The Short- and Long-Run Effects of Private Law Enforcement: Evidence from University Police

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Abstract

Over a million people in the United States are employed in private security and law enforcement, yet very little is known about the effects of private police on crime. The current study examines the relationship between a privately funded university police force and crime in a large US city. Following an expansion of the jurisdictional boundary of the private police force, we see no short-term change in crime. However, using a geographic regression discontinuity approach, we find large impacts of private police on public safety, with violent crime in particular decreasing. These contradictory results appear to be a consequence of a delayed effect of private police on crime.

1. Introduction

With constrained public budgets, falling crime rates, and high pay rates of sworn officers, there is increasing debate about whether there is an appropriate role for the private sector in delivering public safety services. As of May 2014, over 1.05 million people in the United States were employed as private police (such as private detectives, investigators, and security guards), up 8 percent from 2004.[1] That is slightly more than all people employed in computer systems design and related services and more than three times the number of people working in motor vehicle parts production. With the rise in the number of private police, however,

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[1] See US Bureau of Labor Statistics, Occupational Employment and Wages, May 2014: 33-000 Protective Service Occupations (Major Group) (http://www.bls.gov/oes/2014/may/oes330000.htm).

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there have been debates about the lack of their accountability to the public, use of excessive force, and violations of constitutional rights (Davis et al. 2010; Stenning 2000; Boghosian 2005; Mahony and Prenzler 1996). Despite significant investments in and the potential legal implications of private police operating in public spaces, there are only a handful of studies that examine the effectiveness of any type of private police on public safety outcomes.

The few studies in the area find that private police enhance public safety outcomes, with campus police in particular decreasing crime across almost every measure (MacDonald, Klick, and Grunwald 2012), and private security employed for business improvement districts improve public safety outcomes as well (MacDonald et al. 2010; Brooks 2008; Cook and MacDonald 2011). However, the generalizability of these findings to other communities, the time frame over which any gains from private policing are realized, and the comparative impacts of private versus public police remain poorly understood.

This paper seeks to provide new evidence on the impact of private police on crime, exploiting a unique setting that permits a credible examination of the causal effect of police in both the short and long run. We focus on the University of Chicago Police Department (UCPD), the largest private police department in Chicago (Reaves 2008), which operates in a comparatively crime-prone region of a major US city that is dense, racially diverse (US Bureau of the Census 2002), and subject to the same types of crimes as other Chicago neighborhoods.[2] The UCPD is an excellent test case for examining the effect of private police on crime because it is a highly professionalized and accredited department working in a very well-defined and discrete geographic space that extends well beyond campus and into the surrounding neighborhoods of the South Side of Chicago.

Using two methods, a difference-in-differences (DD) approach exploiting temporal and spatial variation in jurisdictional powers and a geographic regression discontinuity (GRD) design exploiting the location of the UCPD's jurisdictional border, this study examines the effect that the UCPD has on crime and violence. Using the DD approach, this study finds that the short-term effect of the expansion of private police is not statistically significant. The GRD design allows us to examine the long-term effect of the UCPD by using cumulative crime levels on either side of the UCPD's jurisdiction border. Using this approach, we find a large and statistically significant effect—areas just outside the border of the UCPD's patrol area experienced 55 percent more overall crime and 63 percent more violent crime. Given that the UCPD represents an approximate doubling of police presence relative to surrounding neighborhoods, with 92 full-time-equivalent (FTE) and three part-time-equivalent (PTE) officers working within a 6.5-square-mile jurisdiction in 2014, these effects translate to an elasticity of overall crime with respect to police of about $-.7$ and a violent-crime elasticity of $-.9$, which is somewhat higher than the elasticities estimated for public police (Chalfin and McCrary 2013). These differing estimates can be reconciled if it

[2] *Chicago Tribune,* Crime in Chicagoland: Hyde Park (http://crime.chicagotribune.com/chicago/community/hyde-park).

takes a substantial period of time—potentially many years—for criminals to recognize and respond to the ongoing presence of private police.

2. Background

2.1. The Role and Scope of Private Policing

The term "private police" refers to law enforcement bodies funded and operated by nongovernmental entities. Many of the spaces commonly considered to be public, such as shopping malls, residential areas, parks, and educational campuses, are in fact controlled by private entities (Stenning 2000; Wakefield 2008; Joh 2005; Shearing and Stenning 1981; Jones and Newburn 1999). Referred to as "mass private property," these spaces, which are privately owned but routinely used and frequented by the public, now make up a substantial portion of the public space in urban areas (Stenning 2000). The owners of these areas may hire their own security to ensure both the safety of the public who frequent their space and the protection of their business interests.

Private police or security typically follow different licensing, registration, background check, and training procedures than public police officers, and regulations for them vary by state. Once licensed and approved, private police have the authority to patrol, stop, detain, and search individuals without probable cause. Most private security officers and investigators do not have full powers of arrest, however, and are typically unarmed (Boghosian 2005).

In many settings, lines have blurred between the private police and public police (Wakefield 2008; D'Alessio, Eitle, and Stolzenberg 2005; Joh 2005; Kakalik and Wildhorn 1971; University of Chicago Department of Safety and Security 2013; Nalla and Heraux 2003; Shearing and Stenning 1981, 1983; Spitzer and Scull 1977; Stenning 2000; Wakefield 2002), and many of the remaining distinctions may be more theoretical than practical. Private and public officers seek to both reduce crime and increase safety, although the "client base for private police [is] smaller" (Nalla and Heraux 2003, p. 238). Thus, private police may be more concerned with creating a favorable business climate for their employers than with justice. The private police are also subject to different oversight and accountability laws than public police (Stenning 2000), which may result in unequal services that disproportionately benefit the private sector.

The size of the private police or security industry has stabilized after years of substantial growth. Between 1988 and 1997, the size of private police forces grew by almost 800 percent, while the public police force grew by only 15 percent and has leveled off since then. There remain approximately 300,000 more private than public police nationwide.[3] Some researchers and legal scholars believe that the increase in the number of private police, including campus police, may be part

[3] According to the US Bureau of Labor Statistics, Occupational Employment and Wages, May 2013: 33-9032 Security Guards (http://www.bls.gov/oes/2013/may/oes339032.htm), as of May 2013 there were 740,340 public patrol officers or detectives and 1,066,730 private security guards in the United States.

of the wider trend of privatizing government services (for example, Joh 2005; Kempa, Stenning, and Wood 2004; Sklansky 1999), in line with the privatization of prisons (Sklansky 1999; Joh 2005) and, to a lesser extent, the judicial branch (Savas 1987; Sklansky 1999). Others believe that the use of private policing has grown because of the changing nature of the built environment, such as the increasing desire to have outdoor private property open for public access and use.

