

The Effect of Privately Provided Police Services on Crime: Evidence from a Geographic  
Regression Discontinuity Design

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**Summary**

Research demonstrates that police reduce crime. A literature in criminology and economics has attempted to document the effect of extra police on crime using field experiments and natural experiments. We study this question by using a natural experiment based on geography, where a patrol boundary of a private university police department was created that added extra police to city blocks that are adjacent to the university. The assignment of extra police to this area was made without consideration to the population in the adjacent blocks or other covariates of crime. We capitalize on the discontinuity in police force size at the geographic boundary to estimate the effect of the extra police on crime. We find that the extra police provided by the university generated approximately 45-60 percent fewer crimes in adjacent city blocks. These estimates are also similar to those found in other natural experiments that do not rely on geographic discontinuity designs. This paper demonstrates the utility of geographic discontinuities for estimating the effects of social policy decisions on a variety of outcomes.

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## 1. Introduction

Policing accounts for a substantial portion of local government budgets. Local, state, and federal governments spent a total of \$104 billion on police services in 2007, at a cost of approximately \$344 per capita (Kyckelhahn, 2011). Crime reduction is the primary social welfare benefit associated with police. Skepticism among prominent legal scholars and criminologists about the capacity of police to reduce crime (Bayley, 1994; Gottfredson and Hirschi, 1990; Harcourt, 2001) has not withstood empirical scrutiny. A number of studies indicate that crime drops when police are vigilant (MacDonald, 2002; Sampson and Cohen, 1988) and when there are more of them (Evans and Owens, 2007; Levitt, 2002; Zhao, Scheider, and Thurman, 2002). This drop is largest in areas targeted directly by police activities, but it is also observed in surrounding areas as well, suggesting that police are not just chasing crime around the corner (Berk and MacDonald, 2010; Di Tella and Schargrodsy, 2004; Draca, Machin, and Witt, 2011; Klick and Tabarrok, 2005; Weisburd et al., 2006). Large police effects have led several scholars of crime prevention to argue that investing in the police is more sensible than spending on prisons (Cook, Ludwig, and McCrary, 2011; Durlauf and Nagin, 2011).

Despite an abundance of scholarly work on the effects of extra police services on crime, two significant gaps in the empirical literature remain. First, the most methodologically rigorous studies have focused natural experiments that rely on exogenous changes in police strength due to terror events, terror warnings, short-term “crackdowns,”<sup>1</sup> or field experiments that briefly manipulate the level of police in specific geographic areas. As a result, these studies focus on episodic and temporary deployments of extra police that are the exception to normal practices. Less is known about the effect of increasing police deployment over *sustained* periods of time. A sustained investment of additional police deployed to small geographic areas may exert a lasting effect on crime if there is a noticeable difference to criminals that suggests they are more likely to be caught committing crime.

Second, relatively little is known about the effect of supplemental police services provided by private entities. This observation is surprising given that private security officers

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<sup>1</sup> Sherman (1990) notes that crime reductions continue to occur temporarily after the police have left a location following a crackdown, a term he refers to as “residual deterrence.”

outnumber public police officers in the United States.<sup>2</sup> Economists have explored the effect of certain forms of private investment on crime (Ayres and Levitt, 1998; Clotfelter, 1977; Cook and MacDonald, 2011a), but few studies examine privately funded police agencies.<sup>3</sup> The role of private police departments in crime reduction is especially pertinent for colleges and universities that are located in high-crime neighborhoods and that are concerned about the safety of employees and students.<sup>4</sup>

Clearly, examining the long-term investment of additional police in geographic areas is important for understanding the effect of police on crime and the benefits that private entities may receive from providing additional police services. The purpose of this paper is to estimate the long-term effect of supplemental police services provided by a private university on crime. To do so, we use a geographic regression discontinuity design that was proposed by Hahn, Todd, and Van der Klaauw (2001) as a potential broad application in research where “geographic boundaries or rules governing programs often create discontinuities in the treatment assignment mechanism that can be exploited” (p. 201). Rao et al. (2011) use this method to estimate how Walmart locates new stores in the face of state labor laws. Keele et al. (2014) provide a more recent example of different methods of estimating geographic discontinuities for their estimates of the effect of ballot initiatives of voter turnout. We follow the identifying approach that Hahn, Todd, and Van der Klaauw (2001) outline and rely on a nonparametric model of the geographic discontinuity that provides more flexibility in functional form restrictions to estimate the effect of extra police on crime. We compared our estimate from this model to those generated from permutation tests where the geographic concentration of crime is independent of the police boundary. Our results indicate that the additional privately funded police substantially reduced crime and generated benefits that exceed direct operating costs of paying for extra police. The estimates are even larger when we use an alternative nonparametric estimation method outlined by Imbens and Kalyanaraman (2012).

The remainder of the paper proceeds as follows. Section 2 provides a review of the literature on the effect of police on crime, how private entities supplement public police services,

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<sup>2</sup> In 2007, for example, roughly 950,000 private security officers and only 800,000 public police officers were employed in the United States (Bureau of Labor Statistics, National Employment and Wage estimates, 2007).

<sup>3</sup> Quasi-experimental studies of business improvement districts provide one potential source of insight on the effect of private security. These studies suggest that hiring private security may reduce crime (Brooks, 2008; Cook and MacDonald, 2011b)

<sup>4</sup> Cook and MacDonald (2011a) note the Economic Census shows that in 2007 \$40 billion was spent on employees in private security compared to \$99 on public police in 2006 (p. 335).

and the case study of University of Pennsylvania's investment in extra police in University City, Philadelphia. Section 3 provides a description of the data, the geographic regression discontinuity design, results, elasticity estimates. Section 4 provides our concluding remarks.

## 2. Background

### 2.1. The Effect of Police on Crime

Police scholars often claim that the number of police officers on the ground matters less than the activities they perform (Sampson and Cohen, 1988; Sherman and Weisburd, 1995; Wilson and Boland, 1978). Nevertheless, prior work has shown that the raw number of deployed police officers - the proverbial "boots on the ground"- does matter. Levitt (2002) used the hiring of firemen as an instrument for police hiring, and found that additional police officers significantly decrease violent and property crimes. Evans and Owens (2007) examined crime rates in cities before and after they received federal grants to hire additional police officers. They found that hiring officers is associated with reductions in crime of 2% to 5%.<sup>5</sup> Although the effect size of these estimates is small, they represent average effects for *entire cities* and not just the specific neighborhoods where the additional police were deployed.

