The Impact of Street Lighting on Crime in New York City Public Housing

CRIME LAB New York

Science in Service of Cities.

THE IMPACT OF STREET LIGHTING ON CRIME IN NEW YORK CITY PUBLIC HOUSING

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University of Chicago Crime Lab New York[1]

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Introduction:

In 2014 the Mayor's Office of Criminal Justice (MOCJ) identified increased street lighting as a potential strategy to reduce outdoor nighttime crime in and around New York City Housing Authority (NYCHA) housing developments. In response to these discussions, Crime Lab New York (CLNY) partnered with the city to design a randomized study of the effect of temporary outdoor lighting on crime in NYCHA developments in all five boroughs of New York City. The study sought to estimate the impact and cost-effectiveness of temporary street lights on crime and other measures of community well-being.

CLNY and its agency partners worked closely together for nearly two years in planning this study. The New York City Police Department (NYPD) identified which developments were priorities for receiving additional lights, based, in part, on their elevated crime rates and perceived need for additional lighting. In early 2016, CLNY randomized 39 into treatment and 38 into control sites. Among the treated developments, approximately four hundred light towers were allocated to the treated developments where each development received a number of additional light towers according to a randomly-assigned dosage variable, chosen from a uniform distribution of lights per square feet. This research design allows us to ascertain whether the effectiveness of lighting diminishes as more lights are added to a development. The average NYCHA development in the study sample spans approximately 720,000 square feet and received an additional 10 temporary lights.

Light towers were deployed between February 29 and March 7, 2016 to outdoor public spaces around the treated NYCHA developments. These developments remained illuminated during all nighttime hours for the six-month duration of this study period. "Control" developments received no additional outdoor lighting ("business-as-usual").

Model description:

Primary results are derived from what we refer to as our "dosage" model of the effectiveness of lighting, which takes advantage of the randomized allocation of the number of light towers per development, creating treatment effect heterogeneity.[2] Specifically, we test whether developments that received a greater dosage of lighting experience larger reductions in crime. While dosage was intentionally varied from the start, we also initially attempted a simple comparison of treatment and control

[2] Our methodology utilizes Poisson regression models. These models are naturally well-suited to this particular research question due to the count nature of the main outcome variables: crime in and around the NYCHA developments. In the original project design, the dosage of lighting was randomized among the treatment developments. Consequently, we measure treatment dosage as a continuous measure of the number of lights per square foot. We estimate two main models, models which include the numbers of lights per square feet, and models with log of the number of lights per square feet. The first model's estimated coefficient informs about the effect that adding one more temporary lighting tower has on local crime, while the second model's estimated coefficient informs about the effect of increasing the amount of temporary lighting in percentages. In all regression models we include a variety of controls for lagged crime, neighborhood demographics, and development characteristics.

developments, but as we had anticipated, the simple comparison left us with insufficient statistical power to detect differences in crime between the treated and control developments. Because there is a great deal of variation in the amount of lighting received by each development, the dosage model maximizes statistical power – that is, our ability to detect an effect of lighting if one exists.

It is critical to establish that the dosage model that we use is appropriate to estimate the effectiveness of lighting. In order to test the validity of the dosage model, we employ two "placebo" tests which allow us to scrutinize the legitimacy of our approach. First, we estimate the model for crimes committed during daytime hours among the treated developments. The intuition behind this placebo test is that we should not expect lighting to have an effect (or at least not as large an effect) on daytime crimes – as the lights are not functional during the day.

Second, we make use of the original randomized control group and apply the dosage model to estimate the effect of the randomized dosage of lighting on crime among the developments in the control group. This is a particularly powerful placebo test as there should be no effect of randomized dosage in developments that did not, in fact, receive additional lighting. In both placebo tests, we find **no evidence** that crime is decreasing as a function of dosage, which constitutes important evidence that 1) dosage was, in fact, randomly allocated and 2) that estimated treatment effects are correct.

Primary crime findings:

Our analysis focuses on four main outcomes: 1) index crime complaints, 2) felony complaints, 3) "assault, homicide, and weapons" complaints (in order to study the effect of lighting on interpersonal violence), and 4) misdemeanor complaints. Index crime complaints include murder and non-negligent manslaughter, negligent or unclassified homicide, robbery, felony assault, burglary, grand larceny, and grand larceny of a motor vehicle.[3] Felony and misdemeanor complaints are defined using the law code category variable in the NYPD data file. Assault, homicide, and weapons complaints include murder and non-negligent manslaughter, negligent or unclassified homicide, assault 3 and related offenses, felony assault, and complaints for dangerous weapons. For each of these four complaint types, we examine outdoor nighttime, indoor nighttime, outdoor daytime, and indoor daytime complaints. We also look for displacement, which we define as incidents which occur outside of NYCHA property, but within 750 feet of a development.

Among the study sites, we detect robust crime reductions outside at night, specifically for index crimes, felony crimes and, to a lesser degree, assault, homicide and weapons crimes. The following reductions take account of nearby, off-campus displacement:[4]

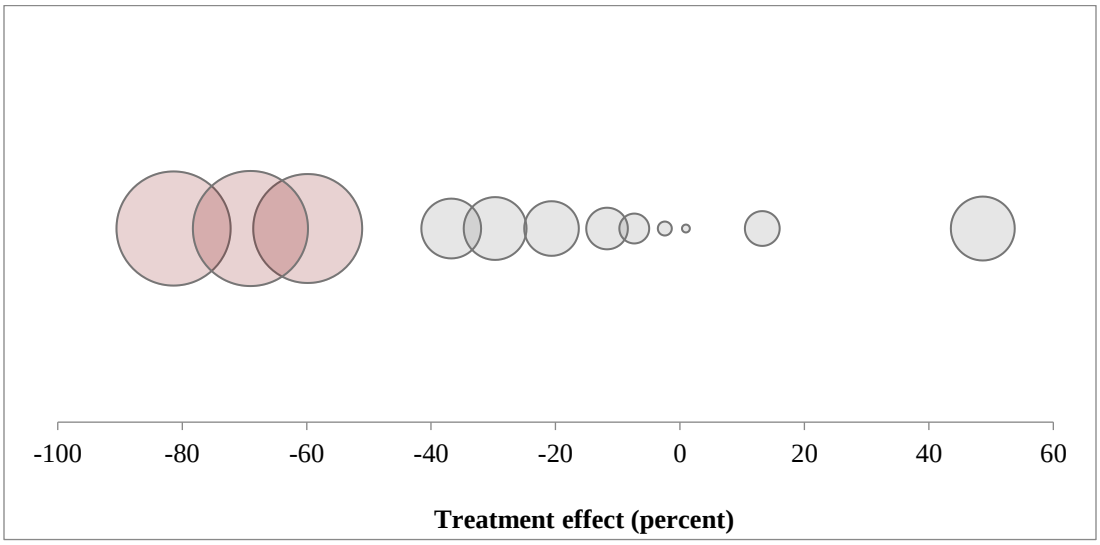
[3] We do not include rape in our analysis because we do not receive data on any sexual offenses from NYPD.

[4] We note that the differences in estimated effects for index and felony crimes is sensible given that index crimes are a subset of felonies. To provide a sense for the degree of overlap between the two

- **Index crimes:** 7% reduction in overall index crimes (day and night). This reduction in overall index crimes was driven by a 39% reduction in index crimes that took place outdoors at night.[5]
- **Felony crimes:** 5% reduction in overall felony crimes (day and night). This reduction in overall felony crimes was driven by a 30% reduction in felony crimes that took place outdoors at night.[6]
- **Assault, homicide and weapons crimes:** 2% reduction in overall assault, homicide, and weapons crimes (day and night). This reduction in overall assault, homicide and weapons crimes was driven by a 12% reduction in assault, homicide and weapons crimes that took place outdoors at night.[7]
- **Misdemeanor crimes:** No detectable change in net misdemeanor crimes in treatment communities.

In order to provide a visual sense for both the magnitude of estimated *on-campus* treatment effects as well as the extent to which these effects are unusual in the absence of some sort of crime control intervention, **Figure 1** presents the estimated effects using the dosage model for *both treatment and control developments*, in both daytime and nighttime hours.

Figure 1. Distribution of placebo vs. actual treatment effects



crime categories, there are seven index crimes with a total of approximately 26 different offenses (note that we only receive data on six of the seven index crime categories). With respect to felony crimes, there are five classes and approximately 360 different offenses. In and around the treatment developments in the study, 59% of felony crimes are also index crimes.

[5] This is because 17% of index crimes occurred outdoors at night.

[6] This is because 17% of felony crimes occurred outdoors at night.

[7] This is because 16% of assault, homicide, and weapons crimes occurred outdoors at night. These crimes include assault 3 and related offenses, dangerous weapons, felony assault, homicide (negligent), and murder and non-negligent manslaughter. Some of these crimes were also categorized as felonies, so these groupings are not additive.

The horizontal axis represents the treatment effect as a percentage change in on-campus crime. The red bubbles are the estimated treatment effects (on index, felony and violent crimes) for nighttime outdoor crimes at treatment developments. The gray bubbles represent estimates crime reductions in either control developments (day and nighttime) or in treatment developments during the day. The position of the red bubbles suggests that the largest reductions are observed precisely where they should be if lighting is the source of the observed crime reductions – reductions in crime outdoors and among nighttime crimes at the treatment developments are well larger than any changes in crime observed either during the day or in control developments. The size of the bubbles indicates the "statistical significance" of the estimated treatment effects. Not only are the estimated effects large; they are precisely estimated – there is well less than a one percent chance that such effects would be observed by chance.

Another way to think about these estimated treatment effects is that one additional light tower per square city block (a standard Manhattan street by an avenue or 125,000 square feet) leads to the following changes in crime:

- Reduction of 1.2 index crimes per development over the six-month study period
- Reduction of 1.2 felony crimes per development over the six-month study period
- Reduction of 0.4 assault, homicide, and weapons crimes per development over the six-month study period

Cost-benefit analysis:

Benefits

In order to estimate the costs associated with crime (and therefore the benefits accruing to NYC residents), we construct per-development cost-of-crime measures utilizing prior estimates from the literature. Based on these estimates, the economic value of crimes abated due to lighting upgrades is projected to be approximately $700,000 per development per year. Notably, these estimates do not include broader benefits of crime reduction, particularly longer-term effects of crime on a community's economic vitality or the intergenerational dividend that is likely to accrue when children are raised in the comparative safety of a more secure and vibrant community.

