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# REDUCING CRIME THROUGH ENVIRONMENTAL DESIGN: EVIDENCE FROM A RANDOMIZED EXPERIMENT OF STREET LIGHTING IN NEW YORK CITY

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## ABSTRACT

This paper offers experimental evidence that crime can be successfully reduced by changing the situational environment that potential victims and offenders face. We focus on a ubiquitous but surprisingly understudied feature of the urban landscape – street lighting – and report the first experimental evidence on the effect of street lighting on crime. Through a unique public partnership in New York City, temporary streetlights were randomly allocated to public housing developments from March through August 2016. We find evidence that communities that were assigned more lighting experienced sizable reductions in crime. After accounting for potential spatial spillovers, we find that the provision of street lights led, at a minimum, to a 36 percent reduction in nighttime outdoor index crimes.

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# 1 Introduction

For decades social scientists have debated the relative importance of the characteristics of the individual—formed by the combination of genes and environmental exposures during childhood and adolescence—versus situational aspects of the environment in which judgments, decisions and behaviors are made. That is, to what degree is behavior driven by, in the words of Lee Ross and Richard Nesbitt, "the person versus the situation?"

A large research literature in behavioral economics and psychology suggests that small changes to the environment ("the situation") can have surprisingly large effects on human behavior. This principle can be seen in the eye-opening findings of Stanley Milgram's classic obedience studies and, more recently, in a host of policy-relevant applications involving "nudges" (see Bertrand, Mullainathan and Shafir 2006).[1] Low-cost interventions that leverage seemingly small environmental changes are particularly attractive as they have the potential to change costly behaviors without the need for resource-intensive interventions which can be invasive, infeasible and difficult to scale. There is perhaps no area in which low-cost interventions are more needed than criminal justice, a domain in which unwanted behaviors lead to enormous social costs, perhaps as high as $1 trillion per year (Chalfin 2015), and in which the primary intervention, incapacitation, has been used with such intensity that

[1]Nudges in behavioral economics vary in scope but the underlying idea is that small yet costly cognitive errors — or other externalities — can potentially be abated by thoughtful and sometimes seemingly small situational changes. For example, minor tweaks to a default rule can have dramatic effects on retirement savings (Madrian and Shea 2001; Benartzi and Thaler 2013), organ donation (Johnson and Goldstein 2003) and the purchase of insurance (Johnson et al. 1993). Likewise, behavioral nudges have been found to reduce theft from public parks (Cialdini, 1993), littering (Thaler and Sunstein 2008) and energy usage (Schultz 2007). In the realm of criminal justice, text-message based reminders have been found to reduce an arrestee's failure to appear at future court appearances (Fishbane, Ouss and Shah 2018). The capacity of behavioral economics to identify behavioral bottlenecks in the quest for human progress captured the attention of the broader public with the publication of *Nudge* by Richard Thaler and Cass Sunstein in 2008 and has led to the proliferation of public sector "nudge units," in particular the Obama administration's Social and Behavioral Sciences Team and the Behavioural Insights Team in the United Kingdom.

the United States now has the highest incarceration rate in the world. Beyond the large financial cost to the government, collateral harms associated with the use of incarceration, including direct economic and social impacts as well as effects on families and communities, fall disproportionately on low income, racially segregated neighborhoods (Western, Kling and Weiman 2001; Aizer and Doyle 2015; Mueller-Smith 2016).

Can crime, in fact, be shifted by a relatively small situational change? Public policy has largely dismissed this idea, focusing either on costly enforcement actions or, alternatively, on resource-intensive social programs which can be both difficult to evaluate and scale.[2] 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 (Weisburd 2015).[3] Second, a litany of evidence from both psychology and economics suggests that offenders are myopic and tend to have high social discount rates (Lee and McCrary 2017). In a world in which potential offenders are myopic, small environmental design changes experienced in the present may well have an outsized impact on behavior relative to the uncertain prospect of prison sentences that, if experienced, will accrue sometime in the future. Finally, the importance of the environment is implicit in research on the importance of peer effects which have been shown to be a salient

[2]This idea that crime is difficult to abate through light-touch interventions is perhaps best captured by Robert Magnus Martinson's seminal Public Interest essay, "What Works" in 1974, which suggested that criminal behavior is difficult to change, as evidenced by high recidivism rates among offenders and the failure of rehabilitative efforts to substantially reduce return to prison. Martinson's essay received substantial public attention, including coverage on *60 Minutes* and in *People* magazine and, as crime continued to climb during the 1970s and 1980s, mass incarceration seemed, to many, like the only viable solution to rising crime.

[3]The idea that the environment matters is likewise implicit in the seminal *Moving to Opportunity* research of the early 2000s (Katz, Kling and Ludwig 2005) and continues to be hotly debated today (Sampson 2008; Chetty, Hendren and Katz 2016; Chyn 2017).

driver of offending, particularly among youthful offenders (Bayer, Hjalmarrson and Pozen 2009; Stevenson 2017).

With public safety in mind, cities have recently expressed renewed interest in making changes to the physical design of public space. In support of this idea, a small research literature notes that certain property crimes can be surprisingly responsive to environmental factors such as the supply of available criminal opportunities (Ayres and Levitt 1998; Cook and MacDonald 2011) and physical disorder, as is evidenced by an influential article in *Science* that reports compelling experimental evidence that physical disorder begets further disorder (Keizer, Lindberg, and Steg 2008). What remains far less well understood is the extent to which changes to the physical environment can be successful in reducing serious crimes, especially crimes involving violence, which drive the majority of the social costs.[4] As such, the key questions of whether and how violence can be successfully reduced by environmental design changes remain entirely unsettled. We offer the first experimental evidence that a small change in environmental design can reduce violence.

We study one of the most ubiquitous — and, surprisingly, understudied —environmental design changes that cities have relied on to maintain public safety for more than two hundred years: street lighting. While many communities cannot convert vacant lots into parks or repair abandoned buildings, every city, regardless of size, faces darkness each and every night. 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.

[4]While there is some evidence that strategies such as increasing the availability of trees and green space (Branas et al. 2011; Kondo et al. 2016), and securing abandoned buildings (Branas et al. 2016) may lead to important reductions in violence, the evidence for effects on violence is non-experimental and mixed (Bogar and Beyer 2016).

That is, lighting, like many traditional behavioral interventions, provides participants in the market for crime with information — in this case, information that is usually masked by a veil of darkness. We further note that lights may also complement more traditional deterrence-based strategies, such as police patrols (Sherman and Weisburd 1995; Evans and Owens 2006), surveillance cameras (Priks 2015) and "eyes on the street" (Carr and Doleac 2017), thus potentially driving down crime through an array of additional mechanisms.[5]

Surprisingly, despite wide interest in street lighting among scholars, policymakers, and the public at large, there has not been a randomized experiment that studies the effectiveness of street lighting in controlling crime. This study, made possible by a unique partnership between the New York City Mayor's Office for Criminal Justice (MOCJ), the New York City Police Department (NYPD), and New York City Housing Authority (NYCHA), offers the first and only experimental evidence on the effectiveness of street lights in controlling street crime, especially violent crime.

Our field experiment was conducted in 2016 in NYC, where crime has declined precipitously over the last three decades, coinciding with a series of well-documented and cutting edge innovations in policing — a number of which have become a model for law enforcement agencies around the world (Zimring 2012). However, despite the prominent decline in crime throughout most of the city, violent crime remains disproportionately high in public housing. As such, NYC's public housing communities were selected as the preferred setting for the intervention.

With any place-based experiment, there are two core research challenges that must be

[5]For example, if peer effects are present, then even if only a few individuals directly modify their behavior due to the presence of lights, the resulting spillovers could magnify their overall impact (Bayer, Hjalmarrson and Pozen 2009; Stevenson 2017).

addressed. The first is statistical power. Intervening on a place, particularly a large place like a housing project, is enormously costly. While more than 80,000 people live in the areas we study, only a small number of locations can 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. It is not uncommon for the number of index crimes to fluctuate by several hundred percent on a monthly basis. 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. Because there is greater variation in a continuous dosage variable than in an indicator variable for treatment, this design maximizes statistical power.

