.13	-0.14	0.27
Time	0.33	0.17	0.04	0.60
Time ^ 2	-0.11	0.08	-0.24	0.02
Month-of-Year Effects				
February	0.02	0.05	-0.07	0.10
March	0.18	0.05	0.10	0.27
April	0.20	0.05	0.11	0.28
May	0.33	0.05	0.24	0.42
June	0.36	0.05	0.28	0.44
July	0.35	0.05	0.27	0.44
August	0.36	0.05	0.27	0.44
September	0.30	0.05	0.22	0.39
October	0.25	0.05	0.16	0.34
November	0.22	0.05	0.14	0.31
December	0.09	0.06	0.00	0.19
Control Variables				
% Male	-0.57	0.75	-1.80	0.66
% Black	0.28	0.49	-0.53	1.04
% No HS Degree	-0.66	0.68	-1.80	0.45
% HH Poverty	0.01	0.82	-1.35	1.29
% HH Income < $30k	-0.18	0.77	-1.49	1.06
% HH Rent > 30% Income	0.38	0.81	-0.93	1.68
% HH on Food Stamps	0.04	0.74	-1.13	1.18
% Female-Headed HH	-0.25	0.52	-1.08	0.63
% Unemployed	-0.53	0.76	-1.75	0.74
% Vacant HH	-0.13	0.62	-1.12	0.86
% HH Renting	-0.16	0.67	-1.24	0.91
Property Crime Rate per 1000	0.07	0.16	-0.19	0.34
Violent Crime Rate per 1000	0.00	0.15	-0.25	0.24
Drug Arrest Rate per 1000	-0.12	1.00	-1.77	1.49
Demolition Rate per 1000	0.08	1.04	-1.65	1.77
Blighted Property per 1000	0.19	0.96	-1.29	1.77
Business Type				
Bar & Food Establishments	-0.08	0.24	-0.45	0.33
Liquor Stores	-0.09	0.25	-0.50	0.33
Retail Shops	-0.25	0.38	-0.87	0.38
Other	-0.59	0.28	-1.03	-0.12

APPENDIX K: Model 2 Calls for Service (Informative Prior)