Costs

Based on the cost of prior infrastructure projects in NYCHA, the up-front cost of a development-wide lighting upgrade is forecasted to be between approximately $3 and $4 million for a development of approximately 720,000 square feet.[8] Note that these

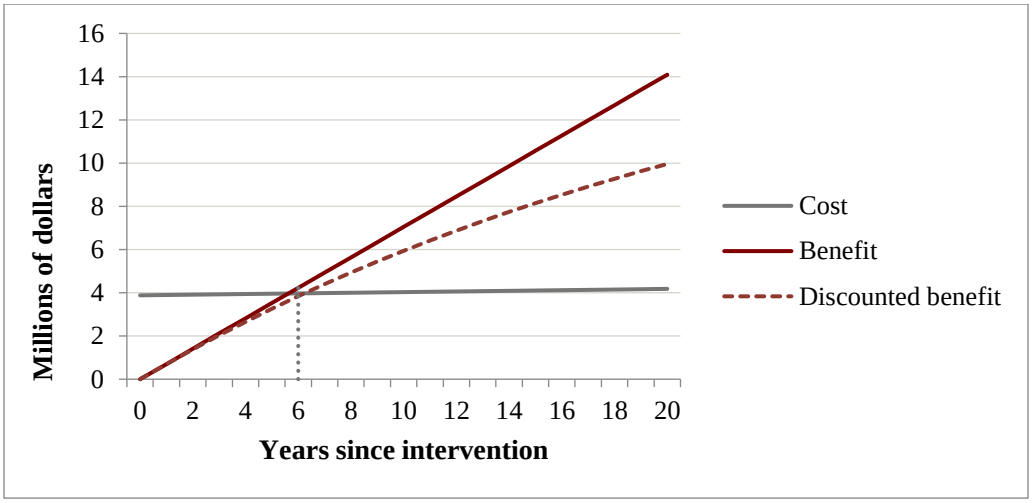
[8] Based on the available data, NYCHA lighting upgrade projects are projected to cost approximately $11,250 per light. This translates to $562,300 to $787,200 per square city block.

estimates assume the removal of old, non-LED hardware and installation of upgraded LED lighting, as is typical in NYCHA lighting upgrade projects.[9]

Our study findings suggest that upper and lower bounds for the number of additional lighting installations are expected to be between 50 and 70 lights per square city block (approximately 125,000 square feet). If we perform a straight conversion based on lumens from the temporary light towers (Allmand 1250w metal halide towers with 150,000 lumens/lamp at 4 lamps/tower = 600,000 lumens total), the total permanent LED-style lamps will be somewhere between 50 (assuming 50w, 4-bulb posttops rated at 16,000-20,000 lumens) and 70 (assuming 25w, 4-bulb posttops rated at 8,000-10,000 lumens). These upper and lower bounds also correspond with actual data from recent NYCHA lighting upgrade projects.[10] While it is extraordinarily difficult to provide an exact mapping from temporary to permanent lights, the fact that two independent approaches to answering this question yield similar estimates makes us optimistic that these estimates are reasonable. The annual cost of providing electricity to the additional lights is expected to be roughly $15,000 per development annually.

Cost-benefit time horizon

Figure 2. Cost-benefit time horizon for NYCHA lighting upgrade



We forecast that lighting upgrades will become cost-effective in six years' time. **Figure 2** displays the number of years following lighting upgrades on the horizontal axis; costs and benefits (in millions of dollars) are shown on the vertical axis. The estimated overall cost of the lighting intervention for a 720,000 square foot development is captured by the gray line. The red lines represent the project's benefits to community residents—that

[9] Previous lighting upgrade projects of which we are aware—including all completed MAP lighting upgrade projects—involved the demolition of nearly all old equipment and the research team assumes that the projects under consideration here would adopt similar practices.

[10] Note that all recent MAP lighting upgrade projects involved the installation of between approximately 50 lights per square city block and just over 70 lights per square city block. These projects include those at Butler, Saint Nicholas, Polo Grounds, Brownsville, Ingersoll, Tompkins, Van Dyke I and II, Castle Hill, Stapleton, Boulevard and Bushwick.

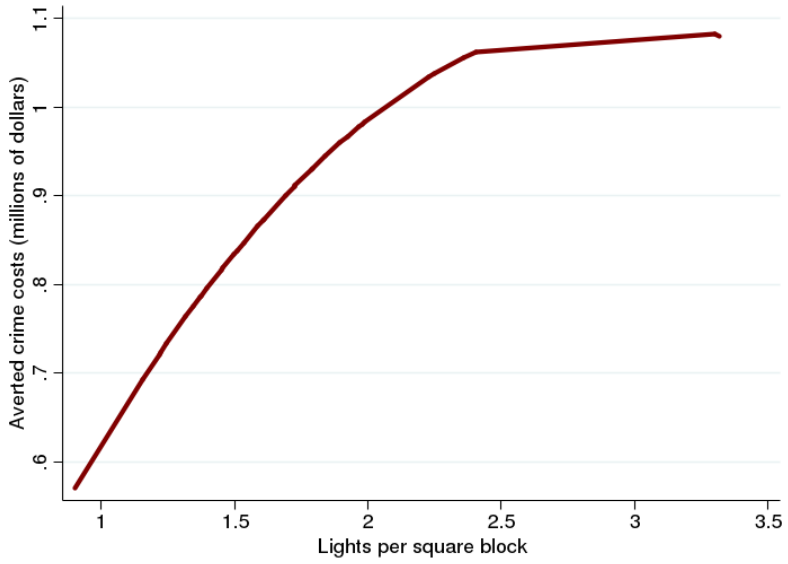
is, the economic value of crimes abated as a result of the lighting upgrades. The solid red line represents the total value of abated crimes assuming no discounting of future crimes. In this case, we anticipate that over twenty years additional lighting will reduce victimization by approximately $14 million per development. The dotted red line represents the value of abated crime assuming future crime abatement is discounted by 4%, a figure which is intended to account for the “time value” of crime (i.e., that people care more about current crime than future crime). Using a 4% discount rate, we estimate that after twenty years lighting provide approximately $10 million in benefits to community residents.

An alternative way to think about the cost of an infrastructure upgrade such as additional lighting is to consider the way that lighting is implicitly financed. In particular, cities often seek financing (e.g., a bond issuance) in order to smooth out large up-front costs over a period of time. Smoothing out the costs of a lighting upgrade project over a twenty-year period at an annual interest rate of 4%, we estimate that lighting upgrades will cost, on average, $200,000 per development annually. Given these annual costs, over two decades, we anticipate that the ratio of benefits to the costs of additional lighting would be approximately 3.5:1.

Diminishing Marginal Returns to Lighting

In order to assist policymakers in making critical decisions about the cost-effectiveness of deploying additional lights, we have constructed a graph that shows how the economic value of abated crimes changes with additional temporary lights. In particular, it would be reasonable to suppose that there are diminishing marginal returns to lighting – that the effects of lighting begin to decline past some saturation point. **Figure 3** demonstrates the diminishing marginal returns of lighting associated with the additional of one more temporary light per square block, per development.

Figure 3. Diminishing marginal returns to lighting



Crime abatement due to lighting is approximately linear until approximately 2.5 lights per square block are added; thereafter the effect diminished considerably which is reflected by the flattening of the line in Figure 2. Until an additional 2.5 lights per square block are added, we estimate that each light per square block is associated with approximately $400,000 in abated harms to crime victims, over a six-month period.

Other outcomes:

Indoor crimes

In our standard model specification, we detect 43% fewer indoor assault, homicide, and weapons crimes (primarily simple assaults) as a result of the additional lighting. However, when we include updated NYPD controls for vertical patrols, Neighborhood Coordinating Officer (NCO) deployment, gang takedowns and home visits, these results become more tenuous.[11] The magnitude and statistical significance of the result is largely dependent on which control variables we include and how these controls are specified. For instance, when we control for vertical patrols alone, the estimated reductions in indoor crimes remain largely identical. When we control for NCO deployments alone, the estimated reductions in indoor crimes falls to 22%. And when we control for both verticals and NCO deployments in the same model, the estimated reductions in indoor crimes become considerably smaller and statistically insignificant. We therefore cannot confidently conclude the models produce evidence that indoor crime falls in response to improved community lighting, though it should be stated that estimated outdoor results are remarkably robust to all combinations of control variables.

Impact on 311 outcomes

We find that the lights had no detectable effect on noise complaints.[12]

Survey results:

In September 2016, a non-random sample of residents in treatment and control developments were sent an online survey intended to collect feedback regarding perceptions of the lights,[13] community sentiments,[14] public use of space,[15] and safety

[11] When including NYPD controls, we exclude Rangel and Samuel (City) due to inconsistencies in the data from the 32nd precinct.
[12] Interestingly, we found statistically significant increases in *daytime* reports of homelessness and drinking as lighting dosage increased.
[13]Lights-related questions were only asked in treatment sites. These questions include: 1) *The lights are not bright enough*; 2) *The lights are placed in the right locations*; 3) *The lights make too much noise*; 4) *The lights were installed because the city cares about my neighborhood*; 5) *The lights are too bright*; 6) *The lights unite my development and the surrounding neighborhoods*; and 7) *The lights are a positive addition to the neighborhood*.
[14] Community sentiment questions included: 1) *My neighbors in this development usually get along with each other*; and 2) *If a group of children from the building were skipping school and hanging out on a street corner, my neighbors would do something about it*.
[15] Use of space questions include: 1) *The lights make me feel more comfortable outside my building at night*; 2) *The lights change the way I walk home*; 3) *A well-lit courtyard makes me more likely to leave my*

and victimization.[16] The survey was initially sent to 22,362 residents. From this initial population, 1,769 residents responded; therefore, the response rate was 7.9%. Responses were fairly evenly split between treatment and control developments (47% treatment; 53% control). Respondents skewed, older and female: 80% were female and 20% were male. While 15% of respondents were 30 and under, nearly 40% were over 50 years of age.

Responses indicate that residents overwhelmingly liked the lights: 67% of respondents had a favorable opinion toward the lights; only 13% of respondents had a negative opinion. We were unable to detect statistically significant impacts on other indicators of resident attitudes and activities, such as community sentiment, public use of space, and victimization.

When thinking about these results it is important to bear in mind that our sample was non-random (consisting of emails that NYCHA had collected from residents over the years and resident association meetings and through the MyNYCHA system). Furthermore, respondents skewed heavily toward older and female residents. Therefore we are not confident that these responses are representative of how residents on average (or by finer-grained demographics) would have responded.

apartment at night; 4) *The lights welcome non-residents to the development at night*; and 5) *Over the past summer, did you leave your apartment more after dark compared to last summer*.

[16] Safety and victimization questions include: 1) *I usually feel safe walking around my development during the day*; 2) *I usually feel safe walking around my development after dark*; 3) *Thinking about my closest family member or friend in this building: I am worried about this person being robbed or assaulted outside of the building*; 4) *In the last month, has anyone ever used violence (such as a mugging, assault, or sexual assault) against you or any member of your household*.

TECHNICAL APPENDIX

Background:

In 2014 the Mayor's Office of Criminal Justice (MOCJ) identified increased street lighting as a potential strategy to reduce outdoor crime in New York City Housing Authority (NYCHA) housing developments. In brainstorming ways to study the provision of additional lighting in order to inform future policy decisions, Crime Lab New York (CLNY), in partnership with MOCJ, the New York City Police Department (NYPD), and NYCHA, designed a randomized evaluation to study the effect of outdoor lighting on crime in housing developments across all five boroughs of New York City. The study sought to estimate the impact of temporary street lights on crime and other measures of community well-being in order to evaluate whether lighting can be a cost-effective strategy to reduce street crime.

