Street Light Outages, Public Safety and Crime Displacement: Evidence from Chicago *

Aaron Chalfin
University of Pennsylvania

Jacob Kaplan
University of Pennsylvania

Michael LaForest
University of Chicago

Abstract

For more than one hundred years, street lighting has been one of the most enduring capital investments to maintain public safety. In this study we provide a comprehensive examination of the effect of street lights on crime, by estimating the effect of nearly 300,000 street light outages in Chicago neighborhoods on crime. We find that outdoor nighttime crimes change very little on street segments affected by street light outages, but that crime appears to spillover to nearby street segments during these outages. These findings suggest that crime may follow patterns of human activity and that the impact of localized street light outages can reverberate throughout a community.

Keywords: Street lights, Crime Displacement, Place-based interventions

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1 Introduction

When crime peaked in the United States in the early 1990s, national policymakers responded by strengthening criminal sanctions and by allocating billions of dollars to cities to promote the hiring of more police officers.[1] States followed suit, passing "three strikes" and "truth-in-sentencing" laws, expanding the use of mandatory minimum sentences and increasing criminal sanctions (Pfaff, 2017). Municipal police departments, inspired by the idea of "broken windows" policing, invested in a more proactive approach to order-maintenance, increasing their use of street stops and the number of arrests made and summonses issued for misdemeanor crimes. While the role of enhanced sanctions in explaining the great crime decline of the 1990s is thought to be relatively small (Johnson and Raphael, 2012; Nagin, 2013) there is a strong consensus that the number of police officers (Marvell and Moody, 1996; Evans and Owens, 2007; Chalfin and McCrary, 2017; Weisburst, 2018; Chalfin and McCrary, 2018; Mello, 2019; Kaplan and Chalfin, 2019) combined with their presence and visibility (Sherman and Weisburd, 1995; Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Braga and Bond, 2008; MacDonald et al., 2016; Weisburd et al., 2016) reduces crime — at least to a modest degree.

The importance of police presence in controlling crime remains relatively uncontroversial. However, the strategies that the police deploy, including directed patrol and excessive use of field interrogations may create high collateral cost for disadvantaged communities (Weitzer et al., 2008). In recent years, the public at large has taken note of these issues. In the context of increased media coverage of police shootings and political movements like *Black Lives Matter*, public support for police has fallen to its lowest levels in twenty years despite the dramatic decline in crime over this time period.[2]

Because of these concerns and others, cities are searching for novel strategies to reduce crime and minimize

[1] The Violent Crime Control and Law Enforcement Act of 1994 was the largest crime bill in U.S. history, ultimately allocating nearly $30 billion to cities to hire additional police officers and $10 billion to fund prison construction.

[2] http://www.gallup.com/poll/183704/confidence-police-lowest-years.aspx

the unintended costs of traditional crime prevention approaches. One of the approaches in which cities have expressed renewed interest is changing the nature of public space, an approach which is informed by situational crime prevention and its theoretical antecedent, crime prevention through environmental design (CPTED) which seeks to leverage changes to the built environment to influence criminal decision-making (Jeffery, 1971; Newman, 1972; Clarke, 1995; Cozens et al., 2005; Robinson, 2013; Cozens and Love, 2015). Such an approach has the potentially attractive quality of circumventing the criminal justice system and relying on managerial and environmental changes that make offending less viable without the need for greater enforcement (Clarke, 1980, 2009). Recent research has shown that CPTED-inspired strategies such as increasing the availability of trees and green space (Branas et al., 2011; Bogar and Beyer, 2016; Kondo et al., 2016), reducing the presence of litter and graffiti (Braga and Bond, 2008; Keizer et al., 2008) and securing abandoned buildings and reducing blight (Branas et al., 2018) can lead to important reductions in crime and disorder (MacDonald et al., 2019).

Street lights are widely thought to be an effective tool in reducing crime and therefore have become an ubiquitous type of investment in environmental design (Farrington and Welsh, 2002; Welsh and Farrington, 2008). Research in criminology, public health and urban planning suggests that improvements in lighting are welcomed by residents and tend to reduce fear of crime and improve perceptions of community safety (Painter, 1996; Atkins et al., 1991; Herbert and Davidson, 1994).[3] The available evidence on street lighting suggests that the evidence of its impact on crime prevention is promising, reducing crime by on average 20 percent (Welsh and Farrington, 2008). However, with one recent exception, a field experiment conducted in New York City by Chalfin et al. (2019), the evidence is based on observational studies that rely on pre-post designs of community-

[3]Inadequate street lighting may also contribute to concerns over safety. In a recent survey conducted by the New York City Mayor's Office of Criminal Justice, many public housing residents expressed not feeling safe walking around their communities, especially after dark. Survey responses suggest that while 50 percent of residents feel safe walking around their community during the daytime, just 21 percent feel safe doing so at night. With respect to community sentiment towards lighting, among the approximately 15 million complaints reported to NYC's 311 system from 2010 to 2016, nearly 5 percent related to street light conditions, the third most common complaint type out of a universe of more than 300 complaint types. Among the complaints related to a street lighting condition, nearly 80 percent of complaints related to a "street light outage" (Chalfin et al., 2019).

level interventions or relatively simple comparisons of small samples.[4] As a result, despite a number of positive research findings, over the last two decades, the promise of street lighting to control crime has been a topic of considerable debate with scholars such as Marchant (2004) and Doleac and Sanders (2015) suggesting that past research may be biased due to secular trends in crime, regression to the mean, and, critically, the strategic placement of street lights by city planners. Based on these concerns, a 1997 National Institute of Justice report to the U.S. Congress concludes that "we can have very little confidence that improved lighting prevents crime."

In this paper, we rely on a natural experiment that is uniquely suited to identify the effectiveness of municipal investments in street lighting in controlling crime. We leverage the fact that publicly owned street lights, on occasion, become non-operational and must be fixed by municipal workers. Street light outages are sometimes addressed immediately, but often outages take several days or even weeks to fix. During the time that a street light is non-operational, the amount of nighttime ambient light on a particular street segment is substantially reduced and generates a credible natural experiment to examine the short-term impact of changes in ambient lighting on criminal activity.

We use data on nearly 300,000 lighting outages in Chicago to study what happens to crime on street segments when street lights are non-operational.[5] The volume of data generated by this natural experiment is enormous, as there are nearly 50,000 street segments that experience at least one outage over the eight year period that we study (2,808 days) in Chicago. As a result, we are able to rely on nearly 140 million observations to estimate the impact of street lights on crime. Leveraging "big data" enables us to estimate the effectiveness of lighting in controlling crime with considerable precision — far greater than can be found

[4] While the meta-analysis by Welsh and Farrington, referenced above, describes prior research as using "experimental" and "control" groups, these studies did not control treatment assignment via randomization. They are best described as non-experimental studies with a treatment group and a naturally available or matched comparison group.

[5] The largest city in the United States, New York City, has public data on street light outages but repair dates often occur prior to report dates and, as such, we regard these data as insufficiently reliable. Los Angeles, the second largest city, has relatively few years of crime and 311 call data available, leading to large standard errors and imprecise estimates.

in the extant literature. We also have a sufficient number of observations to identify crime displacement, a topic of considerable interest in the prior literature but which is often addressed by studies that, due to small sample sizes, lack sufficient statistical power to draw strong inferences (Johnson et al., 2014).

In order for this study to provide an unbiased estimate of the impact of street light outages on crime, the timing of outages must be conditionally random. A key threat to an unbiased test of street lights on crime in this context is that outages typically only become known to municipal authorities when they are reported by community residents. To the extent that there is a delay between a street light outage and its official report date, our natural experiment may be compromised by measurement error in the treatment variable. To address this issue, we focus on a discrete period of time just prior to and after an outage is repaired since the repair date is well-documented. In other words, we investigate whether crime is lower just after a light is fixed by municipal workers relative to the period just before. By focusing on a discrete time window around the repair date, this design also has the virtue controlling for community-specific time trends that are difficult to fully control for with a traditional fixed-effects regression design.[6] Our research design is thus closely related to a regression discontinuity design in time, in that it relies on the sudden change in crime that occurs at the time of the street light repair.

We find evidence that outdoor nighttime crimes are sensitive to street light conditions. During the period of time when multiple street lights on a given street segment are out, there is little evidence that crime changes appreciably on the affected street segment. However, we find evidence that most types of crimes — and robberies in particular — increase on adjacent street segments. This type of crime displacement is consistent with the idea that the most salient type of light outages have the effect of re-allocating potential victims and would be offenders to better-lit areas. As a result, street light outages that go unfixed have

[6]By conditioning on street segment fixed effects, researchers can control for time-stable unobserved differences between communities.

effects that extend beyond a discrete street segment and which reverberate throughout the larger community. This insight — that the effect of place-based public safety initiatives can be mediated substantially by victim behavior — is a topic of discussion in prior literature (see e.g., Cozens et al. (2005)) but has been difficult to detect empirically due to constraints on statistical power in most empirical applications.

The remainder of the paper is organized as follows. Section 2 provides some historical context as well as a brief discussion of the extant literature on street lighting. Section 3 introduces our empirical strategy and Section 4 discusses the data used in the research. Section 5 presents empirical findings and Section 6 concludes.

2 Background

2.1 Ambient Lighting

Street lighting has been around not for years but, in one form of another, for millennia.[7] In more recent history, street lights are thought to have been introduced in the United States by Benjamin Franklin, who designed his own candle-based street light, first used in Philadelphia as early as 1757. Newport, RI become the first U.S. city to introduce gas lighting in 1803 and, after the invention of the electric light bulb, Wabash, IN became the first U.S. city to use electric street lighting in 1880. Today, street lights can be found in varying degrees of abundance in every city in the United States and throughout the rest of the developed and developing world (Chalfin et al., 2019).

The presence of ambient lighting can affect crime through numerous mechanisms, which may operate by changing the behavior of potential offenders, potential victims or both. Perhaps the most obvious way in which lighting can affect crime is by increasing the certainty (or perceived certainty) of apprehension for a given crime, thus deterring criminal activity (Becker, 1968; Akers, 1990). This might be because a police officer can detect criminal activity more easily in an area that is well lit, because lighting increases the probability of a witness (Jacobs, 1961; Painter and Farrington, 1999a,b) or because lighting increases

[7] Oil lamps were used to improve nighttime public safety in the Greco-Roman world at least as far back as 500 B.C. (Ellis, 2007).

the effectiveness of complimentary technology like surveillance cameras (Priks, 2015; Piza et al., 2015). To the extent that lighting increases the actual probability of apprehension, it may also decrease crime by incapacitating offenders (Doleac and Sanders, 2015). For high-volume crimes, even a small increase in arrests could lead to an appreciable decline in crime (Cook, 1986; Ratcliffe, 2002; Roman et al., 2009).

A second way through which the presence of lighting can affect crime is by changing how public space is used during nighttime hours. For instance, individuals tend to feel safer in well-lit areas (Painter, 1996; Chalfin et al., 2019) and may increase their outdoor activity in response to an increase in ambient lighting, thus giving rise to two potentially countervailing effects. On the one hand, more outdoor activity means that there may be more "eyes on the street" (Cozens and Hillier, 2012; Cozens and Davies, 2013) thus deterring crime by increasing the certainty of apprehension (Carr and Doleac, 2018). On the other hand, more human activity, in general, means more potential victims and therefore a greater supply of criminal opportunities (Roncek and Maier, 1991). Greater visibility also might empower potential offenders by reducing their search costs, enabling them to locate more vulnerable victims or lucrative criminal opportunities (Ayres and Levitt, 1998; Welsh and Farrington, 2008).[8] The effect of ambient lighting on crime is therefore theoretically ambiguous.

Finally, as noted by Welsh and Farrington (2008), other theoretical perspectives on the role that lighting plays in the crime production function have emphasized the importance of lighting as an investment in neighborhood conditions that may strengthen community social cohesion (Skogan, 1990). An improvement in the physical environment of a neighborhood, such as the installation of new street lights, may also serve as a cue that an area is cared for and that criminal behaviors violate community norms (Sampson et al., 1997). Under this theory, street lighting might impact crime at nighttime and daytime hours by signaling higher levels of collective efficacy in communities.

[8]These impacts may be further mediated by the extent to which the composition of individuals who spend time outdoors changes.

