Street Light Outages, Public Safety and Crime Attraction*

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Abstract

Objectives: For more than one hundred years, street lighting has been one of the most ubiquitous capital investments in public safety. Prior research on street lighting is largely limited to ecological studies of very small geographic areas, creating substantial challenges with respect to both causal identification and statistical power. We address limitations of the prior literature by studying a natural experiment created by short-term disruptions to municipal street lighting.

Methods: We leverage a natural experiment created by the differential timing of the repair of nearly 300,000 street light outages in Chicago. By conditioning on street segment fixed effects and focusing on a short window of time around the repair of a street light outage, we can credibly rule out confounding factors due to area-specific time trends as well as street segment-level correlates of crime.

Results: We find that outdoor nighttime crimes change very little on street segments affected by street light outages, but that outages cause crime to spill over to nearby street segments. Effects are largest for robberies and motor vehicle theft.

Conclusions: Despite strong environmental and social characteristics that tend to tie crime to place, we observe that street light outages are sufficiently salient to disrupt longstanding patterns. While the impact of localized street light outages can reverberate throughout a community, the findings imply that improvements in lighting can be defeated by the displacement of crime to adjacent spaces and therefore do not necessarily suggest that localized investments in municipal street lighting will yield a large public safety dividend.

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

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1 Introduction

Given concerns about the net-widening effects of the intensive use of police patrols (Weitzer et al., 2008) and the expanded use of incarceration (Pfaff, 2017), policymakers have expressed an interest in alternative strategies to reduce crime which minimize the unintended costs of a law enforcement-driven approach to crime prevention. One of the strategies in which cities have expressed renewed interest is changing the nature of public space. Such an approach is informed by situational crime prevention and its theoretical antecedent, crime prevention through environmental design (CPTED) which seeks to leverage changes to the physical environment to influence criminal decision-making (Jacobs, 1961; Newman, 1972; Clarke, 1983, 1995; Cozens et al., 2005; Robinson, 2013; Cozens and Love, 2015). This strategy 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 place-based 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), changing the design of housing (Armitage et al., 2011) and securing abandoned buildings and addressing abandoned properties (Branas et al., 2018) can lead to important reductions in crime and disorder (MacDonald et al., 2019).

Like disorder reduction, street lights are widely thought to be an effective tool in reducing crime and therefore have become a 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 (Atkins et al., 1991; Herbert and Davidson, 1994; Painter, 1994, 1996). The available evidence on street lighting suggests that its impact on crime is promising, reducing crime by, on average, 20 percent (Welsh and Farrington, 2008). These impacts are especially encouraging given the relatively low cost of maintaining street lights as compared to other crime control interventions like CCTV (Armitage, 2002; Piza et al., 2019). Likewise, improving street lighting requires only minimal technical know-how, indicating that there is extraordinary promise in further scaling street lighting in many jurisdictions. However, with one recent exception, a field experiment conducted in New York City's public housing communities by Chalfin et al. (2020), the available evidence is based on observational studies that rely on relatively simple comparisons of very small samples. As a result, despite a number of positive research findings, 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 expanded street lighting by public works professionals. Based on these concerns, a 1997 National Institute of Justice report to the U.S. Congress, written after the lion's share of the extant literature on street lighting was completed, concludes that "we can have very little confidence that improved lighting prevents crime" (Sherman et al., 1997).

In this paper, we study 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. Street light outages provide an informative natural experiment for several reasons. First, as virtually all cities use street lighting, 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 data available in Chicago allow us to differentiate between major street light outages involving more than two street lights and more minor outages. Accordingly, we are able to consider the impact of different dosages of lighting, a feature which has been hypothesized to be of critical importance but which is not commonly addressed in the literature (Painter and Farrington, 2001).

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. The volume of data generated by this natural experiment is extraordinarily large, allowing us to estimate the effectiveness of lighting in controlling crime with considerable precision. We also have a sufficient number of observations to identify crime spillovers, 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 (Weisburd et al., 2006; Johnson et al., 2014).

We find evidence that outdoor nighttime crimes are sensitive to street light conditions, albeit in subtle ways. 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 that most types of crimes — and robberies in particular — increase on adjacent street segments. This type of "crime attraction" is consistent with the idea that a decrease in ambient lighting has the effect of re-allocating both potential victims and would be offenders to better-lit areas. 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 (Cozens et al., 2005) but has been difficult to detect empirically due to constraints on statistical power in most empirical applications.

These findings have a number of implications for our understanding of both offender behavior and public policy. First, while crime tends to be highly concentrated (Sherman et al., 1989; Farrell, 2015; Weisburd, 2015) and crime hot spots tend to persist over time (Weisburd et al., 2004), our findings suggest that changes in ambient lighting are sufficiently salient to shift crime even in the presence of strong environmental and social characteristics that tend to tie crime to place (Weisburd and Green, 1994; Weisburd et al., 2006). Relatedly, evidence from many place-based interventions suggests that criminal opportunities are not indiscriminately spread throughout urban areas and therefore that offenders are often "coupled to place" (Weisburd et al., 2006). While the majority of the recent literature finds that displacement is "seldom total and often inconsequential" (Barr and Pease, 1990; Guerette and Bowers, 2009; Weisburd et al., 2006), in contrast, this research suggests that the availability of better-lit, familiar terrain that is only a short distance away leads to a re-allocation of crime. While this research does not necessarily challenge the general empirical regularity that offenders are coupled to place, it does suggest that public safety interventions which are particularly salient for victims have the potential to de-couple offenders from place by re-distributing criminal opportunities to nearby areas. An alternative possibility is that street light outages affect only a small portion of an individual's offending radius and so it may be relatively costless for an offender to shift his or her attention to an adjacent street. The observation that an offender's area of operation is likely to be larger than a single street segment has informed prior place-based research which often focuses on a small collection of blocks (Weisburd et al., 2006).