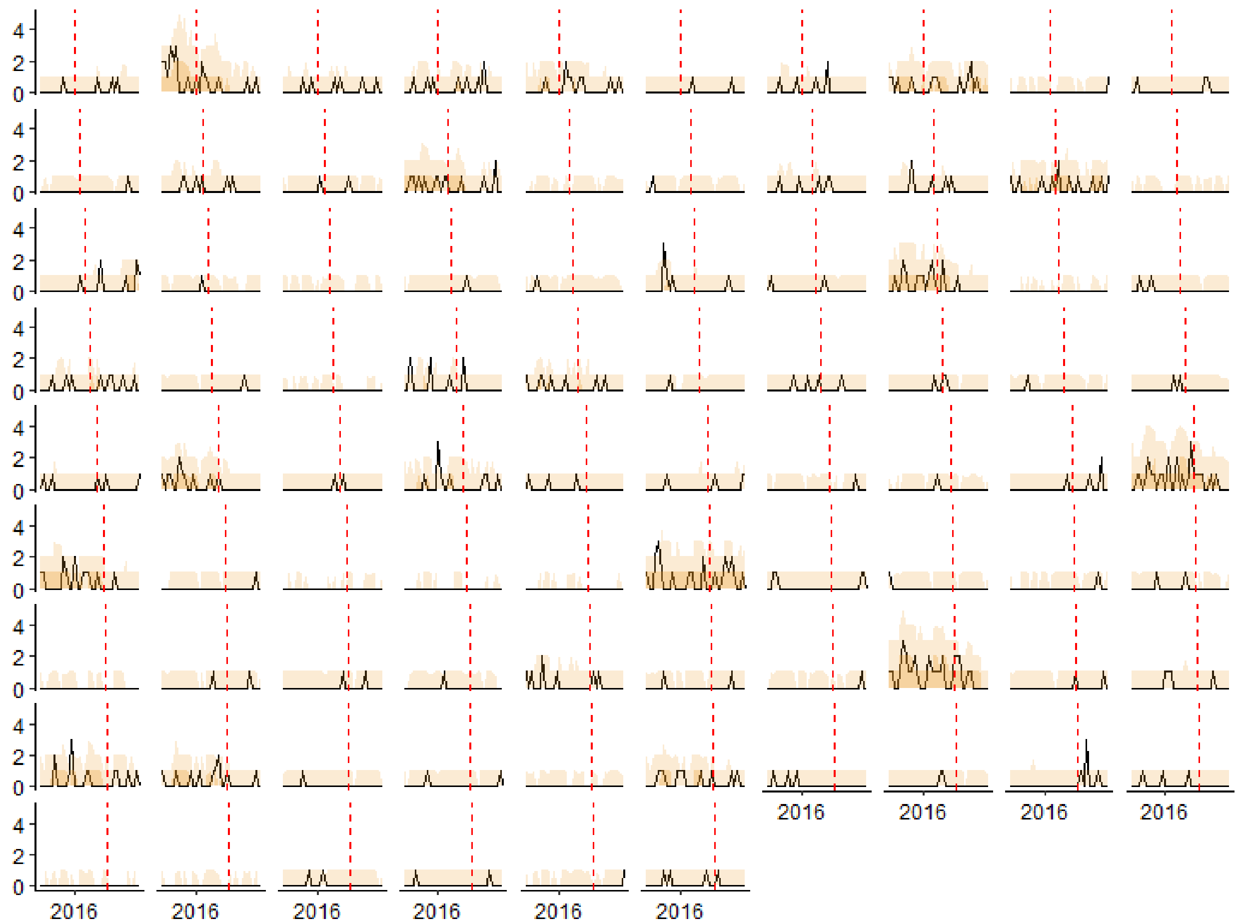
Variable	estimate	est error	2.50%	97.50%
Intercept	0.32	0.68	-0.73	1.42
Green Light	0.20	0.09	0.05	0.34
Green Light * Post-Intervention	0.07	0.12	-0.14	0.28
Time	0.35	0.17	0.08	0.64
Time ^ 2	-0.12	0.08	-0.25	0.01
Month-of-Year Effects				
February	0.02	0.06	-0.08	0.11
March	0.18	0.05	0.10	0.26
April	0.19	0.06	0.10	0.28
May	0.33	0.05	0.24	0.41
June	0.35	0.05	0.27	0.44
July	0.35	0.06	0.26	0.44
August	0.36	0.06	0.26	0.45
September	0.30	0.06	0.21	0.39
October	0.25	0.06	0.15	0.34
November	0.22	0.06	0.12	0.31
December	0.09	0.06	-0.01	0.19
Control Variables				
% Male	-0.47	0.75	-1.72	0.76
% Black	0.22	0.43	-0.47	0.91
% No HS Degree	-0.59	0.62	-1.62	0.43
% HH Poverty	-0.01	0.79	-1.28	1.31
% HH Income < $30k	-0.08	0.68	-1.22	1.12
% HH Rent > 30% Income	0.34	0.82	-0.96	1.70
% HH on Food Stamps	0.03	0.73	-1.17	1.22
% Female-Headed HH	-0.27	0.51	-1.10	0.60
% Unemployed	-0.49	0.76	-1.74	0.74
% Vacant HH	-0.22	0.68	-1.36	0.88
% HH Renting	-0.18	0.68	-1.27	0.94
Property Crime Rate per 1000	0.09	0.15	-0.15	0.33
Violent Crime Rate per 1000	0.00	0.13	-0.22	0.21
Drug Arrest Rate per 1000	-0.11	0.91	-1.63	1.44
Demolition Rate per 1000	0.07	0.95	-1.50	1.65
Blighted Property per 1000	0.24	0.91	-1.12	1.75
Business Type				
Bar & Food Establishments	-0.05	0.24	-0.44	0.32
Liquor Stores	-0.09	0.26	-0.52	0.35
Retail Shops	-0.26	0.40	-0.88	0.44
Other	-0.56	0.30	-1.08	-0.07