A second and related challenge 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 and Pischke 2008; Imbens 2009). While we have rich data upon which to condition, the large number of potential covariates relative to the number of observations means that there are many reasonable models that can be used to estimate treatment effects. This issue is compounded by the lack of theory in selecting the functional form of available covariates—for instance, should we control for population or its natural log? 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 (Belloni, Chernozhukov and Hansen 2014; Varian 2014; Athey and Imbens 2015).

In particular, we use LASSO regression (Tibshiriani 1996) to select predictors that have the greatest out-of-sample predictive power, thereby automating away researcher discretion.

We estimate that the introduction of marginal lighting reduced outdoor nighttime index crimes by approximately 60 percent and, by at least 36 percent, once potential spatial spillovers are accounted for. These findings provide the first evidence that the physical environment of cities and communities is a key determinant of serious crime.

# 2 Background

## 2.1 Street Lighting

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. Newport, RI become the first U.S. city to introduce gas lighting in 1803 and, after the invention of the electric light bulb, Wabash, IN became the first U.S. city to use electric street lighting in 1880. Today, street lights can be found in varying degrees of abundance in every city in the United States and throughout the rest of the developed and developing world.

In addition to being ubiquitous, lighting is also popular among community residents.[6] In our study location — public housing communities in New York City — a recent survey conducted by the NYC Mayor's Office found that only 21 percent of public housing residents felt safe walking around their neighborhood at night, compared to 50 percent who felt safe during the daytime. More broadly, from 2010 to 2016, complaints about street lighting

[6]See prior studies from Painter (1989), Atkins, Husain and Storey (1991), Herbert and Davidson (1995), and Painter (1996) for evidence on reactions among residents to increases in street lighting.

outages were the third most common complaint to the city's 311 system, indicating that residents notice and register concern when lights are not functional.[7]

The academic literature on street lighting—produced predominantly by criminologists—is ably described in a 2008 meta-analysis by Welsh and Farrington, who report that, among thirteen studies in the U.S. and U.K., the addition of street lighting reduces crime by 27 percent. However, all of the prior research is non-experimental and relies either on sometimes questionable comparison groups or pre-post comparisons of community-level interventions. While suggestive of a causal relationship, these comparisons may be biased due to seasonality, secular trends in crime and the strategic placement of street lights by city planners (LaLonde 1986).[8] Similarly, the most promising findings are over forty years old and many consider the effect of lighting on vehicle theft from garages, not the type of street crimes that are of central concern to policymakers. Despite the plethora of positive findings, the state of the literature ultimately led a 1997 National Institute of Justice report to the U.S. Congress to conclude that "we can have very little confidence that improved lighting prevents crime."

The strongest evidence to date that ambient lighting has appreciable effects on street crimes comes from a natural experiment motivated by Doleac and Sanders (2015) who study variation in lighting induced by daylight savings time. Using both a differences-in-differences and regression discontinuity approach, they find evidence that DST reduces crime, particularly robbery.[9] While their findings suggest a role for ambient lighting, experimental evidence remains critically important for several reasons. First, Doleac and Sanders use mi-

[7]This is the author's computation based on publicly available microdata accessed from NYC's *Open Data Portal*.

[8]While the review refers to treatment groups as "experimental and "control groups, all of these studies are actually quasi-experimental.

[9]Research by Dominguez and Asahi (2017) finds similar effects in Chile.

crodata 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. In particular, potential victims may find it difficult to adjust their behavior during the 6:00 to 7:00pm hour, whereas time spent outside during the entire evening may be more discretionary. Finally, street lighting is a policy that communities can directly influence and potentially use to target high-crime areas in which the majority of a city's crimes are clustered.

## 2.2 Field Experiment

The field experiment described in this paper was conducted in the Spring and Summer of 2016 in NYC. Through a unique partnership between NYPD and MOCJ, we randomized the provision of street lights to the city's public housing developments, allowing us to avoid the potential challenges that could result due to spurious time trends as well as selection bias. Managed by NYCHA, NYC's public housing developments are officially home to more than 400,000 New Yorkers (and perhaps an additional 100,000 non-official residents), making NYCHA the second largest landlord in the United States after the U.S. military. NYC's official public housing population is indeed large enough to place it among the forty largest cities in the United States, making it an ideal setting to study the effect of street lighting in urban areas.

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 nearly two years to develop the field experiment described in this paper. 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. Towers were equipped with an automatic timer set to switch on at sunset and off upon sunrise. A schematic photo of an Allmand™ lighting tower as well as a photo of towers in the field can be found in Appendix Figure 1.

In order to select developments for the study, NYPD provided a list of 80 high-priority developments based upon their elevated crime rates and perceived need for additional lighting from among the 340 NYCHA developments in NYC. 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; treatment developments were then randomly assigned a lighting dosage.[10]

In order to maximize statistical power, we also randomly assigned the dosage of lighting among the treatment group. Three hundred and ninety-seven lighting towers were available to be randomly assigned amongst the treatment group. For operational reasons, the City decided that two light towers would be allocated to each campus, regardless of square footage. The remaining 319 lighting towers were assigned to the 39 developments according to a random number drawn from a uniform distribution linked to the square footage of the

[10]In practice, one development (East River) was randomized into the control group but subsequently received a randomized dosage of lights because of operational considerations, requiring us to remove this development and its paired treatment development — which received no lights — from the study. Additionally, one control development (Smith) received some lights post-dosage randomization, and is consequently removed from a placebo analysis in which the control group is used.

developments. This created exogenous variation in the the number of lights per square feet across the developments.[11] The average dosage among the treated developments was seven light towers over an area of approximately 700,000 square feet. 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. [12] Control group developments received no additional outdoor lighting ("business-as-usual").

## 3 Data

To measure crime in the study locations, we use NYPD 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.[13] We focus 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. We focus on index crimes for two reasons. First, these are the most serious crimes and drive the vast majority of social costs (Chalfin and McCrary 2017). 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

[11]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, we report intention-to-treat estimates, using each development's *assigned* dosage.

[12]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.

[13]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.

reported, the time stamps are more reliable allowing us to correctly assign crimes to either daytime or nighttime hours. 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 campuses during the 2011-2016 period.[14]

In addition to crime data, we use administrative data from several alternative sources to test covariate balance and to construct controls. 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. Second, we use census block data from the U.S. Census Bureau to construct an unofficial measure of the age and gender composition of the development.[15] Finally, we obtained auxiliary NYPD data that serves as a proxy for the density and type of police activity in each of the campuses.[16] 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.

[14]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.

[15]Because there may be as many as 100,000 unofficial NYCHA residents, demographic measures of official NYCHA residents may undercount certain populations.

[16]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.

# 4 Models and Results

## 4.1 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.[17] 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 campuses 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 2,400 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.

[17]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 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.

## 4.2 Fidelity of Randomization

Despite the difficulties that randomization can face in a small sample, 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. In practice, we use a permutation test similar to the one described above 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. For both the binary and continuous treatment variables, the $p$-values on the $F$-statistic are approximately 0.5 indicating that covariates do not predict treatment, a finding that is consistent with successful randomization.

We further note that, in addition to checking for covariate balance along observable variables, we can test the fidelity of randomization to imbalances along unobservables. Specifically, among the control group, we regress crime on the randomly assigned dosage of its paired treatment development. These results, detailed later in the paper, provide little evidence against the fidelity of randomization. Given that tests for failed randomization may be underpowered, we continue to control for observed covariates in all subsequent models.

