Can Deterrence Persist? Long-Term Evidence from a Randomized Experiment in Street Lighting*

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Abstract

Research summary: For centuries and even millenia, street lighting has been among the most ubiquitous capital investments that societies have made in public safety. Recent research by Chalfin et al. (2021) — the first randomized experiment that studies the effect of street lighting on public safety — demonstrated that a tactical street lighting intervention in New York City's public housing developments led to a 36 percent reduction in serious criminal activity during nighttime hours in the six months after the new lights were rolled out. But do the effects endure? In this study, we examine the longer-term effects of the same street lighting intervention using three years of outcome data. We show that the effects of the lighting intervention persist over time. Critically, the intervention reduced crime without eventually leading to a larger number of arrests.

Policy Implications: As street lighting requires a large up-front capital investment, the attractiveness of enhanced lighting to policymakers depends critically on whether its public safety benefits will be long-lasting. These findings provide some assurance that the impact of street lighting can endure beyond their initial installation. Because the lighting intervention reduced crime without increasing the number of arrests, it did not reduce crime by widening the net of the criminal justice system.

Keywords: Street lights, Place-based crime prevention, deterrence studies

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1 Introduction

This paper considers the impact of one of the most enduring capital investments to promote public safety: street lighting. Among other benefits, street lights are widely thought to be an effective tool in reducing crime and, therefore, have become an especially ubiquitous type of investment in environmental design (Farrington and Welsh, 2002; Welsh and Farrington, 2008). Research in criminology, economics and urban planning suggests that improvements in lighting are welcomed by residents and tend to reduce fear of crime (Painter, 1996; Kaplan and Chalfin, 2021) and improve perceptions of community safety (Atkins et al., 1991; Herbert and Davidson, 1994). Likewise, the available evidence on improved street lighting suggests that its impact on crime is promising, leading to appreciable reductions in serious criminal behavior (Welsh and Farrington, 2008; Doleac and Sanders, 2015; Domínguez and Asahi, 2017; Arvate et al., 2018; Chalfin et al., 2021, 2020). These effects are especially encouraging given the relatively low cost of maintaining street lights as compared to other crime control interventions like CCTV (Piza et al., 2019) and the minimal technical know-how that is required to scale enhanced street lighting in most jurisdictions (Chalfin et al., 2021).

While there is growing evidence that improved lighting generates an important public safety dividend, due to the difficulty of keeping experiments in the field for a long period of time and ever-changing neighborhood conditions, a persistent limitation of the available literature is that we do not know whether the short-term deterrence effects documented in prior research will endure over a longer time period (Welsh and Farrington, 2008).[1] This limitation is not unique to studies of street lighting; indeed a dearth of long-term research findings is a global feature of a great deal of deterrence research more generally (Nagin, 1998; Weisburd and Telep, 2014). Understanding the time-path of deterrence is of critical importance to policymakers who must decide whether to continue to invest in place-based crime reduction strategies as well as to deterrence scholars who have been interested in understanding the ways in which individuals respond and adapt to crime control strategies.

This paper reports the first experimental estimates of the longer-term impact of enhanced street lighting on crime, focusing on a three-year follow-up period. In particular, we study the longer-term effects of a randomized experiment of the effectiveness of additional

[1] In their seminal review paper, Welsh and Farrington (2008) note that "future lighting schemes should employ high quality evaluation designs with long-term followups."

street lighting in controlling crime in public housing developments in NYC. Analyzing long-term followup data from prior experiments has a rich tradition in empirical social science research. In the broader social sciences, prominent examples include the large literatures which have re-analyzed the seminal 1962 Perry Pre-School study (Barnett, 1985; Nores et al., 2005; Heckman et al., 2010b,a), the effectiveness of Head Start programming (Aughinbaugh et al., 2001; Garces et al., 2002; Ludwig and Miller, 2007; Ludwig and Phillips, 2008; Deming, 2009; Anderson et al., 2010), the Moving to Opportunity Studies of the 1990s (Leventhal and Dupéré, 2011; Gennetian et al., 2012; Ludwig et al., 2013; Sciandra et al., 2013; Chetty et al., 2016), and a variety of conditional cash transfer programs including the PROGRESA program in Mexico (Gertler, 2004; Behrman et al., 2011; Parker and Vogl, 2018; Barrera-Osorio et al., 2019). Within criminology, Farrington and Welsh (2013) and Farrington (2021) provide a comprehensive summary of long-term follow-ups of experimental research. Prominent examples include follow-ups to experimental research on juvenile boot camps (Bottcher and Ezell, 2005), the provision of pre-school programming (Schweinhart, 2013), mandatory arrest for domestic assault (Angrist, 2006; Sherman and Harris, 2013) and restorative justice programming (Jeong et al., 2012), among others. To date, the lion's share of these follow-up studies track individuals over the life course. There have been very few follow-up studies of experimental place-based interventions.

The field experiment that we study, originally analyzed by Chalfin et al. (2021), was launched in March 2016 and studied a "tactical" lighting intervention in which temporary light towers were randomly allocated to augment existing lighting in disadvantaged NYC communities. That research, which is the first randomized experiment of street lighting in the literature, found that during the six-month study period spanning the spring and summer of 2016, outdoor nighttime index crimes declined by 36 percent as a function of the intervention leading to a 6 percent overall decline in serious criminal activity in communities which received the intervention. Critically, arrests in these communities declined by a similar amount indicating that the lights did not appear to increase the number of people who were incapacitated by arrest and subsequent incarceration. The lighting intervention therefore seems to have generated large general deterrence effects.

Due to resource constraints which at, the time, were thought to be binding, the temporary light towers which were added to these communities in March 2016 were scheduled to be removed at the end of August 2016, six months after they were first deployed. However, in response to support for the intervention among community members, the temporary light towers remained in the field for at least three more years. This turn of events was entirely unexpected; indeed the researchers only learned in late 2020 that the entire intervention had remained in the field after the original study period had ended.

We leverage this unexpected opportunity to study the longer-term deterrence effects of the NYC street lighting intervention. We show that three years later the effects of the intervention remain strong and attenuate only modestly relative to the initial six-month study period. Seasonality in the magnitude of the effects is likewise minimal. The remainder of the paper is organized as follows. In Section 2, we provide a description of prior research on street lighting as well as other place-based strategies designed to deter crime, focusing particular attention on the limited availability of evidence of long-term impacts. In Section 3, we provide an account of the field experiment, Section 4 provides a description of the data and Section 5 provides a description of the research design and econometric methods used to estimate treatment effects. Section 6 presents our results and Section 7 concludes.

2 Prior Literature

2.1 Street Lighting

During the past fifty years, a large and growing situational crime prevention literature has arisen to study the public safety impacts 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 (Branas et al., 2011; Kondo et al., 2015; Bogar and Beyer, 2016; Kondo et al., 2018), restoring vacant lots (Garvin et al., 2013; Kondo et al., 2016; Branas et al., 2016, 2018; Moyer et al., 2019), public-private partnerships (Cook and MacDonald, 2011; MacDonald et al., 2013), and reducing physical disorder (Sampson and Raudenbush, 2001; Keizer et al., 2008; Skogan, 2012; Braga et al., 2015).

