ROYAL STATISTICAL SOCIETY
DATA | EVIDENCE | DECISIONS

Journal of the Royal Statistical Society
Statistics in Society
Series A

J. R. Statist. Soc. A (2016)
179, *Part* 3, *pp.* 831–846

The effect of private police on crime: evidence from a geographic regression discontinuity design

John M. MacDonald, Jonathan Klick and Ben Grunwald

University of Pennsylvania, Philadelphia, USA

[Received April 2014. Final revision July 2015]

Summary. Research demonstrates that police reduce crime. We study this question by using a natural experiment in which a private university increased the number of police patrols within an arbitrarily defined geographic boundary. Capitalizing on the discontinuity in patrols at the boundary, we estimate that the extra police decreased crime in adjacent city blocks by 43–73%. Our results are consistent with findings from prior work that used other kinds of natural experiment. The paper demonstrates the utility of the geographic regression discontinuity design for estimating the effects of extra public or private services on a variety of outcomes.

Keywords: Crime; Geographic regression discontinuity; Private police

1. Introduction

Policing accounts for a substantial portion of government budgets. In 2007, local, state and federal governments spent \$104 billion, or \$344 *per capita*, on police services (Kyckelhahn, 2011). Crime reduction is the primary welfare benefit associated with police. Scepticism 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. Various observational 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 *et al.*, 2002). This drop is largest in areas that are directly targeted by police activities, but it is also present in surrounding areas as well, suggesting that police are not just displacing crime around the corner (Berk and MacDonald, 2010; Di Tella and Schargrodsky, 2004; Draca *et al.*, 2011; Klick and Tabarrok, 2005; Weisburd *et al.*, 2006). The most persuasive studies use quasi-experimental variation in police deployment in space or time and report consistent evidence that extra police deployments reduce crime (Cohen and Ludwig, 2003; Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Berk and MacDonald, 2010; Draca *et al.*, 2011). Several field experiments also test the temporary effect of supplementary police deployment to crime 'hot spots' and find that extra police patrols reduce crime and disorder (Sherman and Weisburd, 1995; Braga and Bond, 2008; Braga *et al.*, 1999; Ratcliffe *et al.*, 2011; Weisburd and Green, 1995).

Despite an abundance of scholarly work on the effect of police on crime, the most rigorous studies have examined the effects of exogenous changes in police strength due to terror events, terror warnings, short-term 'crackdowns' or field experiments. As a result, the literature focuses on episodic and temporary deployments of extra police that are the exception to normal practice. Less is known about the effect of increasing police deployment over *sustained* periods of time.

Address for correspondence: John M. MacDonald, Department of Criminology, University of Pennsylvania, Suite 483, McNeil Building, 3718 Locus Walk, Philadelphia, PA 19104, USA.
E-mail: johnmm@sas.upenn.edu

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A sustained investment of additional police deployed to small geographic areas may exert more lasting effects on crime if offenders are aware of the change and perceive an increased probability of apprehension. Further, evidence relating to sustained changes in police staffing levels is more relevant from a policy standpoint given that police hiring generally involves a long-term budgetary commitment.

The purpose of this paper is to demonstrate the use of a geographic regression discontinuity design to estimate the effect of increasing police deployment over a sustained period of time. The method that we apply was proposed by Hahn *et al.* (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’

(page 201). Rao *et al.* (2011) used this method to estimate how Walmart locates new stores in the face of state labour laws, and Keele *et al.* (2015) used similar methods to estimate the effect of ballot initiatives on voter turnout.

Following the approach that was outlined by Hahn *et al.* (2001), we apply a non-parametric model of the geographic discontinuity that provides more flexibility in functional form restrictions. Under this design, city blocks near the boundary of a police patrol zone provide good comparisons because the decision on where to place the police is effectively arbitrary, and there are no observable differences in land use or population characteristics at the boundary line. We show how estimates from a geographic regression discontinuity model can be compared with those generated from permutation tests where the geographic locations of observations are independent of the boundary. We also provide falsification tests by comparing estimates of the geographic discontinuity in police force size on outcomes that the extra police should have no influence on. Finally, we discuss the limitations of the method and conclusions for extending geographic discontinuity estimations for assessing the effect of expanded police, fire and emergency medical services.