CLNY, MOCJ, NYPD, and NYCHA worked closely together for nearly two years in planning this study. Initial discussions were held in late 2014. In May of 2015, CLNY, NYPD, and MOCJ met to discuss the research design and plan the process by which NYPD would identify priority NYCHA developments for randomization. NYPD identified a total of 80 priority developments, based in part on their elevated crime rates. In early 2016, CLNY randomized 39 into treatment and 38 into control[17] via paired, random sampling methods, stratifying on the number of outdoor nighttime crimes in a development in the two years prior to the intervention.

Approximately four hundred lighting towers were available to be allocated to the treatment group. Among treated developments, the dosage of lights, measured as the number of square feet per light, was randomly assigned according to a continuous distribution, allowing for an evaluation that would be able to discern the effect of different amounts of additional lights. At least two lights were allocated to all treatment sites to ensure that each development received a minimum dosage, and the remaining 320 lights were assigned according to each site's random number drawn from a uniform distribution over the number of "uncovered" square feet. In response to subsequent feedback from NYCHA residents about either excessive or insufficient lighting, the allocated dosage was slightly different from assigned dosage. To protect against bias, assigned dosage of lighting for each development is used as the measure of lighting.[18]

In early 2016, MOCJ coordinated a series of meetings with CLNY and NYCHA tenant association presidents to inform the association presidents of the study and to receive input from them on the locations within their developments that they believed lights

[17] 39 treatment and 38 control developments rather than 40 treatment and control developments were used because of policymakers' decision to reassign some developments from control to treatment group and, therefore, outcomes for these developments are not included in the final analysis.

[18] We also investigated the relationship between assigned dosage and actual dosage. In those regressions, the coefficients was practically identical to one, suggested that increasing assigned dosage by 1 unit is on average met with a 1 unit increase in actual dosage. This implies that were we to scale our reduced form estimates by the first stage to generate instrumental variables or generalized methods of moments estimators, those models would have practically identical estimates (with slightly larger standard errors due to the first stage).

would be most beneficial. Residents were informed of the total number of light towers assigned to their development and were asked to indicate where they wanted the lights to be placed around their development. Resident groups produced several variants of lighting placements within each development, from which CLNY produced a single, composite “heat map” for each development identifying where residents, in aggregate, wanted the lights to be placed.

In February 2016, MOCJ also coordinated meetings with Executive Officers from the police precincts and Police Service Areas (PSAs) where the lighting project would be taking place. The purpose of these meetings was for officers to identify the specific locations in each development where they would like the light towers to be placed. NYPD officers were provided with the residents’ heat maps to help inform their decision-making. The final maps that contained the locations for each light tower were then used to inform the light tower vendor about where each tower was to be positioned.

Light deployment commenced on February 29, 2016 and the final towers were deployed on March 7, 2016. In the NYCHA developments receiving lights (“treatment group”), light towers were installed in outdoor public spaces and remained illuminated during all nighttime hours for the six-month duration of the study period. The “control group” developments received no additional outdoor lighting (business-as-usual). Incidents and outages were tracked over the six-month study period.

Data Description:

The evaluation of the street lighting uses NYPD complaint data from March 2011 through August 2016. The data are at the complaint level and include the type of offense, date, time, and type of place that the incident occurred. For privacy purposes, the data do not include complaints for sex offenses and rapes, and XY location coordinates have been displaced such that each complaint is assigned the XY coordinate of the middle of the block in which it occurred.

For the purpose of this analysis, we consider complaints that occur on the 39 treatment and 38 control NYCHA campuses involved in the study, as well as complaints that occur within surrounding communities. In order to determine which complaints occurred on NYCHA grounds, we begin by using the development field in NYPD data, which provides the name of the development in which the complaint occurred. In addition to including complaints with this field filled in, we include complaints that are missing the development field but have the same XY coordinates as complaints that are not missing the development field. In order to determine which complaints occurred in the surrounding NYCHA communities, we use ArcGIS to plot the XY coordinates and calculate the distance between the complaint location and the footprint of the closest development. In the main displacement analysis, we include complaints that are within 750 feet of a development that did not occur on the development grounds itself.

Among complaints that occur on or around NYCHA grounds, we focus on four main outcomes: 1) index complaints, 2) felony complaints, 3) assault, homicide, and weapons complaints (in order to study the effect of lighting on interpersonal violence), and 4) misdemeanor complaints. Index crime complaints are defined using the offense type

variable and include murder and non-negligent manslaughter, negligent or unclassified homicide, robbery, felony assault, burglary, grand larceny, and grand larceny of a motor vehicle.[19] Felony and misdemeanor complaints are defined using the law code category variable in the NYPD data file. Assault, homicide, and weapons complaints are defined using the offense type variable and include murder and non-negligent manslaughter, negligent or unclassified homicide, assault 3 and related offenses, felony assault, and complaints for dangerous weapons.

For each of the four complaint types, we examine four primary crime locations: 1) outdoor nighttime crimes, 2) indoor nighttime crimes, 3) outdoor daytime crimes, and 4) indoor daytime crimes. Outdoor and indoor crimes are defined using the location description variable. Outdoor crimes include complaints that occur in front of, opposite of, or in the rear of a building. Indoor crimes include complaints that are designated as being inside of a building.[20] In order to determine whether a complaint occurred during the day or at night, we use the time of occurrence of a complaint and merge in daily civil twilight hours.[21] Civil twilight is meant to approximate when it is actually dark outside, beginning approximately half an hour after the official sunset and ending approximately half an hour before the sunrise. If a complaint occurs during the civil twilight hours, it is considered to be a nighttime complaint. All other complaints are considered to be daytime complaints.

For the main analysis, the data are collapsed to the development by year-level, with the counts for index, felony, assault, homicide, and weapons and misdemeanor complaints summed over that time period. These counts include only data from March 2016 through August 2016 to match the study period. The 2016 counts are used as the outcome variable; data from 2011 through 2015 are used as control variables in the main regression. In additional analyses, we also collapse the data to both the day- and month-level.

In addition to the NYPD data, we merge in data received from NYCHA on development-level characteristics. The number of assigned light towers to treatment developments is used when defining dosage in the main model. We include population data, square footage of the campus, and the male population between the ages of 15 and 24 as controls in the main model. In later iterations, we also include precinct, the height of the development, the density of the development, units per population, number of entrances per building, average household size, and whether the development has an elevator or is a walk-up, all of which is provided by NYCHA.

We have also received data from NYPD that includes the date and location of crew takedowns city-wide, the date, time, location of NYPD vertical patrols, the date, time, and location of NYPD family home visits and the presence of neighborhood coordination officers (NCO). These variables were intended to capture, among other things, any

[19] Because we do not have access to rape complaints in the NYPD data, we are unable to include them in index crime counts.
[20] Approximately 20 percent of complaints are missing a location description; these are not included in outdoor or indoor counts.
[21] Daily civil twilight hours are downloaded from https://www.timeanddate.com/sun/usa/new-york; 2016 hours are applied to the full dataset.

differences in law enforcement presence between developments which may have emerged after the assignment of treatment.

Econometric Modeling:

Our primary statistical analyses are based on Poisson count data econometric models. These are non-linear models utilized in instances where the data generating process follow a count – in other words the data are bounded by zero and are realized in integer values only. This is a standard modeling strategy for crime data, as the number of crimes in a community over a fixed time window naturally follow a count process. The underlying assumption of the Poisson model is $E(Y|X) = e^{X'\beta}$. This is similar to estimating a linear regression model where the dependent variable, *Y*, is transformed using the natural log. The similarities between the models are evident, as the key assumption for an OLS model to be unbiased is $E(\ln(Y)|X) = X'\beta$. In both models, the coefficients on the regressors can be interpreted as a semi-elasticity, or the percentage shift in Y which follows from a one unit change in the regressor. One limitation of the Poisson model is, when estimated via maximum likelihood, the likelihood function assumes $E(Y|X) = V(Y|X)$. We relax this assumption utilizing robust standard errors, which are robust to over-dispersion or under-dispersion occurring when the conditional mean and variance are not equal.[22]

While the randomization of dosage was intended to balance high and low dosage developments with respect to all possible confounding variables, randomization is not guaranteed to work perfectly in small samples. Accordingly, in our econometric models, we statistically control for prior indoor and outdoor crime rates (felony, misdemeanor, index crime, and assault, homicide, and weapons complaints), on- and off-campus, and at night, the square footage of the development, the percent of the male population aged 15 to 24, and natural log of the population overall. We constrain the coefficient for the log of the population to be 1. This is similar to a linear regression with $\ln(\frac{Crime}{population})$ as the dependent variable, but allows for cases in which the number of crimes is zero. This happens more frequently when we aggregate to the monthly or daily level rather than the entire six-month study period, which further motivates the Poisson model.

Note that the results we report below are the product of our "dosage" model, which we found gave us greater statistical power to detect an effect of lighting. While dosage was intentionally varied from the start, we also initially attempted a simple comparison of treatment and control developments, but as we anticipated, this analysis left us with insufficient statistical power to detect differences in average treatment effects using conventional parametric models. This is, in part, a consequence of the fact that treatment dosage was heterogeneous, as randomizing dosage creates treatment effect heterogeneity, which a simple treatment and control comparison ignores. With that in

[22] While negative binomial models are sometimes used because it relaxes the conditional mean and conditional variance assumption, we prefer the Poisson model because the Poisson model exhibits greater robustness. Namely, the Poisson model is a consistent estimator as long as the conditional mean is correctly specified, or $E(\ln(Y)|X) = X'\beta$. Negative binomial models, on the other hand, require both the conditional mean and conditional variance assumptions to hold, and if either assumption is violated, than the estimates will not be consistent.

mind, utilizing the random assignment of dosage offers us the greatest statistical power due to underlying granularity of how dosage was assigned (having been drawn from a uniform distribution).

Results:

Nighttime Crime in Treatment Sites

Table 1 contains estimates for the effect of lighting dosage among the treatment sites on nighttime crime rates. The first through fourth rows, respectively, report estimates of the effect of lighting dosage on index crime, felony crime, assault, homicide, and weapons crime, and misdemeanor crime. We report the impact of lighting across settings by four column groupings: outdoor nighttime crime on-campus, outdoor nighttime crime off-campus but within 750 feet of the NYCHA campus, net outdoor nighttime crime (the sum of outdoor on-campus and off-campus crime), and indoor nighttime crime on-campus. Within each column grouping we report the estimated effect for two distinct measures of lighting dosage. The first is the number of assigned light towers per square city block.[23] The first coefficient can be interpreted as the predicted percentage reduction in crime from adding one additional light tower per square city block to a development (the adjustment by square city block allows for the effect of lighting to differ by development size). The second measure is the natural log of lighting per square city block. Coefficients for that measure can be interpreted as the estimated reduction in crime associated with a 100 percent shift in the number of lights per square city block (i.e., a shift from no additional lights to the average treatment dosage of 1.7 lights per square city block). We translate all of the point coefficients into incident rate ratios so that the model estimates indicate the percentage shift in crime in response to higher dosages of lighting.