The situational crime prevention strategy that we study here — street lighting — has been around, in one form of another, for millennia. 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; Chalfin et al., 2021). 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 world.

Much of the academic literature on street lighting is summarized in a systematic review by Welsh and Farrington (2008), who conducted a comprehensive scan of the literature, identifying thirty-two street lighting studies in the United States and the United Kingdom, published prior to 2008. Among the thirteen "differences-in-differences" studies in which an appropriate comparison group was available, 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.[2]

While research by Sherman et al. (1997) and Marchant (2004) among others has questioned the internal validity of the older literature, since the seminal review of Welsh and Farrington (2008), more recent research has lent additional support to the idea that ambient lighting can appreciably deter crime. In particular, studies by Doleac and Sanders (2015) and Domínguez and Asahi (2017) exploit variation in lighting induced by the discrete shift to daylight savings time (DST) in March and October of each year. Using both a differences-in-differences and regression discontinuity approach, these papers find evidence that DST reduces crime, particularly robbery.[3] A second approach considers the impact of *street light outages* which generate quasi-random variation in the availability of nighttime ambient lighting in cities throughout the world. Using public crime microdata from Chicago, Chalfin et al. (2020) find that robberies and motor vehicle thefts rise during street light outages, albeit on surrounding blocks rather than the block that is directly affected by the outage. In a developing country context, research by Arvate et al. (2018) suggests that a rural electrification project that, among other things, considerably improved street lighting

[2]Among the U.S. studies, lighting was found to be broadly effective in Atlanta, Milwaukee, Fort Worth and Kansas City and ineffective in Portland, Harrisburg, New Orleans and Indianapolis. In the U.K., lighting was considered to be effective in Bristol, Birmingham, Dudley, and Stoke. In the fifth location (Dover), the improved lighting was confounded with other public infrastructure improvements.

[3]Doleac and Sanders (2015) studies the effect of daylight savings time in the United States using data from the National Incident-Based Reporting System while Domínguez and Asahi (2017) studies the effect of daylight savings time using data from Chile.

in Brazil, led to large declines in violence. The lone field experiment in the literature was conducted by Chalfin et al. (2021) who studied the random allocation of temporary street lights to thirty-nine public housing developments in New York City, finding that additional street lights reduced serious outdoor nighttime crimes by approximately 36 percent — at least over the six-month intervention period covering the time just after the new lighting was rolled out.

Throughout the literature, a key limitation is that few studies offer insight into whether increases in ambient lighting yield enduring treatment effects (Welsh and Farrington, 2008). Referring to Welsh and Farrington's meta-analysis, only four of the thirty-two studies they identified in the literature have follow-up periods that are longer than one year. Of those four, only one employs a comparison group and that study is quite old, published in 1977.[4] Among the more recent quasi-experimental research, follow-up periods tend to be even shorter. For example, research that uses daylight savings time as a natural experiment uses a bandwidth of just three *weeks* and research on street light outages in Chicago by Chalfin et al. (2020) uses a study period of only 7-11 *days*. Only the study of rural electrification in Brazil by Arvate et al. (2018) captures long-run impacts, studying the evolution of crime trends over more than a decade of time. However, electrification plausibly affects crime through many different channels aside from lighting and so this intervention likely captures the effects of economic development more broadly in addition to the greater availability of lighting.

In this paper, we present the first modern lighting study to consider the long-run impact of street lighting on crime. We study the longer term impact of a tactical lighting intervention that is widely available to municipal policymakers throughout both the developed and developing world. Critically, since the evidence is generated from a randomized experiment, we can credibly rule out confounding variation 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 Domínguez and Asahi (2017).

[4] This research, by Sternhell (1977) considers the impact of a lighting intervention in New Orleans using a follow-up period of 34 months. The research was not published in a peer-reviewed journal.

2.2 Short vs. Long-Run Deterrence Effects

A large literature in the behavioral and cognitive sciences finds that human beings are capable of adapting their behavior to changes in the situational environment (Wohlwill, 1974; Pearson et al., 2011). In the broader social sciences, evidence of behavioral adaptation over time has been found in areas as diverse as environmental conservation (Allcott and Rogers, 2014; Bernedo et al., 2014), transportation (Carrel et al., 2013) and individuals' response to health shocks (Graham, 2008; Oswald and Powdthavee, 2008) and poverty (Clark et al., 2016).[5] Within criminology, a wide array of deterrence literature suggests that individuals who commit crime and successfully avoid arrest tend to lower their subjective probabilities of apprehension (Lochner, 2007; Anwar and Loughran, 2011; Apel, 2013) and that people with high-propensity toward criminal behavior are more responsive to changes in formal sanctions than low-propensity individuals (Thomas et al., 2013). These findings provide the basis for thinking about deterrence as a learning process that can evolve substantially over time.[6]

Interventions that are intended to change people's behavior are ubiquitous in modern society. As suggested by Frey and Rogers (2014), four behavioral mechanisms explain the extent to which the effects of interventions will persist over time: the degree to which they cause individuals to build psychological habits and engage in more automated behaviors (Ouellette and Wood, 1998), the degree to which they change how individuals think, the extent to which they change future costs, and via peer effects or "external reinforcement."

With respect to ambient lighting, a key question is whether patterns of behavior that are initially disrupted by the provision of enhanced lighting revert back or otherwise adjust in ways that cause crime to return to equilibrium levels. How might this happen? We offer several examples. First, criminally-involved individuals might initially associate the provision of enhanced lighting with a signal that an area is being surveilled to a greater degree by police. But when it becomes apparent that this is not the case, these individuals might recognize that there is little to fear from the new lighting. Likewise, those individuals who spend time congregating in outdoor spaces might be initially put off by enhanced lighting, moving to other locations to engage in criminal activity. Yet they may come to

[5] There is also evidence of behavioral persistence — in particular, in a recent meta-analysis of the impact of behavioral "nudges" (Brandon et al., 2017).

[6] An example with respect to civil enforcement can be found in Dávila et al. (2002) who study the response of undocumented immigrants to changes in the immigration enforcement regime and finds that immigrants adapt over time to enforcement measures.

accept the new lighting after learning that alternative venues to congregate are unsuitable. A third possibility is that individuals may simply learn how to adapt their criminal activities to the new lighting, either by shifting geographically to unlit parts of the same community or by taking advantage of the features of lighting that may create advantages for their activities. For instance, greater visibility might increase the likelihood of criminal activity by reducing crime commission costs (Nagin, 2013), better enabling the location and identification of more vulnerable victims or more lucrative criminal rewards (Welsh and Farrington, 2008; Chalfin et al., 2021). Finally, the salience of lighting might simply decline over time, thus having less deterrence value, especially for individuals who are myopic or who discount hypberbolically (Loughran et al., 2012).

