Reducing Crime Through Environmental Design: Evidence from a Randomized Experiment of Street Lighting in New York City

Aaron Chalfin*
University of Pennsylvania

Benjamin Hansen
University of Oregon and NBER

Jason Lerner
University of Chicago Crime Lab

Lucie Parker
University of Chicago Crime Lab

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Abstract

Objectives: This paper offers novel experimental evidence that violent crimes can be successfully reduced by changing the situational environment that potential victims and offenders face. We focus on a ubiquitous but understudied feature of the urban landscape — street lighting — and report the first experimental evidence on the effect of street lighting on crime.

Methods: Through a unique public partnership in New York City, temporary street lights were randomly allocated to 40 of the city's public housing developments.

Results: We find evidence that communities that were assigned more lighting experienced sizable reductions in nighttime outdoor index crimes. We also observe a large decline in arrests indicating that deterrence is the most likely mechanism through which the intervention reduced crime.

Conclusion: Results suggests that street lighting, when deployed tactically, may be a means through which policymakers can control crime without widening the net of the criminal justice system.

JEL Codes: I1, K4, H7.
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1 Introduction

A host of theory as well as a large empirical literature in the social sciences suggests that small changes to the situational environment can have surprisingly large effects on human behavior. In the broader social sciences, this can be seen in the empirical impact of "nudges," seemingly small environmental changes which have an outsize impact on consumer behavior (Benartzi & Thaler, 2013; Madrian & Shea, 2001; Thaler, 2018; Thaler & Sunstein, 2009). With respect to crime, this insight has culminated in the foundational philosophy of situational crime prevention that seeks to leverage changes to the built environment to influence criminal decision-making (Clarke, 1995). This approach, also sometimes referenced by its theoretical antecedent, CPTED, an acronym that stands for "crime prevention through environmental design" (Cozens, Saville, & Hillier, 2005; Jeffery, 1971; Newman, 1972; Robinson, 2013; Taylor & Gottfredson, 1986) has the potentially attractive quality of operating not through the criminal justice system, but instead through managerial and environmental changes that are intended make offending less viable without the need for greater enforcement (Clarke, 1980, 2009).

Can crime, in fact, be meaningfully shifted by a relatively small change to the situational environment? Public policy has, to a large extent, dismissed this idea, focusing predominantly on costly enforcement actions or, alternatively, on resource-intensive social programs which often hinge on a charismatic leader or a highly specific programmatic model and, as such, are notoriously difficult to scale (Ludwig, Kling, & Mullainathan, 2011). However, two well-documented empirical regularities offer surprising room for optimism. First, the substantial geographic concentration of crime, particularly violent crime, suggests that the social and physical features of the urban landscape might potentially play an important role in the crime production function (Braga & Clarke, 2014; Weisburd, 2015).[1] Second, a litany of empirical evidence suggests that offenders are

[1] The idea that the environment matters is likewise implicit in the seminal *Moving to Opportunity* research of the

present-oriented and that they tend to place relatively little weight on future probabilities (Jacob, 2011; Jacobs, 2010; Lee & McCrary, 2017; Nagin, Solow, & Lum, 2015). In a world in which potential offenders are present-oriented, small environmental design changes experienced in the present may well have an outsize impact on behavior relative to the uncertain prospect of prison sentences that, if experienced, will accrue sometime in the future (Nagin, 2013; Nagin & Paternoster, 1994). Implicit in this idea is that environmental design might be an important input in a typical offender's decision-making process and accordingly that offenders might be *deterred* by tactical changes to the built environment (Cozens, Saville, & Hillier, 2005).

In order to better understand the role that the physical environment can play in generating deterrence and, ultimately, crime reduction, we study one of the most ubiquitous — and under-studied — environmental design changes that cities have relied on to maintain public safety for more than two hundred years: street lighting. By enhancing visibility, lighting has the potential to change crime through many channels, including by empowering potential victims to better protect themselves and by making potential offenders more aware that a public space has witnesses or that police are present (Farrington & Welsh, 2002; Painter & Farrington, 1999b). While the available quasi-experimental literature suggests that street lighting may be an effective crime control strategy, methodological limitations have limited the formation of a scholarly consensus (Marchant, 2004). This conclusion is echoed forcefully and publicly in a 1997 National Institute of Justice report to the U.S. Congress, written after much of the available literature was completed, which concludes that due to "the inadequacy of available evaluation studies ... we can have very little confidence that improved lighting prevents crime" (Sherman et al., 1997).

Despite widespread interest in street lighting among scholars, policymakers, and the public at large, in the more than 250 years since street lighting has been in use in cities around the world,

early 2000s (Kling, Ludwig, & Katz, 2005) and continues to be hotly debated today (Chetty, Hendren, & Katz, 2016; Chyn, 2018; Sampson, 2008; Sciandra et al., 2013).

there has not been a randomized experiment on the effectiveness of street lighting in promoting public safety. Likewise, though prior literature provides a rich discussion of the theoretical mechanisms through which ambient lighting might influence crime, there is little empirical testing of these mechanisms in practice. Do offenders perceive street lighting as a threat to their ability to successfully complete crimes and avoid detection? Or does it merely enhance the ability of law enforcement to apprehend offenders? As the scalability of municipal investments in street lighting depends on whether they operate by deterring crime (which is relatively cheap) or via incapacitation (which is relatively more expensive), disambiguating between deterrence and incapacitation effects is critical not only to developing a richer understanding of offender decision-making, but also to understanding the financial viability of ambient lighting as an evidence-based crime control strategy (Cook & Ludwig, 2011; Durlauf & Nagin, 2011; Kaplan & Chalfin, 2019; Nagin, 2013).

This study offers the first and only experimental evidence on the effectiveness of street lighting in controlling street crime. The field experiment was carried out in 2016 in New York City, where crime has declined precipitously over the last three decades, coinciding with a series of well-documented and cutting edge innovations in policing (MacDonald, Fagan, & Geller, 2016; Zimring, 2012). However, despite the prominent decline in crime throughout most of the city, crime remains disproportionately high in public housing, the setting for this intervention. This field experiment took two and a half years to plan and was made possible by a unique partnership between the New York City Mayor's Office of Criminal Justice (MOCJ), the New York City Police Department (NYPD), and New York City Housing Authority (NYCHA).

We estimate that the street lighting intervention described in this research reduced outdoor nighttime index crimes by approximately 35 percent (90 percent confidence interval: 8-55 percent) once potential spatial spillovers are taken into account. Given that approximately 12 percent of crimes in our study setting occur outdoors and at night, the reduction in outdoor, nighttime index

crimes that we observe led to an overall reduction in serious offending in these communities of approximately 5 percent. These findings provide key experimental evidence that the physical environment is an important channel through which crime can be meaningfully reduced in an urban environment. Critically, the approximate cost of the lighting intervention described in this research is less than $80 per community resident and requires minimal technical know-how to implement, indicating that there is extraordinary promise in taking the intervention to scale in other cities and in other contexts. A remaining question is whether the lighting intervention studied in this research affects crime mechanically through incapacitation or through deterrence. Using public microdata on arrests, we show that the intervention reduced both crime and arrests to a similar degree implying that the dominant mechanism is general deterrence. Importantly, given that additional street lighting did not result in more arrests, the intervention does not appear to have widened the net of the criminal justice system in the disadvantaged communities in which the intervention was deployed.

2 Background

2.1 Theoretical Considerations

During the past fifty years, a large and growing situational crime prevention literature has arisen to study the impact of a variety of place-based crime prevention strategies. That literature now includes high quality evidence in favor of the promise of a host of CPTED-inspired interventions such as increasing the availability of trees and green space (Bogar & Beyer, 2016; Branas et al., 2011; Kondo et al., 2015; Kondo et al., 2018), restoring vacant lots (Branas et al., 2016; Branas et al., 2018; Garvin, Cannuscio, & Branas, 2013; Kondo et al., 2016; Moyer et al., 2019), public-private partnerships (Cook & MacDonald, 2011) and reducing physical disorder (Braga, Welsh, &

Schnell, 2015; Keizer, Lindenberg, & Steg, 2008; Sampson & Raudenbush, 2001; Skogan, 2012).[2] This latter finding — that crime can be sensitive to physical disorder — is echoed by experimental research on policing by Braga and Bond (2008) which notes that crime reduction achieved by police officers at hot spots was generated principally by situational prevention strategies rather than by misdemeanor arrests or social service strategies.

The situational crime prevention strategy that we study here — street lighting — has been around not for years or even centuries but, in one form of another, for millennia.[3] By some accounts, street lighting was introduced in the United States by Benjamin Franklin, who designed his own candle-based street light, first used in Philadelphia as early as 1757 (Mumford, 2002). 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, while there is substantial variation in their usage and intensity, 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.

The presence of ambient lighting can affect crime through numerous channels, each of which may operate by changing the behavior of potential offenders, potential victims, community members, police officers or any combination thereof. With respect to offenders, ambient lighting can potentially affect both the costs and rewards of committing a crime. The costs of crime are typically thought to be a function of the certainty and severity of punishment (Nagin, 2013) but they also include the cost of locating appropriate victims and the risk that a crime victim will fight back, thus turning the tables on the offender (Tark & Kleck, 2004). The rewards of crime include the value of any "loot" stolen during the commission of a property crime and may, for some of-

[2] For a comprehensive review of this literature, see: MacDonald (2015) and MacDonald, Branas, and Stokes (2019).

[3] Oil lamps were used to improve nighttime public safety in the Greco-Roman world at least as far back as 500 B.C. and accordingly, it is probably reasonable to conclude that street lighting is an idea that is nearly as old as civilization itself (Ellis, 2007).

fenders, also include psychological benefits of committing an act of violence. Broadly speaking, an intervention can deter a rational offender either by increasing the costs of crime or by decreasing its rewards. Given that many offenders may be myopic (Lee & McCrary, 2017) or only boundedly rational (Cornish & Clarke, 2017; Trasler, 1993), costs that are experienced with greater certainty and immediacy may be of greater salience than costs which may or may not be borne sometime in the future.

Perhaps the most obvious way in which lighting can affect the perceived cost of crime is by increasing the perceived certainty of apprehension for a given crime, thus deterring criminal activity (Akers, 1990; Becker, 1968; Cornish & Clarke, 2014; Nagin, 2013; Piquero et al., 2011). This might be because offenders believe that a police officer can detect criminal activity more easily in an area that is well lit (Mayhew, 1979). Alternatively, offenders might believe that lighting increases the probability of a witness (Jacobs, 1961; Painter & Farrington, 1999a, 1999b) or the effectiveness of complimentary technology like surveillance cameras (Piza et al., 2015; Priks, 2015). While lighting is often thought to affect crime mainly by changing the certainty of punishment, we note that it also has the potential to change the severity of punishment that an offender will face. For example, if lighting allows police to collect stronger evidence in a case, this may reduce the leverage a criminal defendant has during the charge bargaining process.

The presence of lighting can also affect the costs and rewards of crime in ways that operate partially through changes in victim behavior. In particular, darkness is thought to generate a sense of insecurity because it decreases visibility and recognition at a distance, creating a limitless source of blindspots, shadows and potential places of entrapment (Painter, 1996). While lessened visibility is often thought to provide an advantage to offenders, ultimately, the extent to which visibility is crime reducing or crime creating is theoretically ambiguous. On the one hand, greater visibility might empower potential offenders by reducing their crime commission costs (Nagin, 2013), better

enabling them to locate more vulnerable victims or more lucrative criminal rewards (Welsh & Farrington, 2008). Alternatively, better lighting may improve the actual or perceived ability of a potential victim to defend himself (Tark & Kleck, 2004) or to identify the perpetrator of a crime and carry out an extra-judicial punishment at a later date.