2.2 Displacement and Spillovers

In studying any place-based intervention that is designed to improve public safety, a critical question is whether the intervention has reduced crime as or has merely displaced it to other areas in a city (Reppetto, 1976; Cornish and Clarke, 1987; Eck, 1993; Guerette and Bowers, 2009). While both crime reduction and displacement are interesting from a scientific perspective, an intervention that merely shifts crime from one location to another is far less attractive to a policymaker than one which leads to a genuine improvement in public safety. Because of its central importance in interpreting empirical estimates, testing for displacement has received a great deal of attention in experimental and quasi-experimental studies of hot spots policing (Sherman and Weisburd, 1995; Braga and Bond, 2008; Braga et al., 2014; Groff et al., 2015; Blattman et al., 2017) disorder reduction (Braga and Bond, 2008; Branas et al., 2011; MacDonald et al., 2016; Branas et al., 2018), closed circuit television cameras (Waples et al., 2009; Welsh and Farrington, 2009; Piza et al., 2014, 2015) and other place-based interventions (Grogger, 2002; Ridgeway et al., 2019).

The conventional approach to study displacement is to examine whether an intervention causes crime to rise in adjacent areas.[9] On the other hand, if an intervention causes crime to *fall* in adjacent areas, then there is thought to be evidence of diffusion of benefits, which captures the idea that even untreated locations might benefit from the general perception that an intervention is in use (Clarke and Weisburd, 1994; Guerette and Bowers, 2009).[10] What crime reduction and displacement effects have in common is that they are typically thought of as behavioral responses of potential offenders to an intervention. Of course,

[9]Measuring crime displacement is challenging for a number of reasons, chief among them that it is unclear *a priori* where crime might go upon being displaced. Will crime merely be pushed "around the corner" (Blattman et al., 2017) or will it migrate to some more distal area which shares one or more key characteristics with the treated area? Given the difficulty of exhaustively testing for all forms of displacement, the norm in the empirical literature is to focus on adjacent areas (Guerette and Bowers, 2009).

[10]Consider, for example, a hot spots policing intervention which adds additional law enforcement presence to a particular area with the intent of deterring potential offenders from engaging in crime. If greater police presence deters offending in a treated location, then adjacent areas might be subject to both displacement and diffusion effects. Of course, the two hypotheses are not mutually exclusive as both displacement and diffusion can happen simultaneously — empirical tests of crime displacement are thus a reduced form test to detect whether either of the two effects dominates.

interventions may also affect victim behavior, an idea which has long been appreciated in the CPTED studies (Cozens et al., 2005; Short et al., 2010; Cozens and Love, 2009, 2015) but which generally receives less attention in the applied criminology literature as many public safety interventions are explicitly designed to change the behavior of offenders to a greater degree than victims.

Street light outages differ in two important respects compared with many other place-based interventions. First, unlike an intervention intended to benefit public safety by increasing police presence, fixing abandoned houses or greening vacant lots, it is reasonable to expect that a reduction in ambient light will tend to increase crime. As a result, street lights that are nonoperational might be expected to be a "crime attractor" (Bernasco and Block, 2011) by pulling offenders into an area that was previous lit. As such, under displacement, one would expect crime in adjacent areas to decline. A second issue worth discussing is that, given the importance that communities seem to place on the availability of street lighting (Chalfin et al., 2019), we might expect the behavior of potential victims to be more sensitive to a lighting outage than one which is more offender-focused like hot spots policing.[11] Since the theoretical calculus is potentially more complex, we pause here to consider the ways in which street light outages might affect spatial spillovers.

To see how victim behavior can have critical implications for how we interpret evidence of displacement, consider the following simplified example which we pair with a simple agent-based simulation. Agent-based simulation has a rich history in the study of place-based crime interventions — recent applications can be found in research on hot spots policing interventions (Weisburd et al., 2017), narcotics enforcement (Dray et al., 2008) and urban renewal programs (Malleson et al., 2013), among others.[12] Lets say that there are two identical adjacent areas of a city (A and B) each of which contain 10,000 street segments. Each area is home to 1,000 potential victims and 100 offenders, with no overlap between the offender and victim populations. Each

[11] Short et al. (2010) refer to this idea as a "reaction-diffusion" model of crime.

[12] Excellent summaries of best practice in this area can be found in Brantingham and Brantingham (2004), Malleson (2012) and Groff (2014).

offender and victim are assigned at random, without replacement, to a street segment and a crime occurs if an offender and victim are paired to the same street segment. Given that offenders and victims cannot cross between areas, each area will experience and expected 10 crimes. In total, the city experiences 20 crimes.

Now consider that area A receives an increasing risk to public safety such as a street light outage. For simplicity, we assume the lighting outage impacts perceptions of public safety but does not affect actual offending conditions. What will happen to crime in areas A and B? The answer depends on the relative movements of victims and offenders in response to the street light outage. To see this, consider first that victims do not respond to the street light outage but that 50 offenders from area B migrate to area A to take advantage of perceived favorable offending conditions. Under this assumption, area A will experience 15 crimes and area B will experience 5 crimes. Thus 5 crimes will have been re-distributed from area B to area A as a result of the street light outage and the total number of crimes in this society will continue to be 20. The result is that the intervention will have displaced crime from B to A without generating any net crime reduction.

Next consider that victims can also respond to the street light outage. First, lets assume that 500 potential victims also migrate from area A to area B in response to the perceived safety risk of street light outage in area A. Under this scenario areas A and B experience 7.5 crimes for a total of 15 crimes. Instead, assume that all 1,000 potential victims migrate to area B as a result of the street light outage in area A. Now area B experiences 10 crimes while area A, which has no potential victims, experiences zero crimes. Under this scenario crime goes down by attracting all potential victims away from area B to area A.

The dynamics of this simple agent-based model are presented in **Figure 1** and are based on 500 random simulations of the model described above.[13] In Figure 1, the x-axis represents the number of offenders who leave area A in favor of area B in response to a street light outage. When the number is positive, offenders,

[13]Replication code in `Stata` 15.0 is available upon request from the authors.

on net, migrate away from Area A; when the number is negative, offenders, on net, migrate into area A. The y-axis represents the number of victims who leave area A for area B in response to the same street light outage.[14] The darkness of the shaded area corresponds to the total number of crimes in each area, with the darker region of the figure representing parameterizations that lead to more crimes and the lighter region representing parameterizations that lead to fewer crimes.

Looking at Figure 1 it is easy to see that crime, which was fixed at 10 crimes per area in steady state, can either increase or decrease in both the treated area A and the untreated area B, depending on the extent to which both victims and offenders respond to the public safety shock. When offenders enter area A from area B and victims do not respond, we are in the southwestern portion of both panels in Figure 1. Here, we see that crime rises in area A and falls in Area B. On the other hand, when offenders and victims both leave area A for area B, we see that crime falls in area A and rises in area B. While this is a very simple example, the dynamics of the model are sufficiently general to make clear that, in the presence of both victim and offender movement, it is impossible to theoretically sign the main effect of a street light outage as well as the sign of the displacement, even when the outage only has perceived effects. Likewise, to the extent that the shock to public safety re-allocates potential victims from the treated to the untreated area, crime can potentially decline while increasing the rate of victimization for a given victim. The empirical analysis presented later in this paper captures the reduced form effect of street light outages on crime in both affected and adjacent areas. We return to this agent-based simulation in discussing potential mechanisms in Section 6 of the paper.

2.3 Prior Evidence

In the United States, contemporary interest in the effect of improved street lighting on crime began during the dramatic rise in crime in the late 1960s (Welsh and Farrington, 2008). The earliest systematic review of the

[14] For simplicity, we rule out the possibility that potential victims move into the area experiencing the street light outage.

effects of street lighting on public safety by Tien (1979) characterized the literature as mixed and inconclusive. More recently, the academic literature on street lighting is ably described in a seminal meta-analysis by Welsh and Farrington (2008), who identify thirty-two street lighting studies in the extant literature and report that, among thirteen studies of sufficiently high quality in the United States and the United Kingdom, the addition of street lighting, on average, reduces crime by more than 20 percent.[15] However, the utility of past research is hampered by a number of critical limitations including: 1) internal validity concerns; 2) measurement issues; 3) limited statistical power; and 4) the fact that only one of the eight studies with a pre-post design and a control group was completed after 1980.

One of the most compelling limitations of the prior literature has to do with the fact that there are few high quality research designs to study the impact of street lights on crime. For a variety of political and operational reasons, it is difficult to randomly assign street lighting.[16] The lone field experiment was conducted by Chalfin et al. (2019) which studies the random allocation of temporary street lights to public housing developments in New York City and finds that street lights reduced serious outdoor nighttime crimes by approximately 36 percent. While this research provides a highly credible estimate of the impact of one particular "tactical" street lighting program, it is unclear whether these results apply to the ordinary provision of street lighting in a typical city.

Of the thirty-two prior studies identified by Welsh and Farrington (2008), nineteen do not emply a comparison group. As such, these studies cannot credibly account for citywide crime trends and regression to the mean, both of which could lead researchers to conflate the effects of street lights with the impact of

[15]Studies included in their systematic review utilize a differences-in-differences research design and, as such, have both pre- and post-intervention data and a control group which did not receive the intervention. Among the eight U.S. studies, lighting was found to be broadly effective in Atlanta, Milwaukee, Fort Worth and Kansas City and ineffective in Portland, Harrisburg, New Orleans and Indianapolis. Among the five U.K. studies, lighting was considered to be effective in Bristol, Birmingham, Dudley, and Stoke. In the fifth location (Dover), the improved lighting was confounded with other public infrastructure improvements.

[16]While Welsh and Farrington's review refers to treatment groups as "experimental" and "control" groups, all of these studies are actually observational.

external events or even ordinary fluctuations in crime which are typical in most communities (Marchant, 2004). Even among the thirteen studies which do employ a comparison area, these areas are often chosen in an ad hoc manner and the validity of resulting estimates depends on a common trends assumption that is formally untestable and is infrequently subject to empirical verification. To the extent that city planners make strategic decisions about where to locate newly available street lights, even pre-post designs with a comparison group may yield biased estimates of the effect of street lights on crime. Of particular concern is a scenario in which policymakers apply new street lighting to areas that are experiencing or are expected to experience an increase in criminal activity which would lead, other things equal, to estimated treatment effects that are biased towards zero (Farrington and Welsh, 2002; Doleac and Sanders, 2015; Domínguez and Asahi, 2017). As a result, despite a plethora of positive research findings, over the last two decades the promise of street lighting to control crime has been a topic of considerable debate with scholars such as Marchant (2004) suggesting that past research may be unreliable, a conclusion that was echoed in a 1997 National Institute of Justice report to the U.S. Congress which concludes that "we can have very little confidence that improved lighting prevents crime."

A second set of issues concerns measurement. Two issues, in particular, are worthy of discussion.First, in a number of prior studies, researchers did not disambiguate between nighttime and daytime crimes. As street lighting is typically hypothesized to have a greater impact on crimes that occur at night, conflating daytime and nighttime crimes will tend to have the effect of generating treatment effects that are biased downward. Second, as noted by Welsh and Farrington (2008), when a comparison area was available, the norm in the literature is to select an area that is adjacent to the treatment area. While this is a reasonable heuristic to select a comparison area that is broadly "similar," adjacent comparison regions will lead to an upward biased treatment effect in the presence of spatial spillovers — that is, the degree to which crime in an area that receives lighting is displaced to adjacent areas.

A third limitation of the prior literature is low statistical power and the inherent difficulty in drawing strong inferences from a small amount of data. Among the thirty-two studies in the literature identified by Welsh and Farrington (2008), only four study more than a handful of locations, meaning that confidence intervals are either so large as to be useless or are entirely unreported limiting our ability to understand the extent to which estimated treatment effects could be due to random chance. Even among the thirteen studies which use a difference-in-differences research design and therefore are of sufficient quality to be included in their systematic review, only two studies (Wright et al. (1974) and a 1974 study of street lighting by the Atlanta Regional Commission) employ more than three treated areas.[17]

Given the substantial methodological limitations of the non-experimental literature on lighting interventions, some of the strongest evidence to date that ambient lighting has appreciable effects on street crimes comes from a natural experiment analysed by Doleac and Sanders (2015) who study variation in lighting induced by daylight savings time. Using both a differences-in-differences and regression discontinuity approach, they find evidence that DST reduces crime, particularly robbery.[18] While their findings suggest a role for ambient lighting, further evidence remains critically important for several reasons. First, Doleac and Sanders use microdata from the National Incident-Based Reporting System, which has poor coverage of large urban areas in the United States, limiting external validity for large cities. Second, and most important, an hour of additional natural light is a fundamentally different — and considerably more intensive — treatment than artificial lighting provided by enhanced street lighting. In particular, potential victims may find it difficult to adjust their behavior during the 6:00pm to 7:00pm hour, whereas time spent outside during the

[17]Recently, this issue — estimating treatment effects for small N case studies — has received a great deal of attention in the econometrics and applied statistics literature. In particular, the method of synthetic controls for case studies proposed by Abadie et al. (2010) and extended by Doudchenko and Imbens (2016) among others, is useful for generating statistical inferences when only a small number of areas are treated. The estimator uses randomization inference to generate an implied p-value and works when there are a large number of potential comparison areas.