The data that are analysed in the paper and the programs that were used to analyse them can be obtained from

`http://wileyonlinelibrary.com/journal/rss-datasets`

2. Privately funded police and the University of Pennsylvania

The University of Pennsylvania Police Department (UPPD) provides supplemental police services in the University City district of Philadelphia. The UPPD is the largest privately funded publicly certified police department in Pennsylvania, employing roughly 100 full-time sworn police officers. The department is the third largest university police department in the USA (Reaves, 2008). The primary role of the UPPD is crime prevention in and around the Penn campus. Fig. 1 depicts three key geographic areas to which we refer throughout the remainder of the paper. First, the *inner Penn campus*, which is demarcated by a thick line, refers to the campus proper; it includes residential dormitories, classroom buildings, administrative university offices, restaurants and stores. Second, the *outer Penn campus*, which is demarcated by the finer line, encompasses the neighbourhood that is adjacent to the inner Penn campus (between 30th and 43rd Streets and between Market Street and Baltimore Avenue). Many students and faculty live here. Third, the inner Penn campus and outer Penn campus are both within the larger *University City district*, which is demarcated by the dotted line in Fig. 1. The University City district is a special services district established to ‘coordinate sanitation, security, and other services and to promote the area’s residential retail assets’ (Kromer and Kerman, 2004).

Fig. 1. University City district crime patterns: - - - - - -, University City boundary; ———, outer Penn campus; ———, inner Penn campus; ☐, 0–25 crime incidents; ☐, 26–50 crime incidents; ☐, 51–100 crime incidents; ☐, 101–200 crime incidents; ☐, 201–491 crime incidents

The UPPD deploys police officers on foot, bicycle and mobile patrols to both the inner and the outer Penn campuses. We refer to this combined area as the *Penn patrol zone*. At any given time, at least 16 UPPD police officers are on patrol within the Penn patrol zone, supplementing the police services that are provided by the Philadelphia Police Department. (The training for the 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 that is responsible for conducting criminal investigations, crime scene management and the collection of evidence for crimes throughout the patrol zone.) The UPPD does *not* deploy officers to areas of the University City district that are outside 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 10 a.m. to 3 a.m., such that the only difference in security within the district is the UPPD deployment of uniform police officers (`http://universitycity.org/serving-our-community`) as opposed to other non-police security investments. This is a sizable investment in police in the Penn patrol zone as the entire University City district has only 6–8 Philadelphia Police Department officers patrolling at any given time.

3. Empirical analysis

3.1. Data

We obtained detailed data on all 19588 reported crimes (with x–y-co-ordinates) in the University

City district between 2005 and 2010. (The deidentified crime data were provided to the study investigators from the University of Pennsylvania Police as part of their data sharing access with the Philadelphia Police Department. These data represent all major crime categories reported to the police between 2005 and 2010 in the University City area of Philadelphia.) We classify crimes into four categories: total crimes, violent crimes (assaults, murder, rape and robbery), property crimes (all non-violent offences) and street crimes (assaults, burglaries, purse snatching, robberies and theft from vehicles). We created the street crime classification because these offences are more likely to occur in the direct visibility of police patrols.

Using geographical information system software, we calculated the distance from each block's centroid to the Penn boundary. (We received the geographic information on the boundaries from the UPPD. See `http://www.publicsafety.upenn.edu/assets/General-Web-Photos/PennPatrolpdf.pdf`.) Results were similar if we instead used the average distance from the location of each crime for a given block to the boundary. Weisburd (2008) noted that blocks represent an ideal unit of analysis for assessing the effect of police on crime because they are 'easily identified by the police' and 'provide a natural setting for police interventions'. Blocks also provide physical boundaries and daily routines that make them particularly attractive for studying crime patterns (Taylor *et al.*, 1984). Crimes inside the Penn patrol zone are assigned a negative distance value and crimes outside the zone are assigned a positive value. Crimes on the boundary are assigned a distance of 0 and are, thus, included within the Penn patrol zone boundary.

Incident data were then aggregated to the block level. (We omitted blocks from the analysis that are comprised only of train yards and a cemetery that fall within the data. These blocks involved no crimes and are largely uninhabited. We also dropped two other blocks for a different reason. First, we did not include the block containing Amtrak's 30th Street station as this block is highly unrepresentative with respect to the rest of the data both because of the volume of people who travel throughout the station daily and because the station receives substantially more police coverage than any other area due to the presence of Amtrak officers. Second, we dropped a small 'block' bounded by Market Street, 32nd Street and Lancaster Avenue since the block is little more than a tiny island which contains no buildings or physical structures of any sort. Results do not change much if these blocks are included.) We aggregate without consideration for time (i.e. we collapse 6 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. (We also estimated our models by 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.)