To the extent that better lighting reduces fear (Painter, 1996) and brings more potential victims outdoors, it may also lead to important changes to routine activities (Cohen & Felson, 1979) — especially how public space is used during nighttime hours — thus giving rise to two potentially countervailing effects (Atkins, Husain, & Storey, 1991; Cozens et al., 2003; Crosby & Hermens, 2019; Herbert & Davidson, 1994; Lorenc et al., 2012; Painter, 1996; Vrij & Winkel, 1991). On the one hand, more outdoor activity means that there may be more "eyes on the street," thus deterring crime by increasing the certainty of apprehension (Carr & Doleac, 2015) or through the types of informal social controls that are a mainstay of broken windows theory (Wilson & Kelling, 1982). On the other hand, more human activity, in general, means more potential victims and therefore a greater supply of "suitable targets" (Branas et al., 2018; Cohen & Felson, 1979; Cozens, Saville, & Hillier, 2005). While we cannot test this behavior directly, in order to better understand how changes in victim behavior may affect the estimates reported in this paper, we note that, to the extent that the use of public space rises with better lighting, the estimated effect of lighting on crime will be smaller than the effect of lighting on the implicit rate of victimization. In this way, the effects we report can be seen as conservative.

To the extent that enhanced lighting increases the *actual* and not only the perceived probability of apprehension, it might also generate incapacitation effects (Doleac & Sanders, 2015; Newman, 1972; Welsh & Farrington, 2008).[4] Incapacitation effects are often considered to be a mechanical response of crime to investments in public safety and can drive crime down even in the absence

[4]For high-volume crimes, even a small increase in arrests could lead to an appreciable decline in crime (Ratcliffe, 2002; Roman et al., 2009).

of a change in offender behavior (Chalfin & McCrary, 2017). While crime reduction is typically a welcome outcome, as has been noted by Durlauf and Nagin (2011) and Nagin (2013) among others, deterrence is cheap relative to incapacitation, which requires that municipal and state governments finance the considerable costs of arresting, adjudicating, and confining offenders. As such, the efficiency and therefore the scalability of a given crime control strategy depends critically on the relative magnitude of deterrence effects. Indeed, if deterrence effects are sufficiently large, it is possible for a crime control strategy to reduce both crime and incarceration, allowing society to achieve a "double dividend" in which two costly outcomes — crime as well as resources allocated to crime control — are simultaneously minimized (Durlauf & Nagin, 2011). Distinguishing between deterrence and incapacitation effects is therefore of great importance, both with respect to providing a rigorous test of deterrence theory and with respect to understanding the relative benefits of ambient lighting as a crime control strategy. In Section 6.5.1, we provide evidence on this question leveraging available public microdata on arrests. Following an argument laid out in Owens (2013), we compare the effect of the lighting intervention we study on crime to its affect on arrests. In particular, we note that an intervention that leads to a larger decrease in crime than arrests is likely to be operating at least, in part, through incapacitation. On the other hand, an intervention which generates a similar reduction in crime and arrests operates primarily through deterrence.

Finally, as noted by Welsh and Farrington (2008), there are other theoretical perspectives on the role that lighting may play in the crime production function that sit outside a rational choice framework. These theories emphasize the importance of investments to improve neighborhood conditions as a means of strengthening community confidence, cohesion and informal social control (Skogan, 1990). One particular hypothesis, advanced by Farrington and Welsh (2002), references a seminal article in *Science* by Sampson, Raudenbush, and Earls (1997) which argues that neighborhood crime is a function of a community's underlying level of collective efficacy. In theory, an

improvement in a community's physical environment can potentially serve as a cue that an area is cared for and thus is an inappropriate setting for behaviors that violate community norms (Taylor & Gottfredson, 1986). While the intervention we study is temporary, according to this theory, it might affect crime by changing community perceptions more broadly. As has been suggested by Farrington and Welsh (2002) and Welsh and Farrington (2008), to the extent that lighting operates by changing community cohesion, it might have an impact not only on nighttime crimes but also on crimes committed during daytime hours when lights are not technically operational (Welsh & Farrington, 2008). We discuss empirical evidence on this point in Section 5.3.

2.2 Prior Literature

The academic literature on street lighting is ably described in a systematic review by Welsh and Farrington (2008), who conducted a comprehensive scan of the extant literature, identifying thirty-two prior street lighting studies in the United States and the United Kingdom. Of the thirty-two studies, nineteen do not employ an appropriate comparison group and accordingly are vulnerable to a wide array of methodological critiques. The systematic review considers the remaining thirteen "differences-in-differences" studies in which a comparison group was available. In these studies, the addition of street lighting was found, on average, to reduce crime by more than 20 percent, though the evidence is mixed, with some studies finding little evidence of treatment effects.

Given that all of the prior research is observational, despite each scholar's best efforts, the prior literature suffers from a number of critical limitations which we briefly describe here. First, in the absence of experimental evidence, extant estimates of the effect of street lighting may be biased due to secular trends in crime, regression to the mean and the strategic placement of street lights by city planners, a limitation that has been noted at length by Farrington and Welsh (2002) and Marchant (2004) and which has been echoed by recent research on ambient lighting by Doleac and

Sanders (2015) and Domınguez and Asahi (2017). These critiques are part and parcel of a broader set of concerns about the validity and replicability of observational research in the social sciences and have, in particular, become the focus of a large literature in statistics and econometrics which has largely concluded that experimental treatment effects cannot be reliably recovered using quasi-experimental research designs (LaLonde, 1986; Smith & Todd, 2001, 2005) even when analysts have a rich set of covariates upon which to condition (Arceneaux, 2010; DiNardo & Pischke, 1997). Such findings have, in turn, reinforced the idea that, despite several important limitations, experimental evidence serves as the "gold standard"—or at least the "bronze standard" (Berk, 2005)—on the evidentiary hierarchy, especially with respect to internal validity (Banerjee & Duflo, 2009; Imbens, 2010; Weisburd, 2010).[5]

Second, among the thirteen differences-in-differences studies that compare crime trends among communities treated by lighting and comparison communities, eleven study only one or two treated locations. As a result, standard errors are sometimes not reported and, when they are, they are typically very large, giving rise to considerable uncertainty about the magnitude and even the direction of the true effects. While systematic reviews are helpful, given the small number of studies of sufficient quality, the inference problem remains frustratingly salient. This concern is compounded by the fact that the available evidence is mostly very old — seven of the studies in the systematic review by Welsh and Farrington (2008) were produced prior to 1980 and, incredibly, we are aware of only one paper on street lighting, that of Xu et al. (2018), written after the completion of the Great American Crime Decline.[6]

[5]For important reviews of the limitations of experimental research especially with respect to external validity, see: Deaton (2010), Deaton and Cartwright (2018), Heckman and Smith (1995) and Sampson (2010) for an excellent review that is specific to the study of crime. Also see Nagin and Sampson (2019) for a wonderfully nuanced and equally important discussion of the inherent challenges in identifying a policy-relevant counterfactual in an experimental design.

[6]One potential exception is quasi-experimental research by Arvate et al. (2018) who study a rural electrification program in Brazil and find that electrification which includes improvements to street lighting, led to a reduction in violent crimes in affected communities. However, electrification in a developing country might affect crime through a number of different mechanisms in addition to street lighting.

Finally, as noted by Welsh and Farrington (2008), the norm in the literature for selecting comparison areas is to select an area that is adjacent to the treatment area. While this is a reasonable heuristic to select a "similar" comparison area, such a decision will lead to a downward biased treatment effect in the presence of spatial spillovers that accrue to nearby locations. 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 research by Marchant (2004) and Doleac and Sanders (2015) suggesting that past research may be unreliable. This conclusion is echoed forcefully and publicly in a 1997 National Institute of Justice report to the U.S. Congress, written after all but two of the papers cited in the meta-analysis of Welsh and Farrington (2008), which concludes that due to "the inadequacy of available evaluation studies ... we can have very little confidence that improved lighting prevents crime" (Sherman et al., 1997). As only a handful of lighting studies have since been published, this conclusion continues to carry substantial weight.

Given the substantial methodological limitations of the quasi-experimental literature on lighting interventions, we note that there is useful evidence on the effect of ambient lighting from a natural experiment motivated by Doleac and Sanders (2015) who study variation in lighting induced by daylight savings time.[7] Using both a differences-in-differences and regression discontinuity approach, they find evidence that more ambient lighting reduces crime, particularly robbery. While their findings suggest a role for ambient lighting, experimental evidence remains critically important for several reasons. First, the reseach uses microdata from the National Incident-Based Reporting System, which has poor coverage of urban areas in the United States, limiting external validity for large cities. Second, an hour of additional natural light is a fundamentally different — and considerably more intensive — treatment than artificial lighting provided by enhanced street lighting. Finally, street lighting is a policy that communities can directly influence and potentially use to

[7] This research has recently been replicated in Chile and extended by Domınguez and Asahi (2017).

target high-crime areas in which the majority of a city's crimes are clustered.

3 Field Experiment

3.1 Description

The field experiment described in this paper was conducted in the spring and summer of 2016 in New York City. Through a unique partnership between the the NYC Mayor's Office of Criminal Justice, the New York City Police Department and the New York City Housing Authority, we randomized the provision of temporary light towers to the city's public housing developments.

The New York City Housing Authority which is charged with managing the city's 340 public housing communities, was created in 1934 as a response to the housing crisis brought about by the Great Depression. Today, NYC's 340 public housing developments are officially home to more than 400,000 New Yorkers (and perhaps an additional 100,000 non-official residents). NYC's official public housing population is large enough to place it among the forty largest cities in the United States, making it a potentially ideal setting to study the effect of street lighting in urban areas.

NYCHA developments are heterogeneous with respect to both their size and their layout. Most of the city's housing developments were constructed in the twenty-year period following World War II. These developments typically consisted of multiple buildings set on large campuses that were inset from the street. While more recently-constructed developments tend to be smaller and are set on a typical NYC sidewalk, the larger developments continue to predominate (Bloom & Lasner, 2019). In our sample, 96 percent of developments consist of more than a single building each of which has more than five floors and 64 percent of developments contain at least one building which has at least 10 floors. 80 percent of developments are laid out in a "campus style" arrangement and are inset from the street.

Given the cost of providing enhanced street lighting at scale, the city wanted to launch a pilot study to investigate the extent to which increased lighting would be effective in reducing serious crime. We worked closely with the City for more than two years to develop the field experiment described in this paper.[8] The intervention deployed temporary lighting towers to housing developments across NYC. These towers emit approximately 600,000 lumens—a measure of brightness—making them extraordinarily luminous, considerably more luminous than a standard permanent street light. For comparison, standard outdoor lighting on NYCHA developments typically ranges in luminosity from between 5,000 and 35,000 lumens, depending on the type of lighting fixture. The light towers were diesel-powered and re-fueled manually each day during daylight hours, by temporary city workers who were specifically hired as part of this intervention and who did not have other responsibilities such as servicing existing street lights. The lights were equipped with an automatic timer set to switch on at sunset and off upon sunrise, ruling out the possibility of non-compliance with the intervention. A schematic photo of an Allmand™ lighting tower as well as a photo of towers in the field can be found in **Figure 1**.

Two further items are worth noting. First, because they are diesel-powered, the light towers emit a small amount of noise and, sometimes, a noticeable smell when an individual is situated within a few feet of a light tower. While an individual would need to be located extremely close to a lighting tower to experience these conditions, we nevertheless note that these ancillary features of the light towers might potentially serve as a mechanism through which the towers affect crime. Second, the

[8] One notable aspect of this field experiment perhaps worth mentioning here was the level of interaction between the City and NYCHA residents during the planning of this project. In early 2016, MOCJ and the research team coordinated a series of meetings with NYCHA residents from treatment developments in order to begin planning the study implementation process. The major goal of these meetings was to receive resident input on where they thought additional lighting would be most beneficial within their development. During these meetings, residents found out how many lights towers would be allocated to their development and were asked to hold a vote at their next tenant's association meeting on light tower placements for their development. Using resident voting data, the research team produced a "heat map" of residents' lighting preferences for each development. These maps were presented to NYPD during subsequent meetings to help inform their decision-making about lighting placement. NYCHA residents and NYPD officers also provided constant feedback about the lighting conditions (e.g., excessive or insufficient lighting in specific locations) within the developments, resulting in frequent movement of light towers throughout the study period.

light towers are quite tall and are extremely prominent in the landscape of the communities that received them. As a result, in addition to providing ambient lighting, the towers may be perceived as a sign that the general area in which they are placed has been prioritized or cared for by either law enforcement or city planners. Accordingly, we note that one of the mechanisms through which the intervention may have affected crime is through what we might call a "demonstration effect."[9] Nevertheless, as the dominant feature of the intervention was enhanced lighting, the experiment provides powerful proof-of-concept for the importance of ambient lighting in controlling street crime.