2.2. Campus Police: Private or Public Police?

Rather than public police or private security, this study focuses on a particularly prevalent form of private law enforcement—campus police. Of the US universities and 4-year colleges with more than 2,500 students, nearly three-quarters employ sworn personnel—officers with full arrest powers granted by the government—with some forces consisting of more than 300 officers (Reaves 2008). While some may have full police powers, they are funded by the colleges and universities that they protect, which distinguishes them from public police. In addition to granting campus law enforcement agencies greater policing power, there has been a concurrent increase in the size of the jurisdictions under their control, extending beyond their campuses (Peak, Barthe, and Garcia 2008). This may be part of the larger movement toward privately funding police and security officers to protect individuals and property.

Although campus police fall under the umbrella of private police, they may have more in common with the public police than private security. Campus police departments, as they are increasingly known, are often fully accredited law enforcement agencies employing sworn police officers. Nearly 90 percent of departments employ armed patrol officers (Reaves 2008). Campus law enforcement agencies also engage in a variety of formal police duties, including uniformed patrols, arrests, and investigations (Peak, Barthe, and Garcia 2008). The activities performed by campus law enforcement agencies include investigations, crime prevention, parking enforcement, training, crime reporting under the Clery Act (Janosik and Gregory 2003), public event coordination, planning (time spent planning and preparing for various operations), narcotics, traffic and accident investigations, dealing with hazardous materials, canine (working with police dogs), and special weapons and tactics (Reaves 2008; Peak, Barthe, and Garcia 2008).

While the responsibilities and activities of campus police and public police are quite similar, they also differ in other respects that may be relevant for analysis in this study. Campus police agencies are more likely than local departments to conduct community relations skills assessments of new recruits, whereas local departments are more likely to require written aptitude testing and physical screening. In addition, while more campus officers are college educated, public police officers have more police academy and in-service training requirements (Reaves 2008).

2.3. *Measuring the Effect of Police on Crime*

Standard economic models of crime (Becker 1993; Ehrlich 1981, 2010) posit that rational individuals trade off the gains from crime with the costs of punishment and commit crime when the expected benefit outweighs the expected costs. Potential victims invest in protective measures designed to increase the cost of committing an offense and thus reduce incentives to commit crime (Cook 1986). Such measures can include individual efforts such as installation of locks and alarms or avoiding higher-crime areas but also investments in collective law enforcement activities, both public and private.

Optimal choices regarding the level of private policing activity can take into account that private police may have a wider scope of objectives than public police. Some research suggests that private police are concerned with preventing or reducing losses and, accordingly, are more concerned with monitoring, surveillance, and prevention than with arrest and punishment (Joh 2004). The private police in this study exist in a comparatively high-crime area of the city, and there is significant concern about conventional crime such as robbery or burglary. Therefore, we anticipate that one of its primary objectives is traditional street crime reduction, and in this respect, the UCPD may be atypical in comparison with other private police forces that are more focused on loss prevention.

In the context of Chicago and the UCPD, some blocks are patrolled by both public and private police. This additional supply of policing services relative to areas patrolled by only the Chicago Police Department (CPD) is hypothesized to increase the expected cost of offending by increasing the probability of getting caught and thus reduce the supply of crime. For blocks that become newly patrolled by the UCPD, whether we observe shifts in crime depends not only on the effectiveness of private police but also on the speed with which behavioral change occurs. For example, when UCPD officers first occupy a new expansion area, they may require time to build relationships with people in the community and confidential informants and to learn about the contours of the built environment. Moreover, public police may respond to the presence of private police by reallocating effort toward other areas. In that case, private police substitute for public police, which therefore attenuates any positive effect that private police have on reducing crime.

Ultimately, whether the presence of private police affects crime rates is an empirical question. The basic problem of measuring the effect of police on crime rates is not a new one, although our focus on private rather than public law enforcement is novel. A persistent empirical challenge identified by past researchers (Marvell and Moody 1996) is to cleanly separate the impacts of police from the myriad other factors that affect crime rates. Beyond this typical difficulty due to simultaneity and omitted-variable bias—and an additional obstacle that arises in this context is that of reverse causality—the size of the police force may not only

affect crime but also respond to changes in the crime rate through the political process.[4]

To resolve some of these methodological challenges to estimating the effect of police forces on crime, researchers have turned to a number of quasi-experimental research designs. Levitt (1997, 2002) and McCrary (2002) exploit the fact that police hiring often occurs around mayoral and gubernatorial elections, and the timing of these elections across jurisdictions is not determined by crime rates. Corman and Mocan (1996) and Buonanno and Mastrobuoni (2012) make use of the fact that hiring decisions are often made long before new officers can be deployed because of training lags, which means that the timing of when officers start may be not be correlated with current crime rates. Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), and Draca, Machin, and Witt (2011) focus on personnel reallocations that occur in response to terrorist threats, the timing of which is unrelated to patterns of underlying street crime. Evans and Owens (2007) use a large federal hiring program that created new officer positions in numerous jursidictions across the United States, with slots determined in a manner unrelated to preexisting crime. These studies focus on different lags in the impact of police on crime, yet each finds that an increase in police presence or force size led to a decrease in crime.

On the basis of the existing literature, we might expect a substantial decline in street crime in areas with substantial private-police presence. However, we would expect such an magnitude only if private police are as effective at reducing crime as public police, a proposition that has not been well tested to date and one that is evaluated empirically in this study. Perhaps closest to the present study is MacDonald, Klick, and Grunwald (2012), which uses a GRD to test the effects of extra police provided by a private university and finds significant impacts. However, that study is only cross sectional and does not have the advantage of being able to observe what happens to crime before and after the patrol boundary of the private police is altered. As a result, it can estimate only the long-term effect of private police on neighborhood crime. By contrast, we are able to capitalize on a natural experiment that occurred with the expanded geographic coverage of a private university police force to estimate the short-term effect of adding extra private police to a set of neighborhood blocks and a GRD to test the effect of extra police on crime over the long term.