## 4.3 Econometric Models

To estimate treatment effects, we estimate a series of Poisson regression models in which the log count of crime is regressed on a treatment variable and a vector of covariates. We begin by regressing log crime on a binary treatment variable. This model suggests that the intervention reduced outdoor nighttime index crimes by 12 percent. However, estimates are imprecise (SE = 19 percent) and the binary treatment indicator masks considerable heterogeneity in dosage. Given that some of the treatment sites received a very small and possibly non-clinical dosage of lighting, these estimates will 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, our main analysis is 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 62,500 square feet (i.e., one square NYC block) among the developments in the treatment group only. The parameter on the dosage variable captures the effect on crime of a one hundred percent change in the dosage of additional lighting. Evaluated at the mean dosage level, this is our estimate of the effect of the intervention on crime.

Next, we turn to the issue of what to condition on. Building upon approaches such as Bayesian model averaging (Madrigan, Raferty, Volinksy and Hoeting 1996; Raferty, Madrigan and Hoeting 1998), as well as that of Oster (2015), who motivates a model averaging scheme involving re-randomization, we estimate treatment effects, averaging over a number of models. To select variables in a principled way, we turn to LASSO regression (Tibshiriani 1996), a popular and versatile machine learning classifier that is often applied to variable selection problems in high-dimensional space (Zou and Hastie 2005; Meinhausen and Buhlmann 2006). The LASSO has the virtue of retaining only the subset of predictors that are genuinely predictive of outcomes, using a penalty term which regularizes all of the estimated regression parameters and constrains parameter values that fall below a given threshold, making them equal to zero. The optimal penalty term, $\lambda$, is selected via $k$-fold cross-validation by randomly partitioning the data into $k$ different training sets and associated test sets. For each training set, a series of models are estimated for varying values of $\lambda$ and predictions are computed on the associated test set. The optimal $\lambda$ is chosen by taking the mean of the errors across the $k$ test sets and choosing the value which minimizes this quantity. With this $\lambda$ in hand, the model is then re-run on the full dataset using the optimal $\lambda$. 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 $k$ 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.

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 a Poisson regression model, storing up the coefficient and a bootstrapped standard error. We report the median coefficient and standard error among the estimated models. Estimates are reported for on-campus crimes and, in order to test for spatial spillovers, for crimes that occur within a radius of 550 feet (two standard NYC blocks) from campus. While we do not detect evidence of spillovers, these tests are underpowered and, in order to be conservative, we also report estimates for crimes committed within the 550 square foot catchment area, *inclusive of the campus.* These "net" estimates are thus extraordinarily conservative and represent a lower bound on the treatment effect as they automatically fold in displacement to adjacent areas.

## 4.4 Main Estimates

In Figure 1 we plot the natural log of crime against the natural log of the number of light towers added per square block for the development. There is a striking negative relationship between on-campus crime and randomly assigned lighting dosage. Turning to the middle column, the relationship between crime and dosage is positive for off-campus crimes, 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 two blocks of the campus — the relationship between lighting and crime remains negative, albeit less negative than for on-campus crimes.[18]

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 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-campus and off-campus crimes, as well as for net crimes inclusive of the off-campus 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. [19] We also transform the Poisson coefficient to obtain the incidence rate ratio minus one, which is the estimated treatment effect. The main estimates for outdoor nighttime crimes in the treated developments are presented in the first set of rows. The regression estimates closely replicate the pattern of findings presented in Figure 1. For each 100 percent change

[18]Excluding the highest dosage developments does not substantively change the slope of these regression lines. We formally test robustness to the exclusion of the highest dosage developments later in the manuscript.

[19]The median number of predictors selected by the LASSO model is 2.

in the natural log of dosage, averaged across all developments, we estimate a 59 percent reduction in outdoor, nighttime crimes that occur on campus. It is important to note that while the IRRs are consistent with very large treatment effects, base rates of crime are low. The 59% reduction in index crimes would take an average development which experiences 3.6 index crimes per six-month period prior to the intervention to 2.1 expected index crimes.

Turning to the middle set of columns of Table 2, we find little evidence that lighting displaces crime. However, the estimate is imprecise and does not allow us to detect 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 campus, inclusive of the campus. Since the intervention occurred only on the campus, 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 36%.

Next, we present results for daytime crimes among the treatment sites as a test for temporal spillovers. While point estimates are negative, they are insufficiently precise to provide confidence that spillovers are present. Two points of caution are worth noting with respect to this test. First, even during the daytime, the presence of the temporary light towers is a notable part of the landscape. 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. Second, 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, could be reported as daytime crimes. Hence, it is possible that a portion of the crime reduction observed at night could be re-distributed to the daytime.

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 that was not actually received. These estimates are presented in the final set of rows and are statistically indistinguishable from zero.

## 4.5 Robustness

The previous section presents evidence in favor of a causal effect of lighting on outdoor, nighttime crimes in and around public housing. In this section, we further interrogate these findings to test whether they are robust to alternative specifications and the removal of outliers.

We begin with a discussion of the robustness of model estimates to the inclusion of alternative control variables in the regressions presented in the previous section. As an additional check on the representativeness of the LASSO-selected predictors across the entire model space, we re-estimate the model many 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. Appendix Figure 3 presents the distribution of estimated on-campus 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.[20] Parameter estimates reported in Table 2 are not unusual among the larger universe of potential estimates and are, if anything, conservative.

Next, we test for outliers by re-estimating the models presented in Table 2 excluding one development at a time from the treatment group. Figure 2, Panel A provides a visual depiction of these results for on-campus 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% confidence intervals. For both on-campus 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. Figure 2, 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.

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 Figure 3. The plots show that meaningful effects persist even at very large radii around the treated campuses.

[20] In addition to the random set, each model controls for development population.

# 5 Discussion

In the previous section, we present evidence that street lighting can be effective in reducing urban street crimes. In this section, we discuss the extent to which lighting is likely to be cost-effective.

The effects documented in this paper speak only to the ability of lighting to reduce outdoor, nighttime crimes. On NYC's public housing campuses, approximately 11 percent of index crimes occur outdoors and during nighttime hours. Accordingly, a 36 percent reduction in outdoor, nighttime index crimes means that lighting may reduce serious offending in these communities by approximately 4 percent. We note here that a 4 percent change in index crimes is approximately what would be expected to occur during a very serious recession (Raphael and Winter-Ebmer 2003; Gould Weinberg and Mustard 2005) or in response to a ten percent increase in police manpower (Evans and Owens 2007; Chalfin and McCrary 2018).

In order to measure the benefits of crime reduction that accrued to NYC residents as a result of the lighting intervention, we construct per-development cost-of-crime measures. Recognizing that crime is a non-market good, we use estimates of the social cost of crime generated by Cohen and Piquero (2009) and update these estimates to express costs in 2016 dollars.[21] We use their willingness-to-pay estimates derived from a contingent valuation survey of potential crime victims and limit the analysis to index crimes. [22] Following Cohen and Piquero, we treat homicide as an event that accrues stochastically from other crimes,

[21]See Cohen (2000) and Chalfin (2015) for a more detailed discussion of the historical development of the literature that estimates the social harms of criminal victimization.

[22]In 2016 dollars, the costs estimated by Cohen and Piquero (2009) are as follows: $324,112 per armed robbery, $45,144 per unarmed robbery, $98,391 per felony assault, $21,993 per misdemeanor assault, $40,514 per burglary, $19,678 per motor vehicle theft, and $4,630 per larceny.

such as a robbery or an assault. Based on these estimates, the economic value of crimes abated due to lighting upgrades is projected to be approximately $770,000 per community per year.[23]

There are two ways to think about the cost of this intervention. First, we provide an accounting of the *actual* rental costs of the pilot project. Such an analysis reveals *retrospectively* whether costs of the temporary lighting were justified by the crime reduction benefits that accrued. With respect to retrospective costs, the total cost of the intervention was $5,032,632. On a per-development basis, this is approximately $129,000 or $258,000 per development annually.