Quasi-experimental research on the effect of police activities in specific geographic areas provides more persuasive evidence that police reduce crime.<sup>6</sup> Cohen and Ludwig (2003) exploit 25 to 50 % variations in patrol intensity by day of week (Wednesday through Saturday) in two neighborhoods of Pittsburgh to conduct a difference-in-difference-in-differences design. The authors find that days with increased patrols experienced 34% and 71% decreases in average gun shots fired and assault-related gun injuries. Klick and Tabarrok (2005) use daily changes in the terror alert level in Washington, DC to estimate the effect of increased deployment. The authors find that increased police presence caused a 6.6% drop in total crime. Even stronger effects were

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<sup>5</sup> Similar results are reported by Zhao, Scheider, and Thurman (2002) in examining a reduced-form model of the effects of COPS grants on crime.

<sup>6</sup> This makes intuitive sense, as research has demonstrated in a number of cities that less than 10% of city street blocks generate more than half of reported crimes and that most crimes occur during specific days of the week (Sherman, Gartin, and Buerger, 1989; Weisburd et al., 2006). The one early exception to this general observation is the Kansas Police Experiment (Kelling et al., 1974) which randomly assigned three levels of police patrol to 15 beats (5 in each group were assigned either no patrol; usual patrol; or 2 to 3 times the usual patrol). Kelling et al. found that additional police on routine patrol had no observable effect on crime. But as the authors note, the study examined the effects of routine patrol and not police manpower or visibility. Subsequent commentators have noted a number of other methodological limitations of the experiment which explain the null findings (see e.g., Sherman and Weisburd (1995)).

observed in the Capitol Hill District where the highest numbers of police officers were deployed during terror alerts. Di Tella and Schargrotsky (2004) use a similar design. Following a terrorist attack on a Jewish center in Buenos Aires in 1994, the police deployed additional patrols to areas adjacent to Jewish institutions. Assuming that the resulting geographic allocation of patrols was exogenous to crime, Di Tella and Schargrotsky compare the rate of car thefts before and after the attack in areas with and without Jewish institutions. After the attack, car thefts dropped by 75% on street blocks with a Jewish institution relative to other street blocks. They find no effect on car thefts just one or two blocks away from Jewish institutions, suggesting a highly localized deterrent. Draca, Machin, and Witt (2011) compare crime rates before and after a terrorist bombing in London, which resulted in the immediate deployment of supplementary police officers to central London. They also examine crime rates after this deployment was lifted. Their difference-in-differences design shows that a 10% increase in police deployment reduced crime in central London by 3 to 4%. Berk and MacDonald (2010) use a similar design to test the effects of a police crackdown in downtown Los Angeles by comparing crime rates in targeted and adjacent areas before and after the deployment. They observe a 30 to 39% relative reduction in crimes associated with the extra police officers.

There are also several field experiments that test the effect of supplementary deployment to crime “hot spots.” Sherman and Weisburd (1995) randomly assigned 55 of the 110 highest crime blocks in Minneapolis, MN to receive a “crackdown-back off” deployment pattern. Police cars spent as much as one extra hour per shift in each of the treatment areas. The study found that treatment areas experienced a 6 to 13% reduction in crime, with the largest effects observed for disorder violations. Other field experiments have also found that targeted police patrols in high crime areas reduce crime and disorder (Braga and Bond, 2008; Braga et al., 1999; Ratcliffe et al., 2011; Weisburd and Green, 1995). Because these studies focus on deployments in highly localized areas for short durations in time, they cannot rule out the possibility that deployment displaces crime to adjacent areas. Little empirical evidence supports the displacement hypothesis (Weisburd et al., 2006), but it is possible that positive local effects are offset by crime shifting to other areas not observed.

Taken together, the empirical literature provides strong evidence that deploying additional officers to narrow geographic areas can substantially reduce crime in those areas. However, existing research designs in this literature rely upon idiosyncratic and short-term

variations in deployment: they use, for example, deployment increases due to terror events, police crackdowns, or temporary field experiments. These designs do not measure the effect of long-term increases in deployment in specific geographic areas. We find this limitation important for at least two reasons. First, some empirical evidence suggests that the effect of crackdowns dissipates over time if they become a permanent policing tactic (Sherman, 1990). And second, the crime reductions associated with crack-downs and terrorism-related deployment may derive from increased officer vigilance or alertness. It is unlikely that a heightened sense of vigilance could be maintained for long periods of time without further exogenous shocks.

## **2.2. Privately Funded Police and the University of Pennsylvania**

The University of Pennsylvania (Penn) maintains the largest privately funded publicly certified police department in the Pennsylvania, employing roughly 100 full-time sworn police officers. The department is the third largest university police department in the United States (Reaves, 2008). The primary role of the University of Pennsylvania Police Department (UPPD) is crime prevention within and around the Penn campus. Figure 1 depicts three key geographic areas to which we refer throughout the remainder of the paper. First, the *Inner Penn Campus*, demarcated by a thick line, refers to campus proper; it includes residential dormitories, classroom buildings, administrative university offices, restaurants and stores. Second, the *Outer Penn Campus*, demarcated by the finer line, encompasses the neighborhood adjacent to the Inner Penn Campus (between 30th and 43rd Streets and between Market Street and Baltimore Avenue). Many students, faculty, and other Philadelphia residents live in this area. Third, the Inner Penn Campus and Outer Penn Campus are both located within the larger *University City District*, which is demarcated by the dotted line in Figure 1. The University City District is a special services district established by the University to “coordinate sanitation, security, and other services and to promote the area’s residential retail assets” (Kromer and Kerman, 2004). UPPD deploys police officers on foot, bike and mobile patrols to both the Inner and Outer Penn Campuses. We refer to this combined area as the Penn Patrol Zone. At any given time, at least sixteen UPPD police officers are on patrol within the Penn Patrol Zone, supplementing the

existing Philadelphia Police Department for the entire University City District.<sup>7</sup> The UPPD does *not* deploy officers to areas of the University City District outside of the Penn Patrol Zone; that area is served exclusively by the Philadelphia Police Department. The University City District pays for 42 private security officers (Public Safety Ambassadors) who patrol throughout the district daily from 10am to 3am, such that the only observable difference in security within the district is the UPPD deployment of uniform police officers.<sup>8</sup>