3. The University of Chicago Police Department

The UCPD was formally established in the 1960s and granted full police powers through an Illinois law, the Private College Campus Police Act (110 Ill. Comp. Stat. 1020). The police department is accredited by the Commission on Accred-

[4] Chalfin and McCrary (2013) identify an additional source of bias in estimates that exploit cross-jurisdictional variation in police force size and crime—measurement error. They demonstrate that common sources of information about police staffing levels reveal inconsistencies that lead to downward-biased estimates of the effects of police on crime. Because we consider data from a single jurisdiction, our analysis is not subject to the measurement-error problem they highlight.

itation for Law Enforcement Agencies and operates 24 hours a day, 7 days a week.[5] Officers are required to complete a series of background checks and training including a background investigation, written and medical exams, an oral interview, a psychological test, a drug screen and physical fitness test (POWER test), training consistent with the Illinois Law Enforcement Training and Standards Board, weeks of Chicago Police Academy training, and finally the UCPD Field Training Program. The department has separate, additional emergency and investigation phone lines from the CPD, although individuals can always call 911 for services from CPD.

The size of the UCPD police force grew over most of the period analyzed in this study. The number of FTE officers grew on average 7 percent annually between 2000 and 2010, from 83 to 145 FTE officers, and fell again to 80 FTE officers in 2012.[6] Uniform Crime Reports (UCR) police employee data indicate that there was one police officer for every 212 people in the city of Chicago. As of 2010, the UCPD had one officer for every 464 people within its jurisdiction (Federal Bureau of Investigation 2010, table 78). Therefore, by 2010, the number of officers per capita was 50 percent greater in the UCPD zone than outside the UCPD zone.

The UCPD's patrol area is not limited to the campus but instead extends into the surrounding community, which includes student housing, commercial areas, and residential neighborhoods. The area monitored by the UCPD has expanded over time and is currently approximately 6.5 square miles, containing over 65,000 residents, although formal documentation and details of its size and expansion are spotty (Larson 2012). Importantly for the purpose of this study, the off-campus areas patrolled by the UCPD are similar in many ways to the surrounding neighborhoods patrolled exclusively by the CPD. In particular, there are few qualitative differences in the quality of the housing stock, types of housing (primarily condos, townhomes, and apartments), neighborhood school assignments, or other neighborhood characteristics at the boundary of the UCPD patrol area, a reality we demonstrate more formally later.[7]

Over the past decade, the UCPD has expanded its primary jurisdiction from 39th to 64th Streets and Cottage Grove Avenue to Lake Shore Drive. As shown in Figure 1, the first expansion of the UCPD's authority occurred in October 2001 and included 78 census blocks (shown in blue) south of the original UCPD area. And in April 2004, the UCPD expanded its jurisdictional boundary by 105 additional census square blocks north of the original UCPD area. There was a third expansion in May 2012 (highlighted in orange), which is not included in these analyses because there is not a long enough follow-up period. These expansions were in response to concerns from students, faculty, and other security officers

[5] University of Chicago, Department of Safety and Security: University of Chicago Police Department (http://safety-security.uchicago.edu/police/).

[6] Unpublished data provided by the UCPD.

[7] Perhaps the main exceptions are portions of the western and southeastern boundaries of the UCPD patrol area, which are contiguous to large parks.

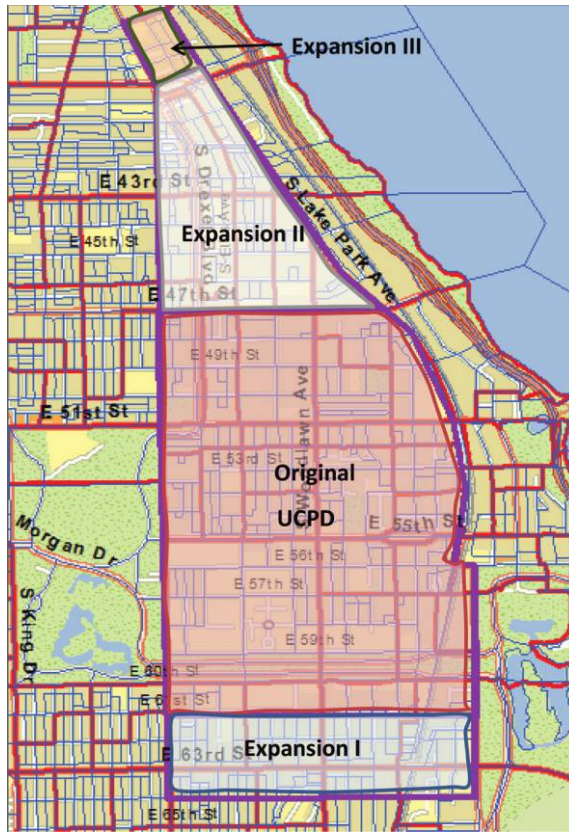

Figure 1. The University of Chicago Police Department's extended patrol areas

that the crime in the surrounding area was directly impacting school safety and the viability of the institution (Larson 2012).

Since crime data are not available before the formation of the UCPD in the 1960s, this study analyzes only the impact of the expansions that occurred in 2001 and 2004. The use of consistent crime definitions and data collection methods started in 2001, so there are only 9 months of reliable preperiod crime data before the first expansion occurred in October 2001. We also investigate whether there are any effects on crime over time in these expansion zones.

4. Empirical Strategy

4.1. Short-Run Effect: Natural Experiment

Our first identification strategy exploits the change in the UCPD's boundaries over time. In October 2001, 52 populated census blocks went from being under the jurisdicition of the CPD only to also being under the UCPD's jurisdiction

(expansion 1), and in April 2004, an additional 77 populated census blocks came under the UCPD's jurisdiction (expansion 2). These two areas are considered the treatment blocks. We define the control area as the 1,954 populated census blocks surrounding UCPD jurisdictions where the UCPD never had jurisdiction, including parts of the Douglas, Grand Boulevard, Greater Grand Crossing, Hyde Park, Kenwood, Oakland, South Shore, Washington Park, and Woodlawn neighborhoods. We conduct a DD estimation in which we compare the change in average monthly crime rates before and after the introduction of the UCPD in the treatment blocks to the change in monthly crime in the control blocks. Models include block and month-year fixed effects, and specifications adjust standard errors for clustering at the block level. Monthly block-level crime counts are the outcome, and we estimate the model using Poisson regression to account for the count nature of the data.[8,9]

A key assumption of the DD methodology is that the outcome in the treatment and control groups is assumed to follow the same time trend in the absence of the treatment (the parallel-trends assumption). To analyze pretrends, we also include a DD model with leads and lags (Stevenson and Wolfers 2006). In particular, pertaining to time relative to the first month of the UCPD entering a jurisdiction, we include a series of dummies coding the month-year of jurisdiction expansion and each month pre- and postjurisdiction expansion.