Finally, while our findings suggest that rapid street light repairs may help to maintain public safety, in contrast to recent discussions in the extant literature (Doleac and Sanders, 2015; Chalfin et al., 2020), our findings do not necessarily suggest that large-scale investments in municipal street lighting will yield a large public safety dividend. Indeed our principal finding — that a loss of lighting causes crime to shift to nearby areas — implies that discrete improvements in lighting can be defeated by crime spillovers to adjacent spaces. This finding potentially rationalizes the observation that crime hot spots tend to have more, not less, ambient lighting than other areas in a city (Sherman and Weisburd, 1995; Weisburd et al., 2014). An alternative and noteworthy policy implication is that lighting may be most effective when it is spread relatively evenly across areas within a community thus providing fewer opportunities for crime to "move around the corner."

2 Background

2.1 Ambient Lighting

Street lighting has been around, in one form of another, for millennia.[1] Street lights are thought to have been introduced in the United States by Benjamin Franklin, who designed his own candle-based street light in Philadelphia, as early as 1757. Newport, RI become the first U.S. city to introduce gas lighting in 1803 (Stinson, 2018) and, after the invention of the electric light bulb, Wabash, IN became the first U.S. city to use electric street lighting in 1880 (Tocco, 1999). Today, street lights can be found in every city in the United States and throughout the rest of the developed and developing world.

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

[1]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).

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 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. 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, 1994, 1996; Chalfin et al., 2020) 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).[2] 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.

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

2.2 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 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.[3] Critically though, the evidence is mixed and the utility of past research is hampered by a number of important 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.[4] Of the thirty-two prior studies identified by Welsh and Farrington (2008), nineteen do not employ 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 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 municipal officials 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 (Farrington and Welsh, 2002; Doleac and

[3] 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.

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

Sanders, 2015; Domínguez and Asahi, 2017). As a result, despite a plethora of suggestive positive 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, written after much of the extant literature was completed, 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 a biased treatment effect in the presence of spatial spillovers.

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 of little use or are entirely unreported limiting our ability to understand the extent to which estimated treatment effects could be due to random chance. Statistical power is a first order issue not only for identifying sufficiently precise treatment effects but also for identifying the magnitude of any resulting crime displacement.

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 difference-in-differences and regression discontinuity approach, they find evidence that increased evening light reduces crime, particularly robbery.[5] While their findings suggest an important role for

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

ambient lighting, further evidence remains critically important as an hour of additional sunlight is a fundamentally different treatment than artificial lighting provided by enhanced street lighting.

The lone field experiment in the literature was conducted by Chalfin et al. (2020) who study the random allocation of temporary street lights to thirty-nine public housing developments in New York City, finding 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. 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 a larger sample size.

2.3 Displacement and Crime Attraction

In studying any place-based intervention that might impact public safety, a critical question is whether the intervention has reduced crime or has merely displaced it to other areas (Reppetto, 1976; Cornish and Clarke, 1987; Eck, 1993), or to other time periods, targets, tactics or offenses (Guerette and Bowers, 2009). While both crime reduction and spatial 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 (Weisburd et al., 2006).[6] The conventional approach to studying spatial displacement is to examine whether an intervention leads to a rise in crime in adjacent areas.[7] 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

[6]Because of its central importance in interpreting empirical estimates, testing for spatial 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). Of course, displacement can also take the form of crime, target or tactical "switch" (Johnson et al., 2014) and can also include temporal displacement.

[7]Measuring spatial 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" (Weisburd et al., 2006; 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 spatial displacement, the norm in the empirical literature is to focus on adjacent areas (Guerette and Bowers, 2009).

and Weisburd, 1994; Weisburd et al., 2006; Guerette and Bowers, 2009).[8] What crime reduction and displacement effects have in common is that they are both behavioral responses of potential offenders to an intervention. Critically, the degree to which crimes are actually shifted by place-based shocks will depend on the extent to which offenders are "coupled" to places which have physical, social or demographic features that are familiar or convenient (Weisburd et al., 2006). While Weisburd et al. (2006) finds qualitative evidence in favor of strong coupling effects, we note that, in our context, street light outages have the potential to shift crime around the corner without measurably altering at least some of the attractive conditions offered by a familiar neighborhood.

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, a reduction in ambient light might be expected to lead to an *increase* in crime. As a result, street lights that are nonoperational might be expected to be a "crime attractor" (Bernasco and Block, 2011; Brantingham and Brantingham, 2013; Brantingham et al., 2017), pulling offenders into an area that was previously lit. A second issue is that, given the importance that communities seem to place on the availability of street lighting (Painter, 1996; Chalfin et al., 2020), we might expect the behavior of potential victims to be especially sensitive to a lighting outage relative to an intervention which is more offender-focused like hot spots policing (Cozens et al., 2005; Short et al., 2010; Cozens and Love, 2009, 2015).[9] To the extent that the shock to public safety re-allocates potential victims from the treated to the untreated area, crime can potentially decline in treated areas 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.