Behavioral adaptation can also be brought about through changes in victim behavior. For example, research suggests that ambient lighting reduces fear (Painter, 1996) and that individuals might be more likely to use public space as a result. As such, to the extent that victim behaviors adapt over time, this might create upward pressure on crime mechanically via routine activities theory (Cohen and Felson, 1979). On the other hand, to the extent that more human activities creates more "eyes upon the street" (Jacobs, 1961), the effect of enhanced lighting can become self-perpetuating leading to a permanent reduction in crime in a community. This concept is a close cousin to broken windows theory (Wilson and Kelling, 1982) which suggests that disorder in public spaces can set off an unwelcome chain of events in which the streets ultimately end up being dominated by individuals involved in crime.

Ultimately, the extent to which community crime adapts to place-based crime prevention efforts over time cannot be signed theoretically; it is an empirical question. By studying the long-term impacts of the first field experiment in street lighting, we can estimate the net impact of any behavioral adjustments to the intervention in NYC's public housing communities.

3 Field Experiment

The field experiment described in this paper was placed into the field in the Spring and Summer of 2016 in New York City. Through a partnership between the the NYC Mayor's Office of Criminal Justice, the New York City Police Department and the New York City

Housing Authority, additional temporary light towers were randomly allocated to the City's public housing developments. These towers, pictured in **Figure 1**, emit approximately 600,000 lumens—a measure of brightness—making them extraordinarily luminous, considerably more luminous than a standard permanent street light which ranges in luminosity from between 5,000 and 35,000 lumens, depending on the type of lighting fixture. The temporary light towers are diesel-powered and re-fueled manually each day during daylight hours by temporary city workers who were hired specifically to staff this intervention and they did not have other responsibilities such as maintenance of existing street lights. The lights had an automatic timer set to turn on at sunset and off at sunrise, ruling out the possibility of non-compliance with the intervention.

As noted in Chalfin et al. (2021), several features of the light towers merit further discussion. First, because the light towers are diesel-powered, they generate a small amount of noise and, sometimes, a noticeable smell when an individual is situated within a few feet of a light tower. Second, the light towers are very tall and are extremely prominent in the landscape of the communities that received them. As a result, in addition to providing more lighting, individuals may perceive the addition of the towers as a sign that either law enforcement or other city officials care about the general location where the towers have been placed. As such, in addition to the impact that they have on ambient lighting, the light towers might also reduce crime, in part, through a "demonstration effect."

4 Data

To measure crime in the study locations, we use public NYPD administrative data on criminal complaints and arrests from March 2011 through March 2019. 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 two types of crimes. Index crimes, 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.[7] We also study low-level "quality of life" crimes which include all misdemeanors except assault in the third degree, offenses against the person, and intoxicated and impaired driving. For both types of crimes, 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.

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. We use public microdata on geo-located arrests which are available at NYC's Open Data portal. 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.

5 Research Methods

5.1 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. From this list, the authors 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 geographical size in the two years prior to the intervention.

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 people live in the areas receiving the intervention, given resource constraints, only a relatively small number of locations could be treated. In order to maximize statistical power, we go beyond a simple treatment-control design by randomizing the dosage of

[7] 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.

lighting received by each community in a block randomized design. This method has been shown to be a better alternative to naive randomization in small N place-based experiments (Weisburd and Gill, 2014).

Three hundred and ninety-seven lighting towers were available to be randomly assigned amongst the 40 treated housing developments. For operational reasons, municipal policymakers decided that each of the treated developments would be allocated at least two towers, regardless of the development's square footage. The remaining 317 lighting towers were then 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 number of lights per square feet across the developments. In response to feedback from residents, the allocated dosage was slightly adjusted and so differs to a very small degree from the randomly assigned dosage. 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.

Echoing Chalfin et al. (2021), we pause here to consider the implications that this research design has for interpreting our estimates. 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 variation in the provision of lighting. 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 such a small dosage of lighting it may not be sufficient to induce any behavioral response. As a result, comparisons between treated and control developments will tend to understate the effect of the intervention.

5.2 Statistical Models

We follow the modeling strategy employed in Chalfin et al. (2021) in order to maintain consistency with prior research and to constrain our ability to "p-hack," a first-order concern in a small N experiment such as this one. 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):

$$\log(\gamma_i) = \alpha + \beta D_i + X_i^{'}\rho \tag{1}$$

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 10 percent. However, estimates are very imprecise (SE = 10 percent) 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 here that these estimates will tend to be biased downward.

Because the treatment is heterogeneous and because there is considerable variability in the assignment of a dosage of lighting to each development, 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. In a series of robustness checks, we change the modeling strategy in a number of different ways and confirm that our preferred estimates are replicated regardless of the precise analytic strategy that is employed.

Next, we turn to the issue of which potential control variables we should add to our model to reduce residual variance as well as to guard against finite sample bias due to imperfect randomization (Angrist and Pischke, 2008; Imbens, 2010).[8] However, 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 could be used to estimate treatment effects. Furthermore, in this instance, theory provides incomplete guidance for selecting the functional form of available covariates—for instance, should we control for population or its natural log? Or, should we

[8] This is a necessary process in small N experiments, particularly those with highly variable outcome data, because of the inevitable sensitivity of treatment effects to reasonable differences in modeling assumptions. In contrast, a large experiment can be straightforwardly evaluated using a t-test which makes minimal assumptions and and affords researchers with little discretion.

control for past crime using six-month or one-year windows?

In order to select a model in a principled way, we appeal to a growing literature in statistics that leverages lessons from machine learning to improve the practice of causal inference (Belloni et al., 2014; Varian, 2014; Athey and Imbens, 2015).[9] 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 (Zou and Hastie, 2005; Tibshirani, 2011; Meinshausen and Bühlmann, 2006).

The LASSO has the virtue of retaining only the subset of predictors that are genuinely predictive 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, LASSO regression provides a means of strengthening the use of theory in selecting an outcome model by automating covariate selection from among a pool of theoretically important predictors. This process, that Athey and Imbens (2016) refer to as "honest causal estimation," makes model selection more robust to the problem of false discoveries.

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. Following (Chalfin et al., 2021) and in keeping with a common practice in machine learning applications, we set $k = 5$.[10] 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.

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

[10] As is noted by Casella et al. (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.

Likewise, as described in Chalfin et al. (2021), the small size of the sample and the fairly noisy outcome variable creates a challenge in applying LASSO to our data. Consequently, the variables selected by the LASSO can be sensitive to how the data are randomly partitioned into the five folds. To address this concern, 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.

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. 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.

6 Results

6.1 Fidelity to Randomization

We begin by providing evidence that randomization was carried out properly. **Table 1**, which is adapted from Chalfin et al. (2021), 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.[11] 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 it may seem that outdoor nighttime crimes in public housing developments are 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 outdoor crimes per 100,000 population annually.

The next panel of in the table considers four different measures of population structure: the development's official population, its population density (population per 1,000 square

[11] Because the 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 500 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.

feet), and average household size. Most developments in the sample are large, housing on average 2,400 residents in an area of roughly 700,000 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.

Despite the difficulties of small sample randomization 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.[12] For both the binary and continuous treatment variables, the p-values on the F-statistic are approximately 0.6 indicating that covariates do not predict treatment, a finding that is consistent with successful randomization.

