Private Security and Deterrence

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Abstract: Quasi-experimental estimates indicate that police are a deterrent to crime. Benefit-cost analysis using those estimates indicate that hiring more police is more than justified, with one prominent study indicating that many U.S. cities are under-policed. Less is known about the deterrence potential of private security options. Considering budget pressures and political calls to de-emphasize or even defund the police, examining the relative efficacy of private security guards compared to police takes on added importance. We present results from college campuses suggesting that private police generate deterrence comparable to that of public police, but we do not find evidence that private security guards hired by colleges generate comparable deterrence.

1, Introduction

As "defund the police" became one of the rallying cries of the "Great Awokening"[1], progressive commentators started indicating that police do not deter crime. The ACLU's Paige Fernandez declared, "We have little evidence, if any, to show that more police surveillance results in fewer crimes and greater public safety.[2]" In an article titled "Police don't stop crime, but you wouldn't know it from the news," PRISM, "an independent and nonprofit news outlet led by journalists of color,"[3] stated that "Police don't stop or prevent crime, but you wouldn't know that from how the mainstream media discusses them as the solution, parroting talking points directly from police departments. If larger police forces make us safe, then by that logic, the U.S. would already be the safest society in the world . . ."[4]

This theme was not just touted by progressive activists. Even mainstream news outlets cited the absence of correlation between police spending and reduced crime as evidence against the efficacy of police. For example, writing for the Washington Post, Philip Bump noted, "If we look at how spending has changed relative to crime in each year since 1960, comparing spending in 2018 dollars per person to crime rates, we see that there is no correlation between the two. More spending in a year hasn't significantly correlated to less crime or to more crime. For violent crime, in fact, the correlation between changes in crime rates and spending per person in

[1] https://www.vox.com/2019/3/22/18259865/great-awokening-white-liberals-race-polling-trump-2020
[2] https://www.aclu.org/news/criminal-law-reform/defunding-the-police-will-actually-make-us-safer
[3] https://prismreports.org/about/our-team/
[4] https://prismreports.org/2022/02/23/police-dont-stop-crime-but-you-wouldnt-know-it-from-the-news/

2018 dollars is almost zero."[5] Adding academic heft to the police skepticism, writing on the Brookings Institution blog, senior fellow Howard Henderson and coauthor Ben Yisrael called the evidence that police reduce or prevent crime "minimal."[6] Perhaps the most damning criticism came from Ben and Jerry's when they asked "Data Shows No Correlation Between Policing Spending and the Crime Rate — So Why Is Funding Going Up?"[7] When the police have lost the confidence of the nation's foremost number-crunching, policy-analyzing, monkey-chunking ice cream purveyors, it's all over.

These claims are misguided in principle and are wrong in their description of what research shows. Modern empirical designs demonstrate that police generate crime deterrence. Quasi-experimental methods consistently yield a statistically significant and practically important negative relationship between the number of police patrolling an area and crime. Research designs using data from across the U.S., as well as in many other countries, replicate this finding. In addition, a number of field experiment show that placing more police in high crime areas leads to significant reductions in crime with little evidence of displacement.[8] There is little doubt anymore that increasing police presence causally reduces crime.

That said, just because police reduce crime does not mean they do so in a cost-effective way. A finding that police causally reduce crime is likely a necessary condition for it to make sense to hire more officers (or even maintain current levels of police coverage), but it is not sufficient. Many rigorous benefit-cost analyses do suggest police are a good investment in basic public finance terms with their monetized deterrence benefits exceeding at least their budgetary costs. However, there are at least two limitations with these benefit-cost tests. First, they generally compare the net benefit of hiring additional officers to not hiring officers, as opposed to making alternative social investments. Second, the benefit-cost analyses focus entirely on budgetary costs without considering other economic costs of hiring police.