We also obtained data on the number of parking tickets (we received data on all tickets that the Philadelphia Parking Authority issued in West Philadelphia between 2005 and 2010 from a database file that the Philadelphia Parking Authority provided to the UPPD) written by block as well as the number of traffic accidents (we received the accident data from the UPPD; the UPPD has access to all accident reports taken by the Philadelphia Police Department in the University City district) per block over the period that is covered by our crime data. Parking tickets are issued by officers of the Philadelphia Parking Authority which has exclusive authority throughout the city and, therefore, the Penn patrol zone boundary is irrelevant with respect to parking tickets. Similarly, the Penn patrol zone boundary should have no effect on the likelihood of accidents. Accident and parking ticket data were both geocoded to their nearest location (x–y-co-ordinates). We rely on these data for falsification checks against the crime data, as we would expect the Penn patrol zone to be of no consequence with respect to these outcomes.

Table 1. Descriptive statistics of crime by boundary

Variable	*N*	*Mean*	*Standard deviation*	*Minimum*	*Maximum*
Overall Penn zone					
All crime	98	71	77	3	490
Street crime	98	22	16	0	80
Property crime	98	59	67	0	397
Violent crime	98	12	14	0	93
Parking tickets	98	93	106	0	476
Traffic accidents	98	65	70	1	410
Distance	98	−601	404	−1558	−17
Inner Penn zone					
All crime	30	104	101	5	490
Street crime	30	18	15	3	80
Property crime	30	92	87	5	397
Violent crime	30	12	17	0	93
Parking tickets	30	156	138	0	476
Traffic accidents	30	76	75	5	410
Distance	30	−995	315	−1558	−74
Outer Penn zone					
All crime	68	56	59	3	384
Street crime	68	24	16	0	71
Property crime	68	44	49	0	324
Violent crime	68	12	12	0	60
Parking tickets	68	65	75	0	313
Traffic accidents	68	60	68	1	377
Distance	68	−428	306	−1263	−17
University City district (excluding inner and outer Penn zone)					
All crime	245	49	35	1	234
Street crime	245	31	19	0	96
Property crime	245	34	22	0	142
Violent crime	245	15	18	0	178
Parking tickets	245	10	15	0	78
Traffic accidents	245	30	38	1	324
Distance	245	1533	1060	54	3902

Table 1 shows that during the 6-year period for which data are available there were an average of 71 crimes per block in the Penn patrol zone. Blocks in the outer Penn campus experienced more street crime, as did blocks in the University district that were outside the outer campus. As these descriptive comparisons do not take into consideration that blocks further from the campus are different in many respects from blocks that are closer, we cannot draw any credible causal inferences from them.

A visual inspection of the data is more informative. Fig. 2 depicts crime at the block level as a function of the distance to the patrol zone boundary, and it includes a local polynomial smoother that allows for a discontinuity at the boundary. Whereas some blocks within the boundary have relatively high crime, this pattern dissipates for blocks that are close to the boundary, and there is a clear jump in crime just outside the boundary. In all likelihood, the relatively high crime rate inside the Penn campus but far from the campus boundary is a result of the much higher population density. These blocks include campus dormitories and a major university hospital, which present higher concentrations of individuals and property than blocks with individual houses or smaller apartment buildings.