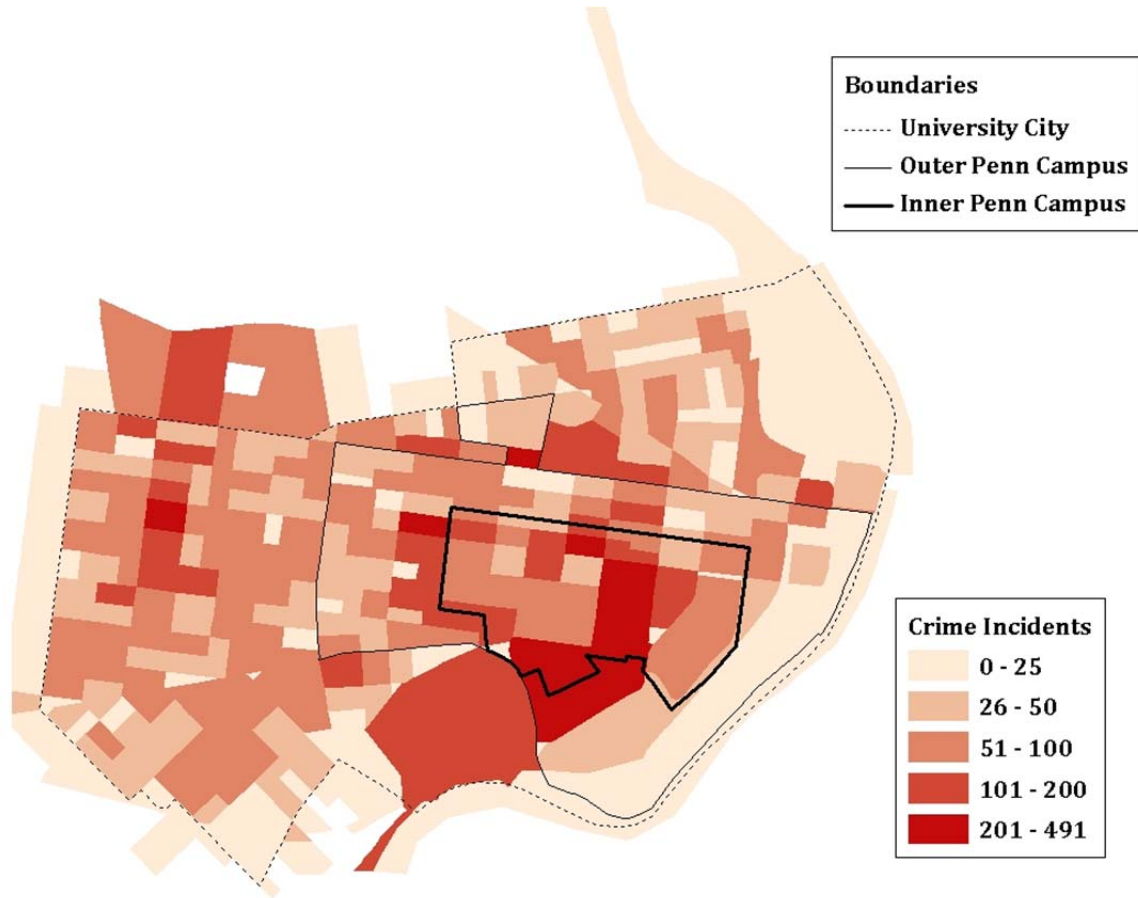
The UPPD is a sizable investment in extra police in the Penn Patrol Zone (Inner and Outer Penn Campus). The entire University City District in which the Penn campus is situated has only six to eight Philadelphia Police Department patrol officers on duty at any given time. In short, Penn is supplying many more officers than would otherwise be deployed in the adjacent neighborhoods. In this study, we exploit the substantial difference in police force size across the Penn Patrol Zone boundary.

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<sup>7</sup> The training for UPPD is similar to the city of Philadelphia Police Department, and a number of officers have served in both agencies. The department also maintains a Detective Bureau responsible for conducting criminal investigations, crime scene management and evidence collection for crimes throughout patrol zone.

<sup>8</sup> <http://universitycity.org/serving-our-community> (retrieved October 4, 2013).

Figure 1: University City District Crime Patterns and Outer Penn Patrol Boundary



### 3. Empirical Analysis

#### 3.1. Data

We obtained detailed data on every crime reported in the University City District between 2005 and 2010. We classify crimes into four categories: total crimes, violent crimes (assaults, murder, rape, and robbery), property crimes (all non-violent offenses), and street crimes (assaults, burglaries, purse snatch, robberies, theft from vehicles). We created the street crime classification because these offenses are more likely to occur in the direct visibility of police patrols.

The crime data includes detailed address locations for 19,611 crimes.<sup>9</sup> Using GIS software, we calculated the distance between the location of each crime and the UPPD

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<sup>9</sup> Crimes reported include aggravated assault, aggravated assault with guns, assault, auto theft, bike theft, burglary, homicide, homicide with gun, purse snatch, retail theft, robbery, robbery with gun, and sex offense, theft, theft from



boundary.<sup>10</sup> We then averaged these distances within each of the 398 blocks in the sample to estimate the distance of each block's crimes from the boundary. For the four blocks where no crimes occurred in the sample period, we used the distance from the block's centroid to the UPPD boundary.<sup>11</sup> Crimes inside the Penn Patrol Zone are assigned a negative distance value and crimes outside the Penn Patrol Zone are assigned a positive value. Crimes on the boundary (e.g., at 40th and Market) are assigned a distance of zero and are, thus, included within the Penn Patrol boundary.

Incident data were then aggregated to the block level.<sup>12</sup> We aggregate without consideration for time (i.e., collapse six years of data into a single cross section) because the source of our identifying variation, the Penn Patrol Zone boundary, did not change during the study period.<sup>13</sup> Of the 398 blocks located with University City District, 148 fall within the UPPD Penn Patrol Zone boundary.

Table 1 shows that during the six-year period for which data are available there were an average of 71 crimes per block in the Inner Penn and 40 in the Outer Penn Campus. The bulk of these crimes were property offenses, especially thefts from buildings. Blocks in the Outer Penn Campus experienced higher rates of street crime, as do blocks located in the University District outside of the Outer campus. This descriptive comparison does not take into consideration that blocks further away from campus are different in many respects (e.g., public housing complexes) from blocks closer to campus. It is, therefore, not plausible to generate causal inference from this simple description of the data.

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building, theft from vehicle. Twenty-three of these cases fall outside of the University City District and are excluded from the analysis, leaving the total count of analyzed crimes to 19,588.

<sup>10</sup> We received the geographic information on the boundaries from the UPPD. See <http://www.publicsafety.upenn.edu/assets/General-Web-Photos/PennPatrolpdf.pdf>.

<sup>11</sup> Our results are qualitatively similar if we use the centroid measurement for all blocks.

<sup>12</sup> For the 50 blocks that lay across both sides of the Penn Patrol boundary, the blocks were split and crime was aggregated to the appropriate side of the patrol boundary for the purpose of our analysis. In other words, these blocks were split and assigned to the share of crime that was on each side of the boundary.

<sup>13</sup> As an alternative specification, we also estimated the model using yearly block-level data and obtained substantively similar findings. Because crime rates dropped overall in the University City District between 2005 and 2010 this provides evidence that the addition of Penn police did not simply displace crime to other parts of the University City District.