In the recent push to defund the police, progressive rhetoric reveals concerns with both limitations. Activists often indicate that police funding ought to be moved to education, mental health treatment, and a range of other social programs the activists assert would do more to improve crime rates. Also, while claiming police engage in corrupt, violent, and biased behavior, many commentators indicate these costs are made worse by constraints on reforming or disciplining police such as qualified immunity in civil trials and employment protections that limit firing police for bad behavior.

Private police and security guards of various types may hold promise on both dimensions. Given that private alternatives such as security guards are often much cheaper than public police, even

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[5] https://www.washingtonpost.com/politics/2020/06/07/over-past-60-years-more-spending-police-hasnt-necessarily-meant-less-crime/

[6] https://www.brookings.edu/blog/how-we-rise/2021/05/19/7-myths-about-defunding-the-police-debunked/

[7] https://www.benjerry.com/whats-new/2022/03/crime-and-police-spending

[8] https://www.nationalacademies.org/our-work/proactive-policing-effects-on-crime-communities-and-civil-liberties-in-the-united-states

if they are not quite as effective in generating deterrence, they might be more efficient on the margin. Also, because private security personnel do not receive liability protections like qualified immunity, nor civil servant employment protections, they may prove easier to incentivize or discipline than the public policing alternatives.

This chapter presents some preliminary evidence that publicly certified private police can be just as effective in deterrence terms as the estimates provided in the literature on public policing. However, our evidence indicates that the much lower cost security guards, who operate without arrest powers, do not appear to generate any deterrence at all. Although private police are not generally cheaper than public police in budgetary terms, they may prove to be more easily disciplined through civil liability and employment incentives, which would lower their true economic cost, making them a viable alternative to regular police at least in some circumstances.

This chapter proceeds by quickly discussing the quasi-experimental literature on police and deterrence as well as some of the attempts to measure the net welfare effects of police spending. We then summarize and extend our previous work on university police as an example of an effective private policing alternative. We contrast this with some new estimates on the deterrence effects of university security guards which indicate no comparable deterrence effects, noting how this distinction in the effectiveness of private police versus private security guards has some support in the existing literature.

2. Police and Deterrence

Examining the relationship between police and crime is difficult given the severe omitted variable bias problems that likely exist, such as reverse causality (places expecting higher crime levels will generally hire more police). While there are examples of field experiments in the literature of deploying more police to high crime areas,[9] these experiments were often under-powered or limited in duration leaving them unable to detect plausible deterrence effects.

Steve Levitt's (1997) paper using election-induced police hiring as a natural experiment to examine the effect of police on crime represents one of the first quasi-experimental designs tackling this issue in the economics literature.[10] Levitt's design, which found large negative elasticities between police and both property and violent crime, was clever and had a large

[9] See, for example, https://www.nationalacademies.org/our-work/proactive-policing-effects-on-crime-communities-and-civil-liberties-in-the-united-states

[10] Some antecedents in the criminology literature include papers that use natural experiments or instrumental variables designs: see Chaiken, J. M., Lawless, M. W., & Stevenson, K. A. (1974). *The impact of police activity on crime: Robberies on the New York City subway system*. New York: New York City Rand Institute; Wilson, J. Q., & Boland, B. (1978). The effect of the police on crime. *Law and Society Review*, 367-390. McCormick and Tollison (1984) represents a clever early quasi-experimental study of this issue albeit in a context that did not deal with actual police or real-world crime; instead, it examined the addition of extra referees in college basketball games, finding a significant deterrence effect with respect to personal fouls due to cleaner play.

impact on the economics of crime field. Unfortunately, the estimates were wrong, as pointed out by Justin McCrary (2002), due to coding issues. Although Levitt (2002) suggested that his results endured if the coding issues were fixed (and other changes to the research design were implemented), there are many reasons to be skeptical of Levitt's findings.[11]
Subsequent natural experiment papers fared better in their credibility. In a series of papers using police increases after terrorist attacks as natural experiments, Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), and Draca, Machin, and Witt (2011) all find remarkably similar crime elasticities with respect to police on the order of -0.3 in Buenos Aires, Washington DC, and London respectively.