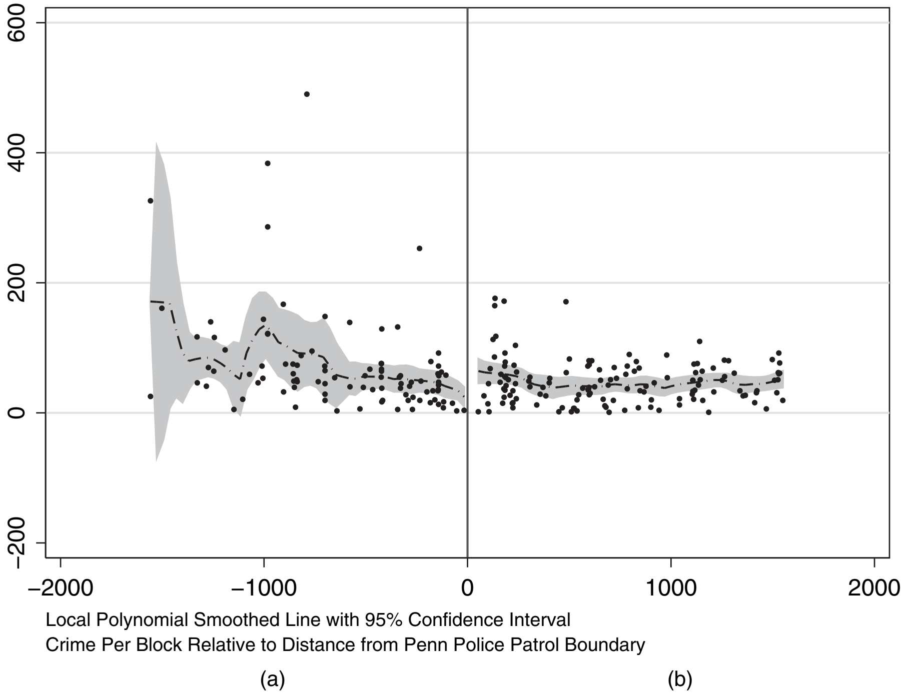

Fig. 2. Local polynomial smoothing of crime by block relative to distance from the Penn boundary: (a) inside boundary; (b) outside boundary

3.2. Regression discontinuity design

The regression discontinuity design enables causal inferences based on weaker empirical assumptions than other standard approaches in empirical microeconomics (Hahn *et al.*, 2001; Lee, 2008). Here, the primary assumption is that blocks just within the Penn patrol zone do not differ systematically on any relevant covariates from blocks just outside the zone.

The Penn patrol zone boundary constitutes a clear geographic discontinuity. The boundary is, in many respects, a historical artefact. (Maureen Rush, Vice-President for Public Safety at Penn, noted on September 27th, 2012, at the Jerry Lee Symposium on Philadelphia Experiments in Crime and Justice, that the boundary was simply chosen as a distance from Penn's campus that the police could adequately patrol without trying to incorporate all of the University City district.) Since the mid-1990s, Penn has tried to merge the university and the surrounding neighbourhood, investing heavily in infrastructure, housing, retail establishments and primary schools (Kromer and Kerman, 2004; Rodin, 2005). As a result, much of the surrounding neighbourhood is populated by Penn students and faculty making it largely indistinguishable from the area within the Penn patrol zone 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. (Close to 6000 Penn students rent apartments or houses in the University City district. The transit service for Penn students and affiliates in the University City district extends to 48th 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 6 p.m. and 3 a.m.

(see `http://www.business-services.upenn.edu/offcampusservices/cms/wp-content/uploads/grad-housing-guide-draft-2012.pdf`). Penn off-campus housing service officially lists 24 buildings for students in the University City district, of which 10 are outside the Penn patrol boundary (see `http://www.business-services.upenn.edu/offcampusservices/?p=graduate_guide/individual_building_profiles`).) The university also administers a housing subsidy programme 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 (`http://www.pennalexanderschool.org/content/what-are-boundaries-penn-alexander-school-can-my-child-attend`). Steif (2012) showed that residential property prices climbed at a substantially higher rate in the areas just inside compared with 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. (Pictures of the block faces are available on request from the authors.) In short, there is good *prima facie* evidence that blocks on either side of the patrol zone are similar in basic sociodemographic characteristics. (The block level of aggregation prevents us from assessing the level of similarity as census blocks cover both sides of the boundary.)

Assuming that blocks just inside the Penn patrol zone do not differ systematically from blocks outside the boundary on relevant covariates, we can estimate the effect of increased police deployment by using a regression discontinuity design. Intuitively, the regression discontinuity estimator compares the mean outcome of the treated units, which are on one side of the threshold (in this case, the Penn patrol zone boundary) with the mean outcome of the untreated units, which are on the other side of the threshold. Units that are far from the threshold may not be good comparators, whereas units near the threshold may represent credible counterfactuals for estimating a local average treatment effect (in this case, the effect of the extra police coverage that is provided by Penn). Imbens and Kalyanaraman (2012) provided an approach for choosing an asymptotically optimal bandwidth in the regression discontinuity setting. Calonico *et al.* (2014a) likewise provided guidance in determining optimal bandwidths, noting that many alternative approaches yield excessively large bandwidths that may result in bias. They also provided an approach to estimate valid confidence intervals. We implement the regression discontinuity estimator by using the Stata package that was described in Calonico *et al.* (2014b), and we report results based on the approaches of both Imbens and Kalyanamaran (2012) and Calonico *et al.* (2014a).