Table 1: Descriptive Statistics of Crime by Boundary

Inner Penn Zone					
Variable	N	Mean	SD	Min	Max
All Crime	43	71.8	81.1	1	341
Violent	43	8.9	10.4	0	55
Property	43	62.0	73.3	0	293
Street Crime	43	12.3	11.3	0	50
Distance	42	-1025	291	-1573	-208
Outer Penn Zone					
Variable	N	Mean	SD	Min	Max
All Crime	105	40.2	46.9	1	309
Violent	105	9	11.3	0	47
Property	105	31.2	39.1	0	271
Street Crime	105	17.7	14.9	0	58
Distance	105	-321	345	-1185	0
University City District (excluding Inner and Outer Penn Zone)					
Variable	N	Mean	SD	Min	Max
All Crime	251	49	49.0	1	626
Violent	251	14.9	14.9	0	257
Property	251	34.1	34.1	0	369
Street Crime	251	31.3	31.3	0	410
Distance	251	1424	1424	1	3952

### 3.2. Regression Discontinuity Design

Regression discontinuity designs (RDD) provide the opportunity to draw causal inferences from non-experimental data when appropriately identified. Hahn, Todd, and Van der Klaauw (2001) and Lee (2008) show that the identifying assumptions in a RDD are relatively weak in comparison to other standard approaches in empirical microeconomics. In our context, the primary requirement is the absence of systematic differences relevant to crime between areas just inside and outside the Penn patrol boundary.

The Penn patrol boundary constitutes a clear geographic discontinuity. The boundary is, in many respects, a historical artifact.<sup>14</sup> Since the mid-1990s, Penn has tried to merge the university and the surrounding neighborhood, investing heavily in infrastructure, housing, retail establishments, and primary schools (Kromer and Kerman, 2004; Rodin, 2005). As a result, much of the surrounding neighborhood is populated by Penn students and faculty making it largely indistinguishable from the part of the neighborhood that is within the Penn patrol boundary. Many, if not most, Penn faculty, students, and staff members are unaware of the location of the patrol boundary, highlighting what has become a seamless relationship between Penn and the University City District. Penn also provides many services, such as transit services, well beyond the formal campus boundary.<sup>15</sup> The university also administers a housing subsidy program to induce faculty to live beyond the campus border in the University City District area of West Philadelphia. In addition, Penn provided \$24 million in capital financing in 2001 and a substantial annual student subsidy to create a public elementary school (the Penn Alexander School) in the University City District that has served residents on both sides of the patrol boundary since 2004.<sup>16</sup> Steif (2012) shows that residential homes sales prices climbed at a substantially higher rate in the areas just inside compared to those just outside the elementary school catchment area. A visual depiction of the block faces at the boundary also shows no clear differences in the built environment.<sup>17</sup> In short, there is good prima facie evidence that blocks on either side of the patrol zone are similar in basic socio-demographic characteristics.<sup>18</sup>

Assuming that blocks just inside and outside the Outer Penn Campus Boundary are not systematically different other than the policy decision to add extra police, we can estimate the effect of increased police deployment within the boundary using a regression discontinuity

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<sup>14</sup> Maureen Rush, Vice President for Public Safety at Penn, noted on September 27, 2012 at the Jerry Lee Symposium On Philadelphia Experiments in Crime and Justice that the boundary was chosen as an adequate distance from Penn's campus that the police could patrol without trying to provide extra police services to all of the University City District area.

<sup>15</sup> Close to 6,000 Penn students rent apartments or houses in University City District. The transit service for Penn students and affiliates in University City District extends to 48<sup>th</sup> street (PennBUSWest), which is on the outer edge of the district. Penn Transit Services also offers free door-to-door transportation to Penn affiliates who live in University City between 6pm and 3am (see <http://www.business-services.upenn.edu/offcampusservices/cms/wp-content/uploads/grad-housing-guide-draft-2012.pdf>). Penn off-campus housing services officially lists 24 buildings for students in University City District, of which 10 are located outside of the Penn patrol boundary (see [http://www.business-services.upenn.edu/offcampusservices/?p=graduate\\_guide/individual\\_building\\_profiles](http://www.business-services.upenn.edu/offcampusservices/?p=graduate_guide/individual_building_profiles)).

<sup>16</sup> <http://www.pennalexanderschool.org/content/what-are-boundaries-penn-alexander-school-can-my-child-attend>

<sup>17</sup> Pictures of the block-faces are available on request from authors.

<sup>18</sup> The block-level of aggregation prevents us from assessing the level of similarity as census block groups cross the boundary.

design. In our initial analyses, we exclude the Inner Penn Campus because it is composed of classroom buildings, university office space, dormitories, a hospital, and sports facilities, and as a result, is less comparable to the residential and retail areas that are on either side of the Penn patrol boundary. As shown later, this exclusion does not affect our analysis. Following Lee (2008), we use a polynomial regression specification where we control for the distance from the city block to the campus boundary. Blocks inside the boundary are assigned negative distance values and blocks outside of the boundary are assigned positive values. We allow these control terms to take the form of  $n$ th degree polynomials that differ depending on whether the given city block is inside or outside of the boundary (i.e., each polynomial term is interacted with a dummy variable indicating whether the block is inside the boundary). The regression model takes the following form:

$$Crime_i = \mu + W_i * \alpha + \theta \sum_{k=1}^4 UPPD * d_i^k + \rho \sum_{k=1}^4 UCD * d_i^k + v_i$$

The variable  $W$  is a dummy indicator that assumes value 1 for each block (denoted  $i$ ) inside the patrol zone boundary, and the terms  $\theta$  and  $\rho$  represent quartic degree ( $n=1$  to  $4$ ) polynomial parameters for the inside (UPPD) or outside (UCD) average block distances (denoted by  $d$ ) for crimes on blocks from the boundary. This polynomial specification is flexible, since it is well known in the regression discontinuity literature that the assumption of a constant treatment effect further away from the cutoff is not identifiable (Cook, 2008; Hahn, Todd, and Van der Klaauw, 2001). Robust standard errors are used, though the results are essentially the same with jackknife or bootstrapped standard errors. We focus on this framework for modeling the RDD because it allows for effects to differ at distances on, inside, or outside the boundary (Lee, 2008). However, we used  $k$ -fold cross validation techniques to examine the sensitivity of model performance to this choice, finding little difference in model performance across polynomial functions of orders zero through five. Further, inspection of the estimated treatment coefficients yielded few differences even through a polynomial of order 10. In short, the estimates we present are not sensitive to the functional choice we used to model the RDD.