3 Empirical Strategy

In order to study the impact of nighttime ambient lighting, 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

[8] The review by Guerette and Bowers (2009) finds little evidence of either spatial displacement or diffusion of benefits in most applications. However, constraints on statistical power mean that displacement is not always detectable even when it exists.

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

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. 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, for a variety of reasons, outages may not be immediately reported to local authorities. To address this concern, 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 up to the one-week period prior to the resolution of a street light outage and the four-day period after the outage's resolution, comparing crimes during the post-repair period to the pre-repair period. While the length of the post-repair period in our primary models is fixed at four days, we allow the pre-repair period to be anywhere between one and seven days in duration, depending upon the number of days that pass between the reporting and the repair of the outage. A visual schematic of our research design can be found in **Figure 1**.

We study outages at the street segment-by-day level using the following difference-in-differences equation which we estimate using a small number of days that are temporally proximate to the repair of a street light outage:

$$Y_{it}=\alpha+\beta D_{it}+\phi_i+\Sigma_{j=1}^{6}DOW_{ijt}+\varepsilon_{it} \tag{1}$$

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 use 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 on segments that generally experience different numbers of crimes or street light outages. Notably, as we focus on a very short time window around the repair of the outage, the fixed effects subsume all block- and community-level correlates of crime that do not vary from day to day. The fixed effects also assure us that we are not comparing crimes on street segments which are exposed to outages of different durations. Second, given that both crimes and the repair of street light outages may fluctuate throughout the week (Cohen and Felson, 1979) we condition on a set of day-of-week dummy variables, DOW_{it}, to account for the possibility that light outages are differentially likely to be resolved 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 also 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.[10] As such, we are able to estimate the impact of ambient lighting under two different treatment intensities. Equation (1) is likewise estimated separately for outdoor nighttime crimes, as well as outdoor daytime crimes and nighttime indoor crimes as placebo checks.

Next, 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 that occur on a street that is 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 over time 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

4.1 Street Light Outages

Data on street light outages are derived 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

[10] 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." More granular information on the precise number of street lights out is not available.

through the city's 311 system website.[11] 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 affects a light in a street or in an alley, and if the light is "completely out" rather than "going on and off."[12] 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.[13] 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).[14] To test for non-linear effects of street lighting, we analyze minor (1-2 lights) and major (more than two lights) outages separately.[15] While this "dosage" variable allows us to study the effects of street light outages under two different intensities of lighting loss, unfortunately we cannot observe the total number of street lights per block. As such, we are not able to directly observe the amount of light lost per block per outage. To the extent that there is slippage between the number of light outages and the amount of ambient lighting lost, so long as these errors are unrelated to the timing of the outage, any resulting bias will be conservative.

We merge these data to Chicago's street center lines shapefile in order to assign street lights to a given street segment.[16] Using the reported latitude and longitude of the reported outage, we created a 50-foot buffer around

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

[12] Among major outages, which is where we observe crime effects, we are told by municipal officials in Chicago that light outages are almost all "completely out." Among minor street light outages, approximately 1 in 5 complaints is for lights "going on and off." The administrative data unfortunately does not distinguish between the two types of outages. Hence, to the degree that 20 percent of these minor outages provides only minimal treatment, our estimates could potentially be attenuated. Happily, we can back out the degree of attenuation under the assumption that flickering lights have no treatment effect. Assuming that 80 percent of the minor outages are, in fact, treated, estimates would be attenuated by a factor of $\frac{1}{0.8} = 1.25$. As the estimated effects are extraordinarily small, this will not substantively affect our estimates. For example, for index crimes arising from a minor light outage, the estimated treatment effect is -0.1 percent. For robbery, it is 2.2 percent. Multiplying these estimates by 1.25, yields estimates of -0.125 percent and 2.75 percent, respectively. These differences are substantively very small and are more than consistent with sampling error. As such, even if we make the assumption that "flickering outages" are associated with no effects, our estimates would not be substantively different.

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

[14] 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.

[15] Approximately 4 percent of post-repair periods experience a new outage and, as such, are exposed to the treatment. In order to avoid introducing post-treatment bias into our models, we follow Chalfin et al. (2020) and report intention-to-treat effects which evaluate the effect of an initial outage and, as such, are if anything conservatively estimated.

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

each segment and used the coordinates from the outages data to determine on which segment each street light outage occurred.[17] In approximately 0.3% of cases there was no match to any segment; 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 (e.g., 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).[18] We use this variable to determine whether the crime occurred indoors or outdoors.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. 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. 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. The available data does not permit us to examine the crime of theft from motor vehicle separately from total thefts. We provide a separate analysis of robbery, assaults and motor vehicle theft, three

[17] The choice of a 50-foot buffer is common in the empirical literature that rely on the geocoding of crimes to blocks or street segments (e.g., (Ratcliffe, 2012).

[18] The dataset contains two variables with a time related to the incident: the time the crime was reported and when the incident report was updated by the police. For this study we use the time when the crime was reported to the police, not the update time.

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 not present. 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." [19] Data on civil twilight 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.[20] As the crime data does not always include the minute that the crime occurred, we consider a crime to occur at night if it happens on the hour that civil twilight begins or after it ends. In the next section of the paper, we address potential concerns about the quality of these data.