We present both conventional estimates and Calonico *et al.* (2014a) bias-corrected estimates. We also provide both conventional standard errors and the robust standard errors proposed by Calonico *et al.* (2014a). In all cases, our forcing variable is the distance (in feet) from the Penn patrol zone boundary (taking negative values for blocks within the boundary and positive values for blocks outside the boundary). Also, in all cases, we use a triangular kernel function for the local linear regression. Our outcome variable is the number of crimes during the time period that occurred in a given physical block. (We do not 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 at the block level. The same census block groups often 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. The actual population at risk of victimization is therefore much higher

Table 2. Effect of Penn Police on all crimes

	Conventional	*Bias corrected*	*Conventional*	*Bias corrected*
Change associated with crossing boundary	44	52	32	61
Conventional standard error	19†	19‡	11‡	11‡
Robust standard error		26§		15†
Bandwidth	Calonico *et al.* (2014a)	Calonico *et al.* (2014a)	Imbens and Kalyanaraman (2012)	Imbens and Kalyanaraman (2012)

†$p<0.05$.
‡$p<0.01$.
§$p<0.10$.

than the residential population. Examining the number of crimes per block is the conservative approach, since arguably more people will be available for victimization the closer one becomes to Penn's campus.)

4. Results

4.1. Regression discontinuity estimates

Table 2 examines the effect of Penn Police on all crimes by block. Depending on the bandwidth approach and whether bias correction is implemented, we find that there are between 32 and 61 more crimes outside the Penn patrol zone boundary. This represents a statistically significant increase of between 45% and 86% relative to the mean number of crimes inside the Penn patrol zone.

To examine whether a coefficient of this magnitude is common at other arbitrarily chosen boundaries, we used a permutation test that randomly reassigned distance values for all blocks 1000 times and we reran our analyses. If we use the conventional estimates for the treatment effect and the standard error (using the bandwidths of Calonico *et al.* (2014a)), we find that our test statistic (Calonico *et al.* (2014a) suggested that bootstrapping the asymptotically pivotal test statistic is the appropriate approach as opposed to bootstrapping the coefficients) of 2.32 (43.445/18.71) is exceeded in magnitude only 1.3% of the time as illustrated in the kernel density graph provided in Fig. 3. We reach a similar conclusion with the bias-corrected coefficients and robust standard errors: our estimated test statistic is exceeded in magnitude only 3.1% of the time (Fig. 4).

In Table 3, we examine street crimes. Earlier research provides some reason to believe that street crimes are more readily deterred by police patrols (e.g. Klick and Tabarrok (2005)). Consistent with this observation, we find that street crimes increase by a statistically significant 49–115% in blocks outside the Penn patrol zone boundary relative to the average number of street crimes per block within the boundary.

Table 4 presents our results for property crimes. The results suggest a smaller effect on property crimes of between 27% and 60%. The statistical significance of these estimates depends on which estimators are used.

Table 5 presents the results for violent crimes. For violent crime we found an increase between 119% and 153% associated with crossing the Penn patrol zone boundary. The statistical significance of our estimates was somewhat dependent on the specification of the estimator.

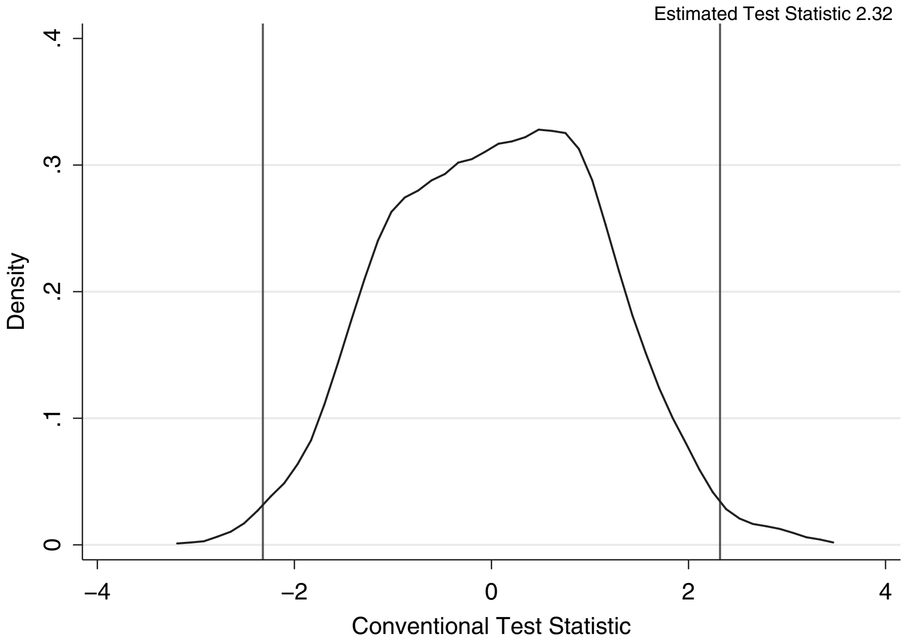

Fig. 3. Kernel density of 1000 test statistics where the distance from the Penn boundary is randomly assigned for each block