6.2 Main Results

We present the main estimates for the full three-year follow-up period in **Table 2**, including estimates for the treated housing developments as well as for a 550-foot catchment area around each treated development, a model which is intended to test for spatial displacement (Weisburd et al., 2006; Guerette and Bowers, 2009; Bowers et al., 2011). We focus on crimes that occur outdoors — that is not within a residential or a commercial space — and present estimates separately for index crimes and less serious crimes, which include all misdemeanors except assault in the third degree, offenses against the person, and intoxicated and impaired driving. For each outcome, we present estimates separately for nighttime and daytime crimes. We also present estimates for housing developments in the control group (D=0) as a falsification check. This falsification check leverages the critical feature that randomization was done in pairs, where each control development was randomly assigned a dosage of additional lighting that it did not receive because of its assignment to the control condition. Accordingly, if randomization worked as expected, we should not expect to observe changes in crime as a function of assigned dosage in the control locations.

[12] In practice, we use a permutation test in which we re-randomize the treatment variable to a given set of covariates 500 times and note the relative position of the F statistic for the model that uses the *actual* data among the distribution of placebo randomizations.

Consistent with the short-run effects of the intervention reported in Chalfin et al. (2021), we observe a large decline in both nighttime and daytime index crimes as a function of assigned dosage of lighting.[13] Overall, we estimate that, over a three-year period, the lighting intervention reduced outdoor nighttime crimes by 45% and outdoor daytime crimes by 39%.[14] On the other hand, we see less evidence for a decline in less serious criminal activity. For nighttime crimes, the point estimate is negative and suggests as 16% decline but it is not precisely estimated. For daytime crimes, we estimate an effect near zero, which is also imprecisely estimated. Consistent with past literature and with Chalfin et al. (2021), there is little evidence for displacement of any type of crime to nearby areas.

In a small randomized experiment with noisy outcome data, there is always a risk that treatment effects could be an artifact of imperfect randomization — that is, it is possible that housing developments assigned to the treatment group may, by chance, differ from the developments assigned to the control group with respect to their potential outcomes. In Table 1, we showed that treated and control developments as well as high versus low dosage developments are broadly balanced with respect to observable covariates. However this is an incomplete test of the fidelity of randomization. Regression estimates for the control developments provide a more complete test of randomization. Notably, while estimates are subject to sampling variability, we see little evidence that crime is sensitive to the assigned dosage of lighting among the control developments — which, critically, did not actually receive any new lighting.

Next, we consider what happened to arrests as a function of the intervention. This is an important outcome in its own right, because it allows us to test whether the lighting intervention widened the net of the criminal justice system, exposing more individuals in a community to arrest and subsequent incarceration. It is likewise important because it allows us to understand whether the intervention reduced crime primarily via deterrence or incapacitation. Consider, for example, an intervention which reduced crime while increasing the number of arrests. In this case, the observed crime reduction would be consistent

[13] In **Appendix Table A.1**, we replicate the estimates in Chalfin et al. (2021) which used privately-obtained NYPD crime microdata which is now publicly available, subject to additional modifications since made by NYPD analysts. The estimates differ only slightly from those obtained using the privately-collected dataset. We further note that these results were derived by an independent analyst who did not work on the original paper, thus providing additional support for the reproduce-ability of Chalfin et al. (2021).

[14] In the six-month follow-up period used in the original evaluation, the impact for on-development outdoor nighttime crimes was 60 percent. The estimate for daytime crimes was likewise large and negative (-35%) but was not significant at conventional levels of significance.

with large incapacitation effects. On the other hand, if arrests and crimes both decline to approximately the same degree, this outcome would be consistent with larger deterrence effects. To the extent that arrests decline, this limits the scope for increased incapacitation to be an important driver of the crime reductions we observe.

Because arrests cannot be linked to crimes in the public microdata, we do not disambiguate between nighttime and daytime arrests as we do for the crime analyses. Estimates for arrests are reported in **Table 3**. Consistent with the decline in index crimes that we observe in Table 2, index crime arrests declined by 35% as a function of the intervention. Overall arrests declined by 28%. There is little evidence of displacement of arrest activity nor there is evidence of an effect of the assigned dosage of lighting on arrests in the control group, where such an effect should not be observed.

6.3 Extensions

6.3.1 Temporal Heterogeneity

We next consider how treatment effects varied over time within the three-year follow-up window. Because the data are noisy instead of dividing the three-year window into a number of discrete sub-periods, we appeal to the idea of a Lowess smoother and use a 12-month rolling window to estimate the time-path of the estimated coefficients. In **Figure 2**, we plot the data for outdoor nighttime index crimes as well as arrests. While the statistical significance of the estimates varies as estimates are less precise within particular time windows than they are in the entire sample, the pattern of the estimates suggests that the crime reduction resulting from the intervention has been remarkably stable over time. With respect to arrests, the data shows that early reduction in arrests reported in Chalfin et al. (2021) appears to have abated over time. Thus while arrests have not risen as a function of the treatment, we cannot reject the possibility that the size of the incapacitation effects has risen in importance over time as the community — and the police — have acclimated to the lights.

As everyday activities change during winter months due to fewer daylight hours and different weather conditions, a second consideration is whether the effects change within the year. **Appendix Table A.2** shows that the treatment effects change very little by season.

6.3.2 Robustness

Finally, we consider the robustness of the results to alternative specifications, an especially important task since the number of units randomized is small and outcome data are sparse. We begin by considering the sensitivity of our estimates to alternative models. First, while using the LASSO to select covariates is a data-driven strategy that reduces researcher discretion, there is no guarantee that the variables selected by the LASSO procedure will produce an estimate that is representative of the entire model space. To provide a sense for the range of estimates that are possible when a different set of covariates are employed, we re-estimate the primary model, controlling for population and a randomly selected set of between one and eight additional covariates. These estimates are plotted in **Appendix Figure A.1**. The figure shows that the LASSO-selected estimates lie well within the range of possible estimates that we obtain from our randomized variable selection procedure. If anything, the LASSO estimates are slightly conservative. Next, recognizing that our sample is small, we test for the impact of highly leveraged observations re-estimating the model dropping each housing development. **Appendix Figure A.2** shows that these leave-one-out estimates are very similar to the main effects, which demonstrates that there is no one housing development driving the results.

We also 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 A.3**. Consistent with the Poisson estimates reported in Table 2, using least squares, we estimate that the intervention led to a significant decline of 4.9 outdoor nighttime index crimes, which is a 67 percent reduction relative to the control mean which is presented in brackets below the standard errors. While the outdoor, daytime index crimes estimate is not statistically significant in this specification, it is consistent to a 21 percent decrease.

Next, we consider an alternative approach to estimating treatment effects which combines the treated and control developments in a single analysis. Following Chalfin et al. (2021), we regress crime on the randomly assigned dosage of light towers where we consider the control group developments to have a randomly assigned dosage of zero additional light towers. We report these estimates, using Poisson regression, in **Appendix Table A.4**. We find that each additional light tower per square block (125,000 square feet) reduces outdoor, nighttime index crimes that occurred in a given community by 19 percent and daytime index crimes by 16 percent. These results are statistically significant at conventional levels and are qualitatively similar to the main estimates.