3.2 Research Design

In order to select developments for the study, NYPD provided a list of 80 high-priority developments based upon their elevated crime rates and NYPD's perception of the need for additional lighting from among the 340 NYCHA developments in NYC.[10] From this list, we randomized 40 developments into a treatment condition that would receive new lights and 40 developments into a control condition via paired random sampling, stratifying on each development's outdoor nighttime index crime rate and size in the two years prior to the intervention.[11]

As Farrington and Welsh (2002) note, statistical power has been a primary challenge to randomizing street lighting — as it has for many place-based experiments. Intervening on a place, particularly a large place like a housing project, is enormously costly. While more than 60,000

[9] With respect to the implications that the latter feature of the intervention has for external validity, we note that demonstration effects are a common feature of place-based crime reduction strategies. For instance, consider a hot spots policing intervention which is designed to maximize the deterrence value of available police officers by making their presence as visible as possible. Such an intervention might impact crime by changing the probability that an offender is apprehended but it might also signal that an area is a priority for city planners or that it is cared for. Naturally, a hot spots policing intervention which places less priority on the visibility of police patrols might have different effects. Given that the intervention described in this research operates by increasing the amount of ambient lighting and potentially also via a demonstration effect, the results are most readily applicable to a tactical lighting intervention rather than a permanent change to urban infrastructure.

[10] We show, in **Appendix Figure 1**, that these 80 developments tend to be drawn from the top of the distribution of developments, ranked by outdoor nighttime crimes.

[11] In practice, one development (East River) was randomized into the control group but subsequently received a randomized dosage of lights because of operational considerations. Additionally, one control development (Smith) received some lights post-dosage randomization. We report intention-to-treat results throughout the paper.

people live in the areas we study, given resource constraints, only a relatively small number of locations could be treated. Compounding this difficulty, the principal outcome of interest, index crimes, while elevated in these areas, are still relatively rare and thus highly variable. In order to maximize statistical power, we go beyond a simple treatment-control design by randomizing the dosage of lighting received by each community in a block randomized design.

Three hundred and ninety-seven lighting towers were available to be randomly assigned amongst the 40 developments that ultimately received the treatment. For operational reasons, the City decided that two light towers would be allocated to each development, regardless of the development's square footage. The remaining 319 lighting towers were therefore assigned to the 40 developments according to a random number drawn from a uniform distribution linked to the square footage of the developments, thus generating exogenous variation in the the number of lights per square feet across the developments. In response to feedback from residents, the allocated dosage was slightly adjusted and so differs from the randomly assigned dosage. As is shown in **Appendix Figure 2**, the assigned and allocated dosages were very similar. To protect against bias due to non-compliance, we report intention-to-treat estimates, using each development's *assigned* dosage. The average dosage among the treated developments was seven light towers over an area of approximately 700,000 square feet.

A simple schematic of the research design can be found in **Appendix A** which shows how we have nested an experiment within an experiment. We pause here to consider the implications that this research design has for interpreting subsequent estimates of the effect of street lighting on community crime. There are two considerable virtues in randomizing the dosage of new light towers among the treatment group. First, power allowing, we can understand whether there are diminishing marginal returns to the additional lighting and, as such, provide guidance to policymakers regarding the optimal dosage of new lighting. Second, because there is considerable variation in a continuous

measure of dosage, this research design goes beyond a simple treatment-control group comparison, providing additional and critically needed statistical power. However, there is also a key drawback. In particular, while some of the treated developments have received a large number of randomly allocated street lights, some of the developments received only a small number of new lights. To the extent that the impact of lighting is, to a degree, non-linear, some of these developments may have received a non-clinical dosage of lighting. As a result, comparisons between treated and control developments will tend to understate the effect of the intervention.

Light towers were deployed in the field between February 29, 2016 and March 7, 2016; the lights remained illuminated during all nighttime hours for the following six months. During the study period, only seven lighting outages were reported. All outages involved the operational failure of a single light tower and were addressed by the vendor within twenty-four hours. Control group developments received no additional outdoor lighting ("business-as-usual").

4 Data

To measure crime in the study locations, we use NYPD administrative data on criminal complaints from March 2011 through August 2016. These incident-level data were provided directly by the NYPD and include the date, time, and type of offense, as well as whether the incident occurred indoors or outdoors. For privacy purposes, the exact location coordinates have been displaced and each complaint is "mid-blocked" — that is, assigned the X-Y coordinate of the middle of the block in which it occurred. Indoor versus outdoor locations were determined using the "premises description" variable in the administrative data which indicates whether a crime occurred in a dwelling. We focus primarily on index crimes, which conform with the FBI's *Uniform Crime Reports* "Part I" crimes and include murder and non-negligent manslaughter, robbery, felony assault, burglary, grand larceny, and motor vehicle theft, but due to data constraints, do not include rape or arson.

Among index crimes, the most common crime types are felony assault and robbery, which together comprise 72 percent of the index crimes that occurred outdoors during nighttime hours within the treated developments during the 2011-2016 period.[12]

We focus on index crimes for two reasons. First, these are the most serious crimes and drive the vast majority of the social costs of crime (Chalfin, 2015). Second, relative to less serious crimes, index crimes are thought to be especially well-measured both because these crimes are more likely to be reported to law enforcement and because when reported, the time stamps are more reliable, allowing us to correctly assign crimes to either daytime or nighttime hours. Critically, we note that while not all index crimes are reported to law enforcement, to the extent that the lighting intervention may have increased crime *reporting* either because more crimes are witnessed or because of an increase in neighborhood collective efficacy (Morenoff, Sampson, & Raudenbush, 2001; Sampson, Raudenbush, & Earls, 1997), this would lead to a mechanical positive correlation between the intervention and crimes in the data. Therefore estimates reported in Section 6.3 will be conservative — that is, the true effect of lighting is at least as large as what is reported — in the presence of reporting effects. In Section 6.5.2 we consider the impact of the intervention on less serious crimes which are primarily comprised of petit larceny and offenses against public order. These crime data are derived from the same NYPD microdata we use to study index crimes.

In addition to crime data, we use administrative data from several alternative sources to test covariate balance, construct controls and disambiguate between deterrence and incapacitation effects. First, we use public microdata on geo-located arrests which are available at NYC's Open Data portal. Next, NYCHA provided the square footage, official population, height of the average building, the number of residents per unit, the number of entrances per building, and whether the development has an elevator. Third, we use Census block data from the U.S. Census Bureau to

[12] 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. Civil twilight generally begins approximately half an hour after the official sunset and ends approximately half an hour before the sunrise.

construct an unofficial measure of the age and gender composition of the development. Finally, we obtained auxiliary NYPD data that serves as a proxy for the density and type of police activity in each of the developments. Specific variables include the date and location of 1) large-scale gang take-downs, 2) home visits conducted by NYPD, and 3) "vertical patrols" conducted by officers within public housing stairwells. Estimates are robust to the inclusion of police activity controls, suggesting that results are not an artifact of post-treatment bias due to the re-allocation of police in response to enhanced lighting.

5 Empirical Methods

To estimate treatment effects, we use a series of Poisson regression models in which the count of crime, $Y_i \sim \text{Poisson}(\gamma_i)$, is regressed on a treatment variable (D_i) and a vector of covariates (X_i). We begin by regressing crime on a binary treatment variable indicating whether a development was randomly assigned to receive some amount of additional lighting. Given that estimates are imprecise and the binary treatment indicator masks considerable heterogeneity in the dosage of allocated lighting, subsequent analyses are derived from a "dosage model" that regresses the log count of crime on the natural log of the number of additional lights randomly assigned to each development per square foot among the developments in the treatment group only. The parameter on the dosage variable, β, captures the effect on crime of a one hundred percent change in the dosage of additional lighting. Evaluated at the mean dosage level, this provides an estimate of the effect of the intervention on crime.

Next, we turn to the issue of what to condition on. A primary challenge in evaluating small N experiments, particularly those with highly variable outcome data, is the inevitable sensitivity of treatment effects to reasonable differences in modeling assumptions. While a large experiment can be straightforwardly evaluated using a t-test which makes minimal assumptions and offers

few researcher degrees of freedom, small experiments require that researchers condition on control variables, both to reduce residual variance as well as to guard against finite sample bias due to imperfect randomization (Angrist & Pischke, 2008; Imbens, 2010). While we have rich data upon which to condition, the large number of potential covariates relative to the number of available observations means that there are many reasonable and theoretically-grounded models that can be used to estimate treatment effects. This issue is compounded by the incomplete guidance that theory provides for selecting the functional form of available covariates—for instance, should we control for population or its natural log? Or, should we control for past crime using six-month or one-year windows?

Inevitably when researchers have broad discretion to select a single "preferred estimate," there are concerns about "p-hacking," the practice of selecting an outcome model — either intentionally or unintentionally — on the basis of statistical significance of a pivotal result (Gelman, 2016). A number of thoughtful critics have, in recent years, identified p-hacking specifically and researcher discretion more generally as potential contributors to the "replication crisis" which has generated concern across the social and behavioral sciences (Benjamin et al., 2018). In order to select a model in a principled way, we appeal to a growing literature in statistics and econometrics that leverages lessons from machine learning to improve the practice of causal inference (Athey & Imbens, 2015; Belloni, Chernozhukov, & Hansen, 2014; Varian, 2014).[13] In particular, in order to automate away researcher discretion and enhance the credibility of our estimates, we turn to LASSO regression (Tibshirani, 1996), a popular and versatile machine learning classifier that is often applied to variable selection problems in high-dimensional space (Meinshausen & Bühlmann, 2006; Tibshirani, 2011; Zou & Hastie, 2005).

The LASSO has the virtue of retaining only the subset of predictors that are genuinely predictive

[13] For excellent reviews of machine learning techniques and their applicability to research in criminology, see Berk (2010) and Brennan and Oliver (2013).

of outcomes — that is, predictive of outcomes in a new sample, unseen to the algorithm. This feature of the LASSO is helpful in our context for two reasons. First, reducing the dimension of the data is necessary when the number of predictors exceeds — or nearly exceeds — the number of observations. Second, by automating covariate selection from among a pool of theoretically important predictors, LASSO regression provides a means of fortifying the use of theory in selecting an outcome model, making model selection more robust to the problem of false discoveries, a process that Athey and Imbens (2016) refer to as "honest causal estimation."

The LASSO works by estimating an ordinary least squares regression with the following constraint on the parameter vector: $\sum_{k=1}^{K} |\beta_k| \leq \lambda$ where λ is a tuning parameter that controls the strength of the penalty term. When $\lambda > 0$, this constraint has the effect of setting parameter values that fall below a given threshold equal to zero, thus performing variable selection. The optimal penalty term, λ^*, is selected via k-fold cross-validation by randomly partitioning the data into k different training sets and associated test sets. In keeping with a common practice in machine learning applications, we set $k = 5$.[14] For each training set, a series of models are estimated for varying values of λ and predictions are computed on the associated test set. The optimal λ is chosen by taking the mean of the errors across the k test sets and choosing the value which minimizes this quantity. With this λ in hand, the model is then re-run on the full dataset using the optimal λ.[15]

While the LASSO is useful for selecting variables, it is not appropriate for estimating treatment effects. Hence, for each of the 500 LASSO-selected subsets of variables, we subsequently run the Poisson regression model outlined in (1) where the covariate vector, X_i, is selected using the LASSO.

[14] As is noted by Casella, Fienberg, and Olkin (2013), there is a bias-variance trade-off associated with the choice of k in k-fold cross-validation. Setting k=5 has been shown empirically to yield test error rate estimates that suffer neither from excessively high bias nor from very high variance.

[15] A challenge in applying LASSO to our data is that the sample size is relatively small and the outcome is fairly noisy. As a result, the variables selected by the LASSO can be sensitive to how the data are randomly partitioned into the five folds. To ensure the stability and robustness of the estimates, we re-run the LASSO 500 times, each time retaining the subset of selected variables. This is done to ensure that a single iteration of the LASSO does not lead to an unusual partitioning of the data and, therefore, a misleading estimate of the treatment effect.