Finding a similar estimate for the elasticity between property crime and police (though only statistically significant at the 10 percent level) at a magnitude just under -0.3 and a much larger (and clearly statistically significant) elasticity with respect to violent crime, Evans and Owens (2007) use funding from the federal COPS grant program to serve as the identifying shock in their research design to test if more police reduce crime. Mello (2019) also finds that federal COPS grant program under the 2009 Recovery act to hire additional police also lead to qualifying departments to see a significant reduction in crime. The implied elasticity of victimization costs was -1.17 per police force size. Although federal hiring grants are arguably less clearly exogenous than the papers that rely on terrorism as mechanism for identifying changes in police deployment, the hiring grant design has the benefit of estimating the effect of police over a longer time span (years) as opposed to the short interventions examined in the terrorism papers (ranging from just a few days to a few weeks). Examining the longer time frame is likely more relevant for the policy choice of how many police to hire, as the terrorism papers might be capturing the maximal effect of police whereas the average effect is more meaningful in the policy decision. On the other hand, the longer time frame likely means that Evans and Owens (2007) and Mello (2019) are estimating the joint effect of deterrence and incapacitation which could necessitate an untangling when calculating the benefits of police, whereas the terrorism papers likely are capturing just deterrence given the short time periods involved.

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[11] Our favorite reason to be skeptical is "The Wire" hypothesis otherwise known as "Juking the Stats." In the HBO series The Wire, police administrators were pushed by local politicians to improve Baltimore's crime numbers in advance of elections to give the appearance that the incumbent politicians were being effective. As described in Season 3 Episode 1 (Time After Time): Police Commissioner Burrell relays Mayor Royce's directive by saying, "Gentlemen, the word from on high is that felony rates, district by district, will decline by five percent before the end of the year," followed by his Deputy Commissioner Rawls emphasizing, "We are dealing in certainties; you will reduce the UCR felonies by five percent or more or, I've always wanted to say this, 'let no man come back alive.'" The district majors in the show respond by falsifying the crime data by purposely reclassifying crimes into lesser categories or hiding the crimes altogether in various ways. If this fictional behavior mimics reality, as suggested by series creator David Simon (see, for example, https://www.pbs.org/moyers/journal/04172009/transcript1.html), it would imply that Levitt's natural experiment is potentially conflating two effects of election-induced police hiring: 1) any actual crime deterrence and 2) downward crime measurement error bias.

Other modern studies have reached similar conclusions when studying quasi-random (MacDonald, Fagan, & Geller, 2016) and randomized field experiments regarding policing tactics, such as hot spot policing (Braga & Weisburd, 2022 for a review of the experimental and quasi-experimental hot spot studies). Other studies have examined the effect of policing particular sub-populations, finding largely consistent results.[12]

Chalfin and McCrary (2017) provide an extensive overview of this literature; the punchline is that most credible modern studies find persuasive evidence that police causally reduce crime, and the effect is substantively large. While the negative relationship between police and crime is not observable in naïve correlational analyses, quasi-experimental methods that isolate causal effects are about as close to unanimous in their conclusions as one can expect in an academic literature. Chalfin and McCrary (2018) themselves add another layer of confidence in this literature by directly addressing measurement error in estimating the effect of police on crime by using multiple police measures as instruments for each other, yielding comparable elasticity estimates as those found in the quasi-experimental literature, with much more precise estimates than available in most of the literature.