Table 3. Effect of Penn Police on street crimes

	Conventional	*Bias corrected*	*Conventional*	*Bias corrected*
Change associated with crossing boundary	26	23	11	26
Conventional standard error	8†	8‡	5†	5‡
Robust standard error		10‡		11†
Bandwidth	Calonico *et al.* (2014a)	Calonico *et al.* (2014a)	Imbens and Kalyanaraman (2012)	Imbens and Kalyanaraman (2012)

†$p < 0.05$.
‡$p < 0.01$.

In Table 6, we provide comparable estimates for each of the specific types of crime identified in Philadelphia crime reports. Perhaps the most striking result is that every category yields a positive coefficient, except homicide which has a trivially small negative coefficient (−0.05 homicides).

Although our results appear robust across crime categories, our causal inference hinges on whether the blocks on either side of the boundary are comparable on relevant observable and unobservable characteristics. Although we have provided some historical and anecdotal evidence supporting this claim, more systematic evidence would increase confidence in our causal inference. Perhaps most importantly, it may be that individuals frequenting the blocks outside the Penn boundary engage in riskier behaviour or otherwise have a higher propensity towards

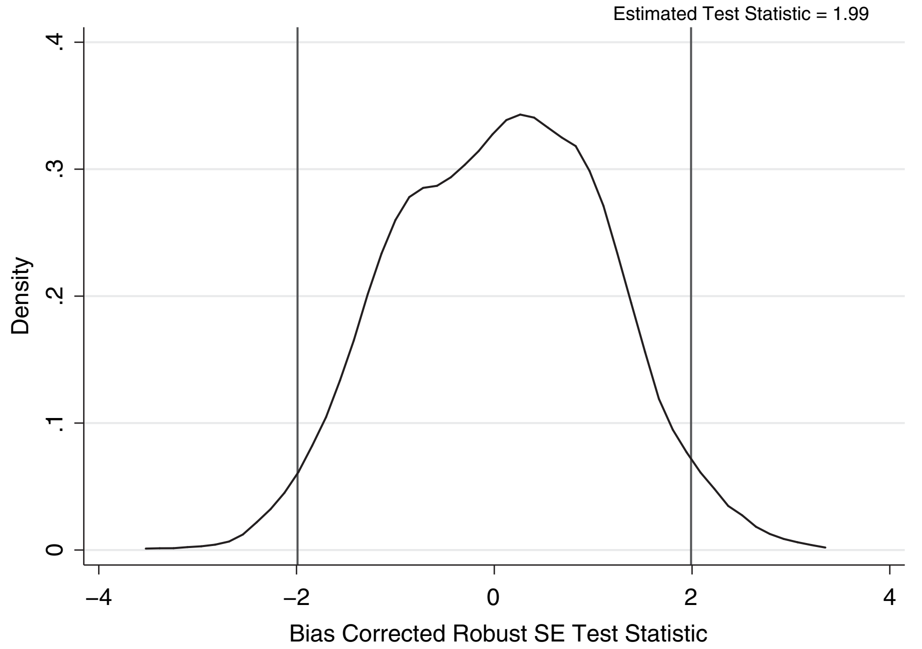

Fig. 4. Kernel density of 1000 test statistics (bias-corrected robust standard error) where the distance from the Penn boundary is randomly assigned for each block

Table 4. Effect of Penn Police on property crimes

	Conventional	*Bias corrected*	*Conventional*	*Bias corrected*
Change associated with crossing boundary	23	25	16	36
Conventional standard error	15	15†	8†	8‡
Robust standard error		22		21
Bandwidth	Calonico *et al.* (2014a)	Calonico *et al.* (2014a)	Imbens and Kalyanaraman (2012)	Imbens and Kalyanaraman (2012)

†$p < 0.10$.
‡$p < 0.05$.

criminal behaviour. Putting aside the fact that many individuals in the area spend a significant amount of time on both sides of the boundary, it is useful to examine some indicator of these risk taking or criminal propensities.