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 Measurement Issues

In this section, we briefly address a potential concern arising from the inevitable errors that exist in administrative data. In particular, we note that precise timestamps on crimes in public crime microdata can be noisy (Felson and Poulsen, 2003). Consequently, it is possible that some nighttime crimes, discovered during daytime hours or reported during daytime hours when it is more convenient to do so, could be reported in the data as daytime crimes (Chalfin et al., 2020). To the extent that this is true, a portion of the crime reduction observed at night could be re-distributed to the daytime. What effect might this issue have on our estimates? We begin by noting that persistent time stamp errors that are unrelated to street light outages will, in expectation, increase our standard errors but will not bias

[19] See: `http://aa.usno.navy.mil/faq/docs/RST_defs.php`.

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

our estimates as such errors would be equally likely to occur in our treatment and comparison conditions (Bound et al., 2001). On the other hand, errors in time stamps will, in fact, bias our estimates if data errors are differentially likely during the time in which a street light is out. For example, if crimes are less likely to be discovered in the darkness, street light outages could mechanically redistribute crimes known to the police from nighttime to daytime hours, creating bias in our estimates. For several reasons, we believe this issue is unlikely to hold in our data. First, as we demonstrate in Section 5.1, we do not, in fact, observe an increase in daytime crimes during street light outages. Second, we observe the largest effects for person crimes like robbery and assault, crimes which presumably have more accurate time stamps than property crimes. Nevertheless, we note that this form of measurement error, if it exists, would bias our estimates downwards, if anything making our reported results conservative.

4.4 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 do 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 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. In **Figure 2**, we provide a visual representation of the distribution of the duration of street light. 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.[21] 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.

5 Results

5.1 Main Results

Our primary regression results are presented in **Table 2A** and **Table 2B** which consider the effect of major light outages (involving more than two street lights) on crime and **Table 3A** and **Table 3B** which consider the impact of minor street light outages (involving two or less 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 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 models (see **Appendix Table 1A** and **Appendix Table 1B**).

Table 2A considers the effect of major street light outages on index crimes, violent crimes, and property crimes.

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

There is a small statistically insignificant increase in index crimes (2 percent), 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). In Panel B, we consider whether daytime outdoor crimes are responsive to street light outages. While estimates are imperfectly precise, 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, in Tables 3A and 3B, we consider whether crime patterns change in response to minor street light outages. Here we do not see evidence that crime is shifted by street light outages. 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 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).

Finally, we consider whether our estimates differ according to land use. We note that testing for heterogeneous treatment effects comes with the caveat that by comparing effects for different types of street segments, we can no longer control completely for differences in the rapidity of street light repair between different types of street segments. 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.

The chief concern with respect to identification is that the timing of a street light outage is correlated with overall 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 communities in which crime is rising. 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, even within this narrow time 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.[22] 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 outages.

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 a 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**. Referring to Table 5, we see that estimated effects are extremely similar to those reported in Tables 2A and 2B.

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,

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

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.[23]

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 a 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 Figure 1B** and **Appendix Figure 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 the figures, we see little evidence that either 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.

A final concern is that offenders might intentionally disable street lights for the express purpose of committing street crimes. While we do not have information on which lights were damaged due to vandalism, we note that our data nevertheless allows us to indirectly test this story. In particular, to the extent that offenders intentionally disable street lights prior to an intended crime spree, we would expect to see that crime is higher on the affected segment during a light outage. We do not observe evidence of such an outcome. Relatedly, we note that, to the extent that offenders disable street lights prior to an intended crime spree, we would expect crime to rise during an outage on the affected street, not in ad-

[23] 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.

jacent 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.

6 Discussion

Street lighting is one of the world's oldest and most enduring place-based 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; Doleac and Sanders, 2015). 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 nighttime ambient lighting as well as the extent to which changes in lighting conditions disrupt spatial crime patterns. Using data on nearly 300,000 street light outages spanning an eight-year period in Chicago, we document evidence that crime is sensitive to ambient lighting but that the effects operate predominantly through subtle 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 observe that crime, in general, and robberies and vehicle thefts, in particular, increase *in nearby areas*. 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; Weisburd, 2016; Chalfin and McCrary, 2018; Weisburst, 2018; Chalfin et al., 2020).

These findings have a number of implications for our understanding of both offender behavior and public policy. First, while a great deal of research establishes that crime is disproportionately concentrated among a small number of street segments in a city (Sherman et al., 1989; Brantingham and Brantingham, 1999; Farrell, 2015; Weisburd, 2015; Chalfin et al., 2020) and that crime hot spots tend to persist over time (Weisburd et al., 2004), our findings suggest that changes in ambient lighting are sufficiently salient to shift crime even in the presence of strong environmental and social characteristics that tend to tie crime to place. Thus, while our estimates are modest in magnitude, they point to lighting as a feature of the urban environment that has the ability to disrupt long-established patterns.