7 Discussion

This research presents the first experimental estimates of the longer-term impact of enhanced street lighting on crime. The intervention, rolled out in NYC's public housing developments between 2016 and 2019, led to meaningful improvements in public safety without widening the net of the criminal justice system. Importantly, the effects persist after three years' time and do not appear to be defeated by displacement to nearby areas, even after several years. As street lighting requires a large up-front capital investment, the attractiveness of enhanced lighting to policymakers depends critically on whether its public safety benefits will be long-lasting. These findings thus provide some further assurance that the impact of street lighting can endure beyond their initial installation.

We pause here to consider how these results fit into the larger research literatures on place-based crime prevention and deterrence theory. First, while most place-based crime control strategies have been evaluated over relatively short follow-up periods, our results do accord with the small number of high-quality studies of place-based crime control strategies which feature longer follow-up windows. For example, prior research suggests that crime reductions have persisted for multiple years in response to the remediation of vacant lots (Branas et al., 2018; Macdonald et al., 2021) and abandoned buildings (Kondo et al., 2015; Hohl et al., 2019) as well as structural repairs to the homes of low-income owners (South et al., 2021).

Second, we observe little evidence of displacement to nearby areas even after the intervention was in the field for several years, a finding which provides additional support for the strong degree to which crime hot spots persist (Weisburd et al., 2004, 2009; He et al., 2017) and to which crime is structurally tied to place (Weisburd et al., 2006; Farrell, 2015; Chalfin et al., 2020). This may be especially important in a public housing setting where residents sometimes reside in multi-generational families with ties to the same community.

Next, we note that the intervention had a larger and more robust effect on the most serious crimes and smaller and less robust effects on "quality of life" crimes. Such a finding comports with how the lighting may interact with law enforcement and has implications for how the intervention may have generated deterrence. To see this, consider robbery, a common and serious outdoor index crime. A robbery committed in a well-lit area is more likely to have one or more viable witnesses. Likewise, the crime is sufficiently serious that police may search for nearby CCTV footage which is made considerably more useful in the presence of bright lighting. Both of these factors suggest that individuals who are considering the commission of a serious crime like robbery may be deterred by the presence of lighting. On the other hand, consider a less serious "quality of life" crime such as public drunkenness. Such crimes are rarely investigated by canvassing an area for witnesses or pulling CCTV footage. The presence of a street light may thus only deter disorderly behavior in the presence of a police officer, which is unlikely given the small number of patrolling officers across an extraordinarily large space.

As arrests declined in tandem with serious crimes, these findings likewise suggest that the deterrence effect of the lighting intervention persisted over several years. However, while crime reductions were stable during the three year follow-up period, arrest reductions which were initially large did decline over time. This suggests that, over time, the relative share of deterrence may have fallen to an extent. The latter finding suggests that while deterrence persists, it is not completely impervious to erosion.

Finally, we reflect upon the large crime reductions that we observe during daytime hours. While the daytime crime reductions may be partially explained by noisy time stamps in administrative data, they are also consistent with a hypothesis put forward by Welsh and Farrington (2008) among others — that lighting towers may affect crime not only through their effect on ambient lighting but also by sending a signal that an area is cared for or under surveillance by police. This finding suggests a different pathway through which deterrence effects might persist.

These findings derive from a tactical street lighting intervention in a specific setting — public housing in NYC — that, while broadly applicable, may nevertheless differ substantively from a number of relevant settings. More research is needed on the long-term impacts of street lighting on public safety, derived from more traditional lighting interventions. Whether the crime and arrests reductions replicate in other settings, such as purely commercial areas, parking garages or suburban residential areas, remains an open empirical question.

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Table 1: Summary statistics, pre-intervention period

	Treatment (1)	Control (2)	p-value (3)
Past nighttime outdoor crimes			
On-development index crimes	3.80	3.14	0.15
Off-development index crimes	3.52	3.74	0.69
On-development nonserious crimes	11.38	9.32	0.20
Off-development nonserious crimes	7.68	7.46	0.88
Past arrests			
On-development index arrests	11.32	9.21	0.57
Off-development index arrests	13.24	9.40	0.62
On-development all arrests	141.85	118.03	0.24
Off-development all arrests	119.68	111.18	0.77
Population structure			
Avg. population	2,452.50	2,325.25	0.66
Avg. population density	184.14	186.62	0.92
Avg. household size	2.44	2.29	0.04
Physical characteristics			
Avg. entrances per building	1.65	1.93	0.34
Square feet (thousands)	718.75	714.37	0.98
Style: elevator	0.82	0.72	0.41
Style: walk-up	0.05	0.07	1.00
F-test			
Treatment vs control			0.67
Dosage			0.14
N	40.00	40.00	

Notes: Columns (1) and (2) report covariate means for the treatment and control groups over the 2011-2015 pre-intervention period, limited to the months between March and August of each year. Column (3) shows the p-value of the difference between both groups. The penultimate two rows of the table report p values on a joint test of the significance of all covariates in predicting treatment. The row 'treatment vs. control' corresponds to a binary indicator of treatment; the 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 1000 simulations from the 'ri2' package in R (Coppock, 2019). On-development incidents are those committed on the development's physical campus. Off-development refers to the incidents perpetrated within 550 feet of the development's campus exclusive of the campus itself. Index crimes and arrests include murder, robbery, felony assault, burglary, theft, and motor vehicle theft. Less serious crimes include all misdemeanors, excepting simple assault, offenses against the person, and intoxication and impaired driving.

Table 2: Poisson estimates on reported crimes, March 2016 - March 2019

	Within development	Off-development
	(1)	(2)
A. Outdoor index crimes		
Nighttime (D=1)	-0.60^{**}	0.09
	(0.24)	(0.17)
	[-45%]	[10%]
Daytime (D=1)	-0.49^{**}	-0.04
	(0.22)	(0.19)
	[-39%]	[-3%]
Nighttime (D=0)	-0.17	0.02
	(0.11)	(0.17)
	[-15%]	[2%]
Daytime (D=0)	0.19	0.07
	(0.17)	(0.17)
	[21%]	[7%]
B. Less serious crimes		
Nighttime (D=1)	-0.18	0.01
	(0.22)	(0.16)
	[-16%]	[1%]
Daytime (D=1)	0.09	0.11
	(0.21)	(0.14)
	[9%]	[12%]
Nighttime (D=0)	-0.07	0.10
	(0.19)	(0.14)
	[-6%]	[10%]
Daytime (D=0)	-0.20	0.13
	(0.15)	(0.15)
	[-18%]	[14%]

Notes: Poisson regression estimates of the relevant outcome between March 2016 to March 2019 on the natural logarithm of each housing development's randomly assigned additional lights per square feet. The D=1 estimates include only the treated units. The D=0 estimates is a placebo test including only the control group and leveraging that developments were randomized in pairs, where each control development has a randomly assigned dosage of additional lighting that was not received. Estimates are reported for two geographic areas: (1) the development's physical campus (within development) and (2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development). Estimates are reported separately for nighttime (after sunset, before sunrise) and daytime (after sunrise, before sunset) crimes. The first cell reports the weighted median coefficient across 500 LASSO runs of the Poisson regression coefficient. The second cell, in parentheses, presents 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). The third cell, in brackets, exhibits the percentage change (incidence rate ratio - 1). Index crimes include murder, robbery, felony assault, burglary, theft, and motor vehicle theft. Less serious crimes include all misdemeanors, excepting simple assault, offenses against the person, and intoxication and impaired driving. *p<0.1; **p<0.05; ***p<0.01.