For each iteration, we store up the coefficient and a bootstrapped standard error. We report the median coefficient (which is less sensitive to outliers) and the standard error among the estimated models. Estimates are reported for on-development crimes and, in order to test for either spatial spillovers or the diffusion of benefits (Clarke & Weisburd, 1994), for crimes that occur within a radius of 550 feet — two standard NYC blocks — from the development. While we do not detect evidence of either negative or positive spillovers, these tests are quite underpowered and, as such, we also report estimates for crimes committed within the 550 square foot catchment area, *inclusive of the development*. By considering the catchment areas as treated (when, in fact, they were not), these "net" estimates are thus conservative and represent a lower bound on the treatment effect as they fold in displacement to adjacent areas.

6 Results

6.1 Descriptive Statistics

Table 1 reports covariate means for the randomized treatment and control groups as well as a p-value from a t-test on the difference between those means. Because our sample is small, asymptotic critical values may provide a poor approximation to the true sampling distribution. Accordingly, we derive p-values empirically using a re-randomization procedure in which we re-estimate each t-test a large number of times, each time randomly assigning the treatment variable. The relative position of the t-statistic for the model that uses the *actual* data among the distribution of placebo randomizations is used to generate an empirical p-value. Past nighttime crimes are expressed as the average count of each type of crime over the 2011-2015 pre-intervention period, limiting the period to the months between March and August of each year. While outdoor nighttime crimes on public housing developments may seem relatively rare — pooling the treatment and control

groups, there are approximately 3.3 outdoor nighttime index crimes over a six-month period at these developments — this is nevertheless consistent with an overall crime rate of approximately 7,500 per 100,000 population annually, more than double the rate of the highest-crime U.S. state.

The next panel considers four different measures of population structure: the development's official population, its population density (population per 1,000 square feet), average household size, and share of the official population that is male and between the ages of 15-24, which generally comprises peak crime ages. Most developments in the sample are large, housing on average 2,500 residents in an area of roughly 720,000 square feet. Finally, we report covariate means for three measures that capture a development's physical layout: the number of entrances per building, whether the building has an elevator or not, and the development's total square footage.

6.2 Fidelity of Randomization

Despite the difficulties that randomization can face in a small sample with highly variable outcome data, past crimes are broadly balanced between the treatment and control groups; none of the differences in means are significant at conventional levels of significance. In order to construct an omnibus test of covariate balance, we regress either the binary treatment variable or, within the treatment group, continuous treatment dosage, on the available covariates and compute the F statistic, testing for the joint significance of covariates in predicting treatment status. In practice, we use a permutation test in which we re-randomize the treatment variable to a given set of covariates a large number of times and note the relative position of the F statistic for the model that uses the *actual* data among the distribution of placebo randomizations. For both the binary and continuous treatment variables, the p-values on the F-statistic are well above conventional significance indicating that covariates do not predict treatment, a finding that is consistent with successful randomization.

6.3 Main Estimates

We begin by regressing crime on a binary treatment variable indicating whether a development was randomly assigned to receive some amount of additional lighting. This model suggests that the intervention reduced outdoor nighttime index crimes by approximately 13 percent. However, estimates are very imprecise and, as noted in Section 3.2, the binary treatment indicator masks considerable heterogeneity in the dosage of allocated lighting. Given that some of the treatment sites received a very small and possibly non-clinical dosage of lighting, we re-emphasize that these estimates will tend to be biased downward and, accordingly, we turn to our analysis of randomized dosage.

In **Figure 2** we plot the natural log of crime against the natural log of the number of light towers added per square block, using raw data. There is a negative relationship between on development crime and randomly assigned lighting dosage. Turning to the middle column, the relationship between crime and dosage is positive for off-development crimes that occurred within the 550-foot catchment area, which is consistent with the possibility that some crimes are displaced to adjacent areas. Finally, turning to net crime — crimes occurring both on and within 550 feet of the development — the relationship between lighting and crime remains negative, albeit less negative than for on-development crimes. As we show in Appendix B, excluding the highest dosage developments does not substantively change the results.

We next turn to generating numerical estimates of treatment effects. **Table 2** presents regression evidence on the effect of lighting dosage on crime within the treatment group. The estimates, presented as incidence rate ratios, are derived from Poisson regressions of the log count of crime on the natural log of dosage, conditional on controls. Results are presented for on-development and off-development crimes that occurred within the 550-foot catchment area, as well as for net crimes inclusive of the off-development catchment area. We present results separately for both daytime

and nighttime outdoor crimes, recognizing that daytime crimes potentially provide a measure of the importance of temporal spillovers. Coefficients and bootstrapped standard errors are the median among 500 different LASSO-selected models.

The main estimates for outdoor nighttime crimes in the treated developments are presented in the first set of rows. For each 100 percent change in the natural log of dosage, averaged across all developments, we estimate a 60 percent reduction in outdoor, nighttime crimes that occur on the development. In other words, the lighting intervention led to a 60 percent reduction in nighttime outdoor index crimes. It is important to note that while the IRRs are consistent with very large treatment effects, base rates of crime are low. The 60 percent reduction in index crimes would take an average development which experiences 3.3 index crimes per six-month period prior to the intervention to 1.3 expected index crimes.

Turning to the middle set of columns of Table 2, we find little evidence that lighting displaces crime. However, standard errors are large and so we cannot estimate the magnitude of spillovers with confidence. In order to be conservative, we estimate a "net" model in which the dependent variable is the count of crimes within a two-block radius of the development, inclusive of the development itself. Since the intervention occurred only on the development, estimating the effect of lighting for the entire area has the mechanical effect of attenuating treatment effects, even in the absence of spillovers. These models are, thus, conservative and provide a reasonable lower bound on the effects of the intervention. Inclusive of potential spillovers, we estimate that lighting reduced outdoor nighttime index crimes by 35 percent.

Next, we present results for daytime crimes among the treatment sites as a test for temporal spillovers. This is an important test as, even during the daytime, the presence of the temporary light towers is a notable part of the landscape in these communities. The towers are prominent, may be associated with public safety in the minds of residents and are tended to regularly by personnel

who re-fuel the lights during the daytime. Therefore, the lights may have a demonstration effect on crime, even during daytime hours. While point estimates are negative, they are insufficiently precise to provide confidence that temporal spillovers are present. Nevertheless the magnitude of the estimates provides some degree of optimism and calls out for replication in future research.

One point of caution is worth noting with respect to this test. In particular, we note that precise timestamps on crimes in police microdata can be noisy. Consequently, it is entirely 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 (Felson & Poulsen, 2003). To the extent that this is true, a portion of the crime reduction observed at night could be re-distributed to the daytime and, accordingly, the test for temporal spillovers to daytime hours would be contaminated. While we do not have an ideal test to rule out the possibility of measurement error bias, we do have a suggestive test at our disposal. To the extent that person crimes — in particular, robberies and assaults — have more accurate timestamps than crimes against property, we might expect that regressions for these outcomes will be less contaminated. We re-estimate the main regressions for robberies and assaults and report the results of this exercise in **Table 3**; results are similar.

The final rows of Table 2 present the results of a key placebo test for the fidelity of randomization. Here, we utilize the randomized control group, leveraging the fact that, since the developments were randomized in pairs, each of the control developments has a randomly assigned dosage of additional lighting that was not actually received. Accordingly, we should not observe that crime falls in response to assigned dosage among the developments in the control group. Consistent with successful randomization, for both nighttime and daytime crimes, the estimates are small in magnitude and are statistically indistinguishable from zero.

6.4 Robustness

In this section, we further interrogate these findings in order to assess whether they are robust to alternative specifications. As an additional check on the representativeness of the LASSO-selected predictors across the entire model space, we re-estimate our Poisson regression model a large number of times using randomly-selected subsets of control variables and show that the estimates from the LASSO-selected models lie within the central tendency of the universe of potential model estimates. **Figure 3** presents the distribution of estimated on-development and net treatment effects from 5,000 models, each of which conditions on a subset of between one and eight control variables randomly selected from among the universe of observed covariates. In addition to the random set, each model controls for development population. The range of possible parameter estimates is appreciable, reflecting the large degree of variability in the distribution of crime among the housing developments in the study. Notably, though, the estimates in Table 2 are not unusual among the larger universe of potential estimates and are, if anything, conservative. Likewise, regardless of the control function employed, estimated treatment effects are uniformly positive and qualitatively important.

We also provide a number of additional robustness checks which are intended to assess the sensitivity of the estimates to outliers and functional form assumptions. We test for outliers by re-estimating the models excluding one development at a time from the treatment group (**Appendix Figure 3**, Panel A) and excluding the developments which were assigned to receive the largest dosages of lighting (**Appendix Figure 3**, Panel B). Next, we consider robustness to changes to the functional form of the outcome models, re-estimating outcome models using ordinary least squares (**Appendix Table 1**) and pooling the treatment and control developments in a single pooled analysis. Finally, we consider the sensitivity of the conservative estimate of the lighting intervention on net crime to changing the radius used to detect spillovers, as shown in **Appendix**

Figure 4. Results of each of these analyses indicate that our principal findings reported in Table 2 are not sensitive to different functional form assumptions. Additional detail on each robustness check can be found in **Appendix B**.

6.5 Theoretical Extensions

The treatment effects reported in Section 6.3 are qualitatively large and robust to several placebo tests and a wide array of modeling strategies. In this section, we consider the behavioral mechanisms through which lighting impacted crime and provide context for how this intervention contributes to our understanding of criminological theory. We begin by providing a test for general deterrence versus incapacitation effects, focusing on the extent to which the intervention changed the number of arrests made in treated communities. Next, we consider the extent to which the intervention had an impact on less serious types of offending, mainly theft and public order offenses. Finally, we consider the extent to which the intervention, apart from its impact on public safety, had an impact on the behavior of police officers.

6.5.1 Deterrence Versus Incapacitation Effects

In Section 2.1, we motivated the importance of unpacking our estimated treatment effects in order to distinguish between general deterrence and incapacitation effects. Using public microdata on arrests, we test whether the effect of the intervention on arrests differs substantively from its effect on crime. To the extent that crime and arrest impacts are similar, this limits the scope for incapacitation effects to be an important contributor to the public safety benefits that we observe. In order to construct an empirical test, we regress (using Poisson regression) arrests on the randomized dosage of treatment, conditional on controls. One limitation of this analysis is that the public arrest data does not contain an incident identifier and so we do not know if an arrest pertains

to a crime that was committed during daytime or nighttime hours. Given that the intervention is primarily intended to affect public safety when it is otherwise dark, to the extent that we observe large declines in the all arrests, whether they pertain to daytime or nighttime crimes, as a function of the intervention, this constitutes strong evidence against the incapacitation channel.

We present arrest outcomes in **Table 4**; Panel A presents estimates for index crime arrests and Panel B presents estimates for all arrests. Referring to the table, we see that arrests that take place on public housing developments decline by nearly 50 percent as a function of the lighting intervention. Despite the fact that that these are arrests for crimes committed during all times of the day, this decline is nearly as large as the effect of the intervention on outdoor nighttime index crimes (60 percent). While imprecisely estimated, the decline in arrests for the net treatment area (-24 percent) is also close in size to the corresponding crime effects (-35 percent). Turning to Panel B of the table, we see a similar pattern when we consider all arrests, and not only arrests for index crimes. More generally, there is little evidence that the lighting intervention improved public safety by increasing the number of arrests made in these communities or that it has subjected public housing communities to surveillance that ultimately widens the net of the criminal justice system.

6.5.2 Heterogeneity in Crime Severity

While limited statistical power makes it difficult to estimate outcome models by crime type, we can nevertheless test whether the intervention had differential effects on several different forms of offending. We begin by noting that three quarters of the outdoor nighttime index crimes in our data are assaults and robberies. Therefore, as is evident in Table 3, we observe strong declines in these types of violent crimes as a function of the intervention. This evidence is consistent with several theories of situational crime prevention, notably that serious crimes may be deterred by more "eyes on the street" (Carr & Doleac, 2015; Cozens & Davies, 2013) or by the perception

that lighting increases the probability of apprehension (Welsh & Farrington, 2008). Likewise, while there is evidence to suggest that robberies increase with the available criminal opportunities (Groff, 2007) and that lighting may facilitate the identification of lucrative criminal targets (Deakin et al., 2007), we see little evidence that these effects outweigh the beneficial effects of street lighting.