For this, we use data on the number of parking tickets written and the number of traffic accidents reported per block as falsification checks. We provide results for both measures in Table 7. There is no systematic effect of the Penn patrol zone boundary on either parking tickets or traffic accidents. Further, to the extent that either of these outcomes proxies for criminal propensity, recklessness or other risky behaviour, the coefficients would appear to cut against our crime findings, given that we find both parking tickets and traffic accidents declining outside the Penn boundary.

Table 5. Effect of Penn Police on violent crimes

	Conventional	*Bias corrected*	*Conventional*	*Bias corrected*
Change associated with crossing boundary	16	18	14	17
Conventional standard error	8†	8‡	6‡	6§
Robust standard error		11		10†
Bandwidth	Calonico *et al.* (2014a)	Calonico *et al.* (2014a)	Imbens and Kalyanaraman (2012)	Imbens and Kalyanaraman (2012)

†$p<0.10$.
‡$p<0.05$.
§$p<0.01$.

Table 6. Effect of Penn Police by crime type†

Type	*Conventional coefficient*	*Bias-corrected coefficient*	*Penn zone mean*
Assault (with gun)	0.6	0.6	0.4
Assault	4.5	4.3	4.5
Assault (aggravated)	2.7	2.9‡	1.6
Burglary	1.0	1.8	4.7
Homicide	−0.0	−0.1	0.1
Homicide (handgun)	0.0	0.0	0.0
Robbery	4.9§	5.8§§	2.8
Robbery (gun)	2.3§§	2.6§§	1.1
Sexual assault	1.5‡	1.6§	1.2
Theft (automobile)	1.7‡	1.6	2.1
Theft (bicycle)	1.1	1.9	6.9
Theft (from building)	7.3	9.4	13.1
Theft (from motor vehicle)	6.6	8.9‡	11.5
Theft (purse)	0.4	0.5	0.3
Theft (retail)	8.9‡	9.5‡	7.4
Theft (other)	2.2	2.2	13.6

†Each row represents a separate regression. The bandwidth choice procedure proposed by Calonico *et al.* (2014a) is used in each regression.
‡$p<0.10$ (conventional standard errors used).
§$p<0.05$.
§§$p<0.01$.

We also secured data on the residential addresses of Penn faculty and staff to examine whether there was a discontinuity in these measures around the Penn patrol zone boundary. We found no systematic evidence of an effect. This provides some confidence that the populations do not differ on either side of the border within the relevant bandwidth. More expansive indicators of the residential populations by block are not available given that census blocks, for which demographic and income information would be available, do not match with physical blocks. (In fact, many census blocks in our sample cover physical blocks both within and outside the Penn patrol zone boundary.)

We also estimated specifications relying on global polynomials of the forcing variable, rather than local linear regressions, finding effectively the same results across all outcomes. Although

Table 7. Falsifications

	Parking tickets		*Traffic accidents*	
	Conventional	*Bias corrected*	*Conventional*	*Bias corrected*
Change associated with crossing boundary	−15	−2	−23	−26
Conventional standard error	15	15	99	99
Robust standard error		20		120
Bandwidth	Calonico *et al.* (2014a)	Calonico *et al.* (2014a)	Calonico *et al.* (2014a)	Calonico *et al.* (2014a)

such specifications are disfavoured because of the potential sensitivity of the estimates to the order of the polynomial used (see, for example, Gelman and Imbens (2014)), we found relative stability in the treatment effect coefficient across orders 1–10. Further, such specifications allowed us to include the parking ticket and traffic accident data as covariates, finding no important change in our estimated effect of the Penn patrol zone boundary on crime.

4.2. Elasticity estimates

Personnel estimates from both the Philadelphia Police Department and the UPPD indicate that approximately twice as many officers patrol the Penn patrol zone than the surrounding University City district. The area that is covered by the Philadelphia Police in the relevant area is twice as large as that covered by the Penn Police, suggesting an effective increase in police presence of the order of 200%. Our estimate that UPPD activity is associated with a 60% (taking the midpoint of our coefficients from the all-crime specifications) reduction in crime suggests that the elasticity of crime with respect to police is about −0.33 for aggregate crime. For property crime alone, our elasticity estimate is of the order of −0.20, whereas, for violent crime, it is of the order of −0.70. These elasticity estimates are strikingly similar to those found in the modern literature on police and crime. Chalfin and McCrary (2013) provides a helpful summary of these previous estimates. Klick and Tabarrok (2005), Draca *et al.* (2011) and Di Tella and Schargrodsky (2004)—all of whom used an exogenous shock in police deployment resulting from terrorism-related events—found an elasticity of approximately −0.30. Our results are also similar to those presented in Berk and MacDonald (2010) who examined a police crackdown in Los Angeles finding 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 examined police hiring related to a federal grant programme, found an elasticity of property crimes of −0.26, though their violent crime elasticity is much higher. Chalfin and McCrary's (2013) study of crime in US cities over the period 1960–2010, which corrects for measurement error bias in prior work, found a violent crime elasticity of −0.35, though their property crime elasticity is only −0.15. As they explained, 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 with those associated with public police forces. Although 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.