Table 3: Poisson estimates on arrests, March 2016 - March 2019

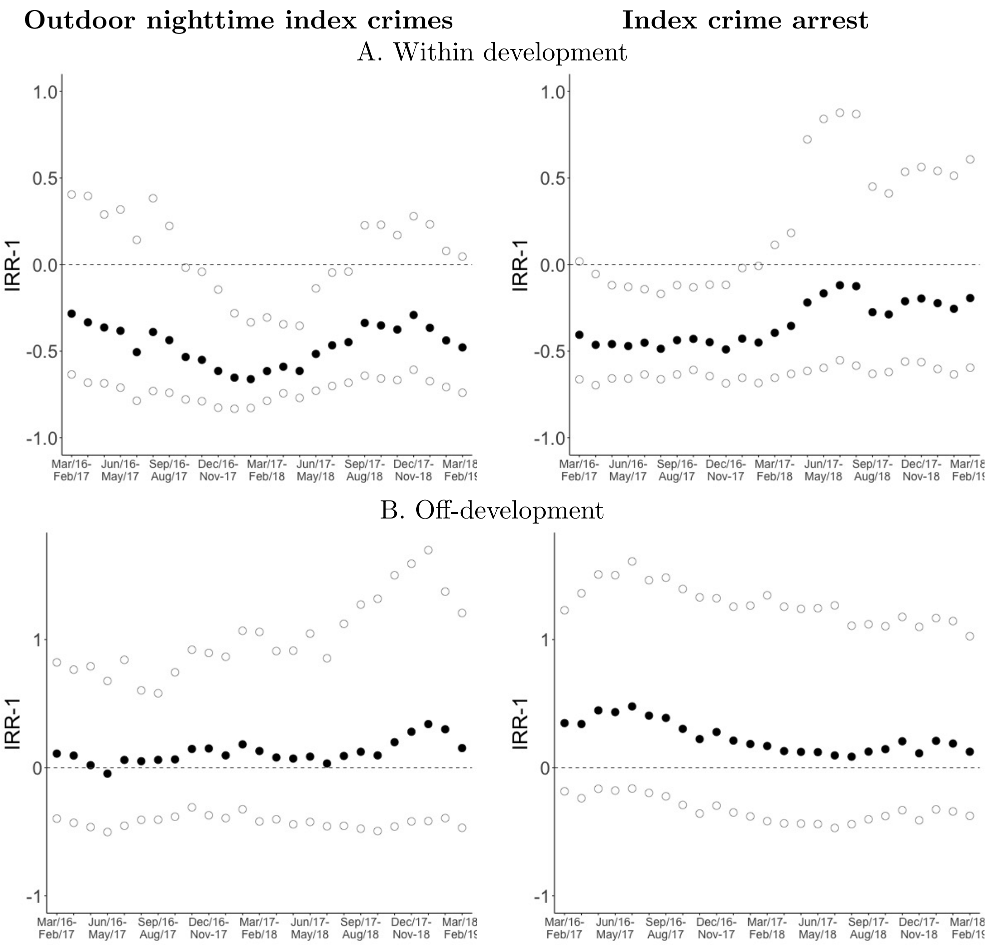
	Within development	Off-development
	(1)	(2)
A. Index crime arrest		
D=1	-0.42^{**}	0.19
	(0.19)	(0.23)
	[-35%]	[21%]
D=0	-0.14	-0.07
	(0.12)	(0.23)
	[-13%]	[-7%]
B. All arrests		
D=1	-0.34^{**}	-0.12
	(0.15)	(0.20)
	[-28%]	[-12%]
D=0	-0.15	0.15
	(0.12)	(0.12)
	[-14%]	[17%]

Notes: Poisson regression estimates of the relevant outcome between March 2016 to March 2019 on the natural logarithm of each housing development's randomly assigned additional lights per square feet. The D=1 estimates include only the treated units. The D=0 estimates is a placebo test including only the control group and leveraging that developments were randomized in pairs, where each control development has a randomly assigned dosage of additional lighting that was not received. Estimates are reported for two geographic areas: (1) the development's physical campus (within development) and (2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development). The first cell reports the weighted median coefficient across 500 LASSO runs of the Poisson regression coefficient. The second cell, in parentheses, presents 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). The third cell, in brackets, exhibits the percentage change (incidence rate ratio - 1). Index crime arrests include murder, robbery, felony assault, burglary, theft, and motor vehicle theft. *p<0.1; **p<0.05; ***p<0.01.

Figure 1: Mobile Lighting Tower, NYC Public Housing

Photo Credit: Edwin Tse.

Figure 2: Incidence Rate Ratio on public safety, 12-month rolling window



Notes: Incident Rate Ratio of the selected outcome over a 12-month rolling window on the natural logarithm of each housing development's randomly assigned additional lights per square feet. Estimates are reported for two geographic areas: (1) the development's physical campus (Within development) and (2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development). The point estimate (solid dot) is the weighted median IRR-1 across 500 LASSO runs of the Poisson regression coefficient. The 95 confidence intervals (hollow dot) use 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). Index crimes include murder, robbery, felony assault, burglary, theft, and motor vehicle theft.

ONLINE APPENDIX

A Appendix: Tables and Figures

Table A.1: Poisson estimates on reported crimes, March-August 2016

	Within development	Off-development
	(1)	(2)
A. Outdoor index crimes		
Nighttime (D=1)	-0.80^{**}	0.02
	(0.38)	(0.30)
	[-55%]	[2%]
Daytime (D=1)	-0.45	-0.27
	(0.33)	(0.55)
	[-36%]	[-24%]
Nighttime (D=0)	-0.17	0.04
	(0.22)	(0.36)
	[-16%]	[4%]
Daytime (D=0)	-0.14	0.51^{***}
	(0.32)	(0.17)
	[-13%]	[66%]
B. Less serious crimes		
Nighttime (D=1)	-0.14	0.12
	(0.29)	(0.24)
	[-13%]	[13%]
Daytime (D=1)	0.0004	0.14
	(0.27)	(0.17)
	[0%]	[15%]
Nighttime (D=0)	0.03	-0.08
	(0.23)	(0.22)
	[3%]	[-8%]
Daytime (D=0)	-0.20	0.08
	(0.23)	(0.15)
	[-19%]	[8%]

Notes: Poisson regression estimates of the relevant outcome between March to August 2016 on the natural logarithm of each housing development's randomly assigned additional lights per square feet. The D=1 estimates include only the treated units. The D=0 estimates is a placebo test including only the control group and leveraging that developments were randomized in pairs, where each control development has a randomly assigned dosage of additional lighting that was not received. Estimates are reported for two geographic areas: (1) the development's physical campus (within development) and (2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development). Estimates are reported separately for nighttime (after sunset, before sunrise) and daytime (after sunrise, before sunset) crimes. The first cell reports the weighted median coefficient across 500 LASSO runs of the Poisson regression coefficient. The second cell, in parentheses, presents 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). The third cell, in brackets, exhibits the percentage change (incidence rate ratio - 1). Index crimes include murder, robbery, felony assault, burglary, theft, and motor vehicle theft. Less serious crimes include all misdemeanors, excepting simple assault, offenses against the person, and intoxication and impaired driving. *p<0.1; **p<0.05; ***p<0.01.