We can also test the effect of the intervention on less serious crimes, including the types of public order offenses which are often thought of as "incivilities" (Swatt et al., 2013). We define less serious crimes as misdemeanor crimes with the exception of simple assaults and traffic offenses — 40 percent of these crimes are petit larcenies and another 30 percent are either criminal mischief or offenses against public order. The latter category consist primarily of disorderly conduct and public intoxication. These estimates are presented in **Table 5**. Overall, we observe that less serious crimes are not as sensitive to the intervention as index crimes. We estimate that less serious crimes declined by 25 percent on treated developments as a function of the intervention; referring to the net treatment area, the estimated crime reduction is just 4 percent. None of the estimates are significant at conventional levels. While we cannot rule out that the intervention may have had a modest effect on less severe forms of offending, if present, effects appear to be considerably smaller than they are for index crimes.

The pattern of effects that we observe is consistent with several different theories of criminal behavior. First, given that public order offenses receive far less punishment than violent offenses, the lighting intervention we study changes the expected cost of serious offending to a greater extent than for lower-level offenses. As such, these results are consistent with a model of rational offending in which offending shifts in accordance with changes in its cost. Second, the intervention may interact with heterogeneity in the skills of the offending population. In particular, while there is limited evidence that the general public has accurate information about the certainty and severity of criminal sanctions (Apel, 2013; Kleck & Barnes, 2014), there is evidence that more experienced

and criminally productive offenders update their perceptions in response to new information about the risk of arrest (Lochner, 2007). To the extent that this is true, we might expect that serious crimes like aggravated assault and robbery which are more likely to be perpetrated by offenders who actively participate in "street culture" (Jacobs & Wright, 1999) will be more sensitive to the design of public space than opportunistic crimes which are more likely to be committed by individuals with less criminal experience. This view of offending is consistent with several ethnographic accounts of the motivations of street robbers who are known to consider the features of the built environment in planning their crimes (Deakin et al., 2007; Hallsworth, 2013).

Finally, we note that the differential effects on serious versus less serious crimes that we observe differ somewhat from the effects of police-oriented situational crime prevention strategies which tend to have fairly uniform effects on both serious crimes and public incivilities (Braga & Clarke, 2014). While the reasons behind these differential impacts are likely complex, one explanation is that the presence of a police officer reduces crime through a different mix of perceptual deterrence channels than lighting. To see this, consider that behaving in a disorderly manner in front of a police officer is very likely to lead to an arrest while being disorderly in a well-lit area will probably only result in an arrest if a police officer observes the behavior. That is, lighting alone is unlikely to change the certainty of capture very much for a public order offense. On the other hand, an offender who commits a violent robbery needs to consider not only the proximity of a police officer but also whether street lighting may affect the certainty of arrest through activities undertaken by the police *after the crime is committed* — for example, interviewing witnesses or reviewing surveillance camera footage. The degree to which police allocate far greater resources towards investigating more serious crimes than less serious crimes can therefore have important implications for the comparative performance of police-oriented versus place-oriented situational crime prevention strategies.

6.5.3 Police Behavior

A remaining issue is that that the intervention potentially could have been associated with a change in the intensity of policing. How police might respond to a public safety intervention such as lighting — to the extent that they do at all — is theoretically ambiguous (Mears et al., 2007). On the one hand, it is possible that police view an increase in ambient lighting as a substitute for police manpower and shift officer resources towards alternative locations that remain poorly lit. On the other hand, police might view officer manpower as a compliment to new lighting, further concentrating resources in the areas that received additional lighting. To the extent that lighting is viewed as a substitute for police manpower, the estimates reported in Section 6.3 would presumably be conservative. However, to the extent that police see lighting as a compliment, we might be concerned that the treatment effect folds in the effect of increased police resources and presence, both of which have been demonstrated to reduce crime.

While it was not possible to obtain geo-located police deployment data, we obtained auxiliary NYPD data that serves as a proxy for the density and type of police activity in each of the public housing developments in the study. Specific variables include 1) the number of large scale gang enforcement activities in each development, 2) the number of home visits conducted by NYPD officers, and 3) "vertical patrols" conducted by officers within public housing stairwells. In order to assess whether these measures of police attention are associated with treatment, we separately regress the natural log of each of our two treatment indicators — binary treatment and dosage — on each of the three measures, conditioning on the square footage of the development. The F-statistics on the excluded police deployment measures from these two regressions are 0.87 and 0.22, respectively, indicating that there is little evidence that police deployments changed as a function of the lighting intervention. We conclude that police officers do not appear to change their activity in these areas, at least during the period in which the intervention was active.

7 Conclusion

Prior studies of street lighting find evidence that street lighting promotes public safety though the evidence is mixed and subject to a number of critical methodological limitations which have prevented researchers from reaching a consensus as to its promise (Doleac & Sanders, 2015; Marchant, 2004; Sherman et al., 1997). This field experiment provides novel evidence that a discrete environmental change brought about through a tactical investment in enhanced street lighting can reduce violent and otherwise serious crimes appreciably in disadvantaged urban areas.

Investments in lighting, including the tactical lighting intervention we study, offers cities a promising method to reduce crime while avoiding unintended costs associated with reliance on incapacitation, which has been shown to have high collateral costs for individuals as well as communities. We report the first evidence that ambient lighting affects crime primarily through general deterrence rather than incapacitation effects. This research thus compliments the large research literature on hot spots (Braga, 2001, 2005; Groff et al., 2015; Sherman & Weisburd, 1995) and problem-oriented policing (Braga & Bond, 2008; Braga et al., 1999), both of which point to suggestive evidence that the effects of intensive policing strategies operates primarily via general deterrence (Chalfin & McCrary, 2017; Nagin, 2013). Importantly, while prior literature tends to find that lighting reduces fear (Painter, 1996) and increases in a general feeling of well-being (Hanslmaier, 2013), there is little evidence that hot spots policing has the same ancillary benefits (Kochel & Weisburd, 2017; Ratcliffe et al., 2015; Rosenbaum, 2006; Weisburd et al., 2011).

The effects documented in this paper speak primarily to the ability of lighting to reduce violent outdoor, nighttime crimes. On NYC's public housing developments, approximately 12 percent of index crimes occur outdoors and during nighttime hours. Accordingly, a 35 percent reduction in outdoor, nighttime index crimes (confidence interval = 8-55 percent) means that lighting reduced serious offending in these communities by approximately 4 percent. To place this estimate in context,

we note here that a 4 percent change in index crimes is approximately what would be expected to occur during an economic recession (Gould, Weinberg, & Mustard, 2002; Raphael & Winter-Ebmer, 2001) or in response to a ten percent increase in police manpower (Chalfin & McCrary, 2018; Evans & Owens, 2007). Interestingly, the intervention's effect on less serious offending including theft and public order offenses is more muted indicating that this sort of intervention may not have a commensurate impact on all types of offending.

Is street lighting a scalable crime reduction strategy? There are a number reasons to be optimistic. First, darkness is a problem that affects every city to varying degrees throughout the year. Likewise, a nontrivial number of violent crimes occur outdoors and during nighttime hours. As such, lighting is an intervention that has broad applicability and is reasonably well targeted. Second, the application of street lighting is well understood and requires neither specialized knowledge nor a charismatic leader in order to successfully deliver the treatment as is often the case in delivering social services interventions to at-risk populations. Third, once installed, street lighting has low variable costs and, as such, is easier to maintain at scale than, for example, a hot spots policing intervention. Fourth, the benefits of improved lighting may extend well beyond their impacts on victimization. In particular, to the extent that lighting makes people feel safer, investments in lighting may contribute to a more active lifestyle among individuals in high crime communities (Roman & Chalfin, 2008; Stodolska et al., 2013), a general feeling of well-being (Hanslmaier, 2013) and, through its impact on crime, to a more robust local economy (Cullen & Levitt, 1999). Finally, the intervention is relatively inexpensive. The 40 developments in this research are home to approximately $60,000$ residents. Given that the intervention cost approximately $5 million, on a per-resident basis, the cost of the intervention is just $80, a tiny fraction of the amount of money it takes to educate a NYC public school student ($17,500) or house an inmate in Rikers Island, the city's jail ($121,000).

Several features of this intervention are worth noting as they may influence the design and scalability of a future candidate lighting intervention. First, the lighting studied here is very bright and provides more light (and more intensively focused light) than a typical investment in permanent street lighting would entail. Accordingly, policymakers should note that the installation of an equivalent number of native street lights may not generate the same magnitude of effects. Second, because the temporary light towers studied here are particularly prominent and are not a natural feature of the urban landscape, the intervention may fold in demonstration effects that may not accrue, in equal amounts, from a permanent intervention. Finally, we are only able to study the short-run impacts of the new lighting in these communities. To the extent that there is substantial seasonal variation in the effectiveness of the intervention or to the extent that victims and offenders adjust their behavior in the long-run, the permanent effect of lighting could differ from the short-run impact identified here. Accordingly, while this research provides powerful proof of concept that changes to ambient lighting reduce crime, the research lends particular support to the promise of a tactical lighting intervention to reduce crime in crime hot spots. Given that demonstration effects may explain a portion of the treatment estimates we observe, we note that the intervention may be especially likely succeed in communities where residents may perceive that there is longstanding municipal neglect.

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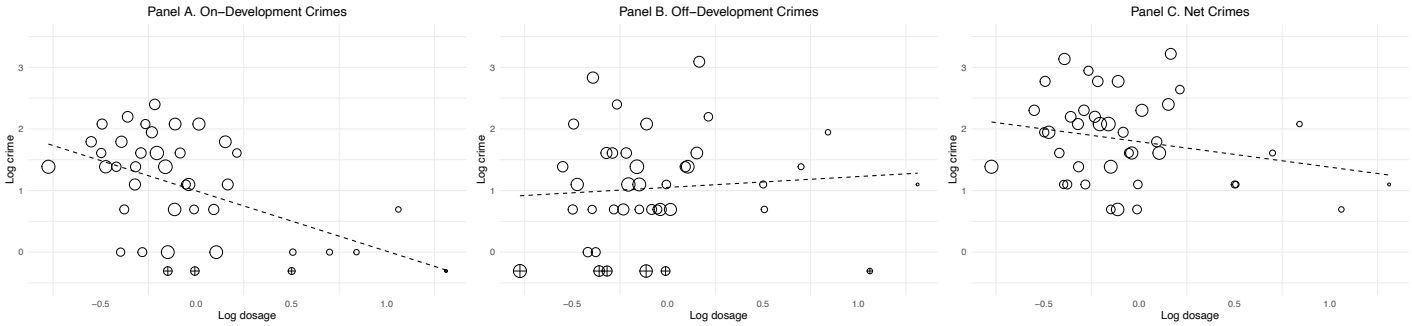
FIGURE 1 Light Towers in the Field