Second, we can think *prospectively* about what a permanent upgrade in lighting might cost, which is more consistent with the choice that policymakers actually face. Projecting what an equivalent permanent lighting upgrade would cost is conceptually challenging. However, based on the cost of recent permanent lighting upgrades in public housing, the up-front cost of a development-wide lighting upgrade is expected to be between $3 and $4 million for a development of approximately 700,000 square feet, the average size of developments in our study. Annual costs of providing electricity to the additional lights is expected to be roughly $15,000 per development. Over a ten-year planning horizon, we estimate that this type of lighting upgrade will cost, on average, $200,000 per development annually. Given these annual costs, if the effects of lighting persist, we anticipate that the ratio of benefits to the costs of additional lighting would be approximately 4 to 1. This is a far larger benefit to cost ratio than the marginal incarceration typically offers, without the collateral costs, and perhaps offering collateral benefits.

[23]This calculation assumes that the effectiveness of lighting will be constant over time.

# 6 Conclusion

The debate over the role of the "individual versus the situation" in the crime production function continues to this day. This field experiment provides novel evidence that changing the situation in urban environments through investments in street lighting can reduce crime in disadvantaged urban areas. Accounting conservatively for potential spillovers, lighting reduces outdoor nighttime index crimes by approximately 36 percent and reduces overall index crimes by approximately 4 percent in affected communities, an outcome which is likely to be cost-beneficial, should the impact of lighting persist over time. Importantly, lighting offers cities a promising method to reduce crime while avoiding potential unintended costs associated with reliance on incapacitation, which has been shown to have high collateral costs. Even more important, these findings highlight a general principle — that violence, like other costly externalities, can be extraordinarily sensitive to situational factors that are experienced in the present relative to longer-term costs that are experienced in the future.

With respect to the promise of lighting as a scaleable crime reduction strategy, we note that some uncertainty about the effects of a permanent infrastructure upgrade remains. In particular, the lighting studied here is very bright and provides far more light than a typical investment in marginal lighting would entail. Likewise, 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 substantial demonstration effects that may not accrue from a more organic intervention.

The cost-benefit analysis presented above also provides some intuition for how cities might choose to trade off between capital investments in their crime control production function. Since the benefits of crime control will accrue over a relatively long time horizon, a social planner who is seeking to optimize over a fixed time horizon may find spending on technology to be an unattractive proposition. The implication of our analysis thus accords with a broader literature on the provision of local or regional public goods which may be over or under-provided based upon the extent of free-riding (Bergrstrom, Garrat, and Sheehan-Connor 2009), or the heterogeneity of production technologies with local spillovers (Ogawa and Wildisin 2009). While Lizzeri and Persico (2001) demonstrate local public goods may be underprovided in winner take all electoral systems, we note that local public capital goods could also be underprovided due to the extended time needed for those investments to pay off, relative to traditional electoral cycles.

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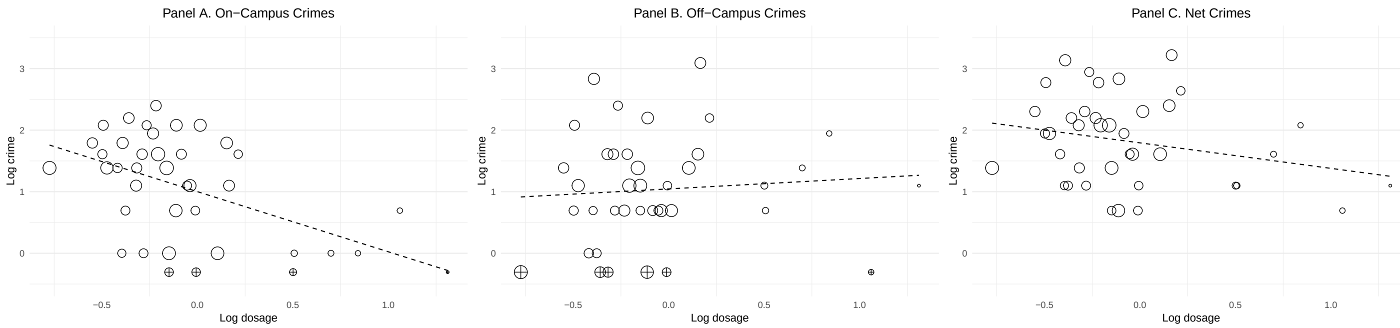
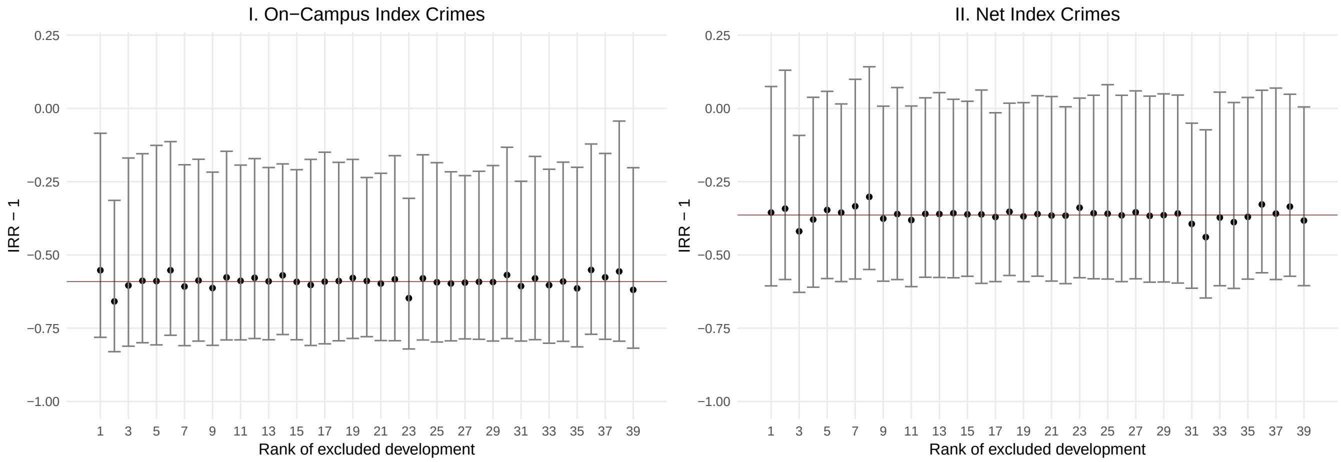
Figure 1. Relationship Between Lighting and Outdoor Nighttime Index Crimes



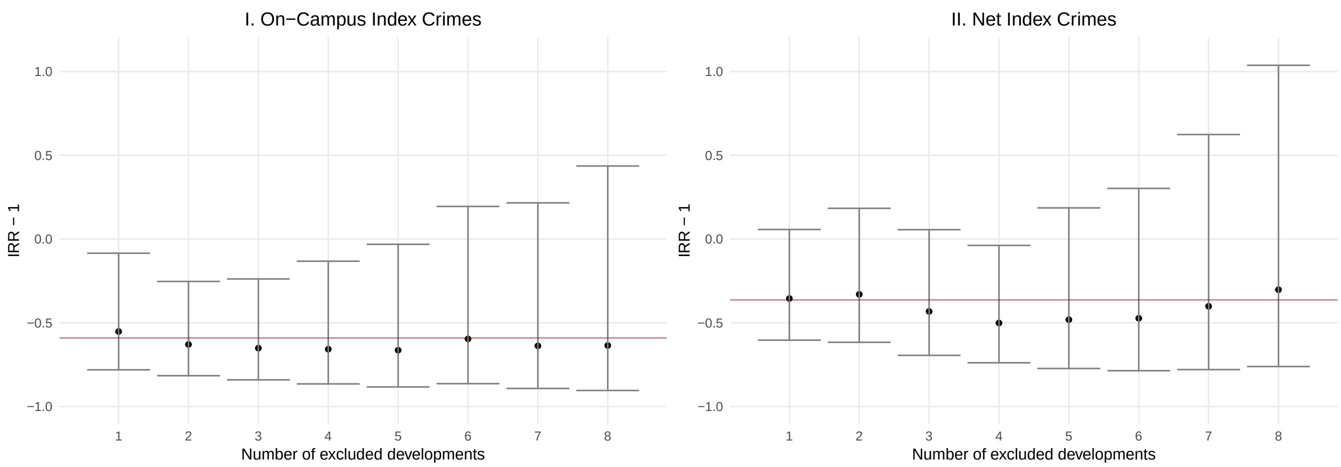

*Note:* Plots illustrate the relationship between the natural logarithm of the number of nighttime outdoor index crimes for the March through August 2016 study period and the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). 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 = 39 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 2. Robustness of Estimated Treatment Effects to Dropping Developments