Table A.2: Poisson estimates by seasonality, March 2016 - March 2019

	Winter (1)	Non-Winter (2)
A. Outdoor index crimes		
Nighttime (D=1)	-0.55^{*}	-0.62^{**}
	(0.33)	(0.26)
	[-42%]	[-46%]
Daytime (D=1)	-0.44	-0.53^{**}
	(0.38)	(0.23)
	[-36%]	[-41%]
B. Arrests		
Index crime arrest (D=1)	-0.56^{**}	-0.41^{**}
	(0.22)	(0.19)
	[-43%]	[-34%]
All arrest (D=1)	-0.39^{**}	-0.31^{**}
	(0.17)	(0.15)
	[-32%]	[-27%]

Notes: Poisson regression estimates of the relevant outcome between March 2016 to March 2019 on the natural logarithm of each housing development's randomly assigned additional lights per square feet. The D=1 estimates include only the treated units. Estimates are reported for incidents within the development's physical campus. They are reported separately for winter (between November 1st to February 28th) and non-winter (March 1st to October 31st). Crime estimates also distinguished between nighttime (after sunset, before sunrise) and daytime (after sunrise, before sunset). The first cell reports the weighted median coefficient across 500 LASSO runs of the Poisson regression coefficient. The second cell, in parentheses, presents 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). The third cell, in brackets, exhibits the percentage change (incidence rate ratio - 1). Index crimes include murder, robbery, felony assault, burglary, theft, and motor vehicle theft. *p<0.1; **p<0.05; ***p<0.01.

Table A.3: OLS estimates on crimes, March 2016 - August 2019

	Within development	Off-development
	(1)	(2)
A. Outdoor index crimes		
Nighttime (D=1)	-4.96^{*}	-3.85
	(2.81)	(4.68)
	[7.3]	[5.97]
Daytime (D=1)	-1.62	-4.86^{***}
	(2.65)	(1.80)
	[7.58]	[6.53]
Nighttime (D=0)	-2.43	2.26
	(1.92)	(2.32)
	[6.61]	[5.56]
Daytime (D=0)	3.05	1.43
	(2.34)	(1.67)
	[8.01]	[6.75]

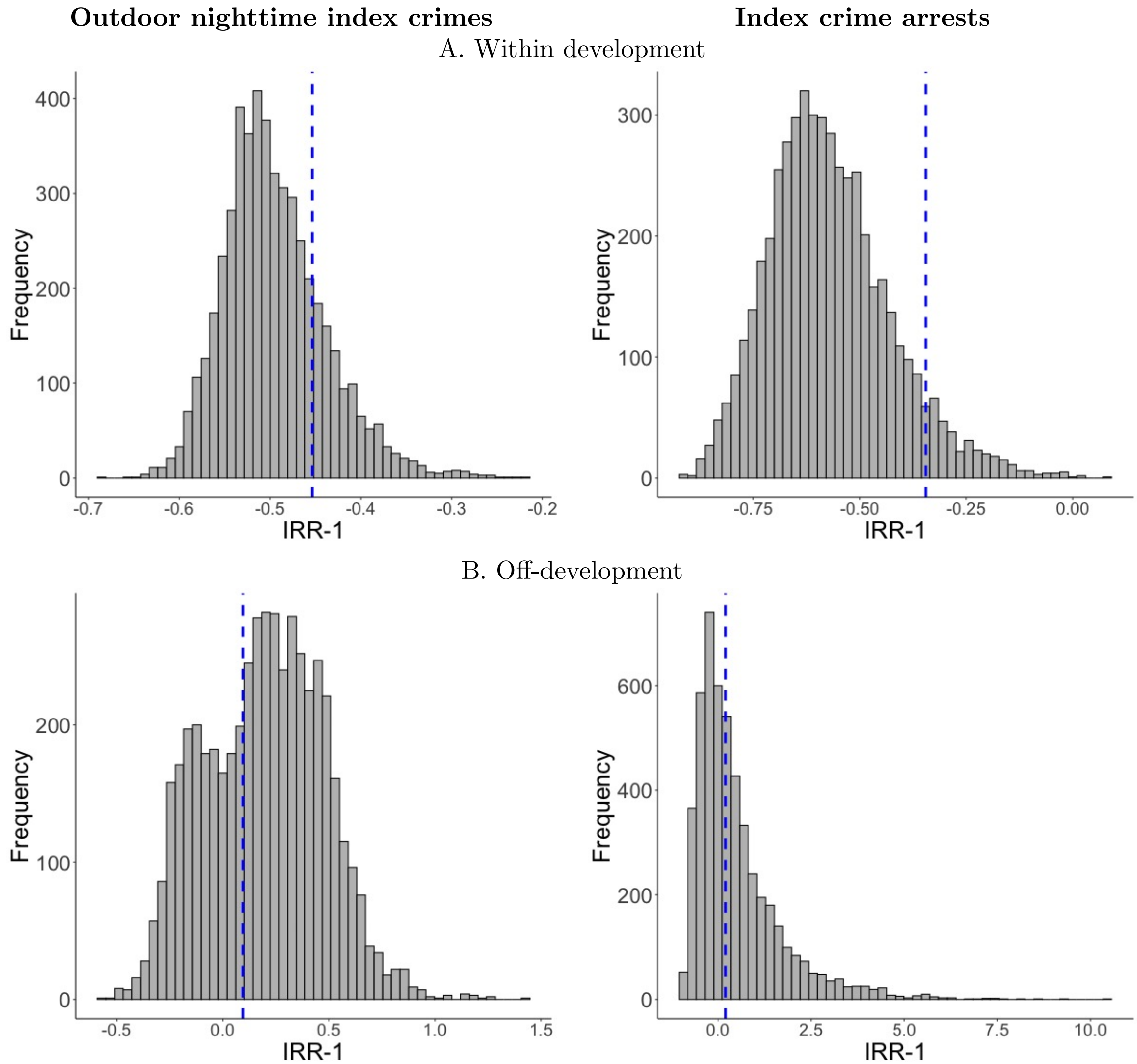
Notes: Ordinary least squares regression estimates of the relevant outcome between March 2016 to March 2019 on the natural logarithm of each housing development's randomly assigned additional lights per square feet. The D=1 estimates include only the treated units. The D=0 estimates is a placebo test including only the control group and leveraging that developments were randomized in pairs, where each control development has a randomly assigned dosage of additional lighting that was not received. Estimates are reported for two geographic areas: (1) the development's physical campus (Within development) and (2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development). Estimates are reported separately for nighttime (after sunset, before sunrise) and daytime (after sunrise, before sunset) crimes. The first cell reports the weighted median coefficient derived from running 500 LASSO identifying the most predictive covariates to include in the OLS model. The second cell, in parentheses, presents 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). The third cell, in brackets, exhibits the pre-intervention yearly mean. Index crimes include murder, robbery, felony assault, burglary, theft, and motor vehicle theft. *p<0.1; **p<0.05; ***p<0.01.