[18]Research by Domínguez and Asahi (2017) finds similar effects in Chile.

entire evening may be more discretionary. Finally, street lighting is a policy that communities can directly influence and potentially use to target high-crime areas in which the majority of a city's crimes are clustered.

Taken as a whole, the limitations of the prior literature suggest that developing a deeper understanding of the role that ambient lighting can play in reducing crime will require both more credible causal identification as well as larger sample size. Next, we discuss the details of our proposed natural experiment.

3 Empirical Strategy

3.1 Identification

We study the effect of street lighting on crime using a unique and, we argue, particularly policy relevant natural experiment which has generated an incredible volume of data. In particular, we leverage the fact that street lights, on occasion, become non-operational and must be fixed by municipal workers. During the time that a light is non-operational, the amount of ambient light at night on a particular street segment is substantially reduced which raises the question of whether street light outages compromise public safety. The identifying assumption that validates this research strategy is that the timing of street light repairs is conditionally random. That is, in order for street light outages to mimic an experiment, there must be no difference in the latent distribution of crimes on a given street segment under the treated (light outage) and control (no light outage) conditions. In this section we note several concerns which potentially undermine this identifying assumption and describe the means by which we address each concern and thus validate our identification strategy.

First, we might be concerned that light outages are correlated with local street conditions. For instance, if municipal neglect is part and parcel of a broader set of social problems that are evolving in a given community, then we might expect that crime and street light outages would be correlated even if the relationship is not causal. To address this concern, we condition on street segment fixed effects which net out all time-invariant characteristics that generate cross-sectional variation in crime among street segments within a city. Second, we focus on a discrete window of between four and eleven days around the repair of a street light outage thus indemnifying our identification strategy against concerns about longer-term street segment-specific crime trends.

Second, our estimates could be biased if there is some other time-varying shock that is correlated with both crime and street light outages — for example, if a neighborhood is deteriorating due to municipal neglect or if a local crime problem itself is the cause of municipal neglect. To address this concern, we motivate a key falsification test that assesses the extent to which street light outages are correlated with latent crime trends in a community. In particular, we consider the effect of street light outages on indoor crimes. While it is possible that a light outage could have redistributed individuals from outdoor to indoor spaces, we might expect any crime effects to be smaller indoors than outdoors where lighting might have a first order impact. We also consider the effect of street light outages on daytime crimes which should be less sensitive to light outages than crimes that occur after darkness has set in — though we note that daytime crimes could potentially be responsive to street light outages under temporal spillovers. In both cases, we find evidence that outdoor nighttime crimes are considerably more sensitive to street light outages than either daytime outdoor crimes or nighttime indoor crimes.

A third concern is that there might be measurement error in the timing of a reported street light outage. Our data allows us to identify the date upon which a street light outage is reported by a community resident and the date upon which the street light issue is resolved by municipal workers. While we are confident that the latter date reflects the date that a street light outage is repaired, the date on which the outage is reported may not reflect the date that the light outage actually began since outages may not be immediately reported. Measurement error in the timing of the treatment creates two difficulties for identification. First, it might be the case that the period of time directly before a reported outage is, in fact, treated by the outage even though we will identify that time period as a control period. To the extent that this is true, resulting estimates would be biased downwards. Second, to the extent that the reporting of an outage by a community member is the result of a crime (for example, if a robbery victim reports an outage after being robbed), the timing of a reported outage may not be exogenous. Resulting estimates would be biased upwards.

To address these twin concerns, we focus on a discrete period of time that is local to the *resolution* of the street light issue rather than the date of the *reported* street light outage. In particular, we focus on the four-day period after an outage is repaired and the up to the one-week period prior to the repair, comparing crimes during the post-repair period to the pre-repair period.[19] A visual schematic of our research design can be found in **Figure 2**. Consider a street light outage that is first reported to municipal authorities at time, s_0. This outage may have begun on s_0 or it may have begun prior to s_0. Panel A refers to a street light outage that is longer than seven days in duration. The outage is repaired at time, s_2. Given this, we study the days that are bounded by the dashed red lines: the pre-repair period are the seven-days between s_1 and s_2; the post-repair period are the four-days between s_2 and s_3. Panel B refers to a street light outage that is less than seven-days in duration — for example, two days. Here, the reported outage date $s_0 = s_1$, the beginning of the pre-period. We continue to study the days that are bounded by the dashed red lines: the pre-repair period are the two days between s_1 and s_2; the post-repair period are the four-days between s_2 and s_3.

Such a strategy has several virtues. First, by focusing on a short time window around a street light repair, we can be confident that the treatment is measured with minimal error and therefore is not compromised by the mechanical correlation discussed above. Second, by focusing on a discrete period of time, we absolve ourselves of the need to control for secular time trends which are difficult to fully account for and which are a central threat to causal identification in differences-in-differences designs.[20] In this sense, our research design is similar to a regression discontinuity design in time under the assumption that the precise timing of the

[19]Formally, the length of the pre-repair period is given by the minimum of one-week and the number of days between the outage and repair.

[20]The standard approach to addressing time trends in differences-in-differences designs is to condition on time fixed effects. However, this does not account for more granular interactive types of time trends such as differential time trends by community or by street segment.

reported outage is conditionally random. Finally, our estimator lends itself to a very simple interpretation. We are simply assessing whether crimes are lower just after relative to just before a street light outage is addressed by municipal workers. In an auxiliary analysis, we consider spatial spillovers to adjacent street segments. We also test the sensitivity of resulting estimates to the temporal bandwidth around the outage.

Fourth, we might also be concerned that individuals — either community residents or first responders — will report a street light outage upon learning that a crime has taken place. For example, police investigating a nighttime robbery might note that a street light is out and report the outage to the city's 311 system. If this is true then we would expect to see a mechanical correlation between light outages and crime that is not, in fact, causal. In order to address this concern, we run an auxiliary analysis in which we exclude crimes that occurred on the date an outage was reported. The virtue of this analysis is that, to the extent that outages are reported by law enforcement responding to a crime, this crime will not be in the time window used to identify the treatment effect.

A final concern is that offenders might intentionally disable street lights for the express purpose of committing nighttime, outdoor crimes. While we are aware of no ethnographic or even anecdotal evidence that intentionally breaking street lights is common, we cannot reject this story out of hand as such, this concern merits comment. While our standard robustness check – examining nighttime indoor crimes — does not fully address the above concern, we note that, to the extent that offenders disable street lights prior to a crime spree, we would expect crime to rise during an outage on the affected street, not in adjacent areas which remain well-lit. Thus, our principal finding — that street light outages shift crime to adjacent areas — should be robust to the concern that offenders intentionally break street lights where they plan to commit crimes.

3.2 Empirical Models

To evaluate the effect of street light outages on crime, we consider whether crime increases during the period in which a street light has burned out, prior to being replaced, on the subset of street segments which experienced at least one outage during the study period. We study outages at the street segment by day level using the following simple differences-in-differences equation:

$$Y_{it} = \alpha + \beta D_{it} + \phi_i + \Sigma_{j=1}^{7} DOW_{ijt} + e_{it} \tag{1}$$

In (1) the dependent variable, Y_{it}, is the count of crimes that occurred on street segment i on day t. D_{it} is a dummy variable for whether the day is prior to ($D_{it} = 0$) or after ($D_{it} = 1$) the repair of the outage.

We condition on two sets of fixed effects. First, we condition on street segment fixed effects, ϕ_i, which account for unobserved heterogeneity that is constant over time, but which varies by street segment. Critically, this term ensures that we are not comparing crime on street segments in different communities or that generally experience different numbers of crimes or street light outages. By focusing on the "within" estimator, we are simply comparing crimes among the days that occur just before or just after a street light outage is repaired on a given street segment. Second, we condition on day-of-week fixed effects, DOW_{it}, to account for the possibility that light outages are differentially likely to be resolved on certain days of the week, for example if a greater number of municipal workers are on duty during weekdays as opposed to weekends, and to account for the possibility that crimes are more likely to occur on certain days of the week.

In our main specification, we include the four days after an outage is repaired and the up to seven days after an outage is reported to municipal authorities but before it is repaired. In a series of robustness checks, we vary the size of this window. We run model (1) separately for outages that affect only 1-2 street lights ("minor outages") and outages that affect more than two street light ("major outages") on a given street segment.[21] As such, we are able to estimate the impact of ambient lighting under two different treatment intensities.

Equation (1) is likewise estimated for outdoor nighttime crimes and separately for outdoor daytime crimes as well as nighttime indoor crimes. Models that identify the effect of street light outages on outdoor daytime crimes test for temporal spillovers. Models that estimate the effect of street light outages on indoor nighttime crimes provide a key falsification test as we would expect lighting conditions to have a greater impact on outdoor crimes at night. Next, we consider potential displacement which is a key consideration in any place-based crime study. In order to test for whether darkness is a crime attractor — that is, whether crime "spills in" to areas treated by a lighting outage — we re-estimate (1) using the number of crimes within 500 feet of the affected street segment (excluding the street segment that experiences the outage itself) as the dependent variable. If crime is being re-allocated by lighting outages to other street segments in a community, then these regressions would indicate that crime on adjacent street segments will change as a function of a street light repair on a given street segment.

In each model, standard errors are clustered at the Census block group level in order to account for spatial autocorrelation amongst observations located in the same block group. Census block groups constitute, on average, 264 street segments and, to the extent that serial correlation exists not only within observations for a given street segment but also amongst street segments within the same Census block group, clustering standard errors at the higher level of aggregation accounts for this feature of the data (Bester et al., 2011).

4 Data

This research studies the effect of street light outages on crime in Chicago. Each crime and street light outage report is merged with street location data for the city of Chicago available on the city's Open Data website

[21] In the administrative data, outages that affect 1-2 lights are called "single outages" and outages that affect more than two lights are called "multiple outages."

to determine the presence of a street light outage or a crime on a given street segment on a given day.[22] In this section we provide detail on how the data were processed in order to generate an analytic dataset.

4.1 Street Light Outages

Data on street light outages come from complaints reported to Chicago's 311 reporting system. In order to report a street light outage, citizens can either call the city's 311 complaint line or they can report a complaint through the city's 311 system website.[23] When submitting a service request on the Chicago 311 website, residents are required to enter an address where the outage occurs. They are then asked whether all lights on the block are out, if the outage is in the street or in the alley, and if the light is "completely out" rather than "going on and off." Residents using the website also have the option to include a photo of the outage. Users must confirm the outage location and details before the request can be submitted.

The 311 data includes the date that the street light outage was reported, the date that the outage was addressed and the location details (i.e., latitude and longitude) of the reported outage.[24] Notably, there are two types of reported street light outages in the data: outages involving 1-2 street lights (47 percent of reported outages) and outages involving more than two street lights (53 percent of reported outages).[25] To test for non-linear effects of street lighting, we analyze minor (1-2 lights) and major (more than two lights) outages separately.

We merge these data to Chicago's street center lines shapefile in order to assign street lights to a given street segment. Using the reported latitude and longitude of the reported outage, we created a 50-foot buffer around each segment and used the coordinates from the outages data to determine on which segment each street light outage occurred.[26] In approximately 0.3% of cases there was no match to any segment;

[22] https://data.cityofchicago.org/Transportation/Street-Center-Lines/6imu-meau

[23] https://www.chicago.gov/city/en/depts/311.html

[24] When a city employee addresses the outage, they also check all nearby street lights.

[25] At first glance, the ubiquity of outages affecting more than two street lights might seem unusual. However, it is important to note that, in Chicago, it is typically the case that a number of lights are connected to each other in a "group." Hence, an electrical issue can disable multiple street lights on a given street segment at the same time.

[26] The choice of a 50-foot buffer is common in the empirical literature that rely on the geocoding of crimes to blocks or

these data were discarded. As outages reported at a street corner or in an intersection will fall within 50 feet of multiple street segments, we use a simple rule to determine on which street segment that outage belongs. In cases where an outage is within 50 feet of multiple streets but only within one foot of a single street, we assign the outage to the nearest street. In cases where the outage is within one foot of multiple streets - as occurs when the outage is coded to the intersection - we assign the outage to each of these streets. Approximately 56% of outages were within one foot of only a single street; 43% were within one foot of more than one street, and, accordingly, were assigned to multiple streets.

4.2 Crime

We obtain microdata on crimes known to the Chicago Police Department from the city's publicly available Open Data website. For each criminal incident, the data provide information about the type of crime (i.e., murder, robbery, motor vehicle theft), the date and time of the reported crime and the type of location of the crime (e.g. playground, school, residence). We use this variable to determine whether the crime occurred indoors or outdoors.[27] We study three key overlapping crime aggregates: violent crimes, property crimes and index crimes which coincide with Part 1 crimes in the Federal Bureau of Investigation's Uniform Crime Reporting program.[28] We also study robbery, assaults and motor vehicle theft, three common street crimes which might plausibly be affected by changes in ambient lighting.