A. Excluding One Development at a Time

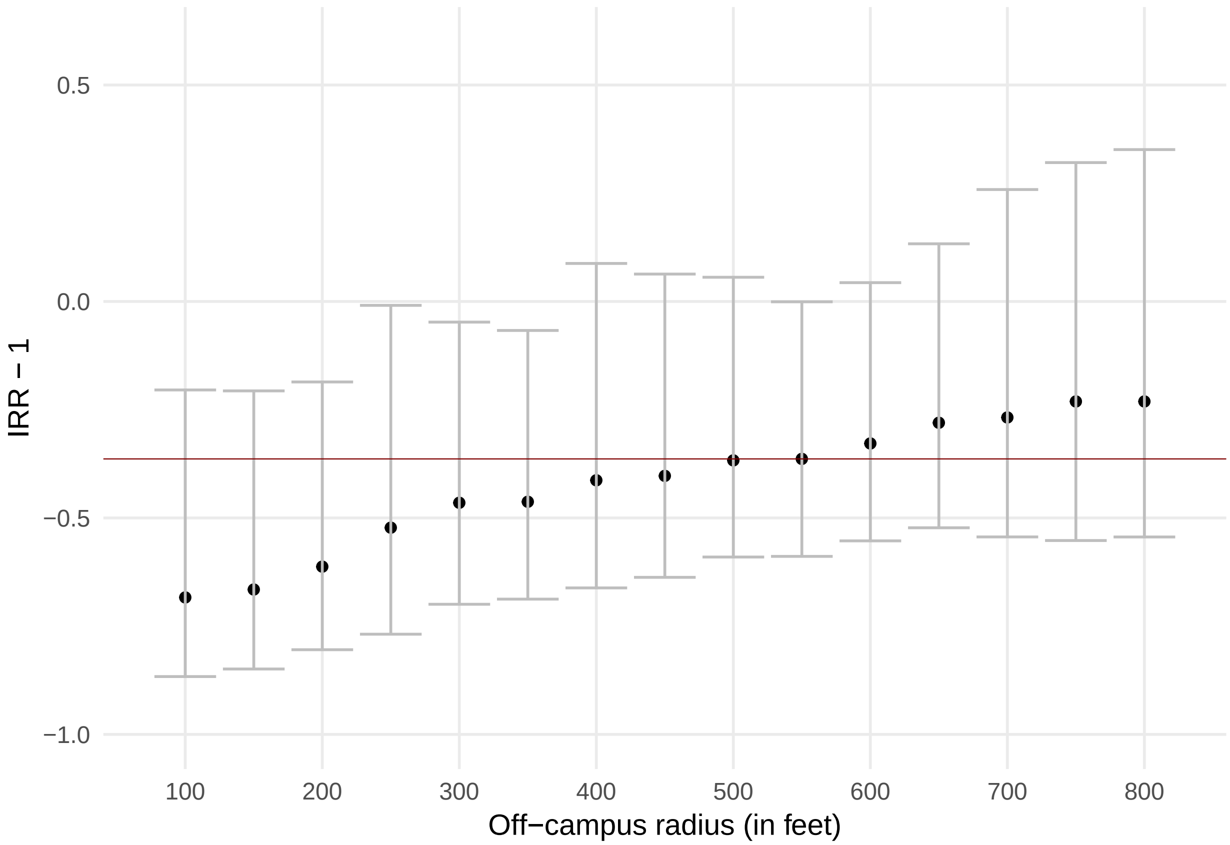



B. Excluding Developments Cumulatively, According to Dosage




*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 39 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.

Figure 3. 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-campus 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.

Table 1. Summary Statistics

| | Treatment | Control | p-value |
|---|---|---|---|
| **Past Nighttime Crime** | | | |
| On-campus outdoor index | 3.60 | 3.03 | 0.20 |
| Off-campus outdoor index | 3.77 | 3.77 | 1.00 |
| On-campus outdoor nonserious | 10.95 | 8.85 | 0.16 |
| Off-campus outdoor nonserious | 7.58 | 8.01 | 0.73 |
| **Population Structure** | | | |
| Avg. population | 2449.68 | 2330.00 | 0.70 |
| Avg. population density | 182.04 | 186.18 | 0.84 |
| Avg. household size | 2.45 | 2.32 | 0.06 |
| Pct. of population male 15-24 | 0.10 | 0.09 | 0.36 |
| **Physical Characteristics** | | | |
| Avg. entrances per building | 1.63 | 1.98 | 0.26 |
| Style | 1.24 | 1.37 | 0.45 |
| Square feet (thousands) | 730.83 | 725.67 | 0.97 |
| **F-test** | | | |
| Treatment vs. control | - | - | 0.51 |
| Dosage | - | - | 0.47 |

*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 final 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-Campus | Off-Campus | Net |
|---|---|---|---|
| Nighttime (D=1) | -0.89 | -0.01 | -0.45 |
| | (0.34) | (0.45) | (0.23) |
| | [-59%] | [-1%] | [-36%] |
| Daytime (D=1) | -0.40 | -0.29 | -0.29 |
| | (0.44) | (0.62) | (0.18) |
| | [-33%] | [-25%] | [-25%] |
| Nighttime (D=0) | -0.15 | 0.18 | -0.06 |
| | (0.35) | (6.73) | (0.35) |
| | [-14%] | [20%] | [-6%] |

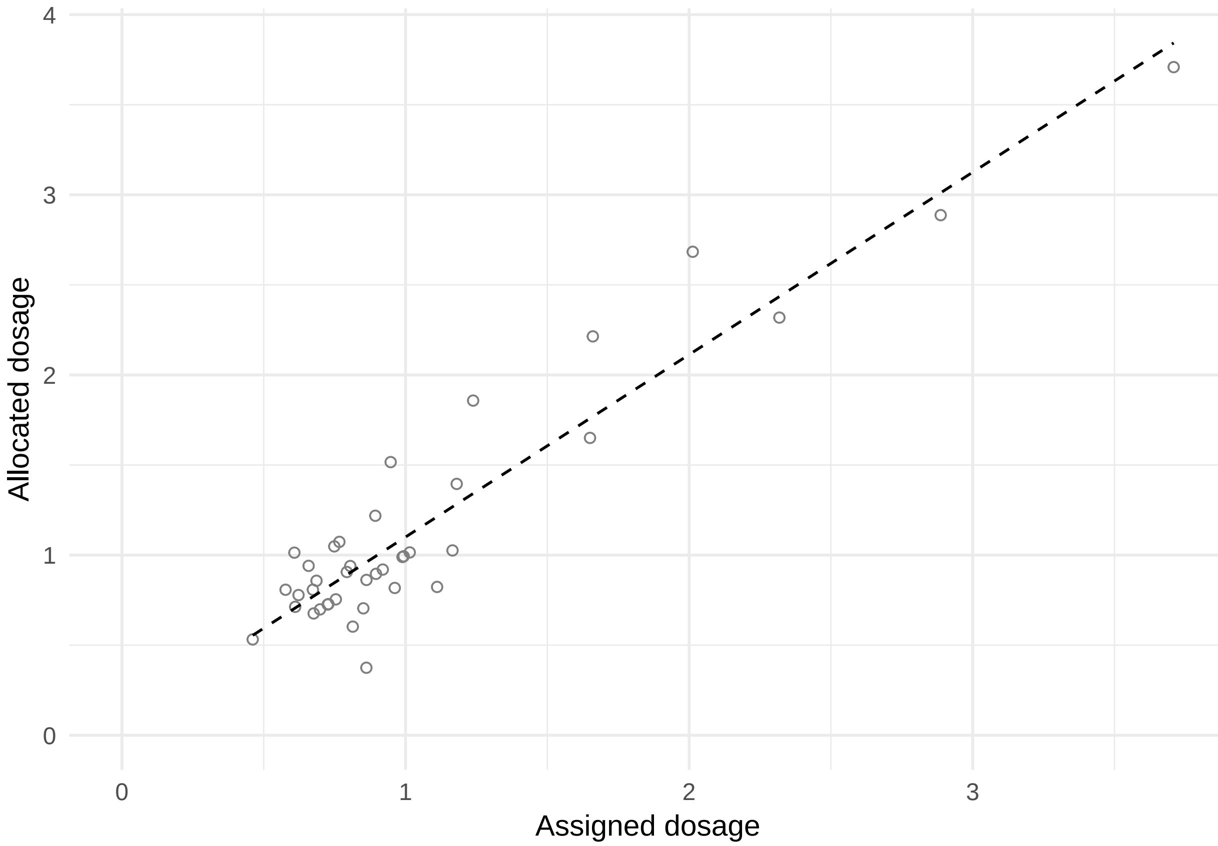
*Note:*
This table reports estimates from a series of Poisson regressions of index crimes for the March through August 2016 study period on the natural logarithm of each housing development's randomly assigned number of additional lights per square block (treatment dosage). Each cell reports a weighted 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-campus), 2) a catchment area within 550 feet of the development's campus exclusive of the campus itself (Off-campus), 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-campus (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 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).