Relatedly, while evidence from many place-based interventions suggests that offenders are "coupled to place"

(Weisburd et al., 2006) and therefore that displacement is relatively uncommon (Barr and Pease, 1990; Guerette and Bowers, 2009; Weisburd et al., 2006), this research suggests that the availability of better-lit, familiar terrain that is only a short distance away allows for the re-allocation of human activity and therefore of criminal opportunities. Therefore, while this research does not necessarily challenge the general empirical regularity that offenders are coupled to place, it does suggest that public safety interventions which are particularly salient for victims or which allow offenders to costlessly shift to a different part of their extant offending radius have the potential to de-couple offenders from place by re-allocating the distribution of criminal opportunities to nearby areas. Importantly, our findings contrast with those from a recent field experiment by Chalfin et al. (2020) in which temporary light towers were added to NYC public housing. This experiment found strong evidence of declines in crime in treated areas and did not find evidence of displacement to adjacent areas. While public housing has a number of environmental and social characteristics that couple offenders to place (Griffiths and Tita, 2009), such characteristics may be considerably weaker for individual street segments within a community. We therefore stress that the extent to which crime will be shifted by lighting depends critically on the degree to which offenders — and victims — are coupled to micro hot spots.

These findings highlight a general principle in place-based crime research which has been suggested 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. 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. As such, we might expect offenders to spill in to a darkened area 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.

With respect to the street lighting literature specifically, we note that our analysis documents strong evidence that the *dosage* of lighting plays an outsize role in promoting public safety. While the loss of lighting from a single street light does not shift crime in Chicago, major outages which implicate more than two street lights shift street crimes to adjacent areas. These findings suggest that the relationship between lighting and crime is non-linear and that the public safety dividend of investments which increase the brightness of lighting (e.g., LEDs) is likely to depend on exactly how much lighting is added and how the new lighting shifts the location choices of both offenders and victims. While prior research has hypothesized that the dosage of lighting might be a critically important input to consider (Clarke, 2008), as is noted forcefully by Painter and Farrington (2001), more research is needed to specify the dose-response curve relating improved street lighting to reduced crimes.

While our findings indicate that rapid street light repairs may help to maintain public safety while reducing fear (Painter, 1996) and promoting active living (Roman and Chalfin, 2008; Roman et al., 2009; Lee et al., 2016), they do not necessarily suggest that large-scale investments in municipal street lighting will yield a large public safety dividend. Taken as a whole, with respect to public policy, our findings suggest that, in contrast with interventions such as hot spots policing (Weisburd et al., 2006; Braga et al., 2014), the public safety benefits of piecemeal improvements in community lighting may be, at least, partially offset by displacement. An alternative and noteworthy implication is that lighting may be most effective when it is spread relatively evenly across areas within a community, thus providing fewer opportunities for crime to "move around the corner."

A related implication of these findings is that they may help to resolve an apparent paradox in the literature. On the one hand, adding additional lighting to a community tends to improve public safety (Farrington and Welsh, 2002; Welsh and Farrington, 2008; Doleac and Sanders, 2015; Chalfin et al., 2020). On the other hand, prior research has noted that crime hot spots tend to be associated with *more* ambient lighting, not less (Sherman and Weisburd, 1995; Weisburd et al., 2012, 2014). One way to rationalize these findings is to acknowledge that while the addition of new lighting may, on net, deter crime, to the extent that additional lighting leads to a re-allocation of criminal opportunities within a given community, crime may shift to relatively well-lit areas. Accordingly, street lighting should be recognized as an intervention which has conditional public safety benefits depending on how it is deployed and operationalized.

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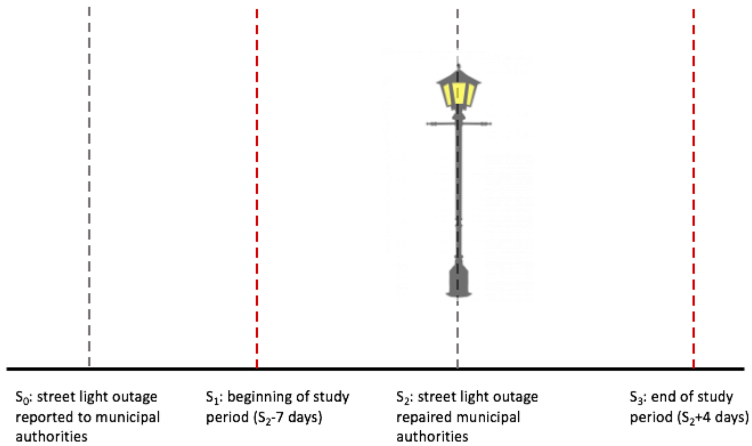
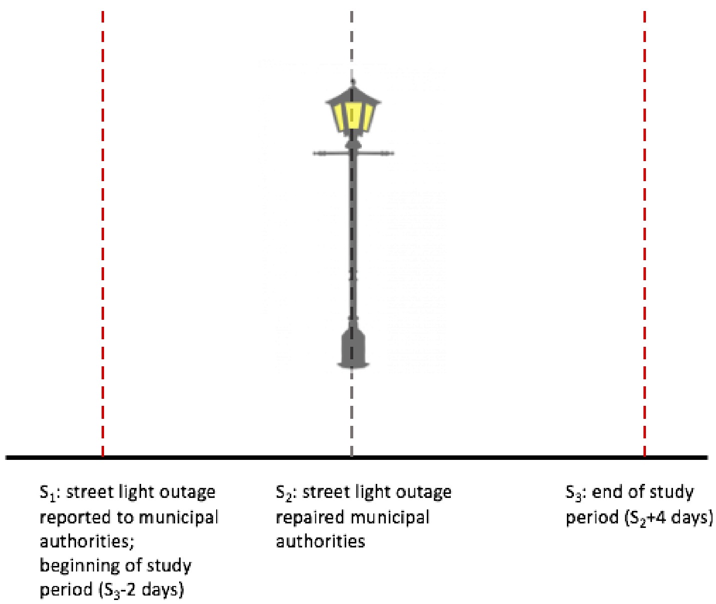
Figure 1: Visual Schematic of Research Design