4.2. Long-Run Effect: Geographic Regression Discontinuity Design

The second identification strategy focuses on the longer-term effect of the UCPD and exploits the geographic discontinuity caused by having extra private police on one side of the patrol zone and not the other. If the UCPD impacts crime, we would expect to see different crime patterns in its patrol zone relative to areas outside its patrol zone. However, simple comparisons of crime between the blocks patrolled by the UCPD and those not controlled by the UCPD may not reveal the true causal effect of police because of potentially unobserved differences in neighborhood characteristics between the two areas. To mitigate bias that might arise because of omitted neighborhood characteristics, we adopt a regression discontinuity (RD) design that focuses on changes in crime immediately surrounding the UCPD's patrol border, which in most cases is miles from the main campus.

The UCPD's boundaries seem well suited to this analytic strategy because they

[8] Since census blocks are relatively small (approximately 60 people per block), there are many zeroes in the data, which might affect the precision of the estimates. We also estimate the difference-in-differences (DD) regressions using census block groups (with approximately 1,000 people per block group). Unlike with the smaller blocks, however, there are block groups that overlap jurisdictional boundaries. Results are similar and are available from the authors on request.

[9] As noted in a number of studies, including Karaca-Mandic, Norton, and Dowd (2012), the interpretation of DD coefficients is somewhat more complicated in a Poisson count model because of its nonlinearity. Puhani (2012) points out, however, that coefficients on the DD interaction term can be used to examine the sign and significance of the treatment effect in a nonlinear model under fairly general conditions.

are precisely defined, known to the police and public, and not contiguous with any campus border but are largely unrelated to neighborhood or other geographic boundaries that would potentially affect crime patterns. By focusing attention on border areas, we ensure that the treated areas with additional private police are as comparable as possible to the comparison areas with only public police. For example, the volume of cars parked on the street is likely to affect the number of vehicle thefts that occur on a particular block, and this characteristic is unobserved and thus cannot be directly controlled for in our analysis. While there may be significant differences on average in vehicular parking patterns around the UCPD's area versus the comparison neighborhoods, it seems less plausible that parking patterns would vary significantly between two blocks that are next to one another in the same neighborhood but that lie on differing sides of the UCPD's border. Thus, a comparison between two border blocks might offer a better opportunity to obtain an unbiased estimate of the effect of the campus police. The GRD approach offers an opportunity to estimate such differences across a large number of blocks within a well-established statistical framework.

In our context, the running variable for the GRD design is distance to the border. We note, however, that unlike in the traditional RD design, in our study the treatment (private police protection) and control statuses are not assigned by a scoring mechanism with a known functional form. Moreover, for the RD approach to recover the true causal effect of the presence of private police, we require that there be no geographic spillovers resulting from the existence of the boundary. If, for example, criminals concentrate just outside the UCPD's patrol boundary in an effort to avoid detection, the border area may provide a poor counterfactual for the amount of crime that would be expected in the UCPD's area absent the UCPD, and we might overstate the impacts of private police.

We estimate the change in the crime rate at the discontinuity using regression methods that are typically applied to RD modeling; in particular, we consider crime counts as an outcome and model them as a function of an indicator of whether a block lies outside the UCPD's border and additional controls, which include functions capturing the distance to the border. We consider a range of alternative parameterizations of the model, including varying the degree of the polynomial in distance and applying local linear regression. We also conduct falsification tests using the same variables and modeling strategy with geocoded data on business licenses and vehicle traffic as outcomes, which should not be associated with the UCPD's boundaries.

5. Data

Street-level crime incident data were taken from the publicly available CPD Citizen Law Enforcement Analysis and Reporting website,[10] which includes

[10] See Chicago Police Department, CLEARMAP: Citizen Law Enforcement Analysis and Reporting (http://gis.chicagopolice.org).

crime incidents from 2001 onward reported to the police and geocoded to the closest block. Individual events are classified into crime categories by the CPD. This study analyzes four major categories, following the Federal Bureau of Investigation's (FBI's) Uniform Crime Reporting Program—a sum of total reported crime and the disaggregates of violent crime (including the part 1 crimes of homicide, criminal sexual assault, robbery, and aggravated assault), property crime (including the part 1 crimes of burglary, larceny, motor vehicle theft, and arson), and public violence (including part 1 violent crimes and firearms-related crimes occurring outside a business or residence, excluding domestic violence). The FBI's overall reported crime measure includes part 2 crimes, which are less serious. We also examined part 2 crimes separately but decided against presenting the findings here since they are much more susceptible to bias in reporting and recording (Gove, Hughes, and Geerken 1985).

Street-level incident data are aggregated to the census block level, and none of the blocks straddle the UCPD's boundary. The comparison blocks are drawn from the community areas surrounding the UCPD's jurisdiction that are most similar in character to Hyde Park, including Douglas, Grand Boulevard, Greater Grand Crossing, Kenwood, Oakland, South Shore, Washington Park, and Woodlawn; blocks in Hyde Park that are not in the UCPD's jurisdiction are also included in the comparison area. Data on block-level demographics, socioeconomic characteristics, and population are from the 2000 and 2010 US censuses and are linearly interpolated to generate annual estimates.

5.1. Short-Run Effect: Natural Experiment

For the DD analysis, we constructed a block-month panel from January 2001 to December 2010, and we analyze the evolution of crime in the expansion areas compared with control areas over time. The total analytical sample includes 2,082 populated census blocks with at least one crime over the period analyzed; 1,953 blocks fall within the control group (blocks never patrolled by the UCPD because they are outside the expansions zones), and 129 are in the treatment group (the expansion 1 area is 52 blocks, and the expansion 2 area is 77 blocks). On average, approximately 1.7 crimes are committed in the CPD's census blocks per month, and 1.4 crimes are committed in the UCPD's blocks. The age and gender distributions are similar between the two groups, but the population and ethnic distribution differ. Population per block is greater in the UCPD's area, and there is a greater proportion of blacks in the CPD's area.