As shown in Table 1, higher dosages of lighting are associated with appreciable reductions in outdoor nighttime crime on campus. Specifically, adding one more light per square city block reduces index crime by 48 percent, felony crime by 37 percent, assault, homicide, and weapons crime by 30 percent, and misdemeanor crime by 5 percent. If we considered instead the effect of increasing dosage by 100 percent (or negative of the effect of going from average dosage to no lighting dosage at all), we estimate that this would decrease index, felony, assault, homicide, and weapons, and misdemeanor crime by 81 percent, 69 percent, 61 percent, and 13 percent, respectively. The estimated impacts on crime are statistically significant at conventional levels of significance ($p < 0.05$) for each of our crime groupings, save misdemeanor crimes. The estimates in Table 1 also show modest evidence that is consistent with geographic displacement. The estimated Poisson regression models suggest that outdoor, nighttime assault, homicide, and weapons crimes committed off-campus increased as lighting dosage levels went up. The point estimates imply that adding an additional lighting tower per city block increases the number of outdoor nighttime crimes occurring off campus by 20 percent. Evidence of geographic displacement for other types of crime is weaker.

[23] A square city block is defined as 125,000 square feet, approximately one city block by one city avenue.

When we combine outdoor, nighttime, on-campus crime with those committed off-campus to examine net outdoor crime, we still find compelling evidence that increased lighting dosages reduce net index and felony crime. Adding one additional light tower per square city block reduces net index crime by 15 percent, and reduces net felony crime by 9 percent. The estimates on net assault, homicide, and weapons and net misdemeanor crime are smaller and statistically insignificant.

The estimates from the models also provide some evidence that increased lighting is associated with statistically significant reductions in indoor misdemeanor and assault, homicide, and weapons crime. One additional light per square block is associated with a 14 percent decrease in indoor nighttime misdemeanor crime and a 17 percent decrease in indoor nighttime assault, homicide, and weapons.

In summary, our main models provide compelling evidence that lighting dosage reduces outdoor, nighttime crime in NYCHA. Even accounting for potential geographically displacement, reductions in index and felony crime remain evident. Furthermore, estimates of the impacts on outdoor nighttime crime are precise enough that statistical significance would remain if we performed conservative adjustment to our critical values for multiple hypothesis testing, such as Bonferroni (which would increase our critical value to 3.16 based on testing 32 hypotheses).

Diminishing Marginal Returns

Next we investigate whether the effectiveness of lighting changes according to the number of lights assigned to a location. In particular, it would be reasonable to suppose that there are diminishing marginal returns to lighting – that the effects of lighting begin to decline past some saturation point. In the first table, our models assume that lights per square city block or that the natural log of lighting dosage has a linear effect on the exponential of crime (as we are using count data models). We note that the latter assumes a specific type of diminishing marginal returns, as this model assumes that lights must increase by a given percentage in order to have a constant percentage effect. In other words, increasing dosage from one light per square city block to two lights per square city block would have the same percentage effect as increasing from two lights per square city block to four lights per square city block, as lighting dosage has doubled in each case. The model fits are more favorable for the model using the natural log of dosage, which, in and of itself, may be evidence of diminishing marginal returns to lighting.

In Table 2 we explicitly test for evidence of diminishing marginal returns. All of the models in this table include both assigned lighting per square city block and assigned lighting per square city block *squared* in the same regression. If the estimated effect for on-campus crimes is negative for the coefficient in the linear variable and positive on the coefficient for the quadratic variable, this would be evidence that the estimated effect of a marginal light per square city block *declines* as lighting levels increase. The estimates in Table 2 are largely supportive of this hypothesis, as estimated coefficients for on-campus outdoor nighttime index, felony, and assault, homicide, and weapons crime are negative for the linear variable, and positive for the quadratic variable (both are statistically significant). While not always significant, the estimates on net crime are

negative for the linear measure and positive the quadratic, also consistent with diminishing marginal returns.

Daytime Crime in Treatment Sites

When examining outdoor nighttime crime, we see evidence of geographic displacement (from on-campus to off-campus). It is also possible that increased lighting could temporally displace crime to daytime hours. In Table 3, we investigate the effect of lighting dosage on daytime crime. The models offer little evidence of any temporal displacement effects on outdoor index, felony, or assault, homicide, and weapons crime. We do find some evidence that misdemeanor off-campus crime increases in the daytime around regions that experienced greater increases in lighting dosage.

Crime in Control Sites

When the dosage of lighting in treatment sites was originally randomized, treatment sites were also matched with a control site that experienced no change in lighting. A central concern in our analysis is whether our estimates are biased due to unobserved factors in regression models. While randomization of dosage should avoid these types of biases, in small samples balance on observables and unobservables may be imperfect, even with randomization. One test of the validity of our findings is to apply to the control site the assigned dosage from the matched treatment site (the original randomized dosage). If we find that outdoor nighttime crime falls significantly in control sites with higher dosages then we would have reason to doubt that increased lighting actually reduces crime, given that these developments did not actually receive lights.

In Tables 4 and 5 we examine the effect of increased lighting dosage on nighttime and daytime crime, respectively, for control sites using the dosage assigned to their matched treatment pairs. In both tables, we find little evidence of statistically significant reductions in crime in control sites. For a few outcomes and specifications, we find some evidence that increased assigned lighting is associated with increases in crime among the control regions. This may suggest that the estimated effects on the outcomes in treatment sites could be viewed as lower bounds (if compared relative to the control group). However, the occasional significant estimates among the control sites could also be the result of conducting multiple hypothesis tests. Indeed, in conducting 32 hypothesis tests we would expect on average two tests to be significant at the 95 percent level, and the Bonferonni adjustment would suggest a more conservative threshold of 3.16 would be warranted. If we utilize this correction in our hypothesis testing, our main conclusion that increased light dosage is associated with less nighttime felony and index crime remain, while potential crime increases in control sites would be insignificant.

Robustness Tests:

We employ several additional robustness checks to confirm the validity of our estimates. In Figure 1, we plot a histogram of estimates generated estimating a series of models that randomly select some number of our potential set of additional control variables to include in the model (varying from only 1 control to 3 of the controls). The distribution of

estimates shown in Panels A - D suggest that, regardless of the subset of controls we might choose, net index and felony crime decrease in response to increases in lighting dosage. The models for assault, homicide, and weapons and misdemeanor crime are more sensitive, with estimates ranging from positive to negative. The range of estimates generated when choosing a random subset of controls provides strong evidence that the model specification does not influence our conclusion that the effect of lighting dosage on net reduced index and felony crimes and that our main estimates have not been selectively chosen from the distribution of potential estimates.

In Figure 2, we consider the role that policing policies enacted both before and after the introduction of additional lights may have had in reducing crime among the developments receiving additional lighting. For both 2015 and 2016 we included counts of home visits, vertical patrols, crew takedowns, any presence of a neighborhood coordinating officer (NCO) program, and the fraction of months an NCO program was in operation from March through September. In order to consider police enforcement as exogenous, we condition upon home visits and verticals in 2015. The distribution of the resulting estimates are provided in Panels A - D. The distribution of estimates emerging using random subsets of the police controls echo the conclusions from Figure 1. In many ways, if anything, this histogram offers even stronger evidence that increased lighting is associated with crime reductions, as the range of estimates are all negative, even for net outdoor assaults, weapons and homicides. We replicate the Figure 2 analyses in Figure 3. The only change is that Figure 3 considers models which additionally include 2016 home visits and vertical patrols. Overall, the range of estimates the models provide are not sensitive to controlling for lagged or current police activity.

In Figure 4, we investigate the sensitivity of displacement to the radius considered outside of the NYCHA grounds (“displacement”). In Panels A through D, respectively, we consider index, felony, assault, homicide, and weapons, and misdemeanor crime. To allow for comparability across different radii, which have different base crime rates, we rescale the estimated coefficients by the base rate to represent the count of crimes displaced. In the figure, the center dot represents the point estimate and the “whiskers” represent the estimated confidence interval. Based on Figure 4A, we find little evidence that index crimes were displaced off-campus during nighttime hours. In Figure 4B, we observe that, at some small and some large radii, we find evidence of felony crime displacement, although the statistical significance oscillates. For assault, homicide, and weapons crime in Figure 4C, we find that, at very small radii, there is slight evidence that lighting dosage displaces assault, homicide, and weapons crime, though it is not statistically significant. At medium radii this effect dissipates, but at larger radii it returns and is statistically significant. Finally, in Figure 4D the estimated models provide little evidence of misdemeanor crime displacement at smaller radii, but estimated displacement effects grow as the radius expands.

We next investigate the sensitivity of the estimates to dropping the top sites ranked by dosage in Figure 5. Once again, the center dot represents the point estimate and the “whiskers” represent the estimated confidence interval. In the figure, dropping Rank 0 refers to keeping all observations (as a reference point), dropping Rank 1 refers to dropping the development with the highest dosage level, dropping Rank 2 refers to

dropping the developments with the two highest dosage levels, etc. These models provide consistent evidence that our results are not sensitive qualitatively to dropping through the top eight dosage sites for each of the crime measures. Predictably, as we exclude more observations, the estimates become noisier.

In Figure 6, we examine whether our results change significantly when excluding any of the treatment developments from our analyses. The vertical axis indicates the size of the estimated effect. The dosage rank of the excluded development is on the horizontal axis. The point estimate including all observations is represented by the horizontal red line. The marker size of each point estimate is determined by the population of the development being dropped. Panels A through D, respectively, report the robustness of index, felony, assault, homicide, and weapons, and misdemeanor crime to dropping a single development. Overall the results show remarkable stability. This provides compelling evidence that the estimated relationship between lighting dosage and net outdoor crime is not driven by any particular housing development.

In our final robustness tests, we examine whether the estimated effects of lights vary by the time of year and weather conditions. In Figures 7, 8, and 9, respectively, we examine the estimated effect of average lighting dosage (the negative of the estimated effect of going from average dosage to no lights) scaled to predict the number of crimes averted or displaced in a given two-month bin (this adjusts for underlying differences in the number of crimes typically reported in a given month). We aggregated to two-month bins to maximize power. Figure 7 suggests the direct effect of lights on reducing index, felony, and assault, homicide, and weapons crime are, on average, larger during summer months. Furthermore, Figure 8 suggests the displacement effects for index, felony, and misdemeanor crimes are largest during March and April. We cannot at this time determine if this is because displacement is larger during that time of the year, or because displacement effects are larger closer to when treatment began. In Figure 9, we examine the predicted net impact on crime (on-campus crime + off campus crime). Given that the estimated direct effects on crime were largest during the July to August period, and the displacement effects were larger during the March to April period, it is not surprising that we find the largest estimated net reductions in index and felony crime in the July to August months.