A. Light Outage Duration $>$ 7 Days

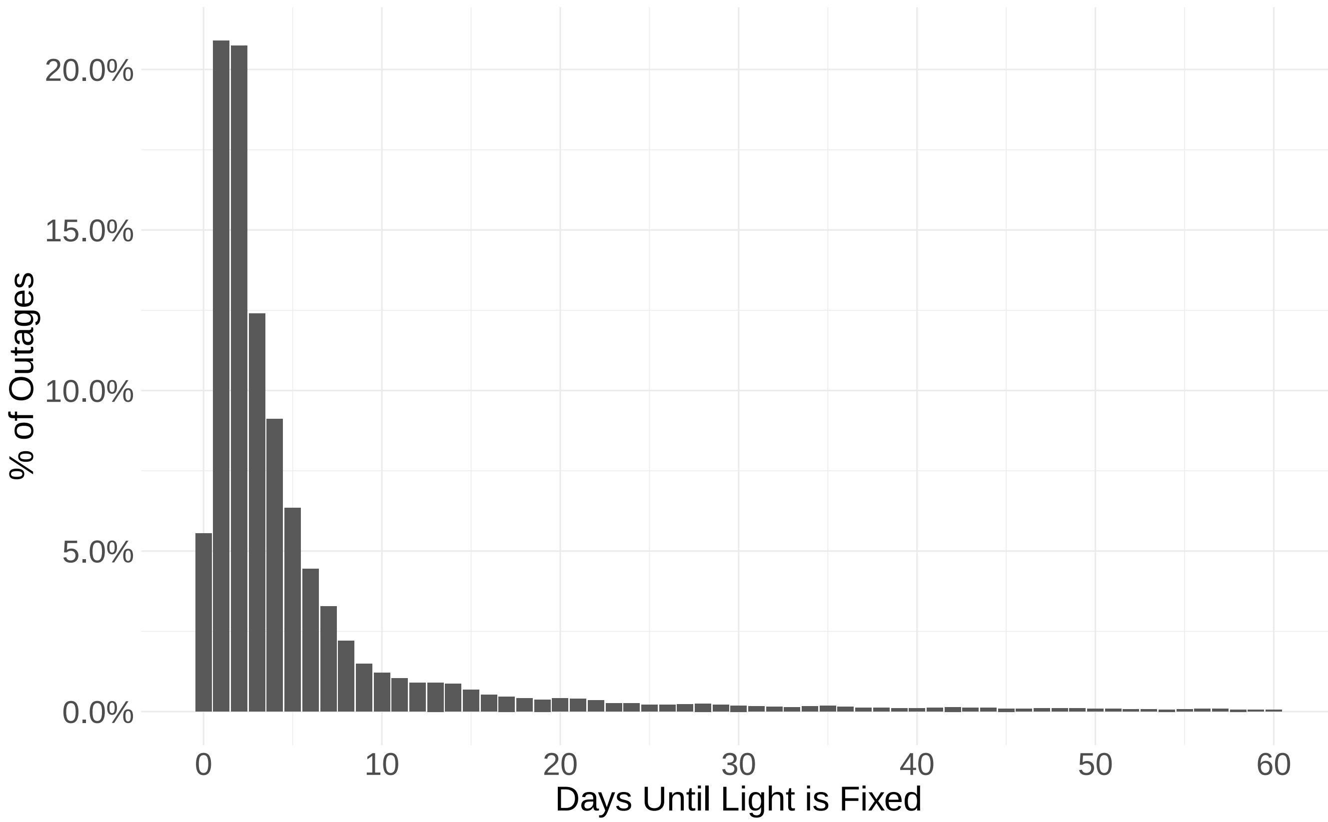


B. Light Outage Duration $<$ 7 Days



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 2: Duration of Time Between Reported and Repaired Street Light Outages



Note: Figure contains a histogram of the known 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.

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)

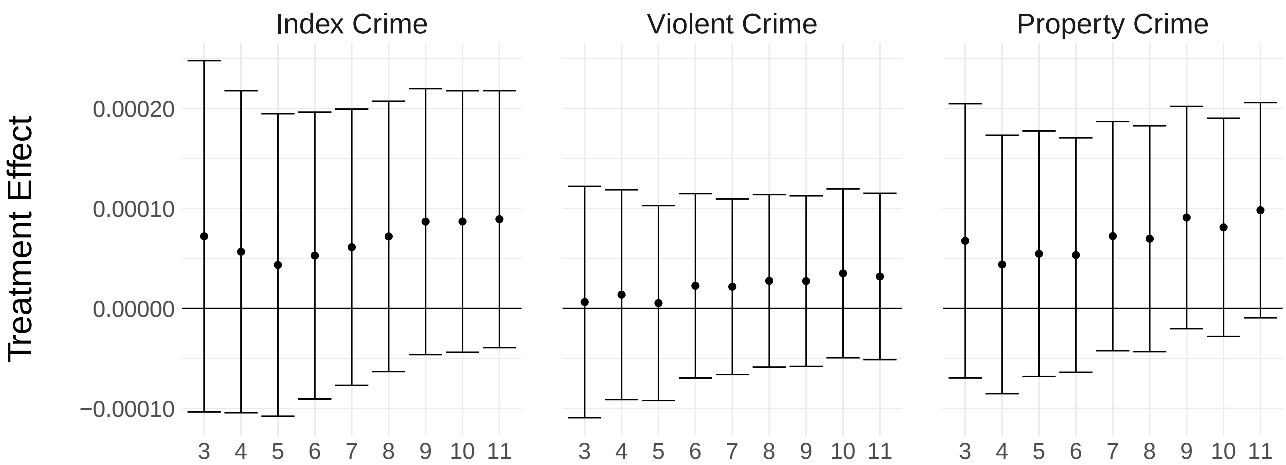
	Index		**Violent Crime**		**Property Crime**	
	Affected Segment	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**	**Affected Segment**	**Segments Within 500 Feet**
$\hat{\beta}$	0.00005	0.00054*	0.00001	0.00013	0.00006	0.00046*
Se($\hat{\beta}$)	0.00008	0.00024	0.00005	0.00015	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%]