APPENDIX L: Property Crime Incidents by Individual Green Light Businesses



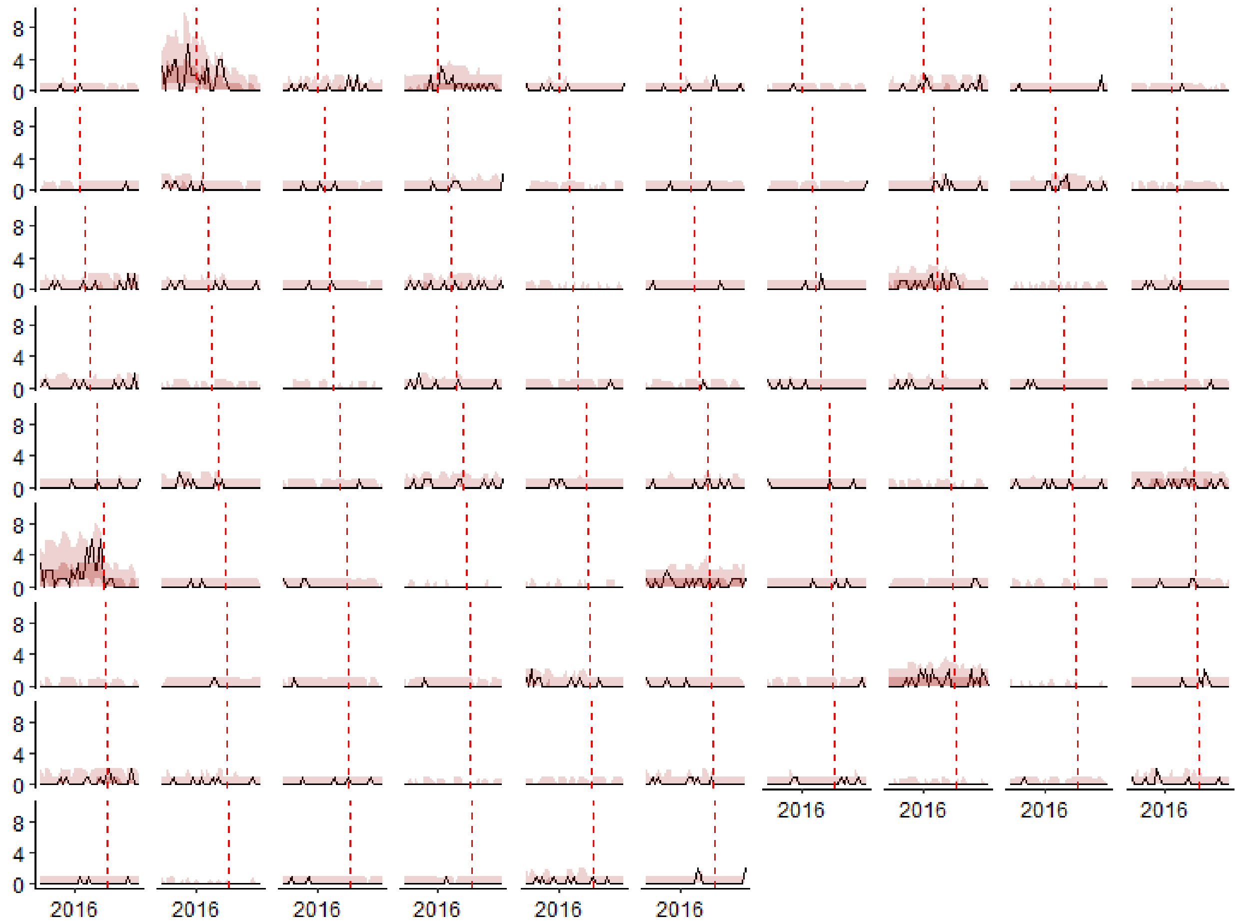
The observed number of property crime incidents in black lines, for each of the 86 Green Light businesses. The 95% and 50% intervals from the fitted model are highlighted in blue. The vertical red line corresponds to the date that the business began Green Light.