In Table 6, we examine the estimated effect of lights based on underlying weather conditions. For this analysis, we aggregated crime to daily-by-development levels and linked them with daily weather taken from the National Oceanic and Atmospheric Administration. For this model, we estimated Poisson models with controls for day of week, holidays, month-of-year, temperature, and precipitation. We then estimated models separately for days on which it rained and days on which there was no precipitation. Based on these models we found index crime and felony crime largely fell by similar amount regardless of the weather (the estimated reductions were slightly larger on days when there was rain). For assault, homicide, and weapons crimes, the estimated effect of lighting dosage is larger on non-rainy days. Furthermore, the estimated displacement effects are driven almost entirely by non-rainy days. These findings have some intuitive appeal: individuals deterred by the lights from committing crimes on NYCHA are more likely to move to other locations further off-campus when the weather is favorable.

Tables:

Table 1: Nighttime Crime among Treatment Group[24]

	Outdoor Nighttime		Outdoor Nighttime Displaced		Net Outdoor Nighttime		Indoor Nighttime	
	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)
Index Crime	-0.48** (-2.59)	-0.81*** (-5.19)	-0.04 (-0.48)	0.05 (0.19)	-0.15*** (-3.43)	-0.39*** (-3.87)	0.03 (0.27)	0.07 (0.22)
Felony Crime	-0.37** (-2.72)	-0.69*** (-5.43)	0.05 (0.83)	0.27 (1.23)	-0.09* (-1.76)	-0.30*** (-2.69)	0.09 (0.72)	0.11 (0.34)
Assault, homicide, and weapons Crime	-0.30*** (-3.35)	-0.61*** (-5.01)	0.20*** (3.24)	0.98*** (3.73)	-0.04 (-1.16)	-0.12 (-1.59)	-0.17* (-1.75)	-0.44*** (-2.82)
Misdemeanor Crime	-0.05 (-1.04)	-0.13 (-0.91)	0.11* (1.84)	0.49** (2.45)	0.05* (1.84)	0.20* (1.84)	-0.14*** (-3.00)	-0.32*** (-3.32)

Each cell contains as estimate for the effect of lighting dosage on crime. All models are estimated using Poisson count data models while controlling for past crime, the fraction of the population male aged 15 to 24, the square footage of the development, and the natural log of the overall population with that coefficient constrained to be 1. ***, **, and * respectively indicate significance at the 1, 5, and 10 percent level, and the relevant z-statistics are provided in parentheses.

[24] Note that the results reported here differ slightly from those reported earlier (including in the final report, issued on March 7, 2017). The reason for this discrepancy is that crime data for some NYCHA developments is coded to NYCHA development names that have been changed, but not updated in NYPD data. For instance, the outcomes reported here include crimes at Sotomayor Houses that were not previously included because they were coded to the Bronxdale Houses (the former name of the Sotomayor Houses).

Table 2: Diminishing Marginal Returns

	Outdoor Nighttime		Outdoor Nighttime Displaced		Net Outdoor Nighttime		Indoor Nighttime	
	Lights	Lights SQ.	Lights	Lights SQ.	Lights	Lights SQ.	Lights	Lights SQ.
Index Crime	-0.82*** (-4.97)	0.19*** (3.43)	0.39 (1.34)	-0.04* (-1.78)	-0.32** (-2.17)	0.03 (1.45)	-0.15 (-0.56)	0.03 (0.69)
Felony Crime	-0.72*** (-5.62)	0.14*** (4.26)	0.30 (1.22)	-0.03 (-1.17)	-0.37*** (-3.35)	0.05*** (3.12)	-0.34** (-2.00)	0.07*** (2.60)
Assault, homicide, and weapons Crime	-0.58*** (-3.57)	0.08*** (2.67)	0.77*** (2.78)	-0.05** (-2.12)	-0.16* (-1.80)	0.02 (1.49)	-0.52*** (-3.27)	0.09*** (3.00)
Misdemeanor Crime	-0.07 (-.34)	0.00 (0.09)	0.39** (2.24)	-0.03* (-1.73)	0.17 (1.28)	-0.01 (-0.98)	-0.29 (-1.94*)	0.03 (1.34)

Each cell contains as estimate for the effect of lighting dosage on crime. All models are estimated using Poisson count data models while controlling for past crime, the fraction of the population male aged 15 to 24, the square footage of the development, and the natural log of the overall population with that coefficient constrained to be 1. ***, **, and * respectively indicate significance at the 1, 5, and 10 percent level, and the relevant z-statistics are provided in parentheses.

Table 3: Daytime Crime among Treatment Group

	Outdoor Daytime		Outdoor Daytime Displaced		Net Outdoor Daytime		Indoor Daytime	
	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)
Index Crime	-0.13 (-0.86)	-0.36 (-1.35)	0.09 (1.20)	0.23 (0.92)	0.04 (0.90)	0.01 (0.05)	0.06 (0.63)	0.08 (0.26)
Felony Crime	-0.08 (-1.12)	-0.19 (-1.11)	0.09 (1.63)	0.30 (1.42)	0.04 (0.99)	0.07 (0.50)	-0.01 (-0.10)	-0.02 (-0.13)
Assault, homicide, and weapons Crime	-0.10 (-1.39)	-0.10 (-0.57)	0.14 (1.50)	0.32 (0.90)	0.06 (0.89)	0.10 (0.57)	-0.01 (-0.11)	-0.09 (-0.49)
Misdemeanor Crime	0.15*** (3.90)	0.39*** (2.69)	0.18*** (3.52)	0.52** (2.40)	0.19*** (4.98)	0.54*** (2.99)	-0.01 (-0.27)	-0.01 (-0.05)

Each cell contains as estimate for the effect of lighting dosage on crime. All models are estimated using Poisson count data models while controlling for past crime, the fraction of the population male aged 15 to 24, the square footage of the development, and the natural log of the overall population with that coefficient constrained to be 1. ***, **, and * respectively indicate significance at the 1, 5, and 10 percent level, and the relevant z-statistics are provided in parentheses.

Table 4: Nighttime Crime among Control Group

	Outdoor Nighttime		Outdoor Nighttime Displaced		Net Outdoor Nighttime		Indoor Nighttime	
	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)
Index Crime	-0.12 (-1.55)	-0.29 (-1.63)	0.06 (0.95)	0.11 (0.46)	-0.03 (-0.51)	-0.13 (-0.81)	-0.01 (-0.09)	-0.17 (-0.52)
Felony Crime	-0.05 (-1.00)	-0.11 (-0.65)	0.10** (2.05)	0.28 (1.43)	0.01 (0.44)	0.02 (0.16)	-0.09 (-1.15)	-0.30* (-1.73)
Assault, homicide, and weapons Crime	0.05 (0.65)	0.10 (0.38)	0.13** (2.42)	0.46** (2.02)	0.07 (1.63)	0.16 (0.93)	0.00 (-0.03)	-0.06 (-0.35)
Misdemeanor Crime	0.03 (0.54)	0.06 (0.37)	0.05 (0.93)	0.13 (0.76)	0.02 (0.52)	0.04 (0.29)	-0.03 (-0.81)	-0.08 (-0.57)

Each cell contains as estimate for the effect of lighting dosage on crime. All models are estimated using Poisson count data models while controlling for past crime, the fraction of the population male aged 15 to 24, the square footage of the development, and the natural log of the overall population with that coefficient constrained to be 1. ***, **, and * respectively indicate significance at the 1, 5, and 10 percent level, and the relevant z-statistics are provided in parentheses.

Table 5: Daytime Crime among Control Group

	Outdoor Daytime		Outdoor Daytime Displaced		Net Outdoor Daytime		Indoor Daytime	
	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)	Lights Per sq. Block	Ln(Lights per sq. Block)
Index Crime	-0.02 (-0.16)	-0.09 (-0.35)	0.08 (1.45)	0.32* (1.69)	0.01 (0.24)	0.04 (0.42)	-0.02 (-0.46)	-0.05 (-0.27)
Felony Crime	0.02 (0.20)	0.06 (0.23)	0.11** (2.52)	0.42** (2.47)	0.04** (2.21)	0.16** (2.45)	-0.02 (-0.67)	-0.05 (-0.48)
Assault, homicide, and weapons Crime	0.13 (1.63)	0.45 (1.35)	0.09 (1.05)	0.26 (0.89)	0.08 (1.34)	0.25 (1.16)	0.02 (0.42)	0.07 (0.46)
Misdemeanor Crime	-0.01 (-0.35)	0.00 (-0.02)	0.03 (0.48)	0.10 (0.67)	-0.03 (-0.85)	-0.06 (-0.58)	0.05 (0.91)	0.22 (1.18)

Each cell contains as estimate for the effect of lighting dosage on crime. All models are estimated using Poisson count data models while controlling for past crime, the fraction of the population male aged 15 to 24, the square footage of the development, and the natural log of the overall population with that coefficient constrained to be 1. ***, **, and * respectively indicate significance at the 1, 5, and 10 percent level, and the relevant z-statistics are provided in parentheses.

Table 6: Nighttime Crime among Treatment Group, Weather Conditions, Days

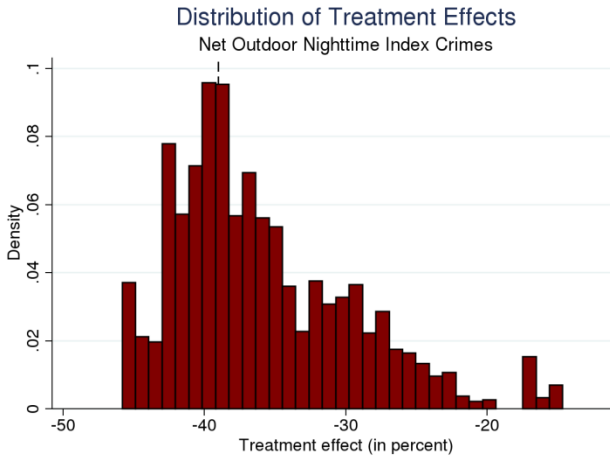
	Outdoor Nighttime		Outdoor Nighttime Displaced		Net Outdoor Nighttime		Indoor Nighttime	
	Non-Rainy	Rainy days	Non-Rainy	Rainy days	Non-Rainy	Rainy days	Non-Rainy	Rainy days
Index Crime	-0.73*** (-2.73)	-0.89*** (-3.92)	0.18 (0.70)	-0.22 (-0.62)	-0.13 (-0.66)	-0.60*** (-4.27)	0.05 (0.16)	0.05 (0.13)
Felony Crime	-0.69*** (-3.67)	-0.70*** (-4.09)	0.26 (1.07)	0.19 (0.72)	-0.26* (-1.78)	-0.31*** (-2.73)	0.21 (0.72)	0.03 (0.07)
Assault, homicide, and weapons Crime	-0.69*** (-5.45)	-0.46** (-2.07)	1.37*** (3.45)	0.39 (1.34)	-0.15 (-1.43)	-0.07 (-0.49)	-0.36 (-1.66)	-0.50*** (-2.64)
Misdemeanor Crime	-0.29 (-1.31)	0.11 (0.50)	1.01*** (2.88)	0.03 (0.14)	0.30 (1.53)	0.07 (0.52)	-0.32* (-1.92)	-0.34*** (-2.72)

Each cell contains as estimate for the effect of lighting dosage on crime. All models are estimated using Poisson count data models while controlling for past crime, the fraction of the population male aged 15 to 24, the square footage of the development, and the natural log of the overall population with that coefficient constrained to be 1. ***, **, and * respectively indicate significance at the 1, 5, and 10 percent level, and the relevant z-statistics are provided in parentheses.