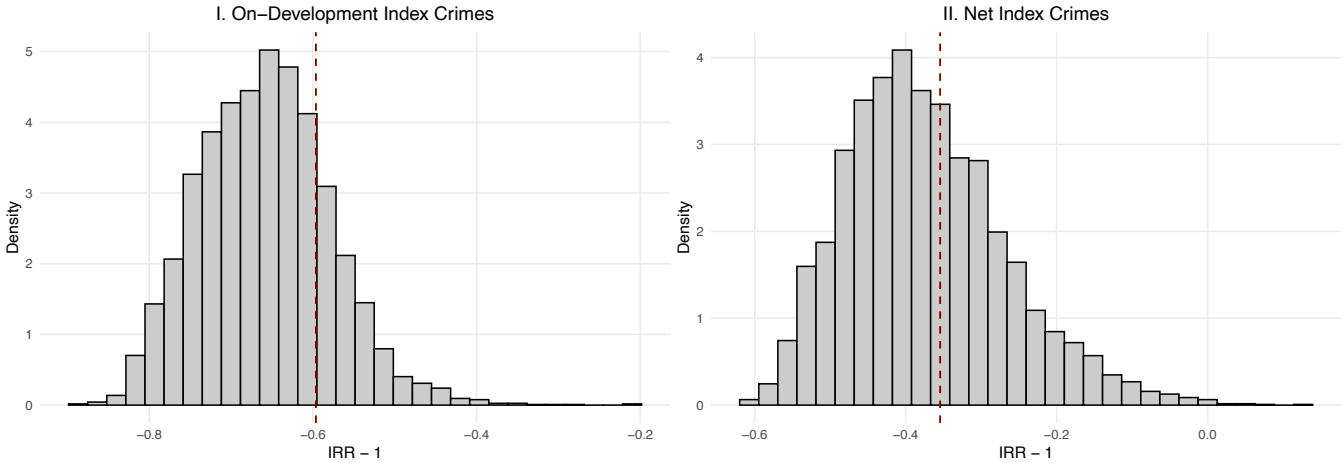
Credit: Ruddy Roye; https://www.wnyc.org/story/spotlight-safety-housing-projects

FIGURE 2 Relationship Between Lighting and Outdoor Nighttime Index Crimes



Note: Plots illustrate the relationship between the natural logarithm of the number of nighttime outdoor index crimes for the March through August 2016 study period and the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). Panel A considers crimes that occurred on the development's campus, Panel B considers crimes that occurred within a 550 foot catchment area of the development and Panel C considers crimes that occurred either on the development's campus or in the catchment area. Each hollow circle represents one of the N = 40 treatment sites with the size of the circle corresponding to the official population of the development. The dashed line represents a linear regression line through the data. A few sites did not experience any crimes over the study period. An approximation to the log value for these data points is obtained using a parametric correction suggested by Chalfin and McCrary (2018). These data points are denoted by a plus sign enclosed within the hollow circle.

FIGURE 3 Robustness of Estimated Treatment Effects to Alternative Sets of Control Variables



Note: Histograms report estimated treatment effects from a series of Poisson regressions of outdoor nighttime index crimes for the March through August 2016 study period on the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). Each model controls for population plus an additional random set of covariates (between 1 and 8). Covariates are drawn from a pool of aggregate crime counts and development demographics. For the full list of potential covariates, see Appendix Table 1, excluding the individual annual crime counts. In order to test the robustness of model estimates to our choice of control variables, we randomly sampled from among candidate control variables, drawing 5,000 samples.

Table 1. Summary Statistics

	Treatment	Control	p-value
Past Nighttime Crime			
On-development outdoor index crimes	3.60	3.00	0.17
Off-development outdoor index crimes	3.65	3.72	0.90
On-development outdoor nonserious crimes	11.04	8.60	0.09
Off-development outdoor nonserious crimes	7.43	7.78	0.78
Past Arrests			
On-development index arrests	6.18	5.00	0.56
Off-development index arrests	10.10	5.71	0.21
On-development all arrests	86.54	67.30	0.13
Off-development all arrests	105.27	76.67	0.23
Population Structure			
Avg. population	2452.50	2325.25	0.67
Avg. population density	184.14	186.62	0.91
Avg. household size	2.44	2.29	0.03
Pct. of population male 15-24	0.10	0.09	0.23
Physical Characteristics			
Avg. entrances per building	1.65	1.93	0.33
Square feet (thousands)	718.75	714.37	0.97
Style: elevator	0.82	0.72	0.41
Style: walk-up	0.05	0.07	1.00
F-test			
Treatment vs. control	-	-	0.52
Dosage	-	-	0.23
N			
	40.00	40.00	-

Note:
This table reports covariate means for the treatment and control groups, as well as a p-value for a t-test on the difference between those means. The penultimate two rows of the table report p-values on a joint test of the significance of all covariates in predicting treatment. The first row ("treatment vs. control") corresponds to a binary indicator of treatment; the second row ("dosage") corresponds to a continuous measure of the intensity of treatment within the treatment group. All p-values reported in the table are calculated using randomization inference and are based on 1,000 simulations from the "ri2" package in R (Coppock 2019).

Table 2. Poisson Estimates for Index Crimes

	On-Development	Off-Development	Net
Nighttime (D=1)	-0.91	-0.00	-0.44
	(0.33)	(0.42)	(0.22)
	[-60%]	[-0%]	[-35%]
Daytime (D=1)	-0.43	-0.23	-0.34
	(0.42)	(0.46)	(0.19)
	[-35%]	[-21%]	[-28%]
Nighttime (D=0)	-0.15	0.28	-0.12
	(0.30)	(23.41)	(0.32)
	[-14%]	[32%]	[-11%]
Daytime (D=0)	-0.03	0.06	-0.01
	(0.33)	(0.27)	(0.20)
	[-3%]	[6%]	[-1%]

Note:
This table reports estimates from a series of Poisson regressions of index crimes for the March through August 2016 study period on the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). Each cell reports a weighted median coefficient and standard error, derived from running 500 LASSO models that identified the most predictive covariates to include in the Poisson model. Estimates are reported for three geographic areas: 1) the development's physical campus (On-development), 2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development), and 3) areas that are within 550 feet of the development's campus inclusive of the campus itself, in addition to crimes that occur indoors on-development (Net). Estimates are reported separately for both nighttime and daytime crimes among the treatment sites as well as for outdoor nighttime crimes among the control sites. For each regression, in the first row, we report the weighted median across 500 LASSO runs of the Poisson regression coefficient. In the second row, we report the median standard error, which is computed for each LASSO run using 500 bootstrap replications via the boot package in R (Canty and Ripley 2017). In the third row, we report the percentage change in index crimes (incidence rate ratio (IRR) - 1).

Table 3. Poisson Estimates for Felony Assaults + Robberies

	On-Development	Off-Development	Net
Nighttime (D=1)	-1.17	-0.30	-0.49
	(0.42)	(0.40)	(0.35)
	[-69%]	[-26%]	[-39%]
Daytime (D=1)	-0.47	-0.47	-0.35
	(0.36)	(0.90)	(0.44)
	[-38%]	[-38%]	[-30%]
Nighttime (D=0)	-0.12	0.43	-0.02
	(0.34)	(0.33)	(0.16)
	[-12%]	[54%]	[-2%]
Daytime (D=0)	0.01	0.25	-0.01
	(0.33)	(0.54)	(0.21)
	[1%]	[29%]	[-1%]

Note:
This table reports estimates from a series of Poisson regressions of all felony assaults and robberies for the March through August 2016 study period on the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). Each cell reports a weighted median coefficient and standard error, derived from running 500 LASSO models that identified the most predictive covariates to include in the Poisson model. Estimates are reported for three geographic areas: 1) the development's physical campus (On-development), 2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development), and 3) areas that are within 550 feet of the development's campus inclusive of the campus itself, in addition to crimes that occur indoors on-development (Net). Estimates are reported separately for both nighttime and daytime crimes among the treatment sites as well as for outdoor nighttime crimes among the control sites. For each regression, in the first row, we report the weighted median across 500 LASSO runs of the Poisson regression coefficient. In the second row, we report the median standard error, which is computed for each LASSO run using 500 bootstrap replications via the boot package in R (Canty and Ripley 2017). In the third row, we report the percentage change in felony assaults and robberies (incidence rate ratio (IRR) - 1).

Table 4. Poisson Estimates for Arrests

A. Index Crime Arrests

	On-Development	Off-Development	Net
D=1	-0.63	-0.19	-0.27
	(0.25)	(0.17)	(0.27)
	[-47%]	[-18%]	[-24%]
D=0	-0.32	-0.14	-0.29
	(0.39)	(0.30)	(0.28)
	[-27%]	[-13%]	[-25%]

B. All Arrests

	On-Development	Off-Development	Net
D=1	-0.36	-0.21	-0.37
	(0.19)	(0.18)	(0.12)
	[-30%]	[-19%]	[-31%]
D=0	-0.37	0.05	-0.12
	(0.22)	(0.11)	(0.10)
	[-31%]	[5%]	[-11%]

Note:
Tables report estimates from a series of Poisson regressions of both index crime (Panel A) and all (Panel B) arrests for the March through August 2016 study period on the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). Each cell reports a weighted median coefficient and standard error, derived from running 500 LASSO models that identified the most predictive covariates to include in the Poisson model. Estimates are reported for three geographic areas: 1) the development's physical campus (On-development), 2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development), and 3) areas that are within 550 feet of the development's campus inclusive of the campus itself, in addition to crimes that occur indoors on-development (Net). Due to data limitations, estimates are reported across nighttime and daytime arrests among the treatment sites as well as for arrests among the control sites. For each regression, in the first row, we report the weighted median across 500 LASSO runs of the Poisson regression coefficient. In the second row, we report the median standard error, which is computed for each LASSO run using 500 bootstrap replications via the boot package in R (Canty and Ripley 2017). In the third row, we report the percentage change in all arrests. (incidence rate ratio (IRR) - 1).

Table 5. Poisson Estimates for Less Serious Crimes

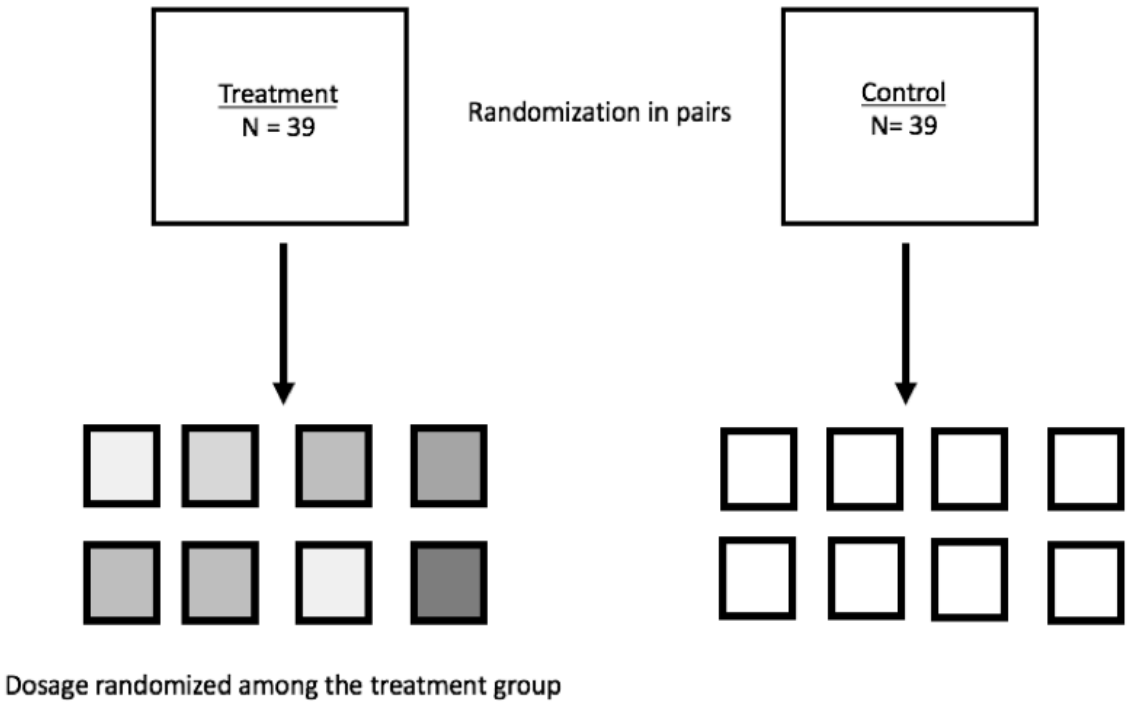
	On-Development	Off-Development	Net
Nighttime (D=1)	-0.28	0.18	-0.05
	(0.19)	(0.27)	(0.15)
	[-25%]	[20%]	[-4%]
Daytime (D=1)	-0.00	0.05	0.01
	(0.31)	(0.14)	(0.31)
	[-0%]	[6%]	[1%]
Nighttime (D=0)	0.25	0.19	0.12
	(0.20)	(13.04)	(0.22)
	[28%]	[20%]	[13%]
Daytime (D=0)	-0.09	0.12	0.03
	(0.24)	(0.24)	(0.15)
	[-8%]	[13%]	[3%]

Note:
This table reports estimates from a series of Poisson regressions of less serious crimes for the March through August 2016 study period on the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). Each cell reports a weighted median coefficient and standard error, derived from running 500 LASSO models that identified the most predictive covariates to include in the Poisson model. Estimates are reported for three geographic areas: 1) the development's physical campus (On-development), 2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development), and 3) areas that are within 550 feet of the development's campus inclusive of the campus itself, in addition to crimes that occur indoors on-development (Net). Estimates are reported separately for both nighttime and daytime crimes among the treatment sites as well as for outdoor nighttime crimes among the control sites. For each regression, in the first row, we report the weighted median across 500 LASSO runs of the Poisson regression coefficient. In the second row, we report the median standard error, which is computed for each LASSO run using 500 bootstrap replications via the boot package in R (Canty and Ripley 2017). In the third row, we report the percentage change in less serious crimes (incidence rate ratio (IRR) - 1).