APPENDIX M: Violent Crime Incidents by Individual Green Light Businesses



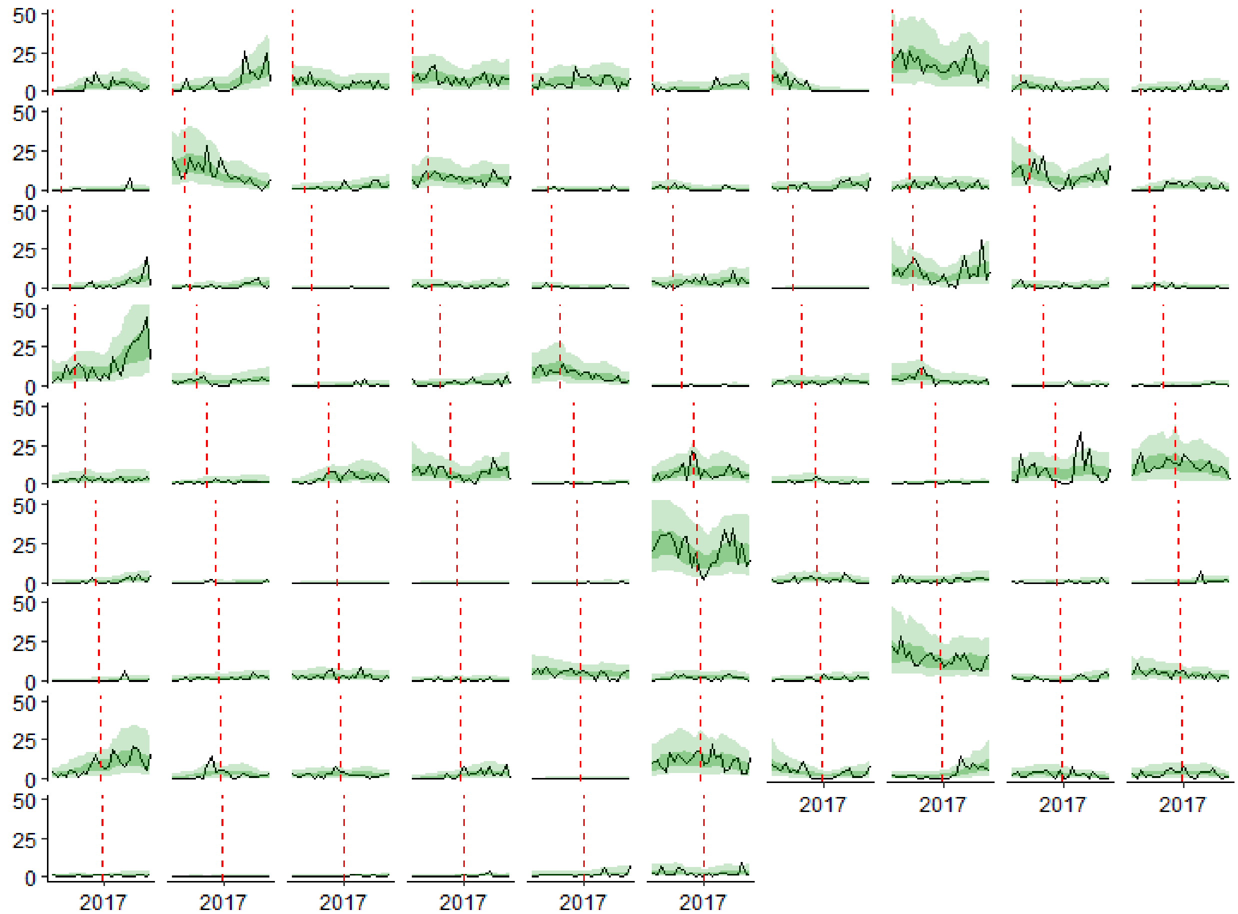
The observed number of violent crime incidents in black lines, for each of the 86 Green Light businesses. The 95% and 50% intervals from the fitted model are highlighted in yellow. The vertical red line corresponds to the date that the business began Green Light.

APPENDIX N: Disorder Crime Incidents by Individual Green Light Businesses



The observed number of disorder crime incidents in black lines, for each of the 86 Green Light businesses. The 95% and 50% intervals from the fitted model are highlighted in yellow. The vertical red line corresponds to the date that the business began Green Light.

APPENDIX O: Calls for Service by Individual Green Light Businesses



The observed number of calls for service in black lines, for each of the 86 Green Light businesses. The 95% and 50% intervals from the fitted model are highlighted in green. The vertical red line corresponds to the date that the business began Green Light.

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