3. Value of Police

With a reasonably confident estimate of the causal effect of police on crime, it is possible to provide some valuation of the contribution of police. Chalfin and McCrary (2018) provide a rigorous analysis based on their police deterrence estimates and a broad range of estimates for valuing the estimated crime reductions. Using a fully loaded average annual cost of a police officer of $130,000 per year and the mean estimate from the value of a statistical life literature of $7 million per murder deterred as well as standard values for other crimes averted, Chalfin and McCrary find that, in the typical U.S. city in their sample, spending an additional $1 on police yields $1.63 in crime costs avoided if all their estimated crime declines come from deterrence. If they account for the possibility that additional police will lead to the incarceration of more individuals, their estimated return on a dollar of police spending declines to $1.31, an amount that still generates a large net social value increase. Based on these calculations, Chalfin and McCrary conclude that U.S. cities are under policed. This is consistent with an earlier welfare analysis done by Klick and Tabarrok (2010) using a range of elasticity estimates from the literature and a range of crime cost estimates that suggested that an increase in spending on police in the range of 5 percent to 155 percent could be justified based on the existing literature.

While the preceding welfare calculations follow standard benefit-cost methods, they are limited in at least two respects. First, optimality requires an analysis of alternative investments even if we restrict ourselves to a very narrow range of options. Specifically, for our purposes here, it is necessary to examine whether there exist police substitutes that might provide an even bigger return for the investment. Second, the foregoing benefit-cost analyses focus only on budgetary costs of police, when the social cost of police is a larger set than what merely shows up in a city's books. For example, settlements for lawsuits filed against the police are not typically accounted for in the total costs of policing. These broader social costs, however, are very

[12] For example, see Berk and MacDonald (2010) for an example of police targeting homeless encampments.

difficult to estimate and monetize and so we will regrettably largely ignore them even though a thorough welfare analysis needs to reckon with these costs. However, we will categorically note that some portion of these off the books costs of police arise due to misbehavior of the police and these costs are compounded when they go unaddressed due to legal and employment protections police receive.

4. Private Police as an Alternative

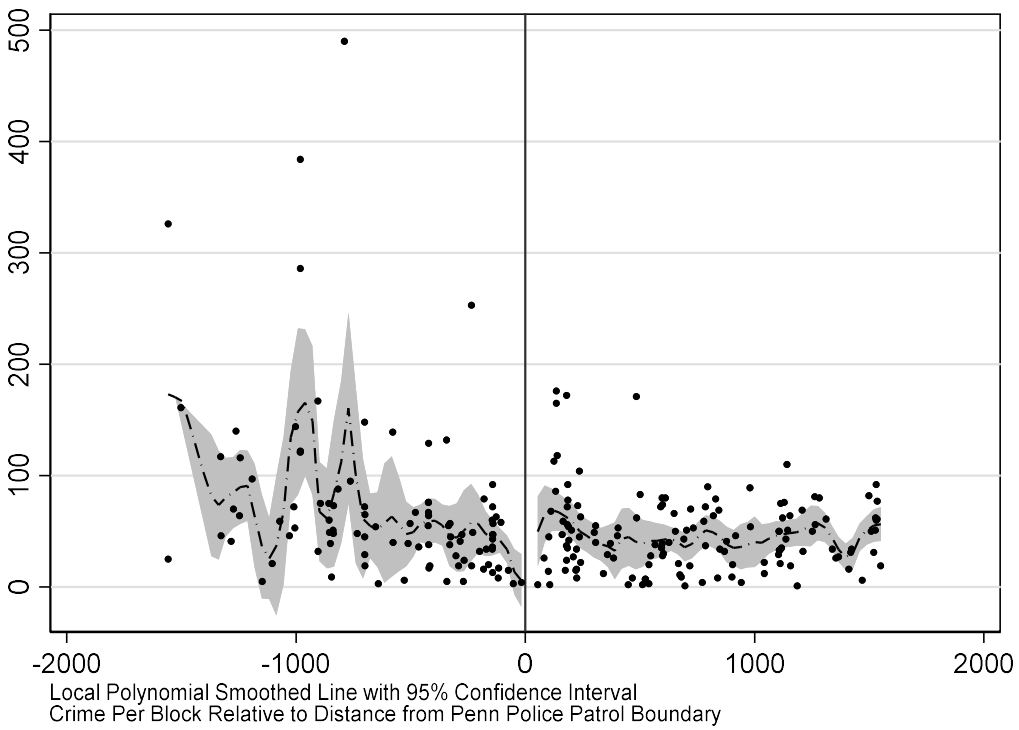
MacDonald, Klick, and Grunwald (2016) examine the phenomenon of private police. These are police paid by private entities but who otherwise are quite similar to public police and are state certified law enforcement officers. They wear uniforms, carry guns, and make arrests. Given their similarity, it is reasonable to ask whether it is appropriate to treat the private police as a separate category. We think there are at least a few reasons why private police could be functionally different from public police. Relevant to a concern raised above, because private police do not benefit the employment protections that come with being a civil servant (and a strong police union), the unquantified costs mentioned above might be assumed to be lower.[13] Beyond that, private police operate under different management structures which could make them less (or, perhaps, more) effective than public police.