ONLINE APPENDIX

Appendix A: Schematic of Research Design

In this appendix we present a simple schematic of the research design which shows how we have nested an experiment within an experiment. Among the $N = 39$ housing developments that received



the intervention, the number of light towers per square feet was randomly assigned. Accordingly, some developments received a large number of new light towers while other developments received only a small number of new light towers. In the control group, assigned dosage is zero by construction. In this schematic, the dosage is represented by the color of the shaded box for each development with darker colors indicating that a development was assigned a greater number of lights per square foot.

Appendix B: Additional Robustness Checks

In this appendix, we further interrogate these findings to test whether they are robust to alternative specifications and the removal of outliers. We begin by re-estimating the models presented in Table 2 excluding one development at a time from the treatment group. **Appendix Figure 3**, Panel A provides a visual depiction of these results for on-development index crimes and net index crimes. In each of the plots, the vertical axis represents the estimated treatment effect and the horizontal axis represents the rank of the excluded development's randomly assigned dosage. Point estimates are indicated by the black dots; the associated whiskers represent the width of the 95 percent confidence intervals. For both on-development and net index crimes, the results are extraordinarily robust to dropping a given development.

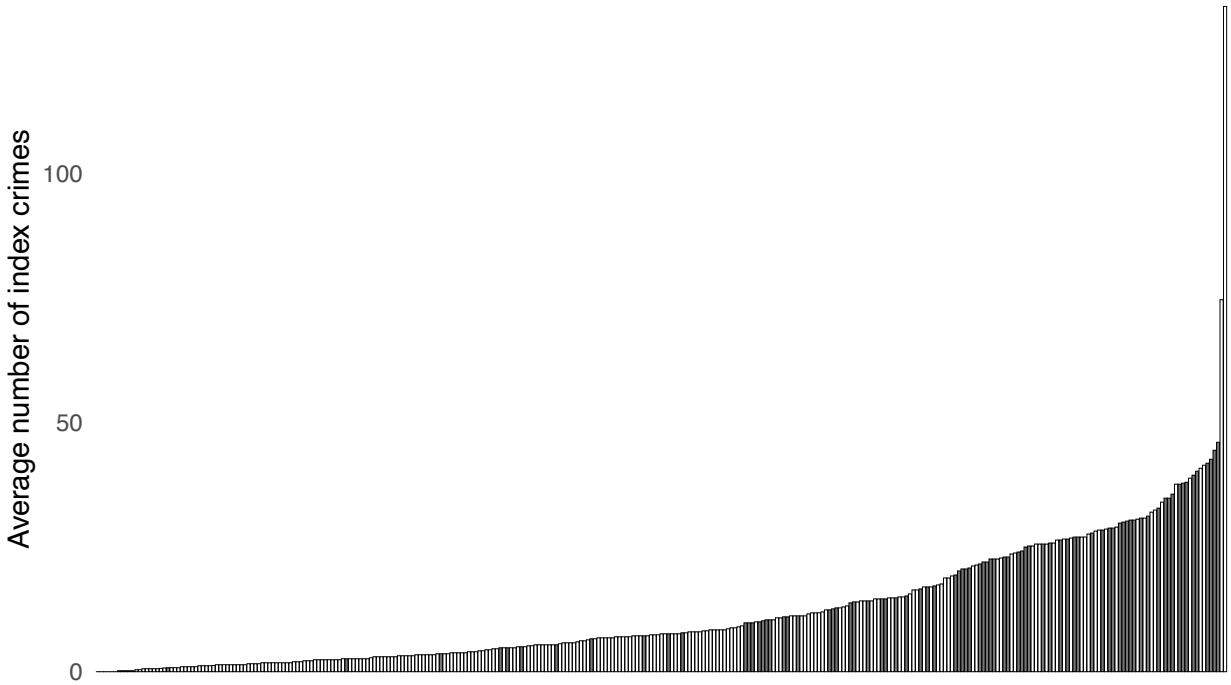
Next, we turn to whether the results are robust to excluding the highest dosage developments. In particular, given that the smallest developments received the highest dosages, we might worry that results would be extremely sensitive to the exclusion of a few small developments that, for idiosyncratic reasons, might have experienced a large percentage change in crime. **Appendix Figure 3**, Panel B plots estimated treatment effects excluding the highest ranked all the way up to the eight highest-ranked developments according to their assigned dosage. The estimates are remarkably insensitive to the exclusion of the highest dosage developments.

Next, we consider robustness to changes to the functional form of the outcome models. First, we re-estimate outcome models using ordinary least squares regression as opposed to Poisson regression. These models are presented in **Appendix Table 1**. Consistent with the Poisson estimates reported in Table 2, using least squares, we estimate that the intervention led to a decline of 2.4 crimes which is a 33 percent reduction in net outdoor nighttime crimes ($p < 0.05$). An alternative approach to estimating treatment effects would have been to pool the treated and control developments in a single analysis, where the treatment variable is the number of additional light towers assigned. In this specification, the treatment variable is equal to the randomly assigned dosage of lighting per square foot among the treatment developments and is equal to zero among the control developments. While such a specification requires that we assume that the difference between the control condition and receipt of one light tower is equivalent to marginal gradations in light towers assigned among the treatment group, we nevertheless report these results for completeness. Given that the log of zero is undefined, we estimated Poisson models of the count of crime on dosage in levels rather than logs. We find that each additional light tower per square block (125,000 square feet) reduces on-development outdoor, nighttime index crimes by 23 percent ($p = 0.07$) and net index crimes by 20 percent ($p <= 0.01$). These results are qualitatively similar to the main estimates reported in Section 6.3.

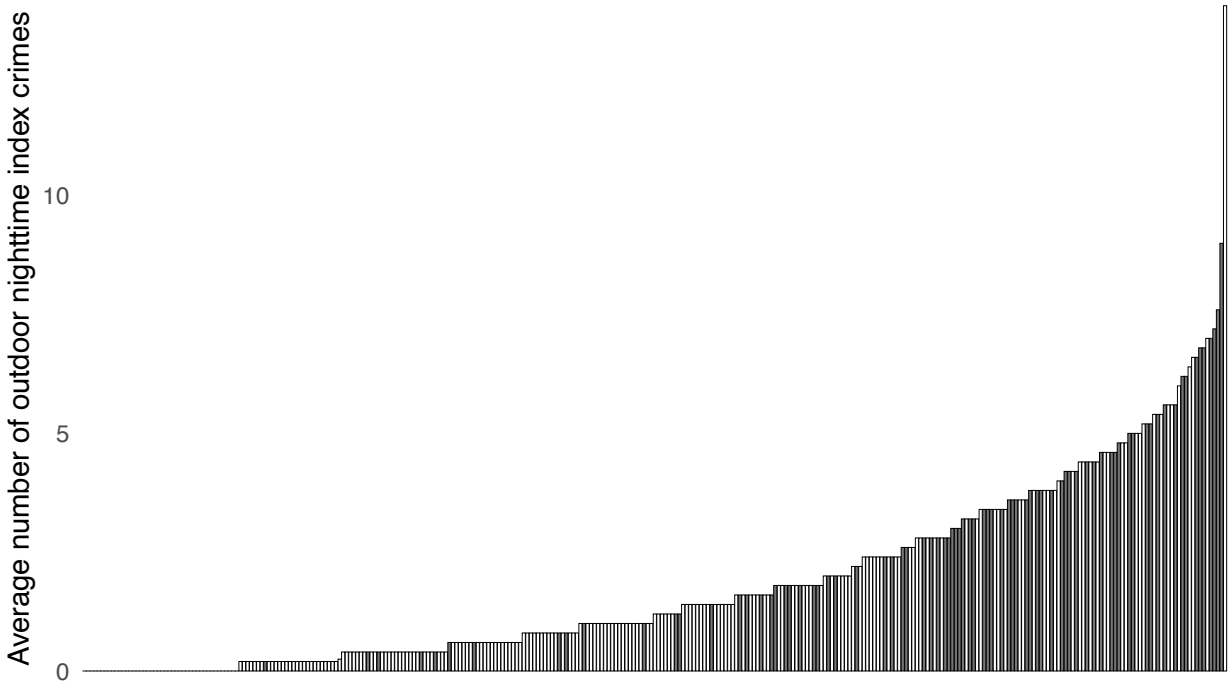
Finally, we consider the sensitivity of the conservative estimate of the lighting intervention on net crime to changing the radius used to detect spillovers, as shown in **Appendix Figure 4**. The plots show that meaningful effects persist even at very large radii around the treated developments.

APPENDIX FIGURE 1 Average Index Crime within NYC Housing Authority Developments

A. On-Development Index Crime

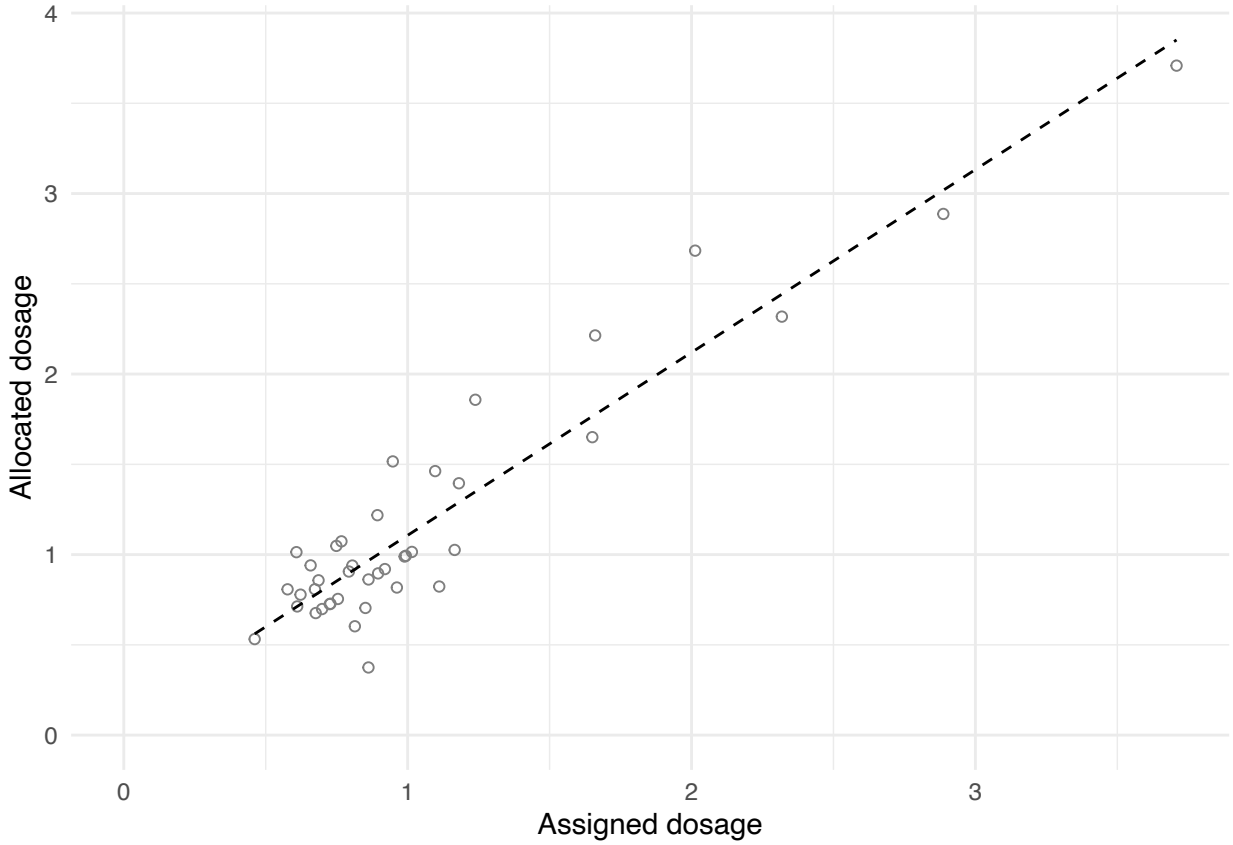


B. On-Development Outdoor Nighttime Index Crime



Note: Figures plot the average number of index crimes occurring between March and August over the 2011-2015 pre-intervention period. Each bar represents a NYC Housing Authority development; dark grey bars correspond to the N = 80 study sample sites.