### 3.3. Results

Table 2 provides results comparing blocks inside and outside the Outer Penn Campus boundary, excluding the Inner Penn Campus. We present results from ordinary least squares

regressions, though the results are effectively the same if estimate crime counts blocks using Poisson, negative binomial regressions, or log of crime. Column (i), which does not control for distance effects, shows that crime is generally lower inside campus. Column (iii) shows that once the polynomial function for distance is included, the model estimates an average of 27 fewer crimes on blocks inside the boundary compared to counterfactual blocks outside the boundary. This effect is statistically significant at the 1 percent level, and represents a reduction of 56 percent.<sup>19</sup>

Table 2: The Effect of Penn Police on All Crimes

	All Crimes		
	(i)	(ii)	(iii)
Penn Campus	-8.22 (5.51)	-21.00*** (6.87)	-27.04*** (9.47)
Distance to Campus Border Effect	None	Linear interacted with Penn and UCD indicators	Quartic interacted with Penn and UCD indicators
Inner-Penn Campus Included	No	No	No
Percentage Change	-17%	-44%	-56%

Note: Results estimated using ordinary least squares regression. Blocks within the inner Penn campus are excluded. Robust standard errors are in parentheses. For all regressions, n = 356.

\*\*\* p < 0.01

\*\* p < 0.05

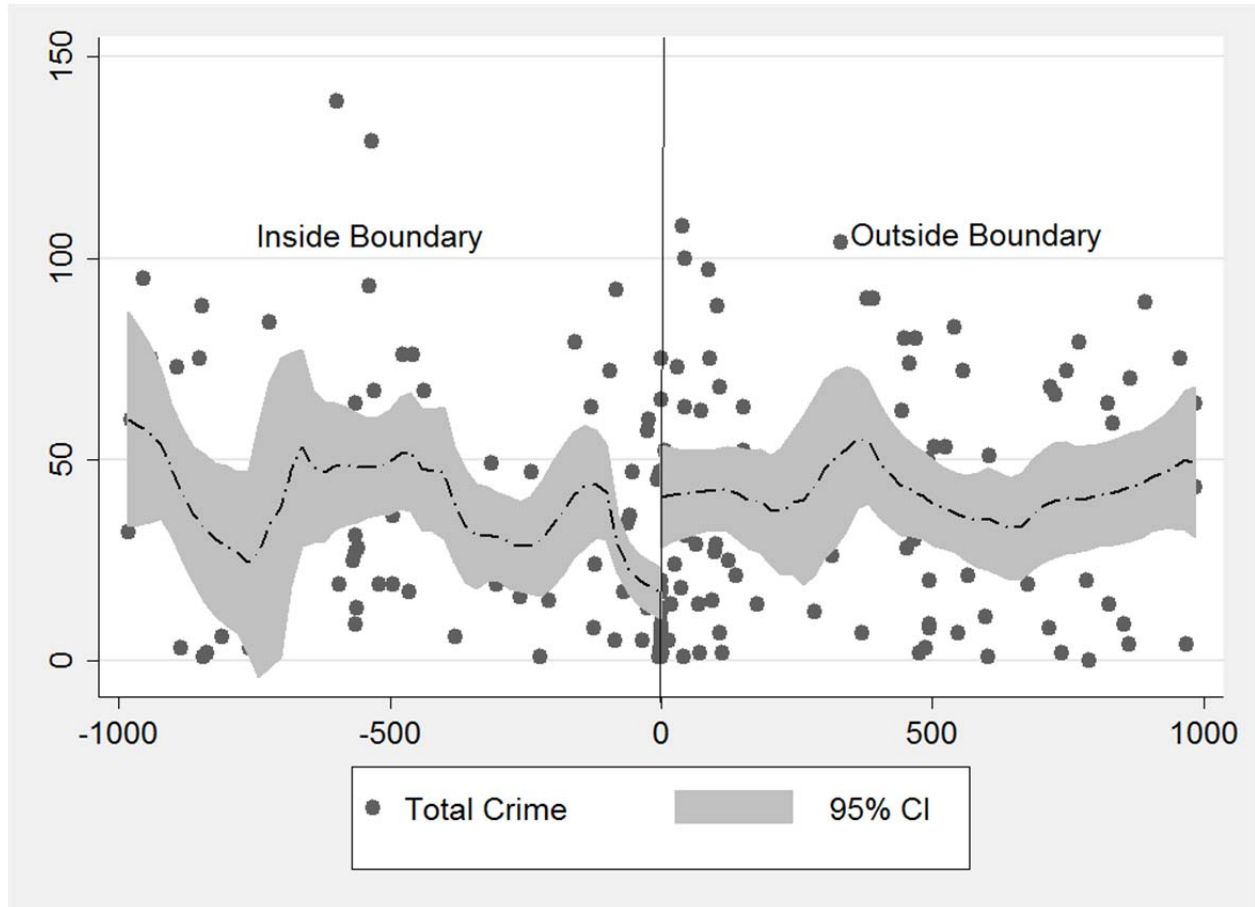
\* p < 0.10

Figure 2 shows the relationship between average block distance to the boundary, and the total number of crimes over the period observation. As is evident from Figure 2, the patterns are non-linear as the distance from the patrol zone boundary increases and there is apparent higher

<sup>19</sup> We don't use the population of residents as a denominator to create a crime rate for two reasons. First, the census data on residential populations cannot be reliably constructed on the block-level. The same census block groups correspond to blocks both inside and outside the boundary. Second, the residential population does not accurately capture the actual population at risk for victimization at this lower level of geography. Penn is a major employer, and the University City District is a shopping and restaurant destination. Therefore, the actual population at risk for victimization is much higher than the residential population. By relying on block-level counts of crime we are being conservative in our estimates, since arguably more people will be available for victimization the closer one gets to Penn's campus.