In order to determine whether a complaint occurred during daytime or nighttime hours, we use daily data on civil twilight hours — those hours in which natural sunlight is present.[29] Data on civil twilight

street segments (e.g., (Ratcliffe, 2012).

[27]We code the following locations as pertaining to outdoor crimes: alley, airport exterior or parking lot, ATM machine, bridge, cemetery, Chicago Housing Authority parking lot or play ground, Chicago Transit Authority platform or tracks, driveways, expressway/highway, forests or lakes, parking lots/garages, vehicles, porch, resident or school yard, sidewalk, street, or vacant land.

[28]Violent crimes include: assault, battery, sexual assault, domestic violence, homicide, intimidation, kidnapping, and other sexual offenses. Property crimes include: theft, motor vehicle theft, gambling, and criminal damage.

[29]Civil twilight generally begins approximately half an hour after the official sunset and ends approximately half an hour before the sunrise. During times between the start and end of civil twilight, there is sufficient sunlight "for terrestrial objects to be clearly distinguished"; in other times, "artificial illumination is normally required to carry on ordinary outdoor activities." http://aa.usno.navy.mil/faq/docs/RST_defs.php

hours come from the United States Naval Observatory (USNO) whose website contains data on the precise time when civil twilight began and ended for each part in the United States.[30] We consider a crime to occur at night if it happens before civil twilight begins or after it ends.

Following how we coded street light outages to street segments, we created a 50-foot buffer around each segment and used the geographic coordinates from the crime microdata to determine which segment each crime occurred on. For crime, nearly 0.5% of incidents were not within 50 feet of any street segment and were excluded for this study. The remaining 99.5% of incidents were within 50 feet of a single street segment; fewer than 0.05% of incidents were coded to multiple streets as a result of being within one foot of multiple street segments.

4.3 Descriptive Statistics

Our street segment-level data is summarized in **Table 1**, where we report the annual prevalence of street light outages and crimes per street segment in Chicago over the 2010-2018 study period. For each variable, in addition to presenting a summary prevalence measure for the entire time period, we present prevalence measures for several salient subsets of our data. First, we divide our data into two periods: 2010-2014 and 2014-2018, reporting prevalence separately for the first half and the second half of our data. Next, we report prevalence separately for high versus low crime districts. Finally, we report prevalence separately for weekends and weekdays.

Street segments experience, on average, approximately 0.8 outages per year, which translates to approximately 8 days per year in which at least one street light is out. The frequency and duration of street light outages does not change appreciably throughout the study period, though they do they vary somewhat according to district-level crime rates. Outages disproportionately affect weekdays as opposed to weekends, which could be a sign that residents are more likely to report outages during the week. Next, we turn to crime prevalence. The average street segment experiences four index crimes per year. Index crimes are

[30] http://aa.usno.navy.mil/data/docs/RS_OneDay.php

fairly evenly divided between violent and property crimes. Assaults, which include both aggravated assaults (which are index crimes) and simple assaults (which are not) are particularly common.

Finally, we discuss the duration of lighting outages. We pause here to remind readers that, while we can measure the number of days that it takes municipal workers to repair a street light once the street light outage has been reported, we do not have an accurate measure of the actual length of the duration of an outage. This is because it is unclear how quickly street light outages are reported by community residents. In **Figure 3**, we provide a visual representation of the distribution of the duration of street light outages using a kernel density plot. Municipal workers in Chicago generally do an excellent job in responding to reported street light outages in a timely manner. The median number of days between a reported street light outage and a repair is just 3 days and 25 percent of outages are resolved within one day. However, there is a long tail of outages that take longer to resolve — the mean duration of an outage is 9.3 days and 10 percent of outages take more than three weeks to resolve.[31] With respect to our identification strategy, we focus on the period of time that is up to 7 days prior to the repair of an outage. When an outage is repaired quickly — for example, in one day, the pre-period will be short. If an outage is addressed less quickly — for example, in 10 days — the pre-period will be 7 days. Overall, 26.4 percent of outages are repaired within a single day, 45 percent of outages are repaired within 2 days and 56.3 percent of outages are repaired within 3 days. Hence, we generate an estimate of the effect of street light outages by comparing crimes during the seven-day post-period window to an average pre-period window of approximately days.

5 Results

5.1 Main Results

Our primary regression results are presented in Table 2A and 2B which consider the effect of major light outages (involving more that two street lights) on crime and Tables 3A and 3B which consider the impact

[31] We consider any report that takes greater than 180 days to resolve to be a data error and exclude it from the data.

of minor street light outages (involving one or two street lights) on crime. Each table provides estimates for crimes on the street segment that experiences the light outage as well as on other street segments within 500 feet of the segment with the outage, excluding the street segment experiencing the outage itself. We provide estimates for outdoor nighttime crimes (Panel A) as well as for outdoor daytime crimes as a test of temporal spillovers (Panel B) and indoor nighttime crimes as a key placebo test (Panel C). We also provide the control mean and the estimated effect size which is calculated by dividing $\hat{\beta}$ by the total mean, along with a 95 percent confidence interval around the effect size. To simplify interpretation we focus on linear regression models that estimate the change in the number of crimes during the period of time in which a street light is out relative to the period of time after the light outage has been repaired. Results are extremely similar using Poisson regression count models (see Appendix Tables 1A and 1B).

Table 2A considers the effect of major street light outages on index crimes, violent crimes, and property crimes. There is a small statistically insignificant (2 percent) increase in index crimes, violent crimes (1 percent), and property crimes (1 percent) during the period in which a street light is out. The estimates are reasonably precise, with the 95 percent confidence interval spanning -3 percent to +6 percent for index crimes. The most reasonable interpretation of these results is that street light outages do not appreciably impact crime in the short-term on affected street segments. When we examine adjacent areas the results indicate that index crimes increase by approximately 3 percent during an outage. Estimates are similar for violent crimes (2 percent) and property crimes (3 percent). While these estimates are modest in size, due to the large size of our data, they are significant at the $p < 0.05$ level. In Panel B, we consider whether daytime outdoor crimes are responsive to street light outages. While estimates have some imprecision, there is no clear pattern in the data that suggest that daytime crimes change during a street light outage — either on the own street segment or in adjacent areas.

Turning to **Table 2B**, we provide estimates for individual crime types. We focus on robberies, assaults and motor vehicle thefts, the common street crimes in the index crime category. The first set of columns pertain to robberies. These do not change appreciably on the segment directly affected by a lighting outage. However, we estimate that robberies on adjacent street segments increase by approximately 7 percent during a light outage (95 percent CI = 0.6%-13.4%). Turning to assault, we see little evidence of an appreciable effect of light outages either on the affected street segment or in nearby locations though the confidence intervals are wide enough to accommodate small positive effects. Finally, we turn to motor vehicle thefts. For these, estimated impacts are large on and around the affected street – we observe a 9 percent increase in vehicle thefts on the affected street segment and a 6 percent increase on adjacent street segments. While, given the relative rarity of vehicle thefts, these estimates are not quite significant at $\alpha = 0.05$, they are very close — p-values are < 0.07 and < 0.06, respectively. As with the aggregate crime categories reported in Table 2A, we do not observe clear evidence of daytime effects for our individual crime analyses.

Next, we consider whether crime patterns change in response to minor street light outages. **Table 3A** and **Table 3B** follow the same structure as Tables 2A and 2B; here we do not see evidence of crime effects. While standard errors are not sufficiently precise to rule out very small impacts, the coefficients are uniformly small and of mixed sign — both for the own street segment models and the models that study adjacent areas. The evidence thus suggests that ambient lighting has a non-linear effect on crime. This finding thus potentially reminds us of the salience of dosage in yielding estimated treatment effects, a finding which has been reported in many other criminal justice research settings, perhaps most notably in research that studies the effect of time served in prison on future recidivism (Loughran et al., 2009; Meade et al., 2013). One caveat is, however, worth noting. To the extent that police re-allocate their efforts to spend greater time on unlit street segments during the period in which the lights are non-operational, additional police presence might put downward pressure on the estimated treatment effects that we observe in our data. We therefore regard these estimates as being a lower bound on the true effect of a street light outage on own-segment crime.

Finally, we consider whether our estimates differ according to land use. To do so, we compute the number of commercial establishments for each of Chicago's 2,174 Census block groups. We then re-estimate equation (1) adding an interaction between the treatment indicator and an indicator variable for whether a street segment is in a block group that is above the median commercial density in the city. Estimates for nighttime outdoor crimes are presented in **Table 4**. We present estimates for the main effect (which corresponds to the treatment effect for low business density areas) as well as the interaction term (which corresponds to the additive effect for high business density areas). For the street segment affected by a street light outage, we see evidence that index crimes increase in low business density areas but not in areas with higher than median business density. While the sign on the interaction terms are predominantly negative, there is no consistent evidence that segments with high commercial density experience differential effects. For adjacent areas, there is no consistent evidence that treatment effects vary significantly by land use for any of the crime categories we study.

5.2 Robustness

We find that outdoor nighttime crimes are shifted during street light outages that involve more than two lights. Effects are particularly large for robberies, a common street crime for which there is evidence that ambient lighting conditions are a determining factor in its incidence (Doleac and Sanders, 2015; Domínguez and Asahi, 2017) and motor vehicle thefts which, in our data, increase on both affected segments as well as in adjacent areas. In this section, we test the robustness of these results to different analytic choices and we defend the identifying assumptions of our model.

5.2.1 Identification

The chief concern with respect to identification is that the timing of a street light outage is correlated with latent crime trends in a community. For instance, we might imagine that the failure to repair broken street lights might be part and parcel of municipal neglect of high crime neighborhoods. We address this concern by conditioning on street segment fixed effects and by focusing on a very narrow window of time around the date that a street light outage is repaired, relying on the exogeneity of the precise date of repair. That said, it remains instructive to test whether the repair of street light outages is correlated with broader crime trends within this window. To do so, we check whether nighttime *indoor* crimes are responsive to street light outages, reasoning that indoor crimes should be less sensitive to street light conditions than outdoor crimes.[32] Referring to the bottom panel of Tables 2A and 2B, we see little evidence that indoor crimes are shifted to adjacent areas by street light outage repairs.

Another concern worth addressing is the possibility that police or other first responders may report street light outages while investigating a crime call. To the extent that this is systematically true, it is potentially serious threat to identification as it would create a mechanical correlation between street light outages and crime, thus biasing us in favor of finding such an effect. Our placebo test for indoor crimes partially addresses this concern — at least to the extent that police report street light outages even when they are investigating a crime that took place indoors (e.g., domestic violence). However, it is also possible to imagine that police report street light outages only when responding to a call for service that is related to an outdoor crime. To fully address this concern, we re-estimate (1) removing the day of the reported outage so that crimes which occur on the outage report date do not contribute to our estimates. The results of this analysis are reported in **Table 5** which presents estimates for the effect of a major street light outage on nighttime outdoor crimes, removing the date of the outage report itself. Referring to Table 4, we see that estimated effects are extremely similar to those reported in Tables 2A and 2B.

[32]Results are similar when burglaries — an indoor crime with some of the characteristics of an outdoor crime — are excluded from the data.