Figure 2 illustrates crime trends in the two areas analyzed from January 2001 to December 2010. The thin vertical line indicates the timing of expansion 1 (October 2001), and the thick vertical line indicates expansion 2 (April 2004). Both jurisdictions have similar seasonal patterns of crime, and the difference between the jurisdictions narrowed over the period investigated. Importantly for the DD design, the crime pretrends appear to be similar between the two groups.

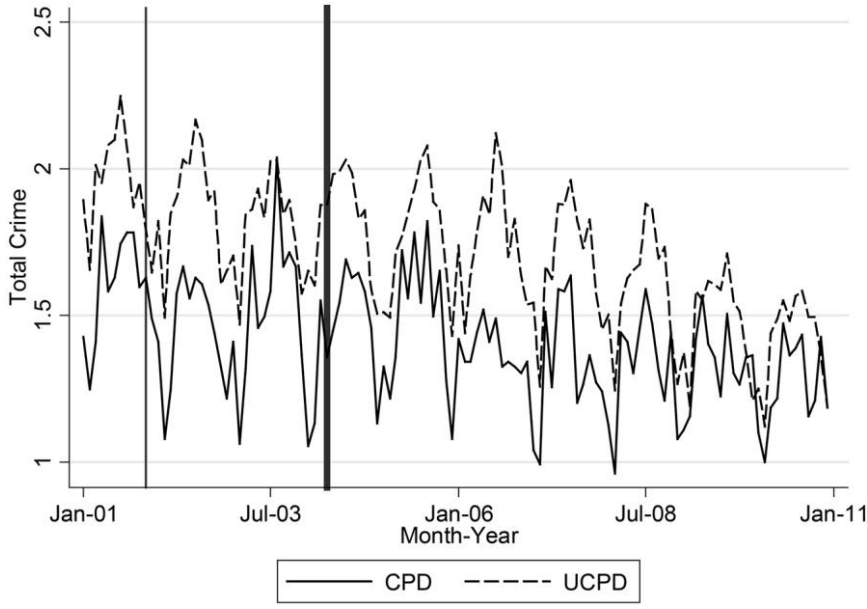

Figure 2. Mean crimes per block per month in areas patrolled by the University of Chicago Police Department and the Chicago Police Department.

5.2. Long-Run Effect: Geographic Regression Discontinuity Design

For the GRD analysis, the data are pooled into a single cross section of blocks by summing the number of crimes committed in each block between April 2004 and May 2012. This means that the GRD analysis is based on only the period after the phased-in expansion, so there are no jurisdictional changes during our observation period.

Table 1 presents summary statistics describing the demographic characteristics of the UCPD's blocks on the basis of 2010 census data. We also report RD estimates of the change in each demographic characteristic at the boundary and each estimate's associated standard error and p-value. There are few differences in the race or other demographics of the resident population, their income, or housing characteristics between the UCPD's blocks and nearby blocks outside the patrol zone. Where differences exist, in practical terms the disparities are small. Because of the large number of tested characteristics, even absent any true discontinuities, some statistically significant coefficients may arise by chance alone when using conventional significance levels. If the UCPD's areas do not differ systematically from the control areas at the discontinuity, then the p-values of the differences in characteristics should be uniformly distributed, and an omnibus Kolmogorov-Smirnov test for balance across the full set of characteristics presented in the table fails to reject the null hypothesis of balance ($p = .99$). This suggests that the CPD's blocks near the boundary are similar to the UCPD's blocks near the boundary.

Table 1
Descriptive Statistics for the University of Chicago Police Department's Patrol Zones

			Regression Discontinuity Estimates		
	Average for Block Groups	SD	Change at the Border	SE	*p*-Value
Total population	1,185.19	493.46	−167.98	80.17	.04*
Fraction black	.76	.29	−.01	.05	.84
Fraction Hispanic	.03	.04	−.01	.01	.43
Fraction young	.06	.06	−.01	.01	.55
Fraction recently moved	.19	.10	.02	.03	.42
Fraction living alone	.44	.16	−.03	.03	.40
Fraction of children not living with parents	.14	.15	.05	.03	.08
Fraction married	.28	.11	.01	.03	.84
Fraction high school dropout	.13	.11	.00	.02	.84
Median household income	37,990	20,736	3,034	3,645	.41
Fraction with no earnings	.27	.15	−.03	.03	.26
Fraction receiving Social Security Insurance	.10	.09	.01	.02	.76
Fraction receiving public assistance	.04	.05	−.01	.01	.11
Fraction veterans	.06	.04	.00	.01	.86
Fraction unemployed	.19	.11	.00	.02	.81
Fraction not in labor force	.39	.13	−.01	.03	.61
Housing units	647.77	293.93	−61.63	55.95	.27
Rental share of housing stock	.69	.18	−.03	.04	.54
Fraction of vacant houses	.21	.13	.03	.02	.17
Median number of rooms	4.61	.87	.22	.18	.23
Share of single-unit houses	.16	.12	.01	.02	.83
Median contract rent ($)	751.39	238.94	127.80	38.73	.00**
Median house value ($)	255,037	108,085	24,583	20,942	.24
Fraction low income	.12	.07	−.01	.02	.58
% Below poverty line	.29	.12	−.07	.03	.01**
% Noncitizen	.05	.05	.01	.01	.51
% Disabled	.15	.06	−.01	.01	.51

Note. The unit of observation is a census block group. $N = 1{,}065$.
* $p < .05$.
** $p < .01$.