Specifically, the police studied by MacDonald, Klick, and Grunwald are campus police at the University of Pennsylvania (Penn). Penn police patrol and have arrest powers within a specifically designated boundary around the campus. Past that boundary, the Philadelphia city police have jurisdiction. Although the boundary originally tracked the university's campus fairly well, in the modern day, much of the university activity spills across the historic boundary, though the Penn police must still abide by the boundary. An uninformed visitor would be hard pressed to be able to tell where the boundary even is. Given the relative wealth of Penn, it is not surprising that Penn employs about three times as many police per geographic unit than does Philadelphia. Putting this effectively arbitrary boundary together with the lopsided police coverage generates a plausible regression discontinuity design through which MacDonald, Klick, and Grunwald attempt to isolate the causal effect of private police on crime.

Figure 1 below duplicates the total crime discontinuity graph from MacDonald, Klick, and Grunwald (2016) where 0 on the x axis represents the Penn boundary and distance from the boundary is measured in feet along the x axis with negative distances falling within the campus boundary and positive distances extending outside the boundary. Crime is aggregated by physical city block in the Penn patrol zone or university city neighborhood surrounding the campus. The crime data cover the period 2005-2010.

Figure 1: Total Crime as a Function of Distance to Penn Boundary (in ft)

[13] At least in some instances, these private police do benefit from legal protections such as qualified immunity. See, for example, Fleck v. Trustees of Univ. of Pennsylvania, 995 F. Supp. 2d 390 (E.D. Pa. 2014) which treated private police paid by the University of Pennsylvania as state actors based on the fact that Pennsylvania state law "endows the Penn Police Department with the plenary authority of a municipal police department in the patrol-zone territory, once the "exclusive prerogative" of the City of Philadelphia," though in the same case, Penn's security guards were not found to be state actors.



There is a statistically significant increase in total crime when crossing the boundary going outside of the Penn police patrol zone. However, in new graphs Figure 2 and Figure 3 presented below (not from the original paper), it is reasonably clear that the jump in crime is driven by property crime as opposed to violent crime.

Figure 2: Property Crime as a Function of Distance to Penn Boundary (in ft.)