5.2.2 Other Robustness Checks

Next, we consider robustness to a number of analytic choices made during the research process. The estimates reported in Tables 2A, 2B, 3A and 3B are derived from least squares regressions of the count of crimes on the presence of a street light outage. We focus on least squares regression models because they are simple and computationally efficient, a first-order issue given the enormous size of our data. Naturally, researchers often prefer to model crime counts using a count data model such as Poisson or negative binomial regression. In **Appendix Table 1A** and **Appendix Table 1B**, we report estimated treatment effects for crime aggregates and individual crime types, respectively, using Poisson regression. We report estimates for major outages (Panel A) and minor outages (Panel B). Point estimates are extremely similar to those reported in Tables 2A and 2B.[33]

We also consider the sensitivity of the estimates to using a different bandwidth around the outage repair date. We test the robustness of our results to bandwidth selection, varying 1) the length of the post-repair bandwidth and 2) the length of the pre-repair bandwidth. As the length of either window increases, the research design is potentially weaker because it becomes more difficult to attribute change in crime to the change in lighting. Thus, all else equal, we prefer estimates using as small a bandwidth as possible. Estimates for our crime aggregates (index, violent and property crimes) are presented in **Appendix Figure 1A** (varying the post-repair bandwidth window) and **Appendix Figure 2A** (varying the pre-repair bandwidth window). Estimates for individual crime types are presented in **Appendix Figures 1B** and **2B**. In each figure, we present estimates for the street segment affected by a major street light outage as well as for street segments within 500 feet of the affected segment. Referring to Appendix Figure 1A, we see little evidence that either

[33]Sometimes crime counts are modeled using negative binomial regression models due to concerns about overdispersion in the data. For several reasons, we prefer Poisson regression in this context. First, tests for overdispersion do not distinguish between overdispersion and misspecification (see Berk and MacDonald (2008); Blackburn (2015). Consequently, it is *a priori* unclear when overdispersion actually exists and is therefore an issue. Second, Poisson regression is first order equivalent to negative binomial regression when robust standard errors are used — as we do. Finally, negative binomial regression yields inconsistent estimates when fixed effects are used in a model (Lancaster, 2000). This is not an issue for Poisson regression (Allison and Waterman, 2002). As our models do include fixed effects, the Poisson regression model is a more appropriate choice.

index crimes, violent crimes or property crimes change significantly on the affected street segment regardless of the bandwidth selected. On the other hand, while estimates sometimes just cross the $\alpha = 0.05$ significance threshold, our finding that index and property crimes increase in adjacent areas is largely robust to varying the length of the post-repair window — point estimates are extremely similar regardless of the choice of bandwidth. Turning to Appendix Figure 1B, our findings that robberies increase in adjacent areas and that motor vehicle thefts increase on both affected *and* proximate segments are largely robust to bandwidth selection.[34]

6 Discussion

Consistent with the theories of crime prevention through environmental design and situational crime prevention, a large body of empirical evidence suggests that crime is sensitive to the design of public space (MacDonald et al., 2019) and the presence of disorder (Braga and Bond, 2008; Keizer et al., 2008; Branas et al., 2018). Street lighting is one of the world's oldest and most enduring CPTED-inspired crime control strategies and yet there is a relative dearth of recent, high quality evidence on the effectiveness of investments in street lighting in promoting public safety (Welsh and Farrington, 2008). This research leverages a natural experiment brought about by the failure — and subsequent repair — of municipal street lights to understand the sensitivity of crime to a short-term change in ambient lighting as well as the extent to which changes in lighting conditions disrupt spatial crime patterns.[35] Street light outages provide an informative natural experiment for several reasons. First, as virtually every city in the developed world uses street lighting each and every night, these results are broadly applicable to a wide range of policy settings. Second, servicing existing street lights has been a municipal responsibility for many years and, as such, improving the servicing of street lights is an intervention that is available to all city policymakers. Finally, the rich administrative

[34] In Appendix Figure 2A and Appendix Figure 2B we vary the length of the pre-repair time window. Similar to estimates in which the length of the post-repair window is varied, estimates are quantitatively and qualitatively similar regardless of the bandwidth selected.

[35] Our approach, studying the short-term impact of ambient lighting is similar to research by Chang and Jacobson (2017) who study the public safety impact of the closure of marijuana dispensaries.

data available in Chicago allow us to identify major street light outages involving more than two street lights. Accordingly, we are able to study a large and salient change in the availability of ambient lighting.

Using data on nearly 300,000 street light outages spanning an eight-year period in Chicago, we document evidence that crime is sensitive to street light conditions but that the effects operate predominantly through subtle but important behavioral channels. During a major street light outage on a given street segment there is little evidence that most crimes change appreciably on the street segment experiencing a street light outage. However, we see evidence that crime, in general, and robberies and vehicle thefts, in particular, increase *in adjacent areas*. Displacement of crimes from "treated" to "untreated" areas is consistent with the idea that light outages have the effect of re-allocating offenders as well as potential victims to better-lit areas. As a result, street light outages that go unfixed for a period of time can have effects that extend beyond a discrete area and can reverberate throughout a community. The effects we observe are qualitatively important — a 6-7 percent increase in robberies and vehicle thefts is equivalent to what we might expect to see if the size of a city's police force were reduced by between 5-15 percent, depending on the estimate (Evans and Owens, 2007; Chalfin and McCrary, 2018; Weisburst, 2018). These findings highlight a general principle in place-based crime research which has been noted by Cozens et al. (2005) and Short et al. (2010) among others — that the effects of place-based crime control strategies can be mediated and indeed shaped to a considerable degree by the behavior of potential victims.

In Section 2.2, we discussed the idea that a street light outage might have effects not only on the behavior of offenders but also on the behavior of the potential victims of crime. Using a simple agent-based model, we demonstrated that the impact of a street light outage on crime will depend on the behavioral response of both victims and offenders. We turn back to this agent-based simulation in order to derive a deeper understanding — even if it is only suggestive — of the behavioral dynamics underlying our reduced form results. The principal lesson of our agent-based simulation is that the impact of a public safety shock on crime in the area affected directly by the shock depends on the extent to which victims allocate their time away from the area and offenders allocate their time towards the area. For example, if a light outage causes both victims and offenders to avoid a dark area, then crime will be "pushed around the corner" (Blattman et al., 2017), declining on the affected street segment. On the other hand, if a light outage does not affect victim behavior but causes offenders to spill into the affected area, then crimes will be re-allocated towards an area experiencing a public safety shock.

This logic has implications for how different crimes will be impacted by a public safety shock like a street light outage. To see this, first consider a common street crime like robbery which requires a victim to be present. In a world in which potential victims are more reluctant than potential offenders to walk down poorly lit streets, we might expect fewer victims to be available to rob on the affected street during a street light outage. Likewise, we would expect that there will be more victims to rob on adjacent street segments. This is exactly what we see in the empirical results presented in Section 5.1 — the magnitude of the spillover is particularly large for robberies as these crimes increase by approximately 7 percent on adjacent street segments during an outage but change little on the street segments directly affected by an outage. The fact that robberies do not change much on the affected street segment is consistent with the idea that there are fewer individuals to rob on these street segments but that the robbery rate increases for those potential victims who do not allocate away from poorly-lit streets. In our empirical models, we observe the net impact of these two competing effects.

On the other hand, consider a crime like motor vehicle theft which does not require a victim to be present. Further consider that cars are sometimes parked for a long period of time in a given space. A street segment may have been well-lit when a vehicle was initially parked only to suffer a street light outage sometime later. We might then expect that potential vehicle theft victims will be less able to respond to a change in street light conditions than potential robbery victims. Furthermore, a street light outage offers ideal conditions to steal a car.

As such, we might expect offenders to spill in to a darkened area perhaps without a proportional spilling out of victims. Given these hypothesized behavioral impacts, we might then predict that motor vehicle thefts would be more likely to rise on the affected street segments than robberies. While estimates are slightly less precise, the results presented in Section 5.1 are consistent with this idea — motor vehicle theft is the only crime type for which there is evidence of an appreciable increase in crime on street segments affected by a lighting outage.

The evidence suggests that street light outages compromise public safety in ways that can have downstream effects on an entire community. A natural question then is whether lighting outages are more common in communities with higher crime rates. To answer this question, we turn to **Figure 4** which plots the average number of street light outages in a police district (Panel A) and the duration of an average street light outage in a police district (Panel B) against district-level crime rates.[36] There is evidence that higher crime areas suffer a greater number of light outages and that these outages that do occur take longer to repair than outages in lower-crime areas in Chicago. This suggests that municipal neglect contributes to unequal crime rates that are experienced by different neighborhoods in Chicago. Speeding up the repair of street light outages is, of course, costly and, as such, we do not reflexively recommend that city planners re-allocate scarce resources towards this activity. Nevertheless, we note that the effect of a perpetual backlog in street light repairs can multiply rapidly given their impact on relatively large areas. We further point out that rapid street light repairs offer a strategy to keep crime low without relying on additional enforcement (Clarke, 1995) and while reducing fear of crime (Painter, 1996) and therefore potentially promoting healthy and active living (Roman and Chalfin, 2008; Roman et al., 2009; Lee et al., 2016). As such, we contribute to the growing literature on the importance of place (Weisburd, 2015) and place-based crime reduction strategies (MacDonald et al., 2019) in controlling crime in an effective, efficient and scalable way.

[36] In each figure, we plot number of index crimes on the x-axis and the street light outage measure on the y-axis. Because police districts vary in population, each measure is residualized to net out the number of street segments in each police district. A linear regression line is drawn through the data.

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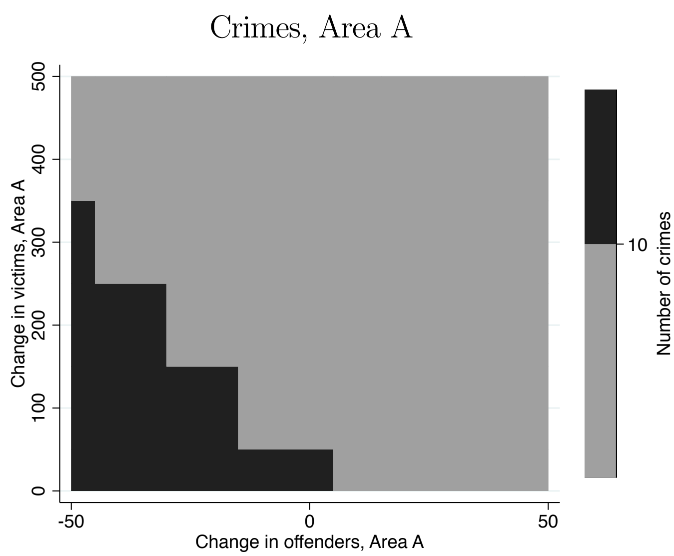
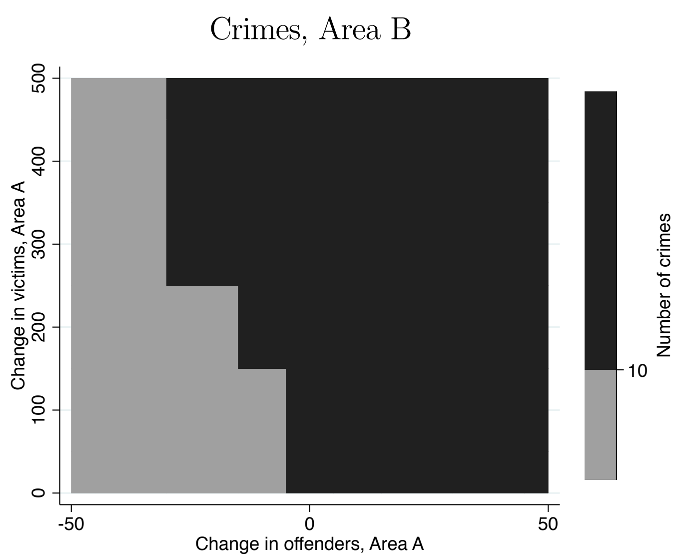
Figure 1: Agent-Based Simulation of Crime Displacement

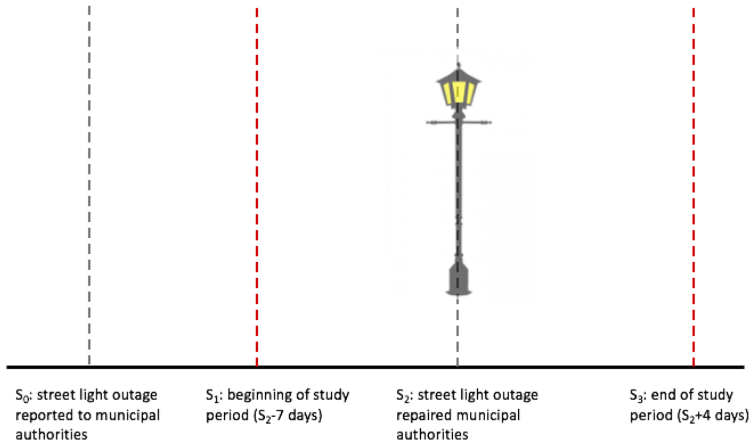




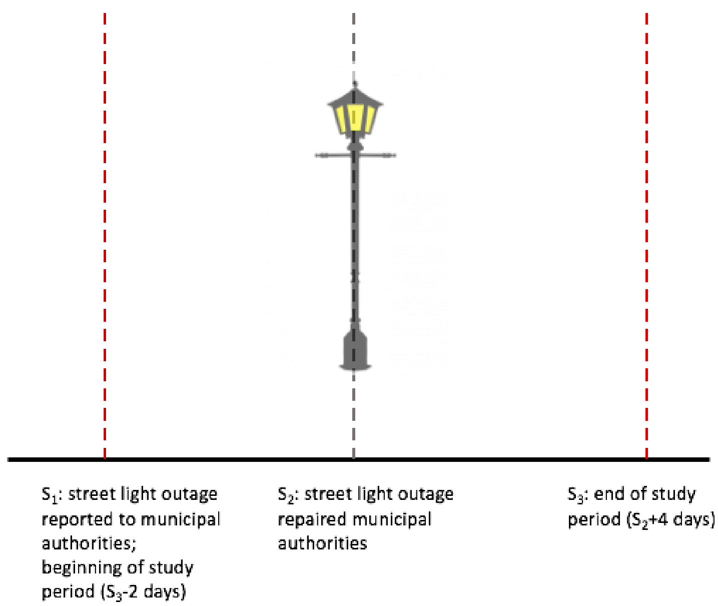
Note: These figures report the results of a simple agent-based simulation covering a society with two areas (A and B) each of which consist of 10,000 street segments. Each society initially has 100 offenders and 1,000 potential victims, with no overlap between the populations. Victims and offenders are allocated to street segments at random; a crime occurs when a victim and an offender are allocated to the same street segment. The figure considers a negative shock to public safety in Area A — for example, a street light outage. As a result of the shock, both offenders and victims can choose to migrate to Area B or remain in Area A. The x-axis in each of the figures refers to out-migration of offenders in Area A; the y-axis refers to out-migration of potential victims in Area A. Negative numbers indicate that offenders are spilling into Area A; positive numbers indicate that offenders are leaving Area A for Area B. The shading of the figure indicates the number of crimes that occur, with a darker shade indicating an increase in crimes and the lighter shade indicating a decrease in crimes. When offenders leave Area B for Area A and victims in area A stay put, this maximizes crime in Area A while minimizing crime in Area B. Simulated results are based on $m = 500$ repetitions.