6. Results

6.1. Short-Run Effect: Natural Experiment

Results of our DD regressions analyzing the effect of the UCPD on total crime and by crime type are presented in Table 2. The results for all crimes indicate that there was no statistically significant effect on monthly crime identified in the areas annexed by the UCPD. When estimating the effect on violent, property, and other crimes separately, the difference in the logs of expected counts is similar for the CPD-only blocks and the UCPD blocks, holding all else constant in the model. Finally, our results for any violent crime occurring in public indicate that the effect of the UCPD is statistically insignificant. When analyzing the effect of

Table 2
Difference-in-Differences Results of the University of Chicago Police Department's Effect on Block-Level Monthly Crime

	All Crime	Violent Crime	Property Crime	Other Crime	Public Violence
Overall effect	−.013	−.015	−.094	.020	−.044
	(.076)	(.064)	(.154)	(.062)	(.116)
N	249,840	245,520	249,360	249,600	249,720
Blocks	2,082	2,046	2,078	2,080	2,081
Expansion 1 effect only	.020	−.031	−.136	.109	−.059
	(.064)	(.137)	(.070)	(.889)	(.087)
N	240,600	236,760	240,360	240,360	240,480
Blocks	2,005	1,973	2,003	2,003	2,004
Expansion 2 effect only	−.021	−.004	−.082	−.0002	−.039
	(.091)	(.072)	(.187)	(.072)	(.143)
N	243,600	239,520	243,120	243,360	243,480
Blocks	2,030	1,996	2,026	2,028	2,029

Note. Reported coefficients are from a Poisson model, with standard errors clustered by block in parentheses. The dependent variable is the number of block-level crimes per month. All models include block fixed effects, month-year fixed effects, and average block demographic characteristics (age distribution, ethnicity, race, gender, and population) as controls. Blocks with no crime in all periods are excluded from the analyses.

each expansion area separately, results are consistent with the main findings, and no statistically significant impacts are apparent.

We next replicate the previous DD analyses but consider lead and lag effects of expanding jurisdiction. Including leads allows us to observe potential confounding effects of preexisting trends and test for possible endogeneity in jurisdiction expansion, and examining lags offers an opportunity to more finely assess the dynamics of postexpansion crime. One month preexpansion is the excluded dummy for each expansion area and is set equal to zero in Figure 3, which presents our point estimates with 95 percent confidence intervals.

These trends support the findings reported in Table 2 of no statistically significant impact of additional private police on crime in the short to medium run. Crime began to trend downward in the treated areas approximately 80 months after jurisdiction expansion, although these shifts are not statistically significant.[11] To the extent that the presence of private police impacts crime, such impacts may be discernable only over the longer term. For this analysis we turn to the GRD results.

6.2. Long-Run Effect: Geographic Regression Discontinuity Design

Given the similarities in demographics and other characteristics of the blocks immediately outside the UCPD's border to areas within the border (Table 1), ab-

[11] We test a number of other time variables to identify the speed with which crime declined as a result of the two expansions. We use quarterly and annual data and test time dummies, time trends, and nonlinear time effects. The results are statistically insignificant overall and for expansion 1 and expansion 2 separately.

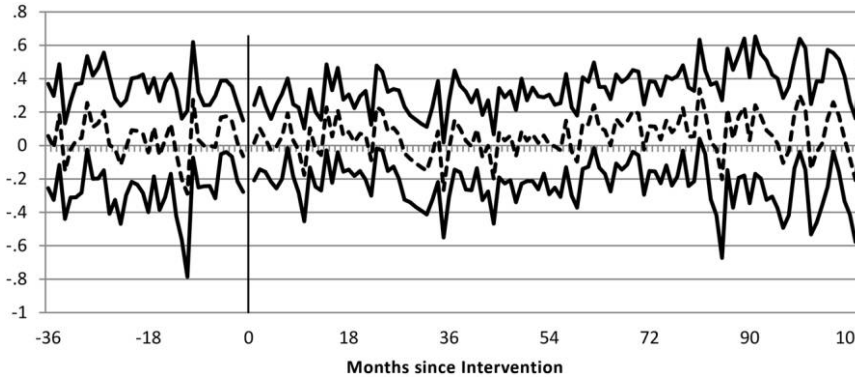

Figure 3. Results from an event-study analysis of jurisdiction expansion, 2001–10

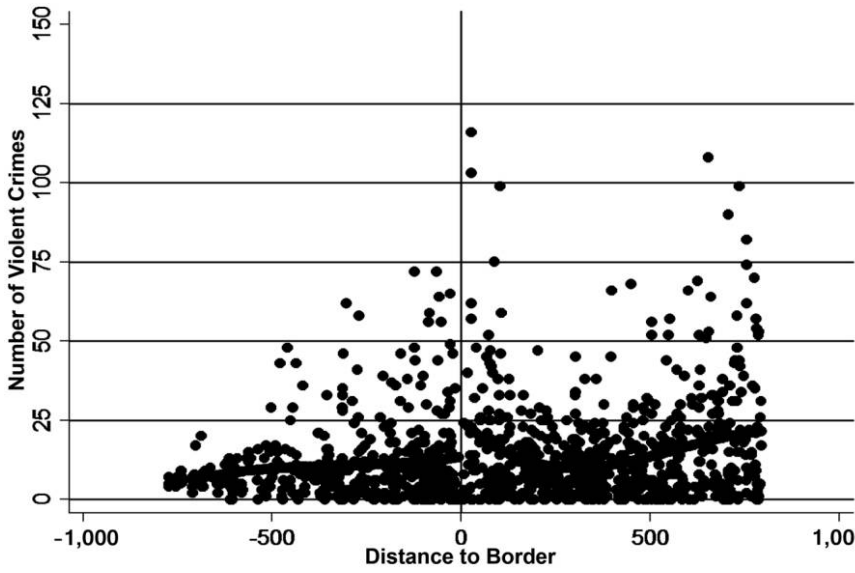

Figure 4. Violent crime by blocks' distance from patrol boundary

sent any long-run impact of private police on crime, we might expect crime rates to be similar across the two areas as well. Figure 4 shows the relationship between blocks' average distance to the boundary and the total number of violent crimes over the period of investigation, along with the polynomials of best fit. As the figure shows, there is a higher density of crimes just beyond the UCPD's border. There is no simple explanation for the drop in crime observed at the boundary other than the fact that areas within the boundary are patrolled by additional private police.

Table 3
Geographic Regression Discontinuity Results

	Coefficient	SE
All crime	68.70*	35.69
Violent crime	9.72**	3.52
Property crime	11.58	10.83
Public violence	47.21	25.00

Note. Results are for the base model estimated using local linear regression. $N = 1{,}065$.
* $p < .05$.
** $p < .01$.