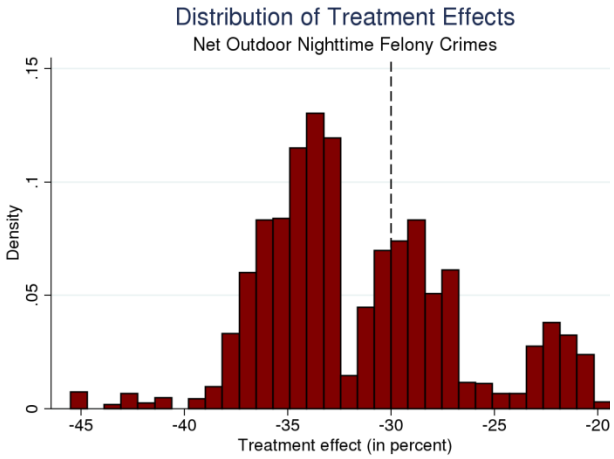
Figures

Figure 1

Panel A



Panel B



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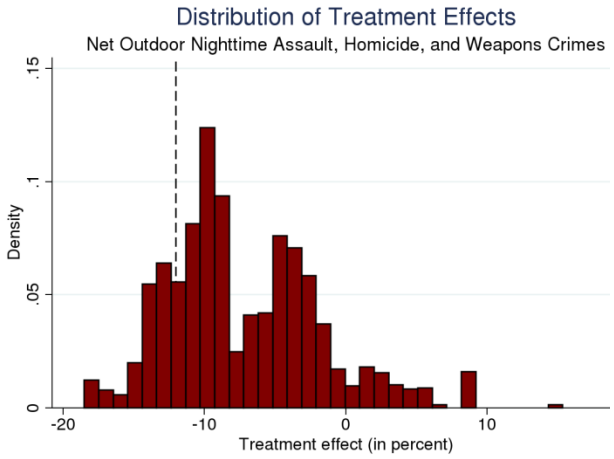


Panel D

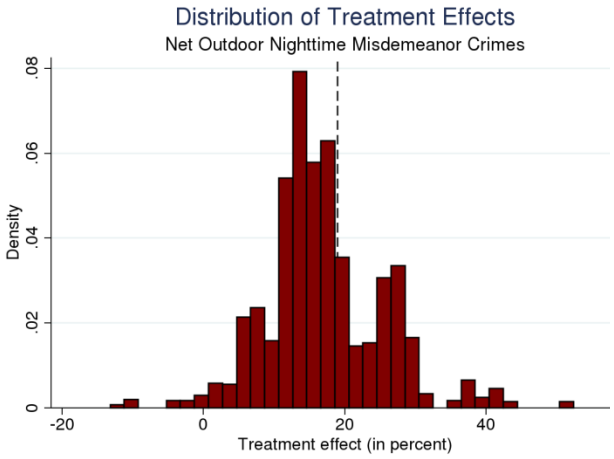
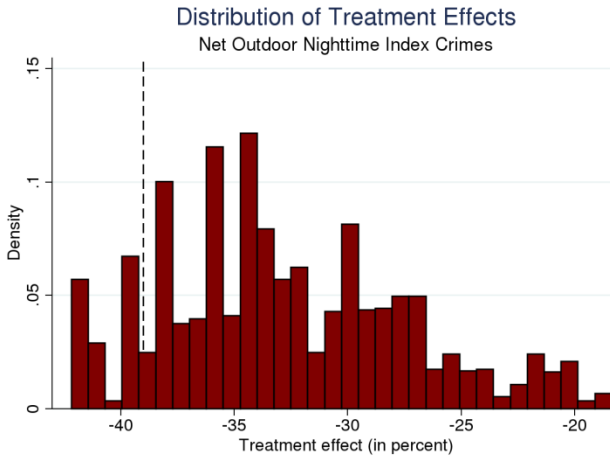


Figure 2

Panel A



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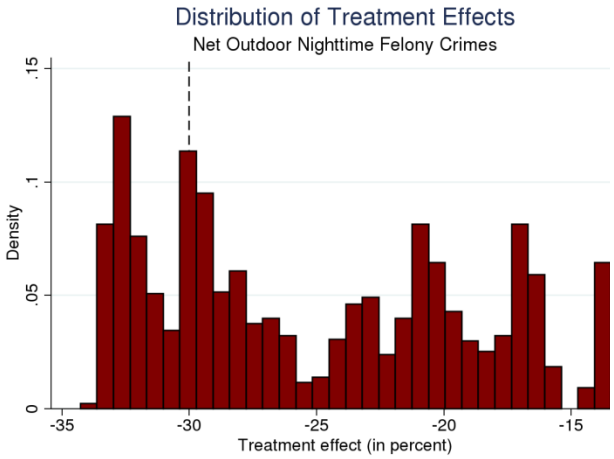

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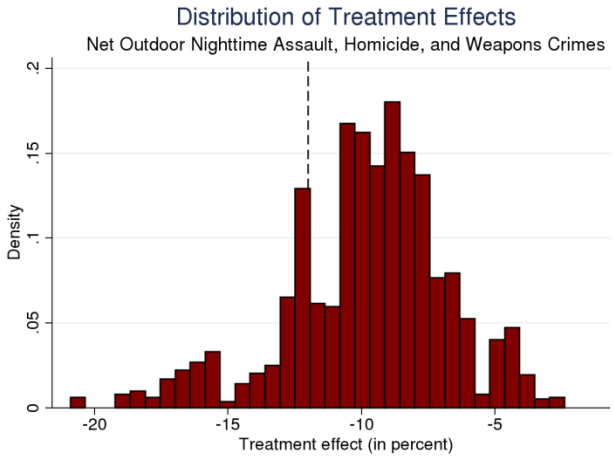

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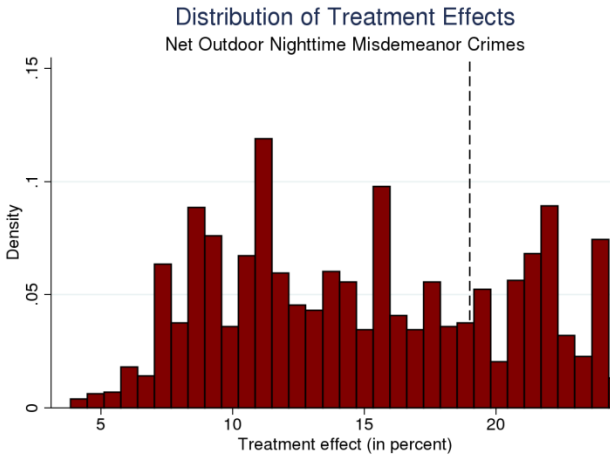

Figure 3

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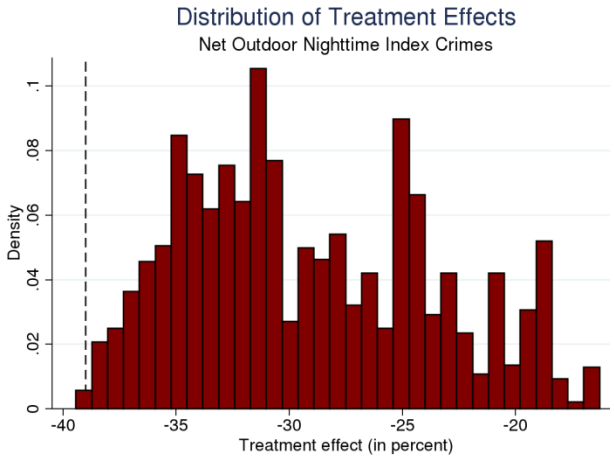

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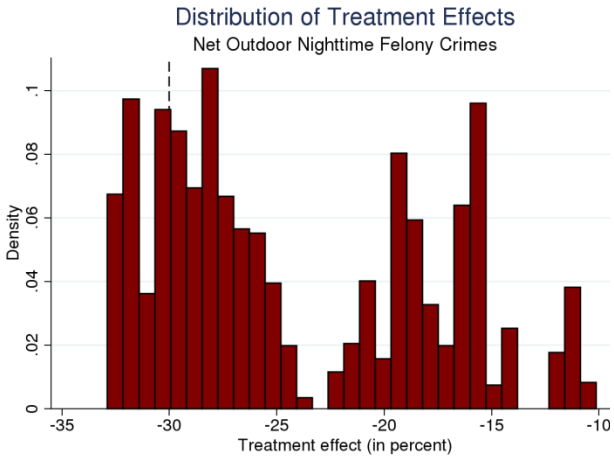

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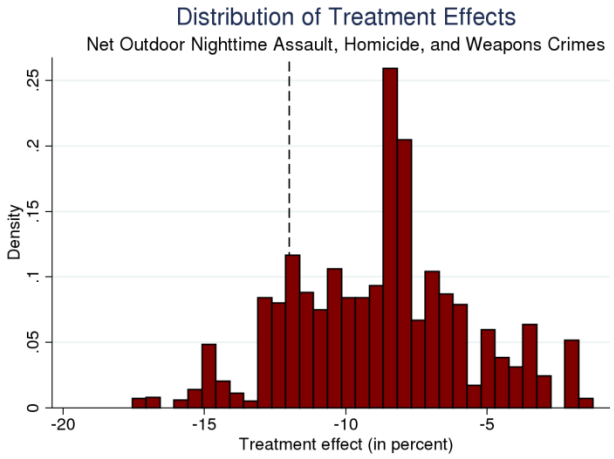


Panel D

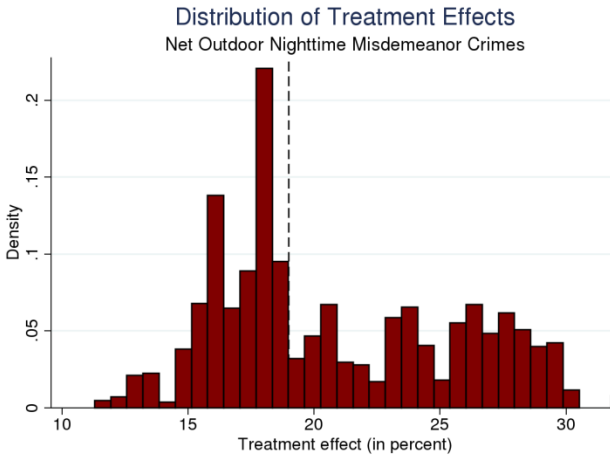
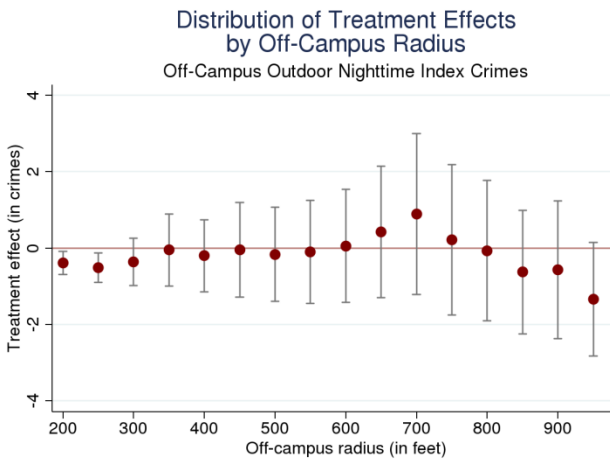
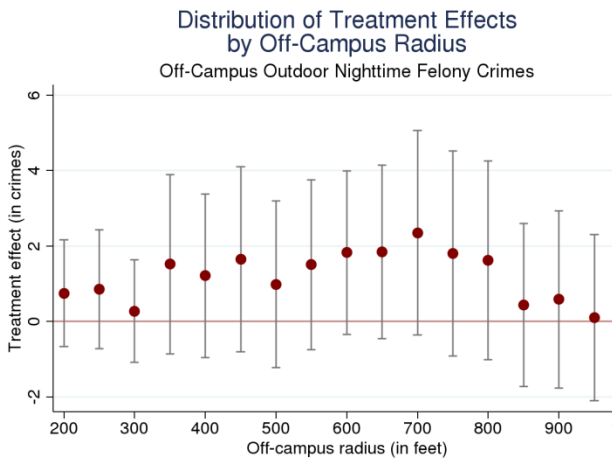