4.3. Limitations

Despite our effort to use a regression discontinuity method to estimate the casual effect of adding extra privately funded public police on crime at the border of the Penn patrol zone, this study has several limitations. First, we could not identify the specific mechanisms by which the UPPD reduces crime at the boundary of their patrol zone. It is possible that their street crimes and mobile patrol units provide more vigilant coverage just inside the patrol boundary. If this is so, the benefits of the extra police may also be felt just across the boundary as criminals would become more acutely aware of the heightened police presence. In contrast, it is possible that the street crime and mobile patrol units also pay attention to crime occurring just across the boundary, so the effects of the extra police on crime may be somewhat muted in our current estimates. Future work should consider measuring how the actual deployment of personnel for a sustained period in a given location impacts crime.

An experiment that randomly varies the dosage of extra police in a given location and time could assess how localized surges of deployment impact crime. If a new lower level of crime occurs after repeated surges of police, this would suggest that it is not the actual visible presence of extra police that matters but their perceived presence. Although criminologists have speculated about the potential temporary or residual deterrence effect that police surges may have on crime (Sherman, 1990), this remains largely untested.

Second, this study cannot identify the extent to which blocks on either side of the patrol zone are similar. It is possible that the daily actions of the police inside the Penn patrol zone boundary also change the social conditions of blocks, such that the residential and daily population mix is different in ways that reduce crime. For example, if people are less fearful of crime just inside the patrol zone boundary rents may increase for shops and restaurants and foot traffic may change in ways that reduce the potential criminal activities. Collecting some daily measures of foot traffic, parking and commerce inside and outside the patrol zone would be an important way to establish the extent to which blocks are equivalent. Although we see no clear discontinuity in staff or faculty residences, it is possible that other unobserved factors are present. Identifying the ways in which blocks are identical to each other, but differ only on the basis of police presence, would strengthen this research design.

Third, it should be recognized that the study is limited in only using official crimes reported to the police. It is possible that crime is reported differently just inside the patrol zone boundary as residents and stores are more prone to report crimes if there is a greater visible presence of police. In contrast, just outside the Penn patrol zone the area is busier with crime and calls for service and has fewer police, so it is possible that crimes are being underreported. Although we have no evidence to suggest that this is happening, it is worth recognizing that we cannot be sure that extra police in an area do not change the crime reporting behaviour of victims. Reporting behaviour inside the campus boundary may also be more fastidious because of the requirements and penalties that the university faces under the Clery Act. (See 20 USC paragraph 1092(f), 'Disclosure of campus security policy and campus crime statistics'.) These effects, if present, would suggest that our treatment effect estimates are actually too low.

Our research design represents the potential to learn much more about the actual mechanisms through which deterrence occurs, leading to possible improvements in policing policies. For example, many universities and other entities use lower cost security patrols. Such patrols may generate different levels of crime reduction which would be relevant to determining the optimal mix of sworn officers and other security professionals. Also, focusing on private university security forces with arrest powers that are different from those studied here might provide insights regarding whether a security presence is sufficient to achieve the levels of deterrence that are identified here or whether the threat of arrest is a necessary element for successful security measures.

5. Conclusions

Given the importance of police protection for private firms and the tremendous welfare effect of crime, the lack of prior studies on the effects of extra police provided by private entities such as universities is an important omission. Although others 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 private entity to its surrounding neighbourhood. More research is needed to determine whether this effect generalizes to other settings.

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 2–3 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 Penn patrol zone boundary. Whether the effects are driven by the increased visibility of the police presence or concomitant increases in arrest and investigation activities we cannot estimate. Our results imply that there are 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.

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

Acknowledgements

The authors thank the UPPD, the University of Pennsylvania Office of Institutional Research and Analysis, and the Philadelphia Parking Authority for sharing data used in this study. We are also grateful to Vice-President for Public Safety Maureen Rush and her staff for sharing institutional knowledge about their police operations.

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