Appendix Figure 1. Light Towers in the Field



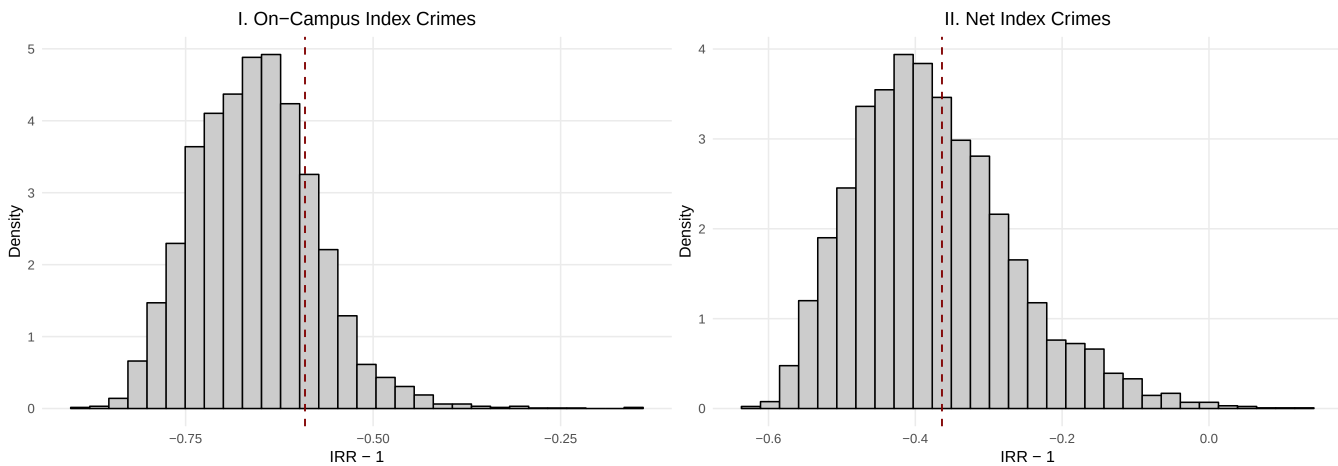
Credit: Ruddy Roye; `https://www.wnyc.org/story/spotlight-safety-housing-projects`

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 = 39 treatment sites. The dotted line plots the linear regression of allocated dosage on assigned dosage.

Appendix 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.

Appendix Table 1. Covariate Selection

| | Nighttime, D = 1 | | | Daytime, D = 1 | | | Nighttime, D = 0 | | |
|---|---|---|---|---|---|---|---|---|---|
| | On-campus | Off-campus | Net | On-campus | Off-campus | Net | On-campus | Off-campus | Net |
| Avg. entrances per building | | 0.128 | | 0.026 | | | | 0.966 | 0.820 |
| Avg. height | | 0.004 | 0.006 | | | | | 0.336 | 0.296 |
| Avg. household size | | | 0.004 | 0.050 | | | | 0.178 | |
| Avg. population | | | | | | | | | |
| Avg. population (ln) | | | 0.056 | | | | | | |
| Avg. population density | | | | | 0.020 | | 0.174 | | 0.036 |
| Avg. units per population | | 0.014 | | | | 0.062 | | | |
| Pct. of population male 15-24 | | 0.102 | | | 0.024 | | | 0.068 | 0.856 |
| Pct. of population male 15-24 (ln) | | | | | | | | 0.424 | 0.200 |
| Square feet | | | | | | | | | |
| Square feet (ln) | | | | 0.812 | | | | | |
| Style | | | 0.004 | | | 0.028 | 0.320 | 0.996 | 0.900 |
| Past assault, daytime, indoor (on-campus) | | | | | | | 0.698 | | |
| Past assault, daytime, outdoor (off-campus) | | | | | | | | | |
| Past assault, daytime, outdoor (on-campus) | | | | | | | | | |
| Past assault, nighttime, indoor (on-campus) | | | | | | | | | |
| Past assault, nighttime, outdoor (off-campus) | | | | | | | | | |
| Past assault, nighttime, outdoor (on-campus) | | | | | | | 0.708 | | |
| Past index, daytime, indoor (on-campus) | | | | | | | | | |
| Past index, daytime, outdoor (off-campus) | | 0.796 | | | 0.026 | | | | |
| Past index, daytime, outdoor (on-campus) | | | | | | | | | |
| Past index, nighttime, indoor (on-campus) | | | | 0.256 | | 0.212 | | | |
| Past index, nighttime, outdoor (off-campus) | | | | | | | | | 0.540 |
| Past index, nighttime, outdoor (on-campus) | | | | | | | | | |
| Past nonserious, daytime, indoor (on-campus) | | | | | | | | | |
| Past nonserious, daytime, outdoor (off-campus) | | | | | | | | | |
| Past nonserious, daytime, outdoor (on-campus) | | | | | | | | | |
| Past nonserious, nighttime, indoor (on-campus) | | | | | | | | | |
| Past nonserious, nighttime, outdoor (off-campus) | | 0.612 | | | 0.018 | | | | |
| Past nonserious, nighttime, outdoor (on-campus) | | | | | | | | | |
| Past serious, daytime, indoor (on-campus) | | | | | | | | | |
| Past serious, daytime, outdoor (off-campus) | | | | | | | | | |
| Past serious, daytime, outdoor (on-campus) | | | | 0.396 | | | | | |
| Past serious, nighttime, indoor (on-campus) | | | | | | | | | |
| Past serious, nighttime, outdoor (off-campus) | | | | | | | | | |
| Past serious, nighttime, outdoor (on-campus) | | | | | | | 0.024 | | |