Table A.4: Poisson estimates on crimes, All sample March 2016 - August 2019

	Within development (1)	Off-development (2)
A. Outdoor index crimes		
Nighttime	-0.22^{***}	-0.07
	(0.08)	(0.07)
	[-19%]	[-7%]
Daytime	-0.17^{**}	-0.04
	(0.07)	(0.08)
	[-16%]	[-4%]

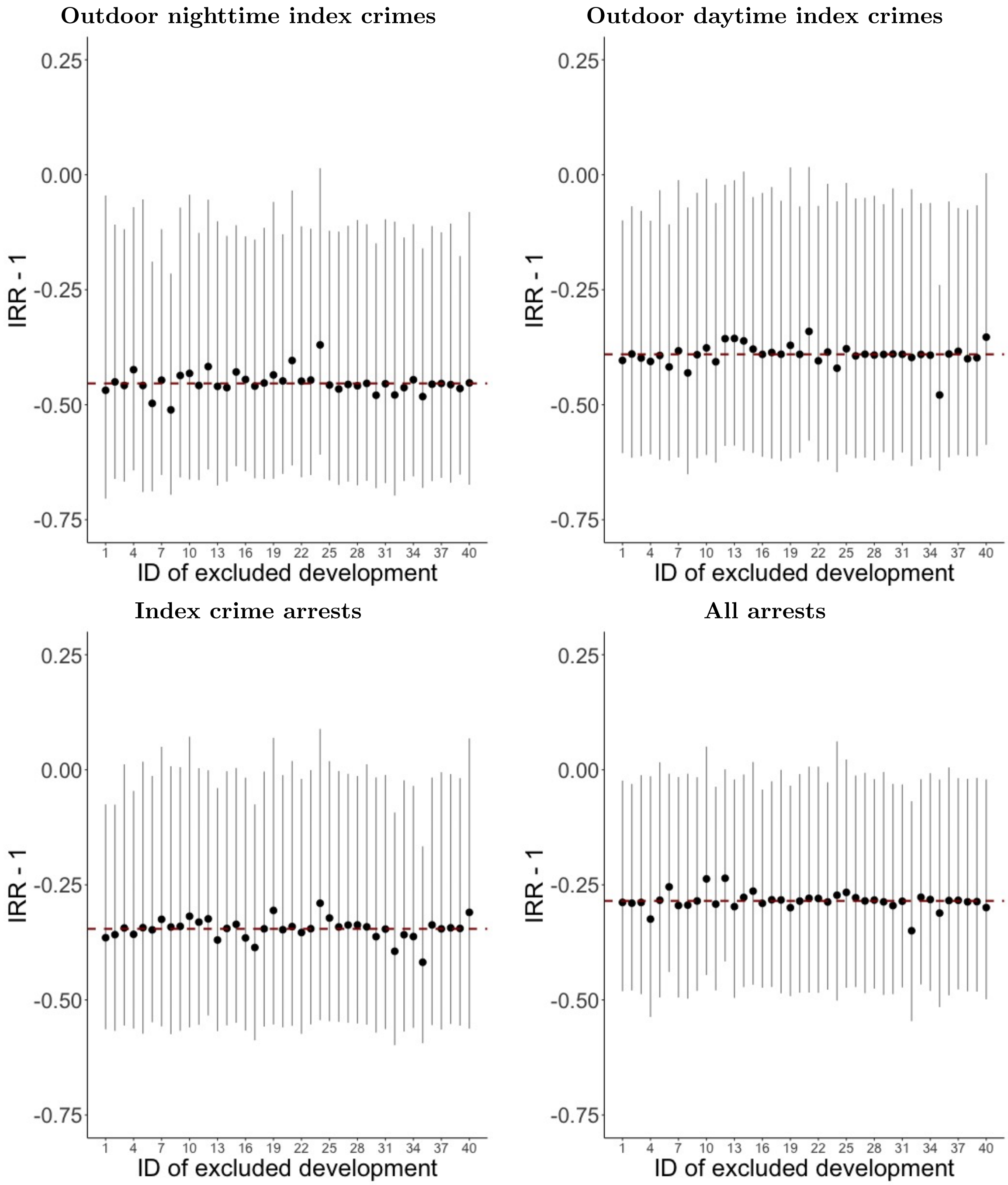
Notes: Poisson regression estimates of the relevant outcome between March 2016 to March 2019 on each housing development's randomly assigned additional lights per square feet. Estimates include the treated and control units, assigning zero dosage to the control group. Estimates are reported for two geographic areas: (1) the development's physical campus (Within development) and (2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-development). Crime estimates are reported separately for nighttime (after sunset, before sunrise) and daytime (after sunrise, before sunset) crimes. The first cell reports the weighted median coefficient across 500 LASSO runs of the Poisson regression coefficient. The second cell, in parentheses, presents 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). The third cell, in brackets, exhibits the percentage change (incidence rate ratio - 1). Index crimes include murder, robbery, felony assault, burglary, theft, and motor vehicle theft. Less serious crimes include all misdemeanors, excepting simple assault, offenses against the person, and intoxication and impaired driving. *p<0.1; **p<0.05; ***p<0.01.

Figure A.1: Robustness of Estimated Treatment Effects to Alternative Sets of Controls Variables, March 2016 - March 2019



Notes: Histograms of estimated treatment effects from a series of Poisson regressions of the selected outcome on the natural logarithm of each housing development's randomly assigned additional lights per square feet. Each model controls for population plus an additional random set of covariates (between 1 and 8). Covariates are randomly drawn from a pool of aggregate pre-intervention crime counts and development demographics, drawing 5,000 samples. Index crimes include murder, robbery, felony assault, burglary, theft, and motor vehicle theft. To improve the visualization, the off-development index crime arrests (bottom-right) plot excludes one outlier with an IRR=19.6.

Figure A.2: Robustness of On-development Estimates, Leave one out method



Notes: Incident Rate Ratio of the selected outcome between March 2016 to March 2019 on the natural logarithm of each housing development's randomly assigned additional lights per square feet, excluding one development at a time. The estimates include only the treated units. Estimates are reported for the development's physical campus (within development). The point estimate (solid dot) is the weighted median IRR-1 across 500 LASSO runs of the Poisson regression coefficient. The 95 confidence intervals use 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). The horizontal red line indicates the main estimate using all the treated developments. Index crimes include murder, robbery, felony assault, burglary, theft, and motor vehicle theft.