Figure 2: Visual Schematic of Research Design

A. Light Outage Duration $>$ 7 Days

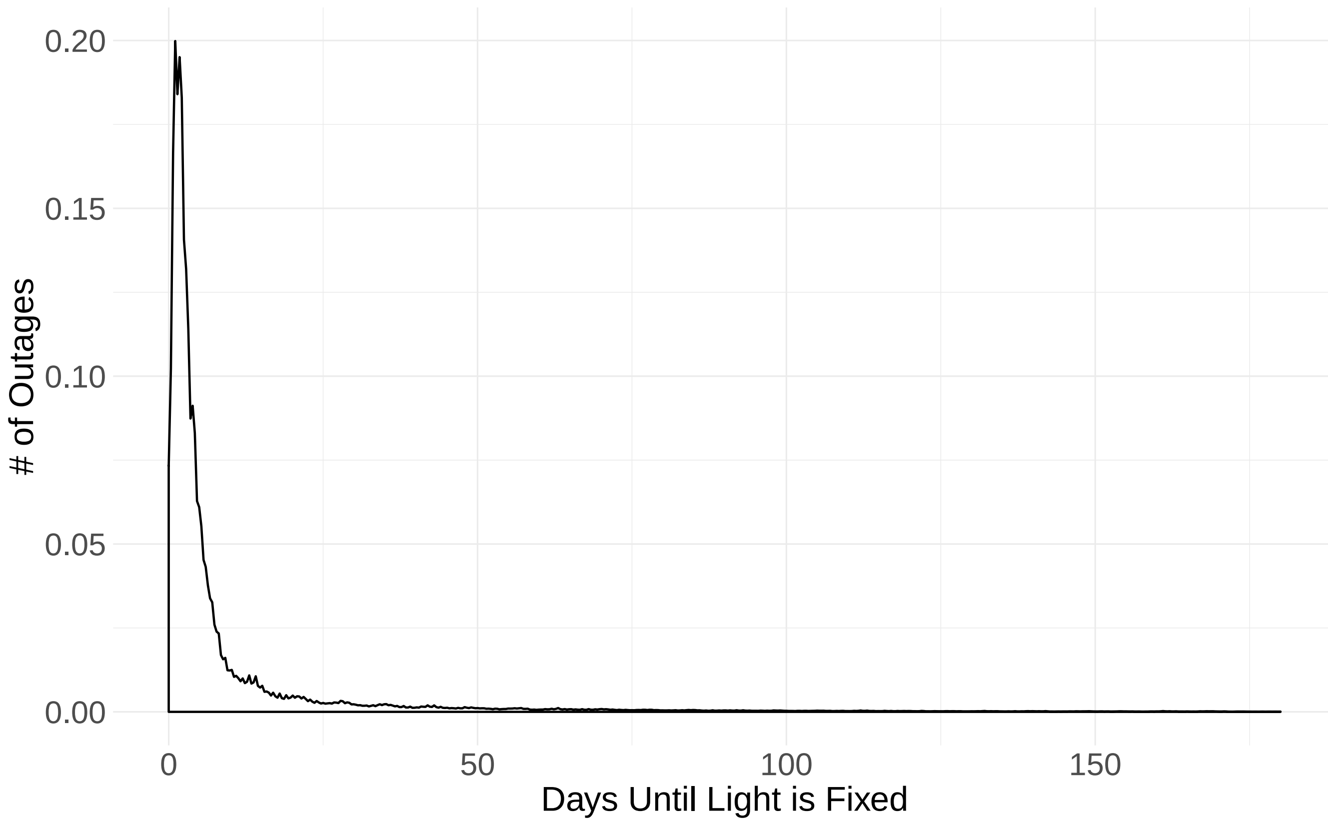


B. Light Outage Duration $<$ 7 Days



Note: These figures (not drawn to scale) present a visual depiction of our research design. Consider a street light outage that is first reported to municipal authorities at time, s_0. This outage may have begun on s_0 or it may have begun prior to s_0. Panel A refers to a street light outage that is longer than seven-days in duration. The outage is repaired at time, s_2. Given this, we study the days that are bounded by the dashed red lines: the pre-repair period are the seven-days between s_1 and s_2; the post-repair period are the four-days between s_2 and s_3. Panel B refers to a street light outage that is less than seven-days in duration — for example, two days. Here, the reported outage date $s_0 = s_1$, the beginning of the pre-period. We continue to study the days that are bounded by the dashed red lines: the pre-repair period are the two days between s_1 and s_2; the post-repair period are the four-days between s_2 and s_3.

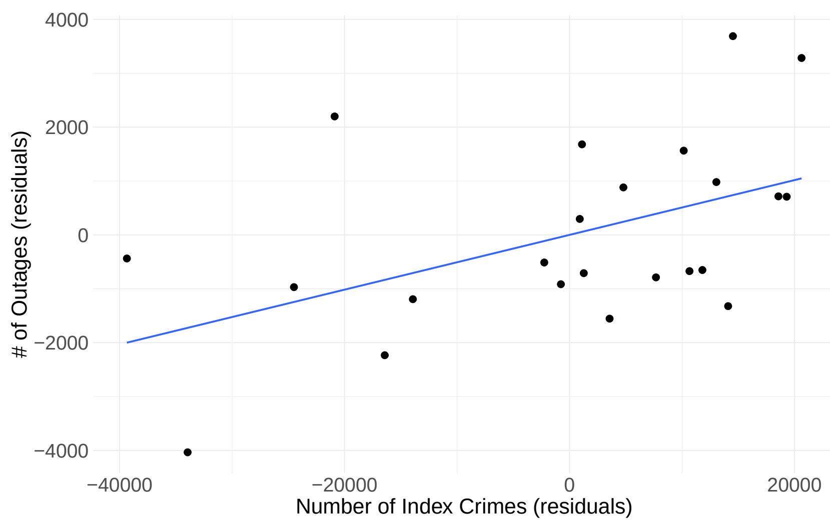
Figure 3: Duration of Street Light Outages



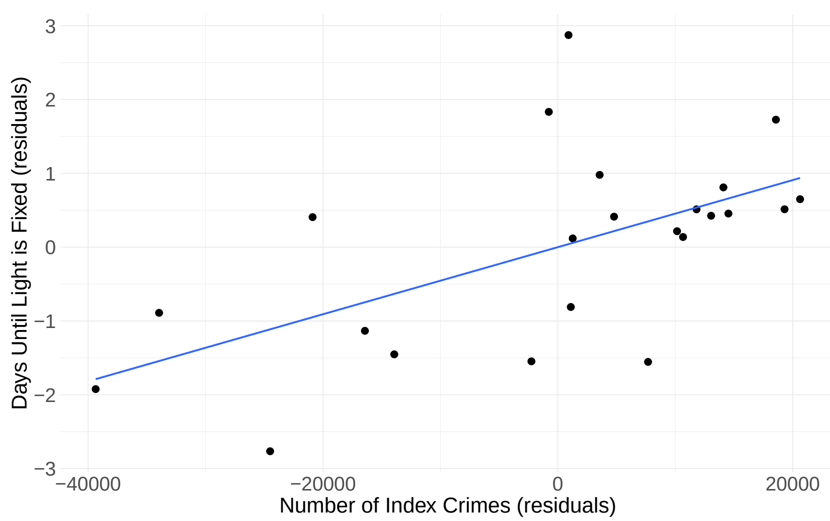
Note: Figure contains a kernel density plot of the duration of street light outages. Duration is measured as the number of days between the initial reported outage and the date that the outage was repaired by municipal workers. Because outages may be reported sometime after they occur, measured duration is likely an underestimate of the actual duration of an outage. The mean outage duration in the data is 9.3 days; the median duration is 3 days. 90 percent of outages are resolved within 21 days.

Figure 4: Relationship Between Street Light Outages and Neighborhood Crime

Panel A: Number of Street Light Outages



Panel B: Duration of Street Light Outages



Note: Figures plot the relationship between police district-level crimes and the number (Panel A) and duration (Panel B) of street light outages. In each figure, we plot number of index crimes on the x-axis and the street light outage measure on the y-axis. All measures are residualized to net out the number of street segments in each police district.

Table 1: Summary Statistics

	Total	**First Half of Time Period**	**Second Half of Time Period**	**Top 50% Safest Police Districts**	**Bottom 50% Safest Police Districts**	**Weekdays**	**Weekends**
Number of Outages	0.84	0.43	0.42	0.81	0.86	0.72	0.13
Outage Days	7.85	3.66	4.19	7.45	8.08	6.75	1.1
Index Crimes	3.85	2.09	1.75	2.97	4.71	2.74	1.11
Violent Crimes	1.62	0.85	0.77	1.29	1.96	1.11	0.52
Robbery	0.25	0.13	0.12	0.19	0.31	0.18	0.07
Assault	1.56	0.82	0.74	1.23	1.88	1.06	0.49
Property	2.33	1.27	1.06	1.8	2.85	1.66	0.67
Motor Vehicle Theft	0.28	0.16	0.12	0.23	0.32	0.2	0.08

Table 2A: Main Results — Major Street Light Outages: Index, Violent and Property Crimes

	Index		**Violent Crime**		**Property Crime**	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.00006	0.00057*	0.00001	0.00015	0.00004	0.00042*
Se($\hat{\beta}$)	0.00008	0.00024	0.00005	0.00015	0.00007	0.00021
Control mean	0.00338	0.01965	0.00139	0.00799	0.00224	0.013
Effect size	1.7%	2.9%	1%	1.9%	2%	3.3%
[CI]	[-3.1%, 6.5%]	[0.5%, 5.3%]	[-6.6%, 8.5%]	[-1.8%, 5.7%]	[-3.9%, 7.9%]	[0.1%, 6.5%]

(a) **Outdoor-Nighttime Crimes**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.00003	0.00026	-0.00001	0.00011	0.00006	0.00029
Se($\hat{\beta}$)	0.00008	0.00025	0.00005	0.00016	0.00007	0.00020
Control mean	0.00371	0.02177	0.00154	0.00891	0.00241	0.01423
Effect size	-0.7%	1.3%	-0.7%	1.3%	2.5%	2.1%
[CI]	[-5.2%, 3.7%]	[-1.1%, 3.6%]	[-7.6%, 6.2%]	[-2.4%, 4.9%]	[-3.1%, 8.1%]	[-0.8%, 5%]

(b) **Outdoor-Daytime Crimes**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.00007	-0.00012	0.00002	0.00003	0.00003	-0.00002
Se($\hat{\beta}$)	0.00007	0.00023	0.00005	0.00016	0.00005	0.00016
Control mean	0.00291	0.01714	0.00163	0.00947	0.0014	0.00858
Effect size	2.4%	-0.7%	1.4%	0.3%	2.1%	-0.2%
[CI]	[-2.6%, 7.3%]	[-3.3%, 1.9%]	[-5.3%, 8%]	[-3.1%, 3.7%]	[-5.1%, 9.4%]	[-3.8%, 3.4%]

(c) **Indoor-Nighttime Crimes**

Table 2B: Main Results - Major Street Light Outages: Robbery, Assault, and Motor Vehicle Theft

	Robbery		Assault		Motor Vehicle Theft	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.00001	0.00020*	0.00001	0.00016	0.00005	0.00018
Se($\hat{\beta}$)	0.00003	0.00009	0.00005	0.00015	0.00003	0.00009
Control mean	0.00049	0.00294	0.00133	0.00767	0.00054	0.00302
Effect size	2.1%	7%	0.8%	2.1%	9.2%	6%
[CI]	[-10.9%, 15.2%]	[0.6%, 13.4%]	[-7%, 8.5%]	[-1.8%, 6%]	[-2.2%, 20.6%]	[-0.1%, 12.1%]