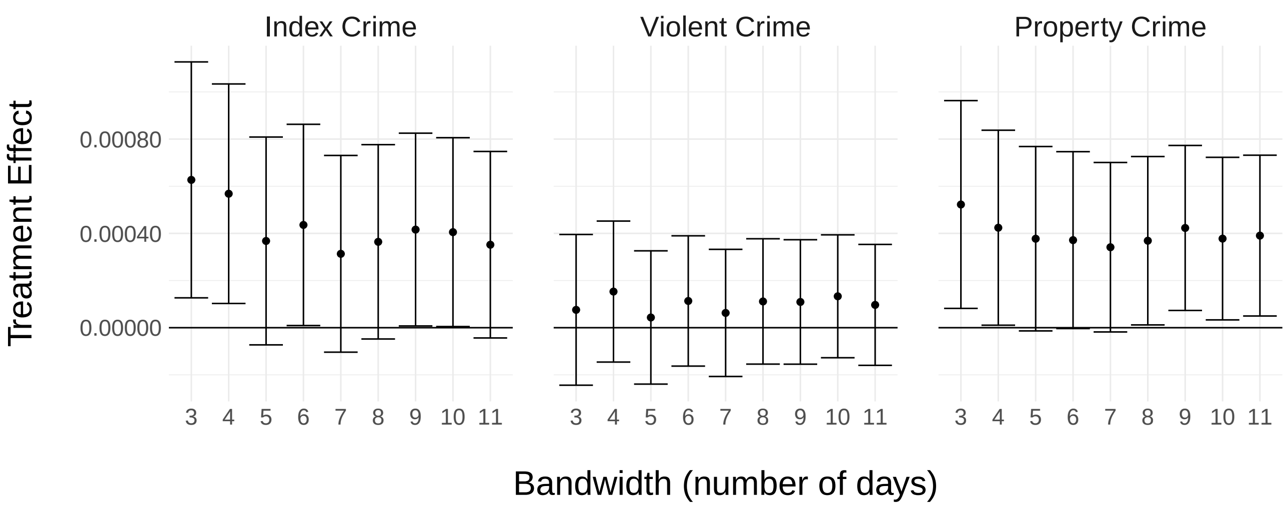
Online Appendix Material

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

Panel A: Affected Segment



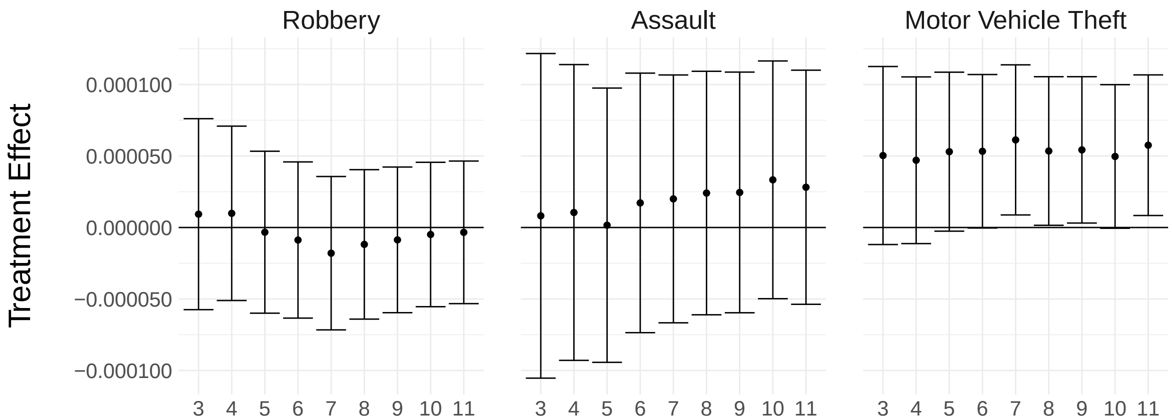
Panel B: Segments within 500 feet



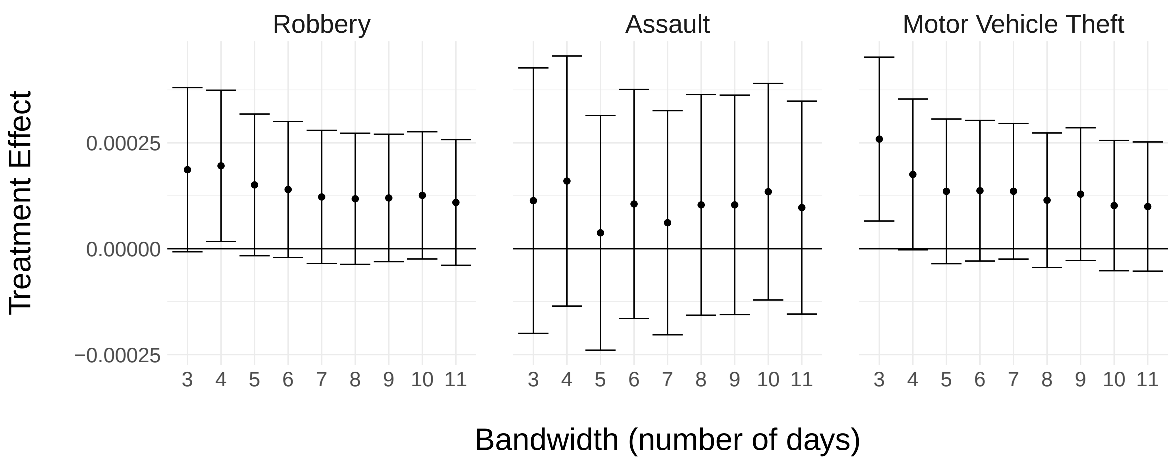
Note: Figures test the robustness of our results to bandwidth selection, varying the length of the post-repair bandwidth to be between 3 and 11 days. Panel A presents estimates for the street segment affected by major street light outages; Panel B presents estimates for street segments within 500 feet 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