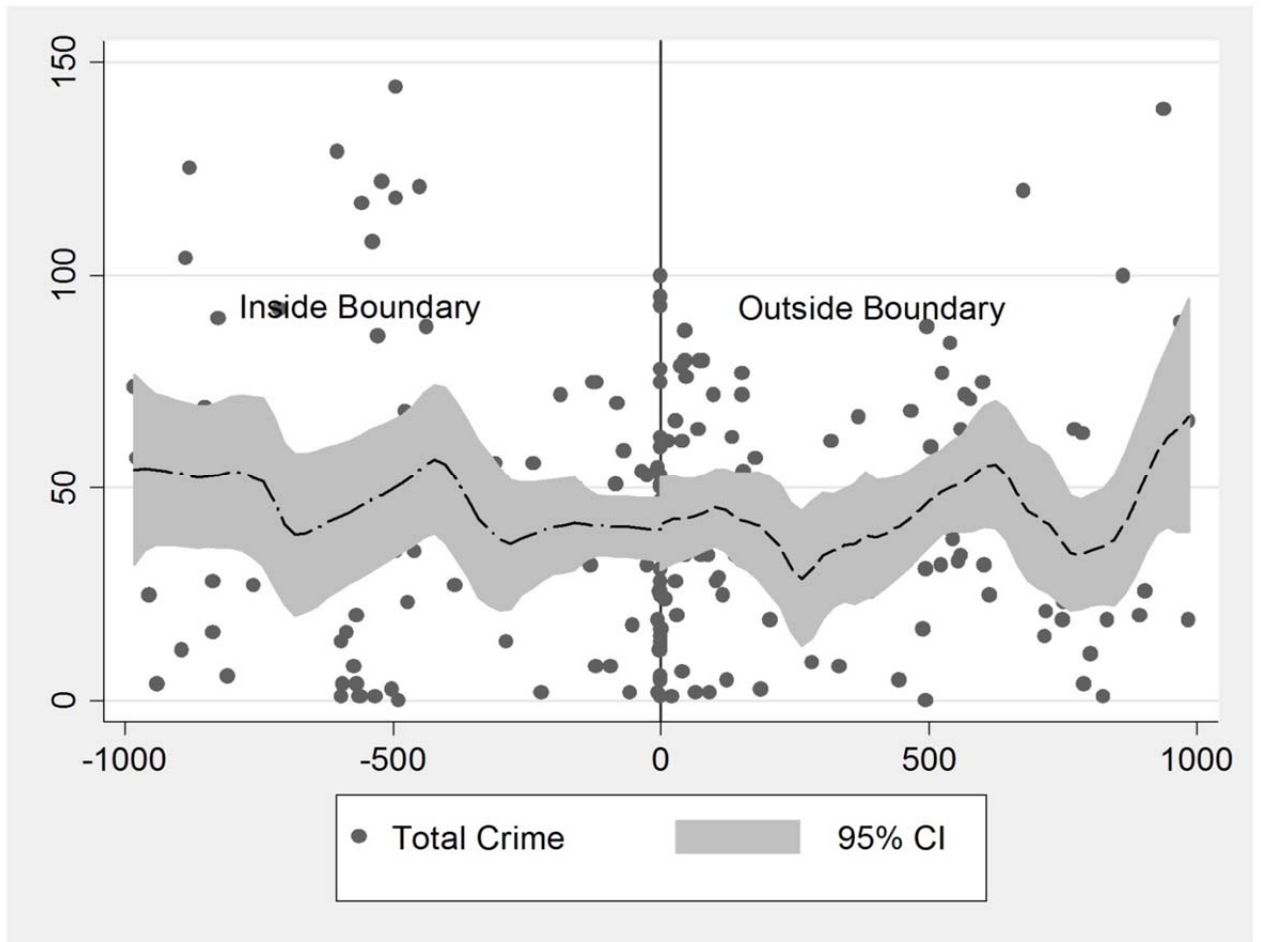
density of crimes at greater distances. The drop in crime at the border is a visual depiction of our point estimate.

Figure 2: Crime by Block Distance from Patrol Boundary



We can also establish a visual baseline to compare under which conditions the average distance from patrol zone boundary does not matter. We constructed such a visual depiction by shuffling block locations and re-plotting the patterns. This makes the block locations completely independent of distances from the patrol zone boundary. Figure 3 presents the result when there is no relationship between average distance to the patrol zone boundary. The pattern is starkly different from that displayed in figure 2. When block distances are randomized we see that there is no longer a drop-off in the crime rates.

Figure 3: Crime with Block Distance from Patrol Boundary Shuffled



In Table 3, we examine the estimates of the effect of Penn’s police deployments separately by crime category. For each category, the estimated coefficient is negative and statistically significant at the 5 percent level or lower. The magnitude of the effects is comparable across crime categories. Violent crime is 60 percent lower, property crime is 55 percent lower, and street crime is 46 lower inside of the Penn patrol boundary.

Table 3: The Effect of Penn Police on Crime by Category

	Violent	Property	Street
Penn Campus	-8.74** (3.59)	-18.31** (7.07)	-14.14*** (3.64)
Distance to Campus	Quartic interacted	Quartic interacted	Quartic interacted

Border Effect	with Penn and UCD indicators	with Penn and UCD indicators	with Penn and UCD indicators
Inner-Penn Campus Included	No	No	No
Percentage Change	-60%	-55%	-46%

Note: Results estimated using ordinary least squares regression. Blocks within the inner Penn campus are excluded. Robust standard errors are in parentheses. For all regressions, n = 356.

\*\*\* p < 0.01

\*\* p < 0.05

\* p < 0.10

Given the nature of a polynomial regression discontinuity, including observations relatively far from the discontinuity point should not influence our estimates if the model is well identified. This issue of appropriate identification, coupled with the institutional knowledge that the Inner Penn Campus is substantively different than the areas around the boundary led us to exclude the 42 blocks in the Inner Penn Campus from our analysis. However, to ensure that our model is well identified, we present results in Table 4 that include the blocks in the Inner Penn Campus. In each crime category, the results are very similar.

Table 4: The Effect of Penn Police on Crime by Category Including Inner Campus

	Violent	Property	Street
Penn Campus	-8.74** (3.57)	-20.16*** (7.07)	-13.73*** (3.64)
Distance to Campus Border Effect	Quartic interacted with Penn and UCD indicators	Quartic interacted with Penn and UCD indicators	Quartic interacted with Penn and UCD indicators
Inner-Penn Campus Included	Yes	Yes	Yes
Percentage Change	-60%	-60%	-45%

Note: Results estimated using ordinary least squares regression. Blocks within the inner Penn campus are included. For all regressions, n = 398. Robust standard errors are in parentheses.

\*\*\* p < 0.01

\*\* p < 0.05

\* p < 0.10



Table 5 provides results for each individual crime type. While there is some variation in the relative effects across specific crimes, the treatment effects are generally negative and substantively important. There are four exceptions. We do not find a negative effect for handgun-homicides, purse snatchings, or thefts from buildings, though none of these positive effects can be distinguished from the null hypothesis of no difference. It is worth noting that handgun-homicides and homicides that are the result of other means are rare events, accounting for only 0.15% (n=31) of all crimes. Purse snatchings are also relatively rare (0.55%; n=55) and occur mostly along central business corridors. Theft from buildings is not a street crime that is easily detectable by police patrols. Table 5 also shows the 95 percent confidence intervals for our estimates shows that the majority of crimes that occur at greater frequency are lower in blocks inside the Penn patrol boundary.