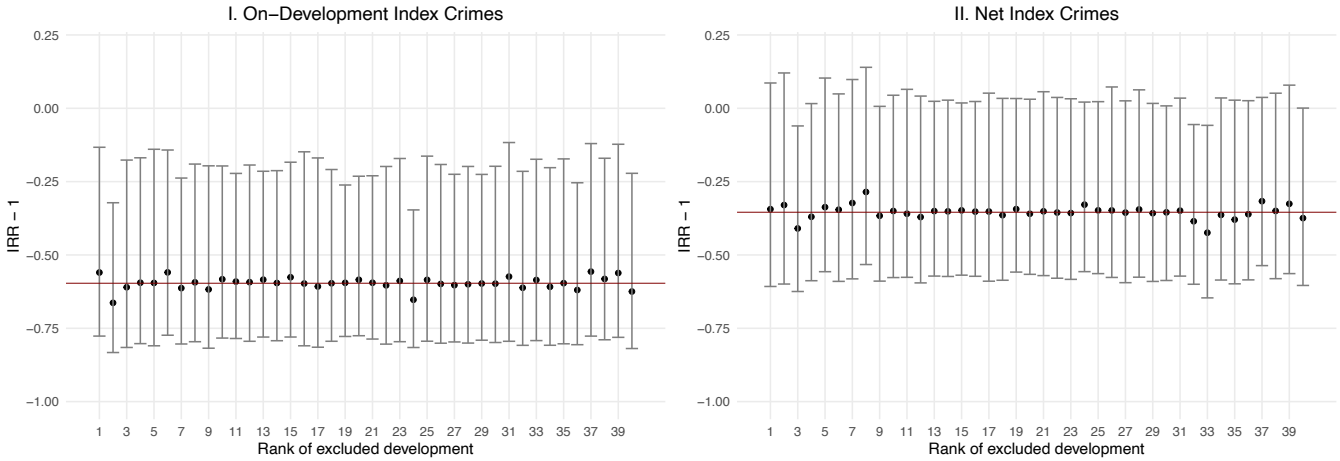
APPENDIX FIGURE 2 Relationship Between Assigned and Allocated Dosage of Lighting



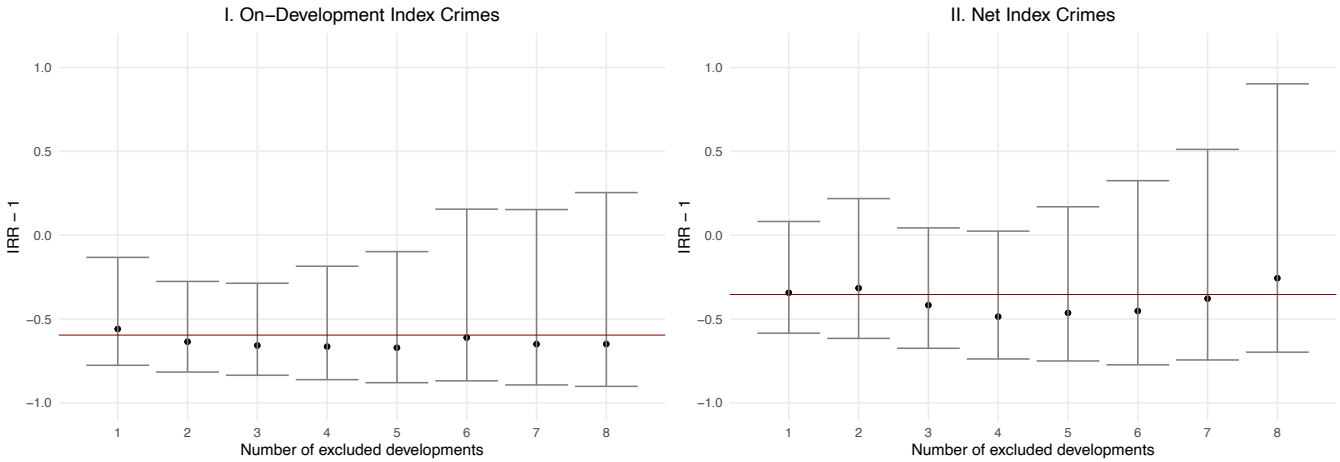
Note: Figure plots the relationship between the randomly assigned number of lights per square city block (assigned dosage) and the actual number of lights per square city block that the development received (allocated dosage). Each hollow circle represents one of the N = 40 treatment sites. The dotted line plots the linear regression of allocated dosage on assigned dosage.

APPENDIX FIGURE 3 Robustness of Estimated Treatment Effects to Dropping Developments

A. Excluding One Development at a Time

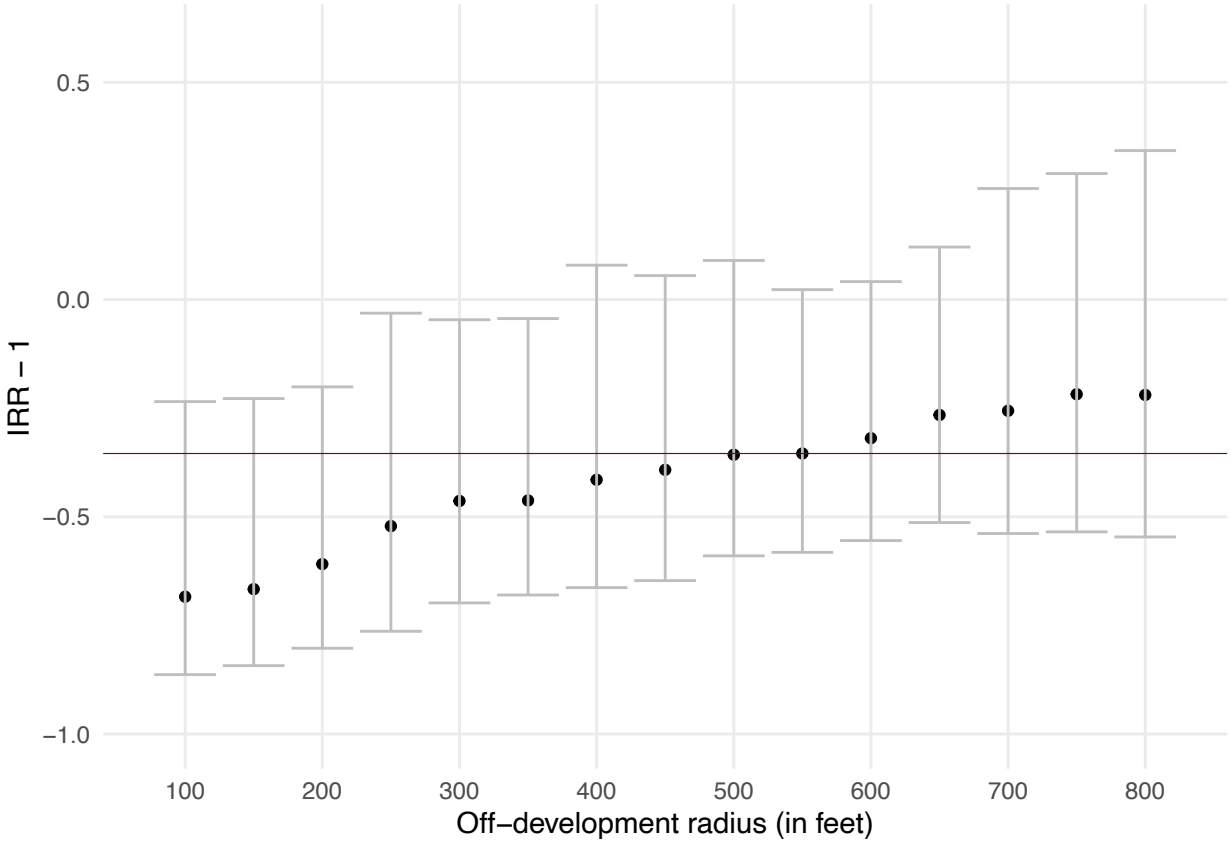


B. Excluding Developments Cumulatively, According to Dosage



Note: Plots report estimated treatment effects from a series of Poisson regressions of index crimes for the March through August 2016 study period on the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). In order to test the sensitivity of the estimated treatment effects to the exclusion of a handful of highly leveraged developments, we re-estimate the model excluding one development at a time (Panel A) and excluding developments cumulatively, according to assigned dosage (Panel B). Treatment effects and corresponding 95 percent confidence intervals are plotted for each of the 40 treated developments. Estimates and confidence intervals are the median among 500 models, each of which selects predictors using LASSO regression. These LASSO selected variables are the same selections that we use in Table 2. Confidence intervals are based on standard errors that have been bootstrapped 500 times using the "boot" package in R (Canty and Ripley 2017). The red horizontal line in all panels represents the primary estimate of the treatment effect reported in Table 2.

APPENDIX FIGURE 4 Robustness of Estimated Treatment Effects for Net Index Crimes to Various Displacement Radii



Note: Plots report estimated treatment effects from a series of Poisson regressions for net index crimes - crimes that occurred either on-development or within a given catchment area of the campus for the March through August 2016 study period. For a given displacement radius, we regress outdoor nighttime index crimes on the natural logarithm of each housing developments randomly assigned number of additional lights per square block (treatment dosage). Estimates and 95 percent confidence intervals are the median among 500 models, each of which selects predictors using LASSO regression. These LASSO selected variables are the same selections that we use in Table 2. Confidence intervals are based on standard errors that have been bootstrapped 500 times using the "boot" package in R (Canty and Ripley 2017). The red horizontal line in all panels represents the primary estimate of the treatment effect reported in Table 2.

Appendix Table 1. Ordinary Least Squares Estimates for Index Crimes

	On-Development	Off-Development	Net
Nighttime (D=1)	-1.32	-0.71	-2.41
	(0.76)	(1.04)	(1.04)
Daytime (D=1)	-0.80	-1.52	-1.11
	(1.32)	(1.20)	(1.30)
Nighttime (D=0)	-0.38	0.72	-0.51
	(0.78)	(1.04)	(1.57)
Daytime (D=0)	-0.73	0.75	0.95
	(0.78)	(0.98)	(1.48)

Note:
This table reports estimates from a series of ordinary least squares (OLS) regressions of index crimes for the March through August 2016 study period on the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). Each cell reports a weighted median coefficient and standard error, derived from running 500 LASSO models that identified the most predictive covariates to include in the OLS model. Estimates are reported for three geographic areas: 1) the development's physical campus (On-development), 2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development), and 3) areas that are within 550 feet of the development's campus inclusive of the campus itself, in addition to crimes that occur indoors on-development (Net). Estimates are reported separately for both nighttime and daytime crimes among the treatment sites as well as for outdoor nighttime crimes among the control sites. For each regression, in the first row, we report the weighted median across 500 LASSO runs of the OLS regression coefficient. In the second row, we report the median standard error, which is computed for each LASSO run using 500 bootstrap replications via the boot package in R (Canty and Ripley 2017).