*(continued)*

| | On-campus | Off-campus | Net | On-campus | Off-campus | Net | On-campus | Off-campus | Net |
|---|---|---|---|---|---|---|---|---|---|
| Assault, daytime, (off-campus), 2011 | 0.002 | | 0.006 | 0.024 | | 0.002 | | | |
| Assault, daytime, (off-campus), 2012 | | | | | | | | 0.056 | |
| Assault, daytime, (off-campus), 2013 | | | 0.004 | | | | | | |
| Assault, daytime, (off-campus), 2014 | | | | | | | 0.570 | | 0.048 |
| Assault, daytime, (off-campus), 2015 | | | 0.002 | | | | | | 0.054 |
| Assault, daytime, (on-campus), 2011 | | | | | | 0.088 | 0.690 | | |
| Assault, daytime, (on-campus), 2011 | | 0.014 | | | | | | | |
| Assault, daytime, (on-campus), 2012 | | | 0.002 | | | | 0.142 | | 0.078 |
| Assault, daytime, (on-campus), 2012 | | 0.142 | 0.142 | 0.002 | | 0.388 | | | |
| Assault, daytime, (on-campus), 2013 | | | | | | | | | |
| Assault, daytime, (on-campus), 2013 | | | | 0.954 | | 0.008 | | 0.630 | 0.318 |
| Assault, daytime, (on-campus), 2014 | | 0.014 | | | | | | | 0.776 |
| Assault, daytime, (on-campus), 2014 | | | | | | | | | |
| Assault, daytime, (on-campus), 2015 | 0.244 | | | | | 0.006 | | | |
| Assault, daytime, (on-campus), 2015 | 0.980 | 0.002 | 0.382 | | 0.002 | 0.526 | | | |
| Assault, daytime, outdoor (net), 2011 | | | | | | | | 0.544 | |
| Assault, daytime, outdoor (net), 2012 | | | | | | | 0.002 | 0.262 | |
| Assault, daytime, outdoor (net), 2013 | | | | | | | | | |
| Assault, daytime, outdoor (net), 2014 | | | | | | | | | 0.036 |
| Assault, daytime, outdoor (net), 2015 | | | 0.720 | 0.026 | | 0.688 | | 0.004 | |
| Assault, nighttime, (off-campus), 2011 | | | | | | | | | |
| Assault, nighttime, (off-campus), 2012 | | | | 0.014 | 0.002 | | | 0.324 | |
| Assault, nighttime, (off-campus), 2013 | | | | | | | | | 0.136 |
| Assault, nighttime, (off-campus), 2014 | | | 0.276 | | | 0.362 | | | |
| Assault, nighttime, (off-campus), 2015 | | | | | | | | | |
| Assault, nighttime, (on-campus), 2011 | | | | | | | | | |
| Assault, nighttime, (on-campus), 2011 | | 0.102 | 0.004 | 0.022 | 0.176 | 0.408 | | | |
| Assault, nighttime, (on-campus), 2012 | | | | | | | | 0.108 | 0.152 |
| Assault, nighttime, (on-campus), 2012 | | 0.014 | 0.006 | 0.060 | | 0.014 | 0.936 | | |
| Assault, nighttime, (on-campus), 2013 | | | | | 0.388 | | | | |
| Assault, nighttime, (on-campus), 2013 | | | | | | | | | |
| Assault, nighttime, (on-campus), 2014 | | | | 0.654 | | 0.332 | | | 0.412 |
| Assault, nighttime, (on-campus), 2014 | | 0.010 | | | | | | | |
| Assault, nighttime, (on-campus), 2015 | | | | | | | | | |
| Assault, nighttime, (on-campus), 2015 | | | | 0.462 | 0.156 | | 0.124 | 0.870 | |
| Assault, nighttime, outdoor (net), 2011 | | | | | | 0.002 | | | |
| Assault, nighttime, outdoor (net), 2012 | 0.244 | | 0.008 | | | 0.002 | | | 0.918 |
| Assault, nighttime, outdoor (net), 2013 | | | | | | | | | |

*(continued)*

| | On-campus | Off-campus | Net | On-campus | Off-campus | Net | On-campus | Off-campus | Net |
|---|---|---|---|---|---|---|---|---|---|
| Assault, nighttime, outdoor (net), 2014 | 0.140 | | | | | | | | |
| Assault, nighttime, outdoor (net), 2015 | 0.398 | | | | | | | | |
| Index, daytime, (off-campus), 2011 | | | | | | | | | |
| Index, daytime, (off-campus), 2012 | | 0.644 | | | 0.970 | | | | 0.136 |
| Index, daytime, (off-campus), 2013 | | | | | 0.530 | | | | |
| Index, daytime, (off-campus), 2014 | | | | | 0.852 | 0.400 | | 1.000 | 0.806 |
| Index, daytime, (off-campus), 2015 | | | | | | | | 0.056 | |
| Index, daytime, (on-campus), 2011 | | | 0.004 | 0.002 | | | | | 0.092 |
| Index, daytime, (on-campus), 2011 | | | | | 0.114 | | | | |
| Index, daytime, (on-campus), 2012 | | | | | | | | 0.004 | |
| Index, daytime, (on-campus), 2012 | 0.244 | | | | | | | | |
| Index, daytime, (on-campus), 2013 | | 0.024 | 0.004 | | | 0.632 | | | 0.412 |
| Index, daytime, (on-campus), 2013 | | | | 0.980 | | | 0.366 | | 0.874 |
| Index, daytime, (on-campus), 2014 | | | | | | | | | |
| Index, daytime, (on-campus), 2014 | | | 0.002 | | | 0.006 | | | 0.218 |
| Index, daytime, (on-campus), 2015 | | 0.004 | | | | 0.878 | | | |
| Index, daytime, (on-campus), 2015 | | | 0.058 | | | | | | |
| Index, daytime, outdoor (net), 2011 | | | | | | | | | |
| Index, daytime, outdoor (net), 2012 | | | 0.870 | | 0.576 | 0.918 | | | |
| Index, daytime, outdoor (net), 2013 | | | | | | | 0.004 | | 0.874 |
| Index, daytime, outdoor (net), 2014 | | 0.014 | | | | | | | |
| Index, daytime, outdoor (net), 2015 | | 0.314 | 1.000 | | | | | 0.164 | |
| Index, nighttime, (off-campus), 2011 | | | | | | | | | 0.436 |
| Index, nighttime, (off-campus), 2012 | | | | | | | | | |
| Index, nighttime, (off-campus), 2013 | | 0.088 | 0.002 | | | | 0.152 | | |
| Index, nighttime, (off-campus), 2014 | | | | | 0.550 | | | | 0.772 |
| Index, nighttime, (off-campus), 2015 | | | | | | | | 1.000 | 0.608 |
| Index, nighttime, (on-campus), 2011 | | | 0.002 | | | | | 0.996 | 0.706 |
| Index, nighttime, (on-campus), 2011 | | | | | | | | | |
| Index, nighttime, (on-campus), 2012 | | | 0.004 | 0.690 | 0.002 | | 0.120 | | 0.772 |
| Index, nighttime, (on-campus), 2012 | | | | | | | | | |
| Index, nighttime, (on-campus), 2013 | | 0.004 | 0.004 | | 0.002 | 0.002 | | | |
| Index, nighttime, (on-campus), 2013 | | 0.006 | 0.002 | | | | | | |
| Index, nighttime, (on-campus), 2014 | | | 0.006 | | 0.156 | 0.002 | | 0.630 | 0.584 |
| Index, nighttime, (on-campus), 2014 | | | | | | | | | 0.158 |
| Index, nighttime, (on-campus), 2015 | | | | | 0.032 | 0.684 | | | |
| Index, nighttime, (on-campus), 2015 | | | | | 0.002 | 0.158 | | 0.996 | |
| Index, nighttime, outdoor (net), 2011 | | | | | | | 0.180 | | |