(a) **Outdoor-Nighttime Crimes**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.00003	0.00005	-0.00001	0.00011	0.00002	0.00001
Se($\hat{\beta}$)	0.00003	0.00008	0.00005	0.00016	0.00003	0.00009
Control mean	0.00037	0.00227	0.0015	0.00864	0.00046	0.00273
Effect size	-8.5%	2.4%	-0.9%	1.3%	4.3%	0.4%
[CI]	[-22.7%, 5.7%]	[-4.8%, 9.5%]	[-7.9%, 6.1%]	[-2.5%, 5%]	[-9.1%, 17.6%]	[-6.4%, 7.2%]

(b) **Outdoor-Daytime Crimes**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.00000	0.00000	0.00002	-0.00003	-	-
Se($\hat{\beta}$)	0.00001	0.00004	0.00005	0.00016	-	-
Control mean	0.00009	0.00048	0.00155	0.00898	-	-
Effect size	4.9%	-0.7%	1.5%	-0.3%	-	-
[CI]	[-24%, 33.8%]	[-16.1%, 14.7%]	[-5.4%, 8.3%]	[-3.8%, 3.2%]	-	-

(c) **Indoor-Nighttime Crimes**

Table 3A: Main Results — Minor Street Light Outages: Index, Violent and Property Crimes

	Index		**Violent Crime**		**Property Crime**	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.00002	-0.00001	0.00003	0.00017	-0.00009	-0.00013
Se($\hat{\beta}$)	0.00009	0.00027	0.00006	0.00016	0.00007	0.00021
Control mean	0.00278	0.01632	0.00113	0.0064	0.00188	0.01104
Effect size	-0.6%	-0.1%	2.3%	2.7%	-4.6%	-1.2%
[CI]	[-6.7%, 5.6%]	[-3.2%, 3.1%]	[-7.1%, 11.7%]	[-2.2%, 7.6%]	[-12.2%, 2.9%]	[-4.9%, 2.5%]

(a) **Outdoor-Nighttime Crimes**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.00001	0.00001	0.00003	0.00014	-0.00001	-0.00016
Se($\hat{\beta}$)	0.00009	0.00026	0.00006	0.00016	0.00007	0.00022
Control mean	0.00302	0.01735	0.0013	0.00722	0.00201	0.0115
Effect size	-0.5%	0.1%	2.4%	2%	-0.4%	-1.3%
[CI]	[-6.6%, 5.6%]	[-2.8%, 2.9%]	[-7%, 11.8%]	[-2.5%, 6.5%]	[-7.7%, 6.9%]	[-5%, 2.3%]

(b) **Outdoor-Daytime Crimes**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.00007	0.00011	-0.00004	0.00009	0.00002	-0.00003
Se($\hat{\beta}$)	0.00009	0.00024	0.00007	0.00019	0.00006	0.00019
Control mean	0.0027	0.015	0.00146	0.00817	0.00133	0.00759
Effect size	2.6%	0.7%	-2.9%	1%	1.8%	-0.4%
[CI]	[-3.7%, 8.8%]	[-2.4%, 3.8%]	[-11.6%, 5.8%]	[-3.5%, 5.5%]	[-7.1%, 10.7%]	[-5%, 4.2%]

(c) **Indoor-Nighttime Crimes**

Table 3B: Main Results — Minor Street Light Outages: Index, Violent and Property Crimes

	Robbery		**Assault**		**Motor Vehicle Theft**	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.00001	0.00005	0.00003	0.00022	-0.00002	-0.00003
Se($\hat{\beta}$)	0.00003	0.00010	0.00005	0.00016	0.00003	0.00009
Control mean	0.00038	0.00243	0.00109	0.00615	0.00041	0.00232
Effect size	-3.6%	2.2%	2.9%	3.6%	-4.3%	-1.3%
[CI]	[-19.2%, 12%]	[-5.6%, 10%]	[-6.6%, 12.5%]	[-1.5%, 8.6%]	[-20.1%, 11.5%]	[-9.2%, 6.5%]

(a) **Outdoor-Nighttime Crimes**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.00003	0.00004	0.00002	0.00010	0.00002	0.00010
Se($\hat{\beta}$)	0.00003	0.00008	0.00006	0.00016	0.00003	0.00008
Control mean	0.00027	0.00163	0.00125	0.00693	0.00037	0.002
Effect size	-12%	2.8%	1.7%	1.4%	4.5%	5.1%
[CI]	[-32%, 8%]	[-7.4%, 12.9%]	[-7.8%, 11.3%]	[-3.2%, 6%]	[-12.6%, 21.7%]	[-3.4%, 13.6%]

(b) **Outdoor-Daytime Crimes**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.00001	-0.00004	-0.00002	0.00013	-	-
Se($\hat{\beta}$)	0.00001	0.00004	0.00006	0.00018	-	-
Control mean	0.00007	0.00045	0.00139	0.00773	-	-
Effect size	-13.9%	-9.1%	-1.3%	1.7%	-	-
[CI]	[-50.5%, 22.7%]	[-26.6%, 8.4%]	[-10.2%, 7.6%]	[-2.9%, 6.4%]	-	-

(c) **Indoor-Nighttime Crimes**

Table 4: Estimated Treatment Effects by Commercial Density (Nighttime Outdoor Crimes, Major Outages)

	Index		Violent Crime		Property Crime	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
Main Effect (Low Density)						
$\hat{\beta}$	0.00022*	0.00073*	0.00010	0.00013	0.00015	0.00061*
Se($\hat{\beta}$)	0.00011	0.00032	0.00010	0.00020	0.00009	0.00027
Interaction						
$\hat{\beta}$	-0.00033*	-0.00032	-0.00017	0.00005	-0.00021	-0.00039
Se($\hat{\beta}$)	0.00016	0.00047	0.00010	0.00030	0.00013	0.00042

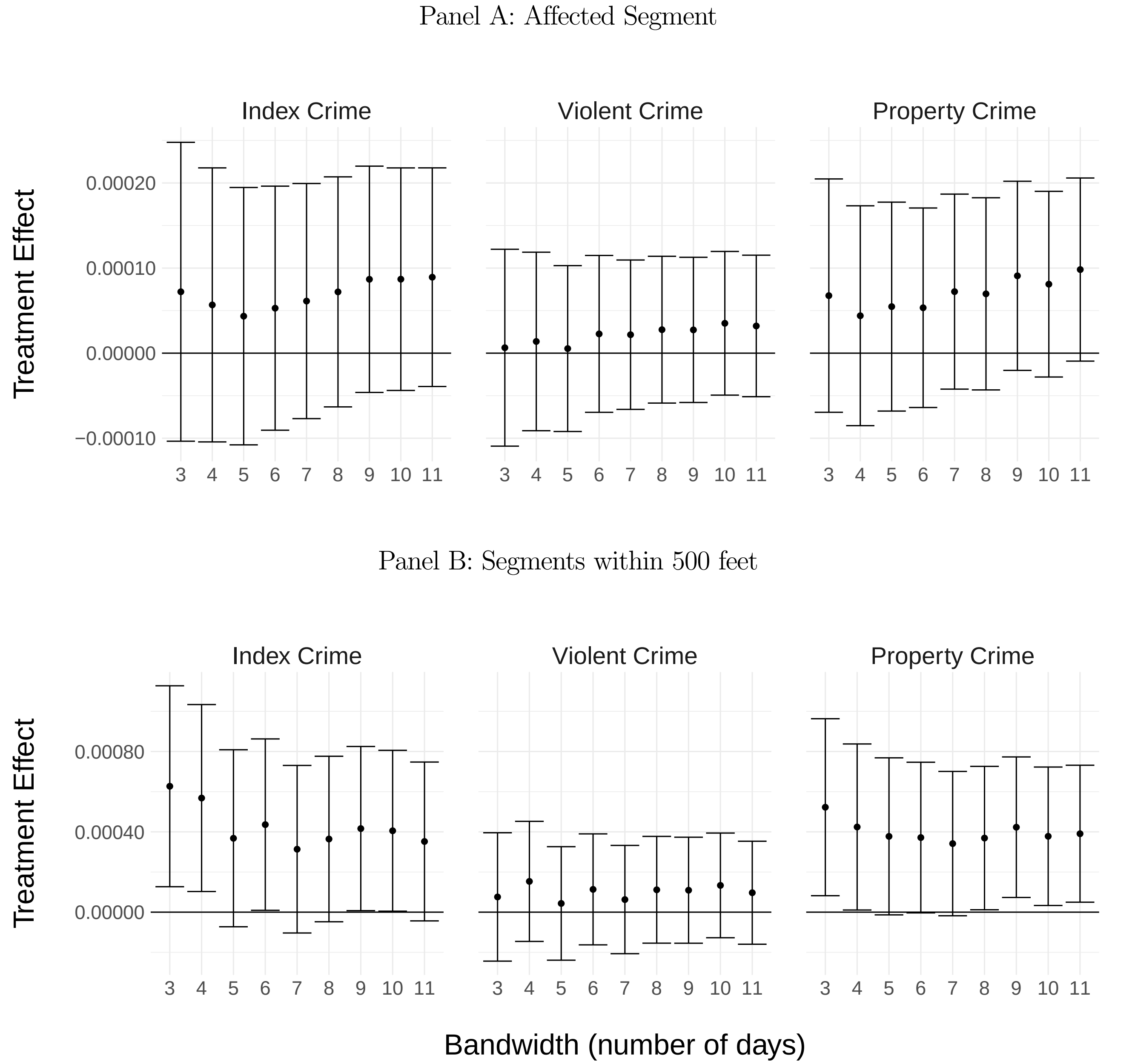
	Robbery		Assault		Motor Vehicle Theft	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
Main Effect (Low Density)						
$\hat{\beta}$	0.00001	0.00030*	0.00008	0.00015	0.00004	0.00018
Se($\hat{\beta}$)	0.00004	0.00012	0.00007	0.00020	0.00004	0.00012
Interaction						
$\hat{\beta}$	-0.00000	-0.00020	-0.00015	0.00002	0.00001	-0.00001
Se($\hat{\beta}$)	0.00006	0.00018	0.00010	0.00030	0.00006	0.00018

Table 5: Estimated Treatment Effects, Day of Outage Excluded (Nighttime Outdoor Crimes, Major Outages)

	Index		**Violent Crime**		**Property Crime**	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.00005	0.00054*	0.00001	0.00013	0.00006	0.00046*
Se($\hat{\beta}$)	0.00008	0.00024	0.00005	0.00015	0.00007	0.00021
Effect size	1.6%	2.8%	0.8%	1.7%	2.6%	3.6%
[CI]	[-3.2%, 6.5%]	[0.4%, 5.2%]	[-6.8%, 8.4%]	[-2.1%, 5.5%]	[-3.4%, 8.5%]	[0.3%, 6.9%]

	Robbery		**Assault**		**Motor Vehicle Theft**	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.00000	0.00016	0.00001	0.00015	0.00005	0.00019*
Se($\hat{\beta}$)	0.00003	0.00009	0.00005	0.00015	0.00003	0.00009
Effect size	0.2%	5.7%	0.5%	1.9%	9.8%	6.7%
[CI]	[-12.8%, 13.3%]	[-0.8%, 12.2%]	[-7.3%, 8.3%]	[-2%, 5.8%]	[-1.8%, 21.4%]	[0.5%, 12.9%]

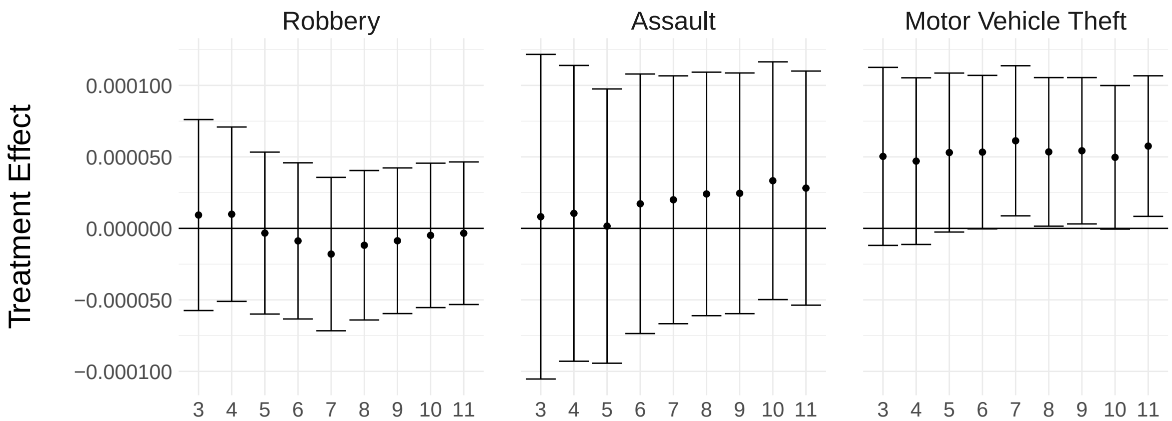
Appendix Figure 1A: Robustness
of Estimated Treatment Effects to Post-Period Bandwidth Selection: Index, Property and Violent Crimes