Figure 4

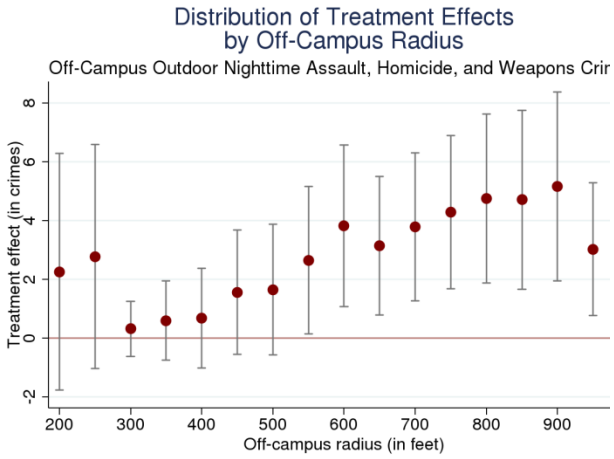
Panel A



Panel B



Panel C



Panel D

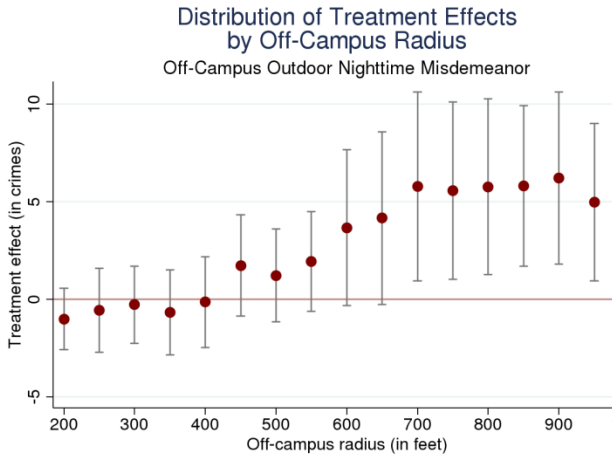
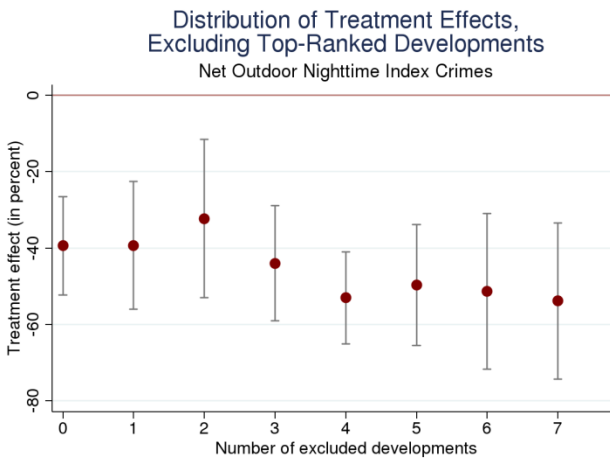
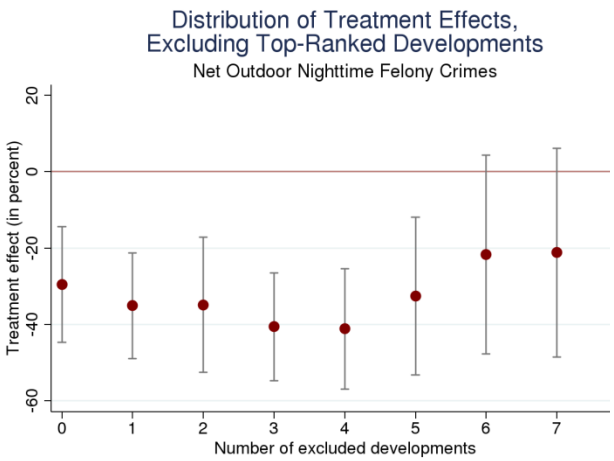


Figure 5

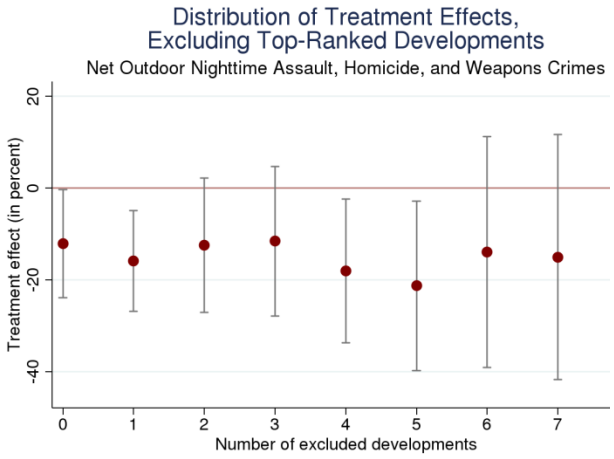
Panel A



Panel B



Panel C



Panel D

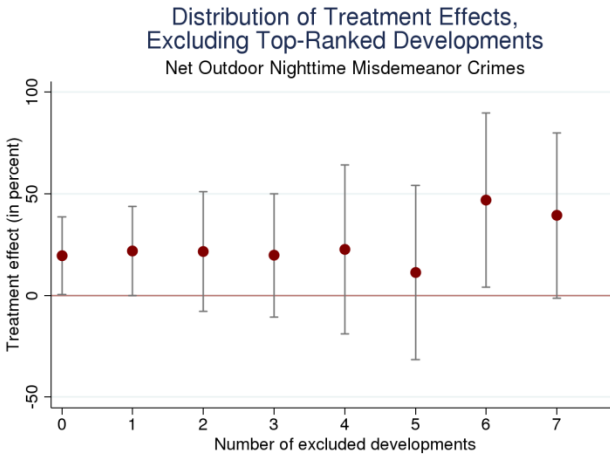
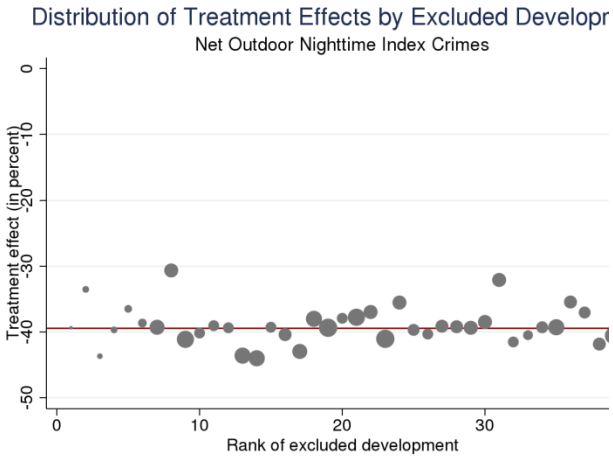
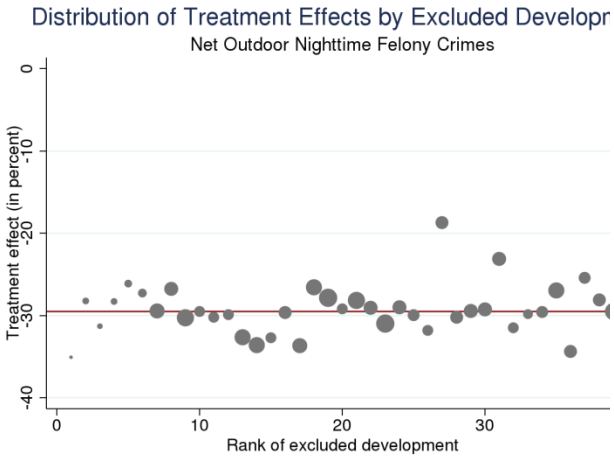


Figure 6

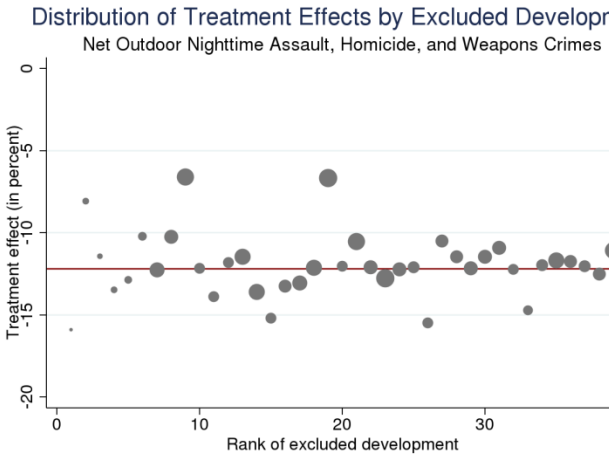
Panel A



Panel B



Panel C



Panel D

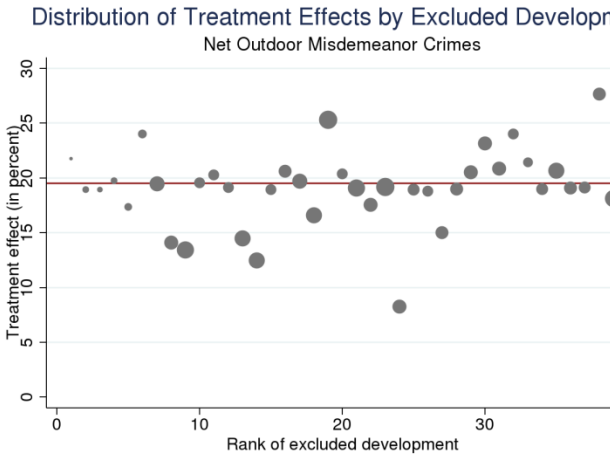
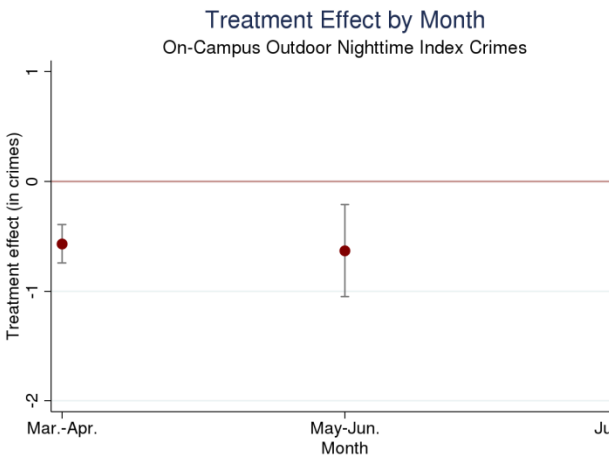
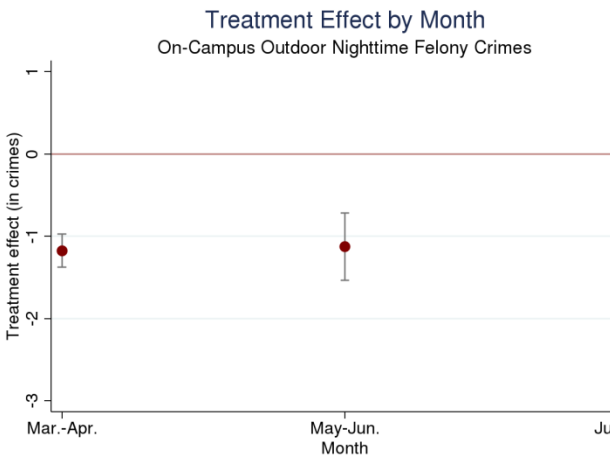