*(continued)*

| | On-campus | Off-campus | Net | On-campus | Off-campus | Net | On-campus | Off-campus | Net |
|---|---|---|---|---|---|---|---|---|---|
| Index, nighttime, outdoor (net), 2012 | | | | | | 0.002 | | | |
| Index, nighttime, outdoor (net), 2013 | | | 0.004 | | | | | | 0.014 |
| Index, nighttime, outdoor (net), 2014 | | | | | | | | 0.004 | |
| Index, nighttime, outdoor (net), 2015 | | | 0.002 | | | | | | |
| Nonserious, daytime, (off-campus), 2011 | | 0.996 | 1.000 | | | 0.110 | | 0.094 | |
| Nonserious, daytime, (off-campus), 2012 | | | | | | | 0.400 | | 0.232 |
| Nonserious, daytime, (off-campus), 2013 | | | | | | 0.124 | | 0.200 | 0.028 |
| Nonserious, daytime, (off-campus), 2014 | | | | | | | | | |
| Nonserious, daytime, (off-campus), 2015 | | | | | | | | | |
| Nonserious, daytime, (on-campus), 2011 | | | | 0.024 | | 0.008 | 0.008 | | |
| Nonserious, daytime, (on-campus), 2011 | | | | | | | | | |
| Nonserious, daytime, (on-campus), 2012 | | | | | | | 0.008 | 0.894 | |
| Nonserious, daytime, (on-campus), 2012 | | | | | | | | 0.996 | |
| Nonserious, daytime, (on-campus), 2013 | | | | | | | 0.224 | 0.580 | |
| Nonserious, daytime, (on-campus), 2013 | | | | | | | 0.040 | | 0.036 |
| Nonserious, daytime, (on-campus), 2014 | | | | | | | | | |
| Nonserious, daytime, (on-campus), 2014 | | | 0.004 | 1.000 | | 0.206 | | | |
| Nonserious, daytime, (on-campus), 2015 | | | | | | | | | |
| Nonserious, daytime, (on-campus), 2015 | | | | | | | | | |
| Nonserious, daytime, outdoor (net), 2011 | | | 0.136 | | 0.536 | 0.998 | | 0.078 | |
| Nonserious, daytime, outdoor (net), 2012 | | | | | | | | | |
| Nonserious, daytime, outdoor (net), 2013 | | | | | | | | | |
| Nonserious, daytime, outdoor (net), 2014 | | | | 0.054 | | 0.926 | | | |
| Nonserious, daytime, outdoor (net), 2015 | | | | | | | | | |
| Nonserious, nighttime, (off-campus), 2011 | | 0.644 | | | 0.938 | | | | |
| Nonserious, nighttime, (off-campus), 2012 | | | | | | | | | |
| Nonserious, nighttime, (off-campus), 2013 | | | 0.002 | | | | | | |
| Nonserious, nighttime, (off-campus), 2014 | | 0.500 | | | 0.852 | 0.408 | | | |
| Nonserious, nighttime, (off-campus), 2015 | | 0.962 | 0.980 | | | | 0.268 | | 0.038 |
| Nonserious, nighttime, (on-campus), 2011 | | | 0.004 | | | | | | |
| Nonserious, nighttime, (on-campus), 2011 | 0.888 | | | | | 0.020 | | 0.114 | 0.014 |
| Nonserious, nighttime, (on-campus), 2012 | | | | | | 0.008 | | | |
| Nonserious, nighttime, (on-campus), 2012 | | | | 0.040 | 0.008 | | | | 0.516 |
| Nonserious, nighttime, (on-campus), 2013 | | 0.074 | 0.004 | | 0.004 | | | | |
| Nonserious, nighttime, (on-campus), 2013 | 0.140 | 0.002 | | 0.988 | | 0.028 | | | |
| Nonserious, nighttime, (on-campus), 2014 | | | | | | | | | |
| Nonserious, nighttime, (on-campus), 2014 | | 0.002 | | 1.000 | 0.002 | | | | |
| Nonserious, nighttime, (on-campus), 2015 | | | | | | 0.002 | | 0.120 | 0.058 |

*(continued)*

| | On-campus | Off-campus | Net | On-campus | Off-campus | Net | On-campus | Off-campus | Net |
|---|---|---|---|---|---|---|---|---|---|
| Nonserious, nighttime, (on-campus), 2015 | | | | | | | 1.000 | | 0.052 |
| Nonserious, nighttime, outdoor (net), 2011 | | | 0.142 | | | | | 0.876 | |
| Nonserious, nighttime, outdoor (net), 2012 | | | | | 0.460 | | | | |
| Nonserious, nighttime, outdoor (net), 2013 | | | | 0.984 | | 0.436 | | 0.986 | 0.434 |
| Nonserious, nighttime, outdoor (net), 2014 | | | | | | 0.126 | | | |
| Nonserious, nighttime, outdoor (net), 2015 | | | | | | | | | |
| Serious, daytime, (off-campus), 2011 | | | 0.002 | | 0.010 | 0.028 | | 0.004 | |
| Serious, daytime, (off-campus), 2012 | | | | | 0.004 | | 0.048 | | 0.012 |
| Serious, daytime, (off-campus), 2013 | | | | | | | | | |
| Serious, daytime, (off-campus), 2014 | | | | | | | | | |
| Serious, daytime, (off-campus), 2015 | | | | | | | | 0.390 | |
| Serious, daytime, (on-campus), 2011 | | | | | | | | | |
| Serious, daytime, (on-campus), 2011 | | | | | | | | | |
| Serious, daytime, (on-campus), 2012 | | | | | | | | | |
| Serious, daytime, (on-campus), 2012 | | | | 0.026 | | | | | |
| Serious, daytime, (on-campus), 2013 | | | | | | | 0.690 | | |
| Serious, daytime, (on-campus), 2013 | | | | 0.070 | | | | | 0.018 |
| Serious, daytime, (on-campus), 2014 | | | | | | | | | |
| Serious, daytime, (on-campus), 2014 | | | | | | | | | |
| Serious, daytime, (on-campus), 2015 | | | | | | | | | |
| Serious, daytime, (on-campus), 2015 | | | | 0.792 | | | | | |
| Serious, daytime, outdoor (net), 2011 | | 0.002 | | | 1.000 | 1.000 | | 0.492 | |
| Serious, daytime, outdoor (net), 2012 | | | | | 0.054 | 0.090 | | | |
| Serious, daytime, outdoor (net), 2013 | | | 0.004 | | | | | | 0.698 |
| Serious, daytime, outdoor (net), 2014 | | | | | | | | | |
| Serious, daytime, outdoor (net), 2015 | | | | | | | | 0.940 | 0.528 |
| Serious, nighttime, (off-campus), 2011 | | | | | 0.790 | | | | |
| Serious, nighttime, (off-campus), 2012 | | | | | | | | | |
| Serious, nighttime, (off-campus), 2013 | | 0.912 | 0.162 | | | | | | |
| Serious, nighttime, (off-campus), 2014 | | 0.968 | | | 1.000 | | | | 0.376 |
| Serious, nighttime, (off-campus), 2015 | | | | | 0.004 | | | | |
| Serious, nighttime, (on-campus), 2011 | | | | | | | | | 0.318 |
| Serious, nighttime, (on-campus), 2011 | | | | | | | | | |
| Serious, nighttime, (on-campus), 2012 | | | | | | | 1.000 | | 0.078 |
| Serious, nighttime, (on-campus), 2012 | | | | | | | | | 0.158 |
| Serious, nighttime, (on-campus), 2013 | | | | | 0.084 | 0.844 | | | |
| Serious, nighttime, (on-campus), 2013 | | | | | | | 0.068 | | 0.100 |
| Serious, nighttime, (on-campus), 2014 | | | | 0.932 | | | 0.014 | | 1.000 |

*(continued)*

| | On-campus | Off-campus | Net | On-campus | Off-campus | Net | On-campus | Off-campus | Net |
|---|---|---|---|---|---|---|---|---|---|
| Serious, nighttime, (on-campus), 2014 | | | | | | | | | 0.018 |
| Serious, nighttime, (on-campus), 2015 | | | | | | | | | |
| Serious, nighttime, (on-campus), 2015 | | | | | | | 1.000 | | |
| Serious, nighttime, outdoor (net), 2011 | | | | 0.014 | | | 0.180 | | 0.132 |
| Serious, nighttime, outdoor (net), 2012 | | | | | | | | | 0.164 |
| Serious, nighttime, outdoor (net), 2013 | | | 0.464 | | | 0.652 | 0.326 | | 0.180 |
| Serious, nighttime, outdoor (net), 2014 | | | | | | 0.430 | | | |
| Serious, nighttime, outdoor (net), 2015 | | 0.996 | 0.994 | | | 0.020 | | | 0.166 |

*Note:* This table reports the percentage among the 500 LASSO regressions in which each covariate was selected to be included in the outcome model used to estimate treatment effects.