Table 5: The Relative Effect of Penn Police by Specific Crime Type

Crime Label	Coefficient	Lower 5%	Upper 95%	Average Crime	Percent Change
Assault (with gun)	-0.302*	-0.662	0.057	0.674	-45%
Assault	-3.040	-6.986	0.905	4.788	-63%
Theft (Auto)	-1.943***	-3.106	-0.782	3.803	-51%
Theft (bike)	-0.689	-1.885	0.505	1.639	-42%
Burglary	-2.522***	-4.327	-0.718	5.937	-42%
Homicide	-0.036	-0.14	0.066	0.039	-93%
Homicide (Handgun)	0.036	-0.019	0.092	0.035	104%
Theft (Purse Snatching)	0.020	-0.293	0.333	0.274	7%
Theft (Retail)	-4.94**	-9.728	-0.167	1.858	-266%
Robbery	-2.114**	-4.138	-0.091	3.282	-64%
Robbery (Gun)	-1.109**	-1.869	-0.349	2.992	-37%
Sex Assault	-0.497	-1.182	0.187	0.70	-71%
Theft (Other)	-2.417	-6.042	1.207	4.792	-50%
Theft (from Building)	0.362	-4.995	5.719	1.392	-26%
Theft (from Motor Vehicle)	-6.166***	-9.742	-2.59	13.875	-44%
Assault (aggravated)	-1.675**	-2.996	-0.354	2.156	-78%

Note: Results estimated using ordinary least squares regression. Each coefficient is estimated by a separate regression that includes distance to the boundary of Penn's campus as a quartic interacted with Penn and UCD indicators. These regressions are estimated on samples that exclude blocks within the inner Penn campus. Percentage changes are calculated relative to the UCD averages for each crime type and robust standard errors are used. For all regressions, n = 356.

\*\*\* p < 0.01

\*\* p < 0.05

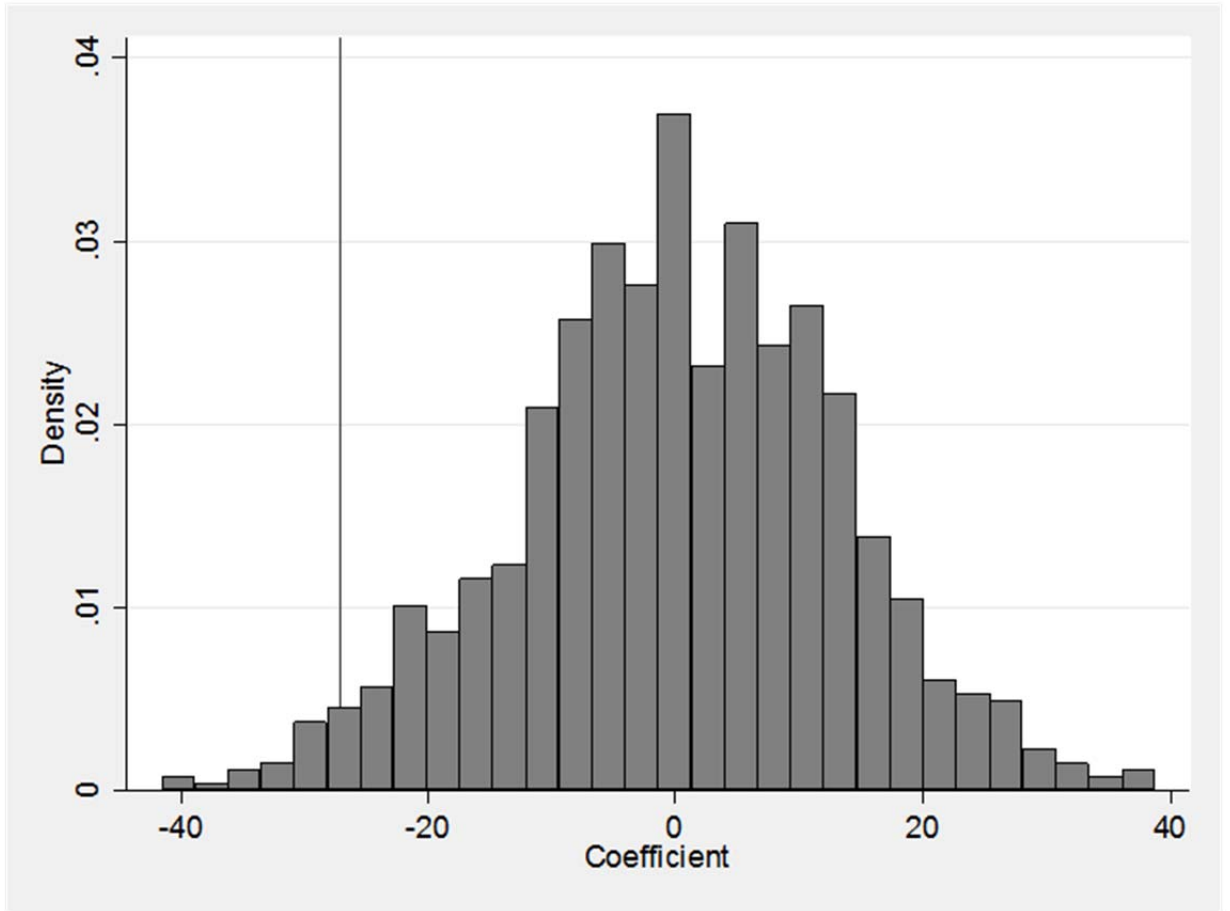
\* p < 0.10

Our results paint a fairly consistent picture: more police protection is associated with less crime. This effect is statistically and practically significant across nearly all crime categories. The results are also largely invariant to the choice of regression specification, and the inclusion of data points farther from the discontinuity.

While we have focused on a polynomial based RDD, local linear approaches provide similar conclusions. Using the approach of Imbens and Kalyanaraman (2012), which optimizes the tradeoff between statistical efficiency and bias in determining threshold bandwidth, we find even larger crime reductions inside the Penn boundary, on the order of 80 percent. This approach, however, adopts a very small bandwidth of less than one city block and incurs relatively large standard errors. We prefer the more conservative estimates detailed above.

Finally, to address the concern that the effects we observe are due to the chance location of the patrol boundary, we simulated 1,000 random reconfigurations (shuffles) of block locations and remodeled the effect of the UPPD patrol zone. Figure 5 provides a graphical depiction of the overall crime estimate from table 2 (column iii) in the vertical line versus the density of estimates obtained from 1,000 shuffled block locations. The estimate of 27 fewer crimes due to the patrol zone is ranked in the bottom 3 percentile of (28 out of 1001) of all estimates for the greatest estimated crime reduction.