Results of our GRD regressions analyzing the effect of a block being on the UCPD's side of a border on four categories of crime are presented in Table 3. For our baseline model we include no demographic controls but account for distance to the border using local linear regression, which has some desirable statistical properties relative to other methods of accounting for distance.[12] The treatment variable indicates being outside the UCPD's patrol area, so a positive estimate indicates a reduction in crime due to the presence of the UCPD. Results are statistically significant and indicate that blocks patrolled only by the CPD had 68.7 more crimes than blocks also patrolled by the UCPD over 8 years, from April 2004 through May 2012, which corresponds to 55 percent more crime (the mean number of crimes per year in the UCPD's and CPD's patrol zones is 126 and 162, respectively). There is also a statistically significant impact on violent crime at the 1 percent level, such that there were 9.7 more violent crimes per block on the CPD's side, which represents 63 percent fewer violent crimes in the UCPD's area (the mean number of violent crimes in UCPD's and CPD's patrol zones is 11 and 17, respectively). Overall, it does not appear that the UCPD had a statistically significant impact on property crime or violent crime committed only in public spaces, although the point estimates suggest potentially large impacts on these crimes in a practical sense. On an annual basis, blocks within the UCPD's patrol boundaries had an average of 8.5 fewer crimes per year, of which 1.2 were violent crimes.

Table 4 demonstrates that these findings are robust to a number of logical specification changes to the RD design. Specification (1) estimates the same model using logged crime and finds similar results. When we control for a multitude of socioeconomic characteristics (specification [2]), results become only marginally significant for total crime yet remain statistically significant for violent crimes at the 1 percent level; there were approximately nine more violent crimes per block in the CPD area. The robust model, which generates confidence intervals robust to differing bandwidth choices for the local polynomial regression according

[12] Gelman and Imbens (2014) and Imbens and Lemieux (2008) provide a more detailed discussion of the advantages of local linear regression in this context.

to the procedure of Calonico, Cattaneo, and Titiunik (2014), indicates an even greater effect of UCPD than found in the base model. The impact of the UCPD's presence was 83.2 fewer crimes per block overall and a statistically significant fewer 11.3 violent crimes per block on the UCPD's side of the border.

Specifications (4) and (5) use conventional polynomials rather than local linear regression to account for distance and yield similar results. In specification (6), we estimate a donut-RD design by omitting blocks less than 50 meters from the border, while in specification (7), we include only blocks within 400 meters of the border, which ensures that identification comes solely through areas near the border. In both cases, the estimated impacts for total and violent crime remain qualitatively similar, although the smaller number of observations in these specifications renders the estimates appreciably less precise. Finally, in specification (8) we measure distance on the basis of block centroids versus block edges and again observe large impacts on total and violent crime. Across the various specifications, we see consistent evidence that the presence of private police reduces violent crime and some evidence of an impact on overall crime.

To put these magnitudes in perspective, we can roughly equate these impacts to elasticities of police with respect to crime, which is the conventional metric reported in studies of the effects of police. Although precise manpower data for the UCPD over the entire period are elusive, in 2014 the UCPD had around 92 FTE and three PTE officers working within its 6.5-square-mile jurisdiction, which represents an approximate doubling of police presence relative to neighboring areas of the city, assuming no crowding out. These effects translate to an elasticity of overall crime with respect to police of about –.7 and a violent-crime elasticity of –.9, which is somewhat higher than the elasticities estimated for public police.

If the GRD design is an appropriate method for modeling the effects of the CPD's presence, we might see changes in crime across the border, but we should not expect to detect changes in other outcomes not logically connected with police presence. To examine this, we geocoded data on business licenses and traffic volume obtained from the City of Chicago's Data Portal[13] and applied a similar GRD analysis to these outcomes as a falsification test. Those falsification tests do not identify any measurable discontinuity along the UCPD's boundary in either business licenses or traffic volume.[14] The lack of an effect when examining these other outcomes supports the use of the GRD methodology as a way of measuring the influence of the CPD.

7. Discussion

Our analysis indicates that the introduction of private police to a patrol area initially had no measurable effect on crime, yet after sufficient time had passed, the presence of private police was associated with economically meaningful reductions in crime. How do we reconcile these findings with those of studies such

[13] See City of Chicago, Data Portal (https://data.cityofchicago.org/).
[14] The full set of results is available from the authors on request.

Table 4

Robustness Check for Geographic Regression Discontinuity Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All crime	.55*	58.69	83.20*	65.07	86.94	44.42	57.67	80.55**
	(.29)	(34.80)	(41.97)	(34.52)	(48.52)	(58.62)	(52.44)	(18.14)
N	1,065	1,065	1,065	1,065	1,065	451	251	1,065
Violent crime	.63**	8.66**	11.30**	8.27**	11.60*	7.01	9.64	6.87**
	(.24)	(3.46)	(4.22)	(3.24)	(4.82)	(4.82)	(5.05)	(1.63)
N	1,065	1,065	1,065	1,065	1,065	451	251	1,065
Property crime	.36	10.32	15.96	7.29	18.22	5.35	8.84	17.50**
	(.26)	(10.53)	(12.56)	(9.63)	(12.90)	(19.26)	(14.08)	(5.40)
N	1,065	1,065	1,065	1,065	1,065	451	251	1,065
Public violence	.49	27.02	43.66	49.51	37.79	−1.67	51.10	71.38**
	(.27)	(23.85)	(30.00)	(24.84)	(36.20)	(49.18)	(32.73)	(12.04)
N	1,065	1,065	1,065	1,065	1,065	451	251	1,065

Note. Specification (1) uses logged outcomes, specification (2) includes covariates, specification (3) is the robust model, specification (4) uses a quadratic polynomial in distance, specification (5) uses a cubic polynomial in distance, specification (6) omits blocks near the border, specification (7) includes only blocks near the border, and specification (8) uses an alternative distance metric. Standard errors are in parentheses.

* $p < .05$.

** $p < .01$.

as Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), and Draca, Machin, and Witt (2011), all of which demonstrate using credible quasi experiments that involve similarly large shifts in police presence that criminal activity can respond rapidly to changes in police presence?

One possibility is that culture is key and that there are cultural or other environmental differences between Chicago and the other locations studied that cause the effects of police in this study to be different from those observed elsewhere. However, given that the existing studies span multiple countries and time periods and yet obtain fairly consistent impacts, it seems less likely that cultural differences alone can explain the difference in results.

An alternative explanation for the disparity might be that private police are separately governed from public police and that the two types of organizations may have distinct goals. Prior studies that demonstrate substantial short-run effects of changes in police presence focus on contexts in which there is central management of enforcement levels across the entire jursidiction. In this study, both the CPD and the UCPD provide enforcement services simultaneously and without direct coordination. The CPD could respond strategically to an expansion of the UCPD in a variety of ways—by cutting back its presence in the newly patrolled areas, which would lead to a partial displacement of the private police, or by expanding its presence in the areas immediately surrounding the expansion zone, in an effort to contain spillover effects, or both. Our short-run results would be expected if the CPD alters its patrol patterns in a way that negates the effects of the boundary expansion.