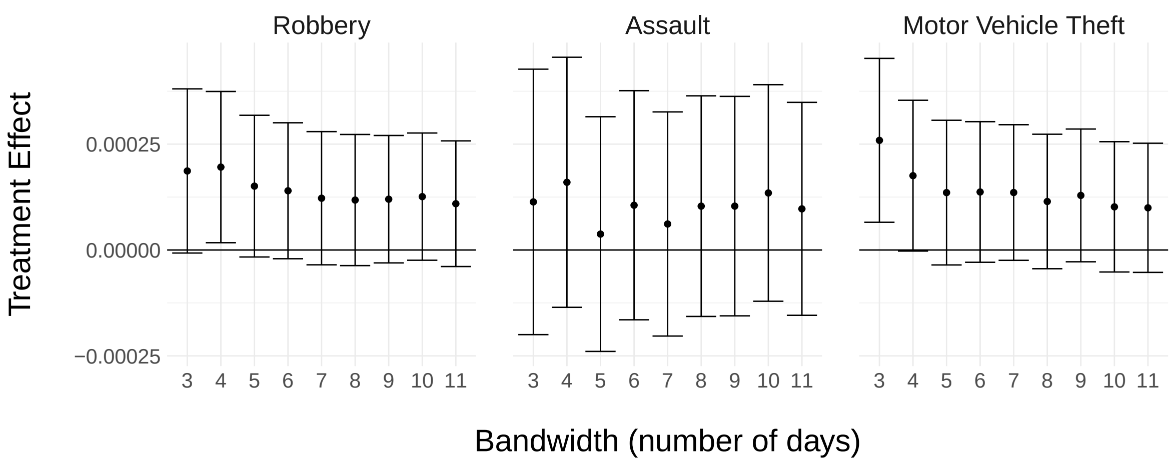
Note: Figures test the robustness of our results to bandwidth selection, varying the length of the post-repair bandwidth to be between 3 and 11 days. Panel A presents estimates for the street segment affected by major street light outages; Panel B presents estimates for street segments within 500 feet of the affected segment. For each of the selected bandwidths, we plot the point estimate as well as the associated 95 percent confidence interval.

Appendix Figure 1B:
Robustness of Estimated Treatment Effects to Post-Period Bandwidth Selection: Individual Crime Types

Panel A: Affected Segment



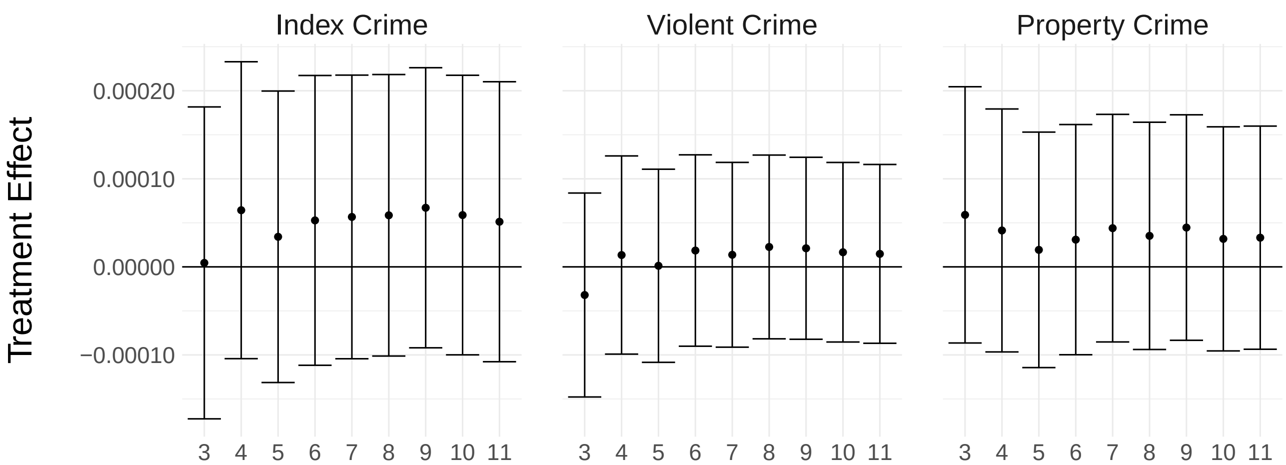
Panel B: Segments within 500 feet



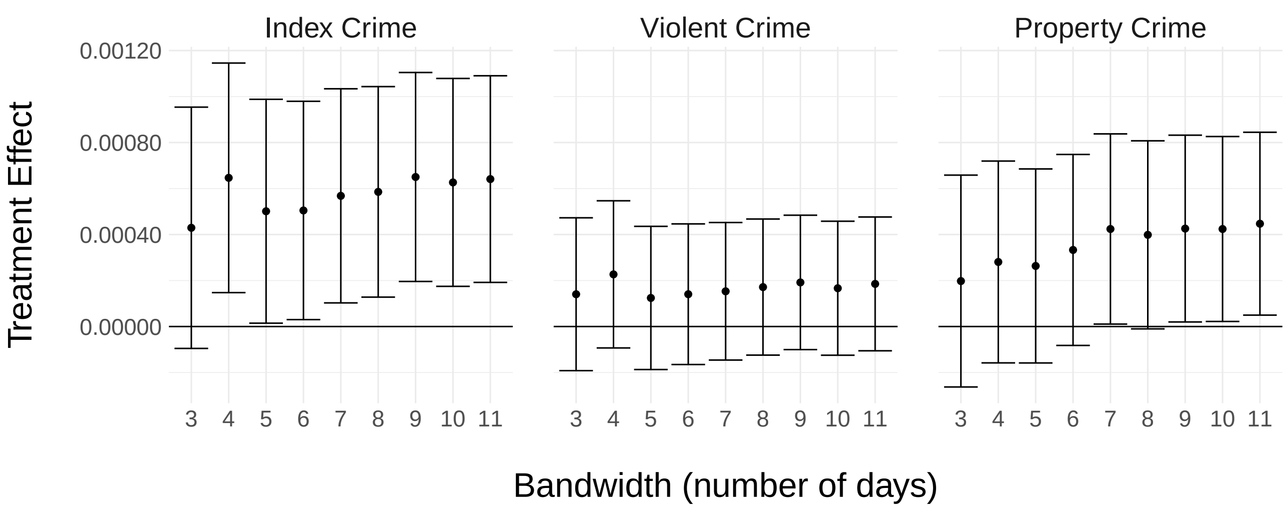
Note: Figures test the robustness of our results to bandwidth selection, varying the length of the post-repair bandwidth to be between 3 and 11 days. Panel A presents estimates for the street segment affected by major street light outages; Panel B presents estimates for street segments within 500 feet of the affected segment. For each of the selected bandwidths, we plot the point estimate as well as the associated 95 percent confidence interval.

Appendix Figure 2A: Robustness
of Estimated Treatment Effects to Pre-Period Bandwidth Selection: Index, Property and Violent Crimes

Panel A: Affected Segment



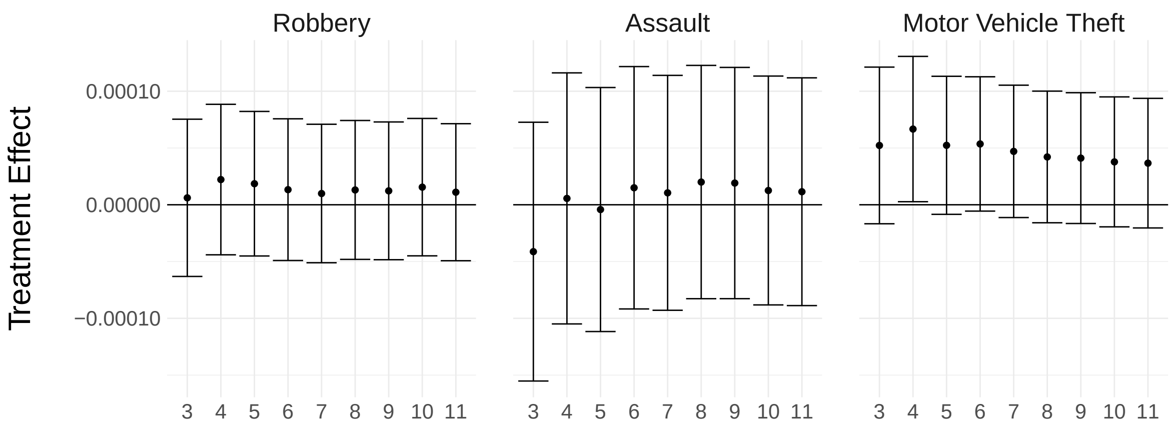
Panel B: Segments within 500 feet



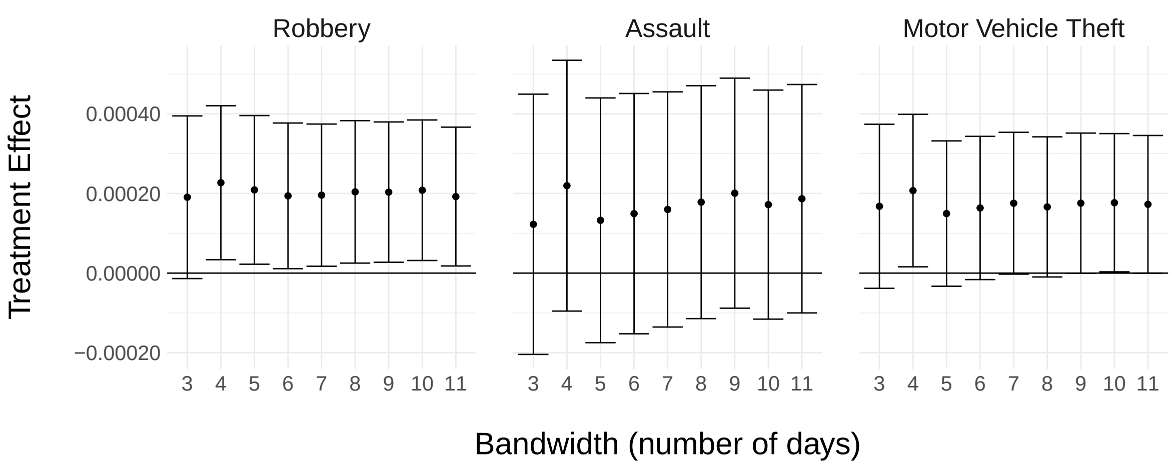
Note: Figures test the robustness of our results to bandwidth selection, varying the length of the pre-repair bandwidth to be between 3 and 11 days. Panel A presents estimates for the street segment affected by major street light outages; Panel B presents estimates for street segments within 500 feet of the affected segment. For each of the selected bandwidths, we plot the point estimate as well as the associated 95 percent confidence interval.

Appendix Figure
2B: Robustness of Estimated Treatment Effects to Pre-Period Bandwidth Selection: Individual Crime Types

Panel A: Affected Segment



Panel B: Segments within 500 feet



Note: Figures test the robustness of our results to bandwidth selection, varying the length of the post-repair bandwidth to be between 3 and 11 days. Panel A presents estimates for the street segment affected by major street light outages; Panel B presents estimates for street segments within 500 feet of the affected segment. For each of the selected bandwidths, we plot the point estimate as well as the associated 95 percent confidence interval.

Appendix Table 1A: Chicago Poisson: Index, Violent, and Property

	Index		**Violent Crime**		**Property Crime**	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.01687	0.02900*	0.00852	0.01749	0.01974	0.03294*
Se($\hat{\beta}$)	0.02443	0.01213	0.03811	0.01894	0.02965	0.01626

(a) **Major Street Light Outages**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.00688	-0.00144	0.02313	0.02534	-0.04677	-0.01183
Se($\hat{\beta}$)	0.03115	0.01609	0.04762	0.02471	0.03808	0.01891

(b) **Minor Street Light Outages**

Appendix Table 1B: Chicago Poisson: Robbery, Assault, and Motor Vehicle Theft

	Robbery		**Assault**		**Motor Vehicle Theft**	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.02200	0.06955*	0.00617	0.01948	0.09124	0.05985
Se($\hat{\beta}$)	0.06573	0.03234	0.03910	0.01950	0.05759	0.03078

(a) **Major Street Light Outages**

	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	-0.03749	0.02125	0.02922	0.03395	-0.04290	-0.01318
Se($\hat{\beta}$)	0.07878	0.03949	0.04826	0.02538	0.07967	0.03986

(b) **Minor Street Light Outages**