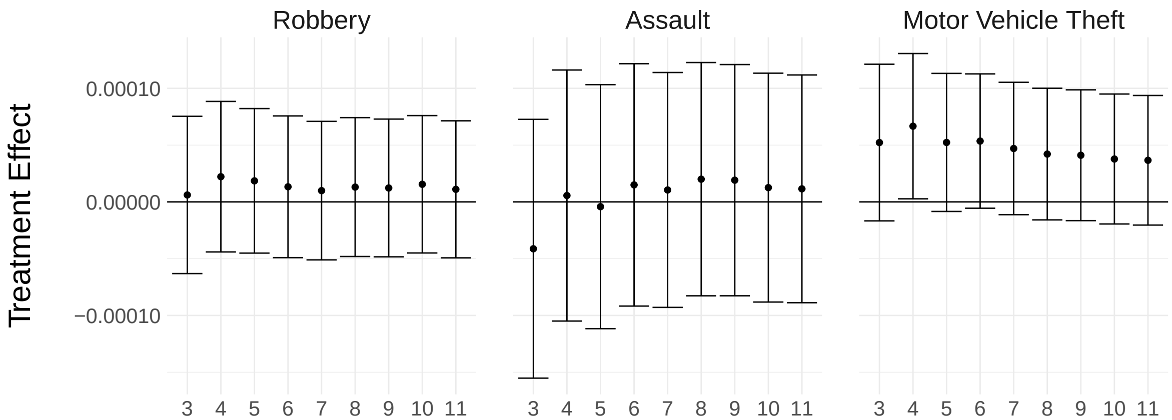


Note: Figures test the robustness of our results to bandwidth selection, varying the length of the post-repair bandwidth to be between 3 and 11 days. Panel A presents estimates for the street segment affected by major street light outages; Panel B presents estimates for street segments within 500 feet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

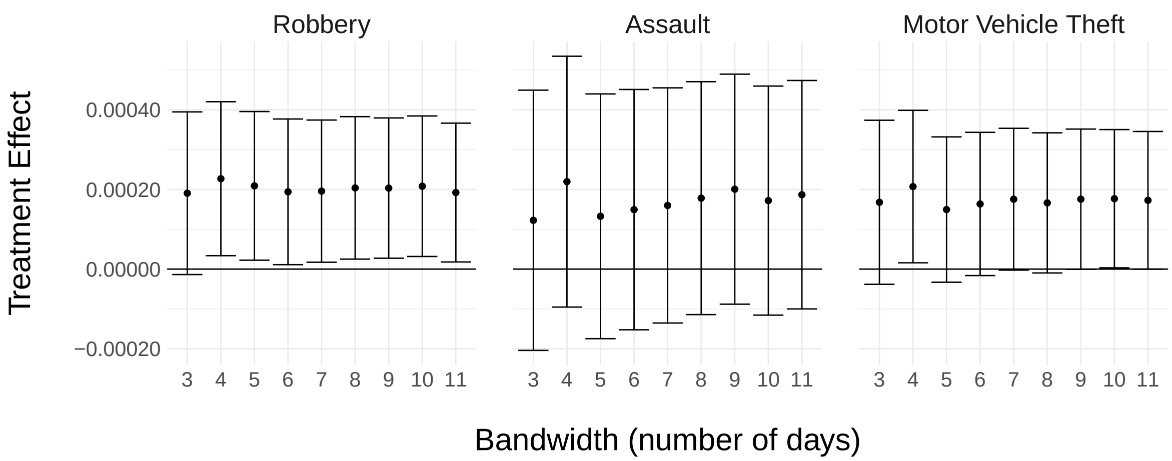
Appendix

Figure 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**