However, given that we ultimately observe differences in crime that are based on the UCPD's boundaries, any explanation that relies on the compensatory behavior by the CPD requires that such behavior be temporary, and there is no obvious explanation for why such differences could persist for several years but not indefinitely. Moreover, a number of factors would have made it difficult for the CPD to appreciably reduce its patrol effort in response to the expansions. There was strong community pressure to increase police presence when the expansions occurred, so a rollback by the CPD would likely have met with community resistance. Also, the patrol structure of the CPD, which is organized around geographic beats that have remained fairly constant over time, would complicate efforts to diminish patrol in a narrowly defined area without endangering neighboring blocks not patrolled by the UCPD. Finally, there is little evidence from media accounts or our discussions with individuals in the UCPD or CPD that suggests that the CPD engages in such strategic behavior. Thus, while acknowledging the possiblity that the patterns we observe might reflect displacement, we do not find this to be a likely explanation for our findings. More generally, however, the issue of crowding out is an important one that is relevant when considering the impacts of private rather than public police.

A related possiblity is that private police crowd out private precautionary measures. If the expansions reduced investment in alarm systems and led citizens in the affected areas to be more willing to engage in risky behaviors such

as walking alone at night or to otherwise diminish private precaution, then the net impact might be small even if police do have some salutary effect. Lacking data on private precaution, we are unable to test this hypothesis directly. However, the timing and magnitude of the effects we observe would seem to disqualify private crowding out as an explanation—under this explanation, individuals in the expansion areas would need to reduce private precautionary measures fairly quickly to fully offset the effects of private patrols but then over time increase private precautionary behavior so as to render the effects of the police eventually detectable. The costs of the private police are borne by the university rather than neighborhood residents, so the expansions had no direct influence on the budget constraints affecting potential victims. Moreover, it seems unlikely that private precaution could be sufficiently protective so as to account for changes in crime of 50 percent or more.

One important difference between our Chicago context and the policing variation studied in past research is that the introduction of private police in Chicago was expected to be relatively permanent, whereas in the other studies policing changes were likely to be transient. One possibility is that criminals might be willing to displace their activity temporarily at fairly low cost but may have difficulty making permanent career changes. For example, individuals who rely on selling drugs or acquisitive crimes for income may be able to defer criminal activity during periods of high police scrutiny so long as such periods are sufficiently brief; at some point, however, the need for income will override the risk of capture. Under this jobcentric view of criminal activity, criminals respond to events such as those in documented in London by Draca, Machin, and Witt (2011) by taking a vacation from crime, which leads to short-run drops in crime, but crime reasserts itself once the period of heightened enforcement has passed. In Chicago, in contrast, when the expansions occurred, criminals may have recognized the permanence of the new arrangement and thus did not respond by taking vacations from crime, which would lead to little short-run effect on crime patterns. A permanent increase in enforcement reduces the returns to being a criminal in the targeted area, however, so over time criminals substitute to other careers or other locations, which leads to the larger long-run effects we demonstrate. If this model explains our findings, one implication is that the speed of job adjustment is fairly slow, as we do not find signficant changes in crime when boundaries are altered even several years postexpansion. The model, if correct, would also imply that private police forces may require sustained investment over a long period of time to be fully effective.

Our analysis also connects with the extensive popular discussion and academic debate surrounding the problem of crime and violence at institutions of higher education. Over the past few decades, the nature of the discourse has changed, as campus crime has evolved into a major social issue, requiring federal and state political action (Sloan and Fisher 2010). Colleges and universities face pressure from governments, students, and activist organizations to ensure campus safety. Campus law enforcement agencies are a central component of crime-reduction

efforts, yet the effect of campus law enforcement agencies on campus crime and violence has received scant attention in the empirical literature.

We note that in this study, as in all studies of the effect of enforcement effort that rely on geographic comparisons, it is impossible to know with certainty whether the additional police presence reduced crime in an absolute sense or merely displaced it to other areas. Displacement is of particular concern for public police because displacement results in a reallocation of workload across different geographic units with no net benefit for the department as a whole. For private police, the calculus is more complicated: because private police exist to further the interests of a particular constituency, to the extent that private police increase safety for their constituents, even at the cost of displacing crime to others, they may view themselves as successfully fulfilling their mission. In addition, if willingness to invest in private police is driven by differences in the costs of victimization, then private police may promote a form of allocative efficiency even if they primarily displace rather than prevent crime.

8. Conclusions

This study demonstrates that investment in private security can have a meaningful impact on serious crime in surrounding areas over the long run. Using two approaches to examine the impact of a privately funded police force on neighborhood safety—a natural experiment in which the authority of the UCPD expanded jurisdictional boundaries in 2001 and 2004 to capture the short-term effect of their expansion on crime and a GRD approach to capture the longer-term effect—we show that private police do not have a measurable effect in the short to medium run, but in the long run, additional policing reduced violent crime by over 50 percent in the target areas.

We offer several potential explanations for these contradictory findings across our two research designs. First, there could be a substitution effect, with the private police simply replacing the public police services in the new area in the short run. Second, there could be a substitution effect with other forms of self-protection (such as alarms, private guards, or selective avoidance of crime-prone areas), such that households and businesses reduced spending in security when the UCPD expanded its jurisdiction. Third, and perhaps most likely, there may be a delay in the effect of the extra police on crime because it takes a long time to change the crime dynamics of neighborhoods as criminals adjust to permanent increases in the cost of committing crime.

The University of Chicago's experience shows that campus law enforcement may be a cost-effective means of addressing crime and violence in higher-education settings, at least with a sustained investment over many years. The implications of these findings may be particularly important as members of Congress turns their eyes once again toward campus safety and crime prevention. In the UCPD's jurisdiction, only 34 percent of violent crimes occurred on campus. The University of Chicago is not an outlier in this respect: nationwide, more

crimes involving college students occur off campus than on campus. Expanding the jurisdiction of campus police departments and allowing officers to patrol beyond their borders may be integral to their success. More broadly, our research suggests that private police can be as effective at crime control as public police, at least in some circumstances. Additional efforts to rigorously measure the impacts of private police are warranted so as to allow us to better identify the contexts in which private police matter most for public safety.

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