Figure 7

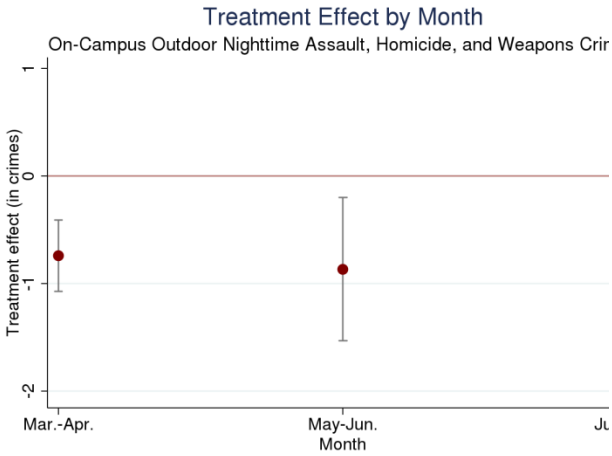
Panel A



Panel B



Panel C



Panel D

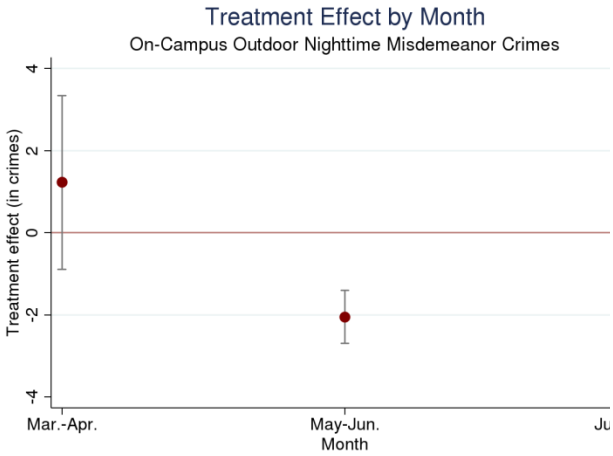
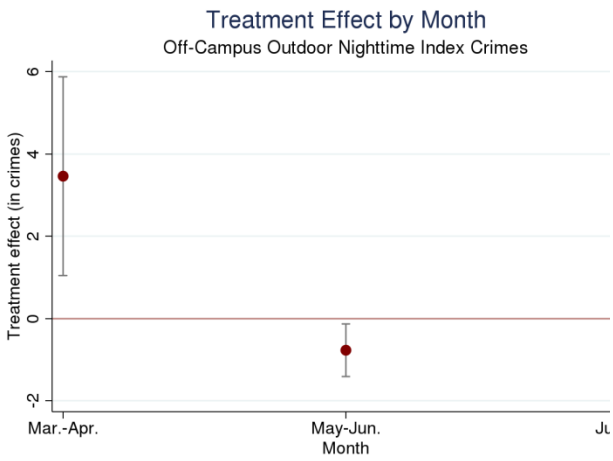
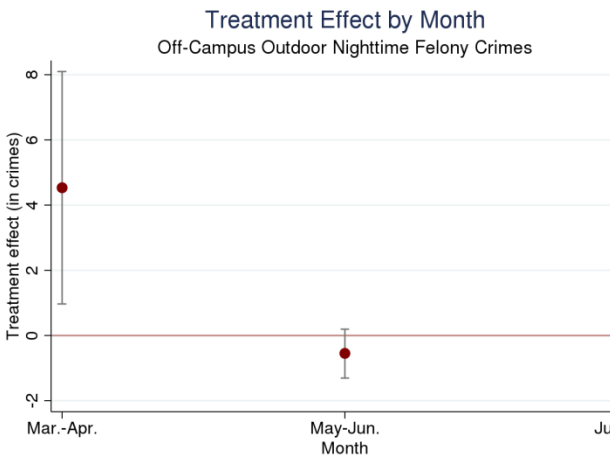


Figure 8

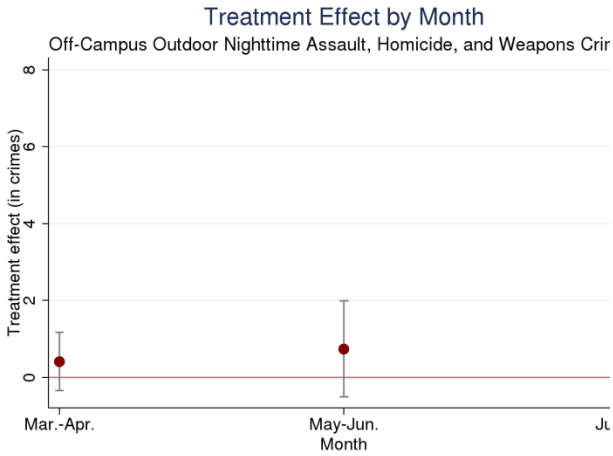
Panel A



Panel B



Panel C



Panel D

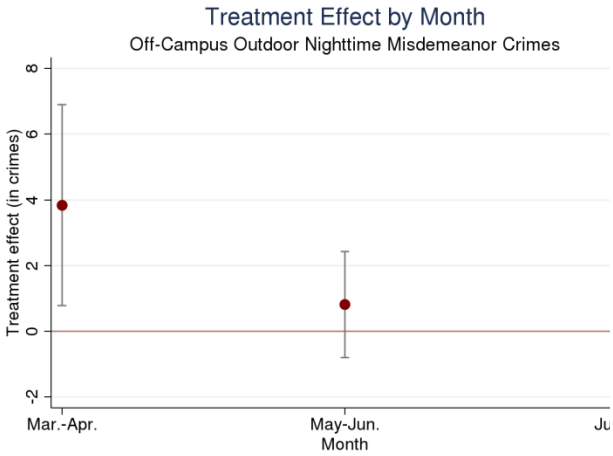
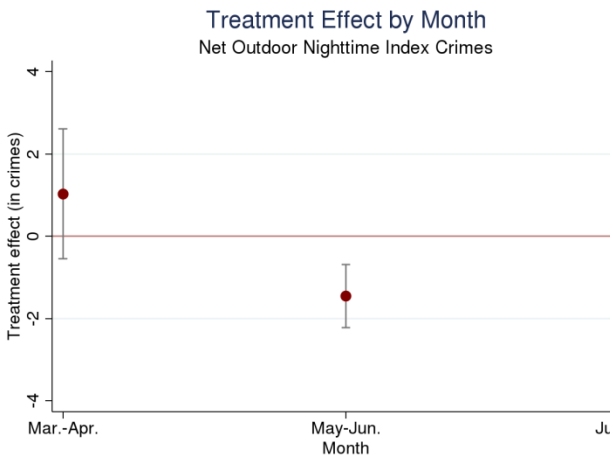
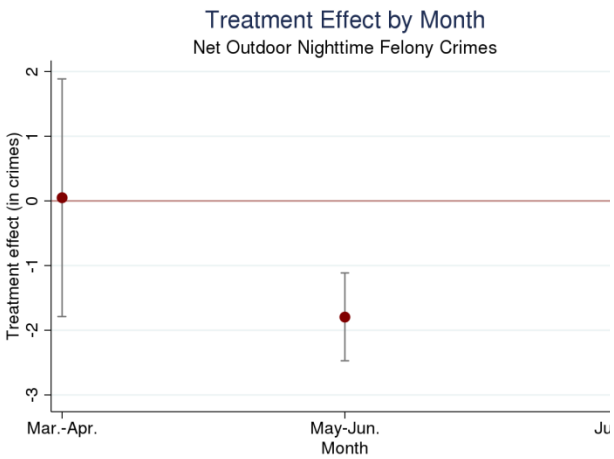


Figure 9

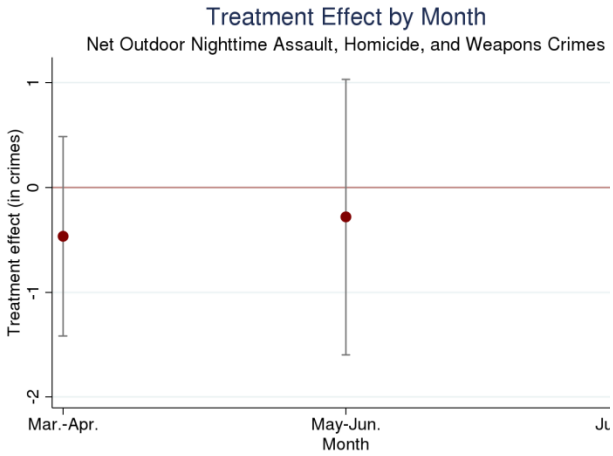
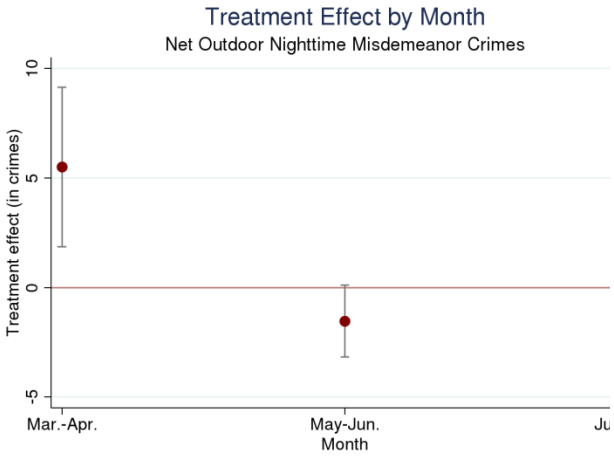
Panel A



Panel B



Panel C



Panel D