Figure 5: Estimate of Penn Police versus One Thousand Shuffled Block Locations



### 3.4. Elasticity Estimates

Personnel estimates from both the Philadelphia Police Department and the UPPD indicate that approximately twice as many officers patrol the Outer Penn Zone than the surrounding University City District. The area covered by Philadelphia Police in the relevant area is twice as large as that covered by the Penn Police, suggesting an effective increase in police presence on the order of 200 percent. Our estimate that UPPD activity is associated with a 60 percent reduction in crime suggests that the elasticity of crime with respect to police is about -0.30 for both violent and property crimes. These elasticity estimates are strikingly similar to those found in the modern literature on police and crime. Chalfin and McCrary (2012)'s recent paper provides a helpful summary of these previous estimates. Klick and Tabarrok (2005), Draca, Machin, and Witt (2011), and Di Tella and Schargrodsky (2004)—all of which use an exogenous shock in police deployment resulting from terrorism-related events—find an elasticity of approximately -0.30. Our results are also similar to those presented in Berk and MacDonald (2010) who examine a police crackdown in Los Angeles and find similar elasticities. The results from our investigation respond to concerns that short-term gains from police crackdowns are not sustainable. Instead, our results suggest that these crackdown studies may be generalizable if increased police presence becomes a permanent tactic in specific areas.

Our results are also consistent with other modern studies that examine the effect of increasing the total number of officers in a city. For example, Evans and Owens (2007) who examine police hiring related to a federal grant program over an eight year period find an elasticity of property crimes of -0.26, though their violent crime elasticity is much higher. Chalfin and McCrary (2012)'s study of crime in US cities over the period 1960-2010, which corrects for measurement error bias in prior studies, finds a violent crime elasticity of -0.35, though their property crime elasticity is only -0.15. As they explain, in the absence of a plausibly exogenous source of variation in their police measures, their results may underestimate the true elasticity. Cities tend to hire more police when they expect crime to increase, and as a result, panel data models tend to underestimate the elasticity of police on crime.

Our results also suggest that privately employed police forces generate reductions in crime that are comparable to those associated with public police forces. While the UPPD employs publicly certified law enforcement officers, their salaries are paid by a private

university. These findings suggest that augmenting public police forces with substantial private investments can have a meaningful effect on crime rates.

Chalfin and McCrary (2012) provide one of the most rigorous estimates of the cost of crime relative to the hiring of police. They find that in a typical U.S. city, investing a dollar in hiring police officers yields an average of \$1.50 in benefits to the public. From these estimates we could assume that the social welfare benefits of crime reduction that comes from paying for UPPD services are a large multiple of the costs of employing the extra police. However, to put the relative costs of police in more concrete terms we estimated the direct crime savings resulting from the additional police officers hired by Penn. The social costs of crime include both direct and indirect costs. We relied on estimates published by Roman (2011) that are based on crime costs estimated from jury awards that have case attributes similar to crimes. Estimates using this approach are displayed in Table 6. We include only the cost savings from crimes that are estimated to be significantly lower due to the UPPD services, as noted in Table 5.

Table 6: Total Estimated Cost Savings from Extra Penn Police on Crime

	Social Cost Savings	5% Lower Bound	95% Upper Bound
Assault (Agg with gun)	0	0	0
Assault	0	0	0
Theft (Auto)	\$31,446	\$12,652	\$50,238
Theft (bike)	0	0	0
Burglary	\$11,950	\$3,402	\$20,500
Homicide	0	0	0
Homicide (Handgun)	0	0	0
Theft (Purse Snatching)	0	0	0
Theft (Retail)	\$2,420	\$82	\$4,760
Robbery	\$629,124	\$27,174	\$1,231,074
Robbery (Gun)	\$330,064	\$103,994	\$556,134
Sex Assault	0	0	0
Theft (Other)	0	0	0
Theft (from Building)	0	0	0
Theft (from Motor Vehicle)	\$6,508	\$2,736	\$10,280

Assault (aggravated)	\$506,856	\$107,216	\$906,496
Total	\$1,518,368	\$257,256	\$2,779,482

Notes: Estimates of incident costs taken from Roman (2011) estimates from jury awards and converted into 2012 dollars. Per incident costs are multiplied by coefficient estimates from Table 5.

The results show the extra investment of police officers in the surrounding area produces a total six year (2005-2011) social-cost savings of \$1,518,368 (range \$257,256 to \$2,779,482) in 2012 dollars. If we assume that only two officers are on average the area of the outer Penn boundary or block areas where crime is affected, this would translate into a total six year cost of \$751,800 (in 2012 dollars). This suggests that the social benefit of crime reduction is approximately two times larger than the costs of an extra police officer.<sup>20</sup> If we were to assume that the fully loaded costs of a police officer were \$100,000 per year because of overtime pay and salary differentials with promotions, this would still translate into a social cost benefit of \$1.26 for Penn’s hiring of extra police. Under either assumption the results are consistent with others and suggest there are substantial cost savings associated with hiring extra police.

#### 4. Conclusions

Given the importance of police protection for private firms and the tremendous welfare effect of crime in city neighborhoods, the lack of prior studies on the effects of extra police provided by private entities such as universities is an important omission. Although others before us laid the groundwork for assessing the causal effect of the police more generally on crime, and their influence on crime when deployed to high crime areas, we provide one of the first examinations of the crime reduction effects of supplemental police services provided on a large-scale by a university to its surrounding neighborhood that uses a geographic regression discontinuity design. More research is needed to determine if this effect generalizes to other urban university settings.

The geographic discontinuity design we employ could be used in other settings that examine the expansion of public services across boundaries, such as the expansion of police patrol services across boundaries to encompass new housing developments.

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<sup>20</sup> The costs of salary and benefits for a new UPPD officer is \$62,650, which is comparable to the starting salary (\$45,420) and benefits (assuming 40%) for the Philadelphia Police is \$63,588.

Our identification strategy would be undermined if the Penn patrol boundary was selected because it reflects some natural geographic discontinuity of student living or commercial properties that the university is particularly interested in protecting. To the best of our knowledge, the patrol zone was selected as part of a negotiation with the University City District, and there is no evidence that the zone is anything but an attempt to secure the outer ring of the campus and off-campus properties that are considered valuable to the university (Rodin, 2005). Fundamentally, there is no reason to think that the demographic or risk profile shifts fundamentally at 2 to 3 blocks beyond the boundary in the University City District. Large numbers of university students live as far as 50th street, suggesting that street blocks have similar exposure to student risk groups within our bandwidth.

The hiring of additional police services appears to be an effective approach to reducing crime, at least for a limited distance around the boundary. These results also imply that there may be greater social welfare benefits of hiring extra police for private entities if their provision of extra police allows municipal police to focus efforts elsewhere at the same costs to the taxpayer.

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