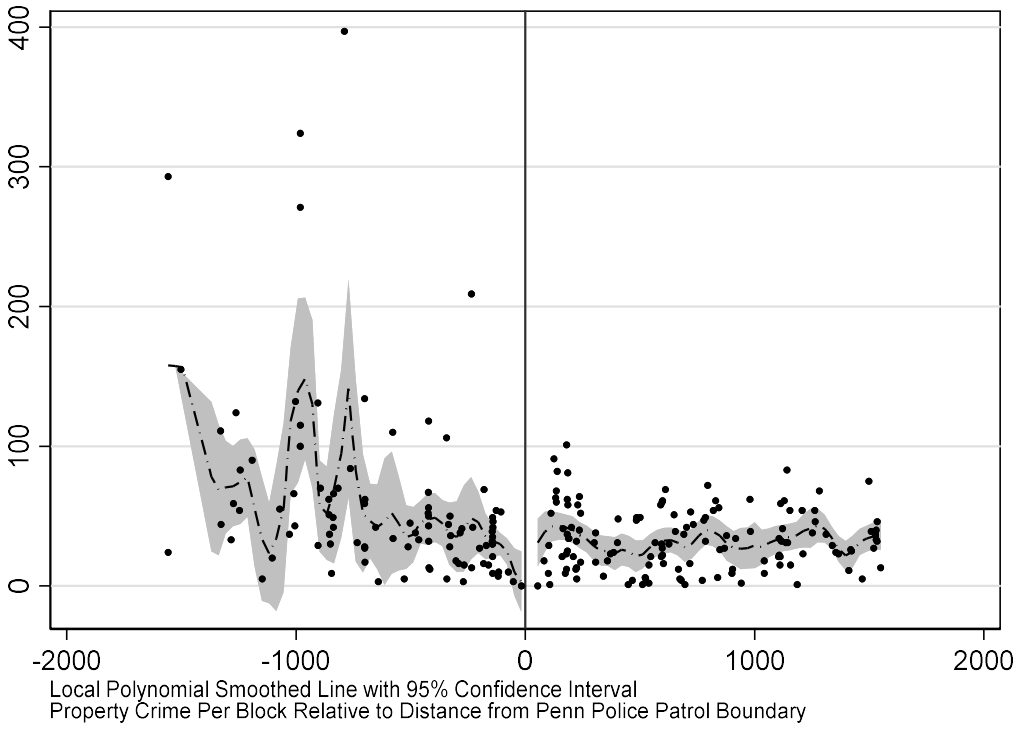
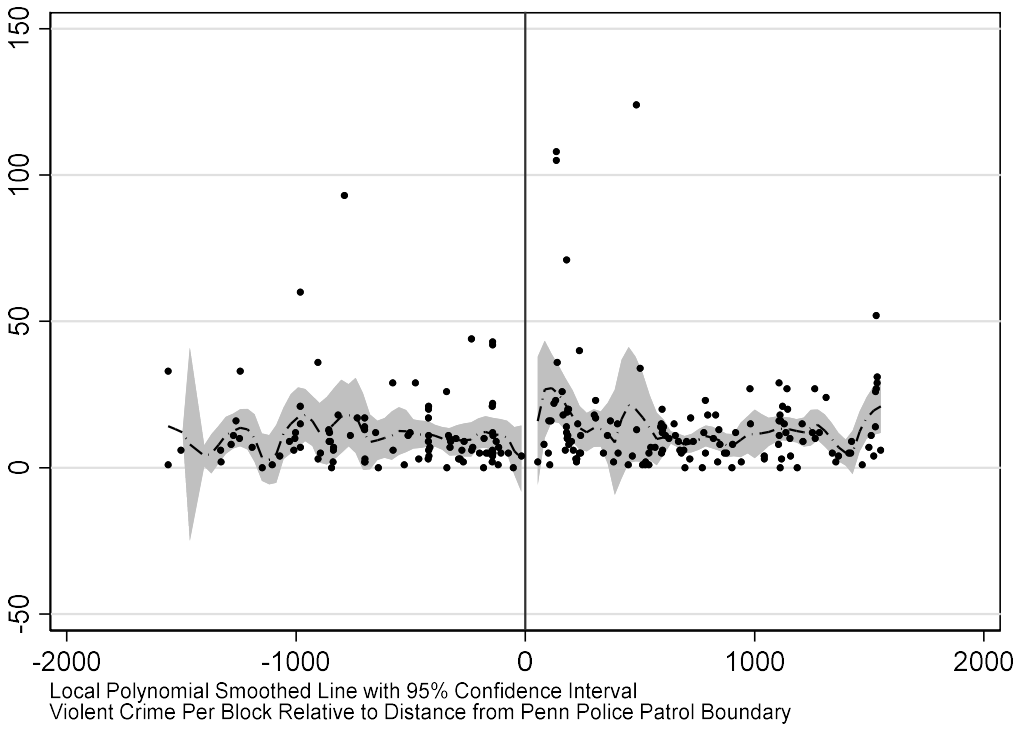


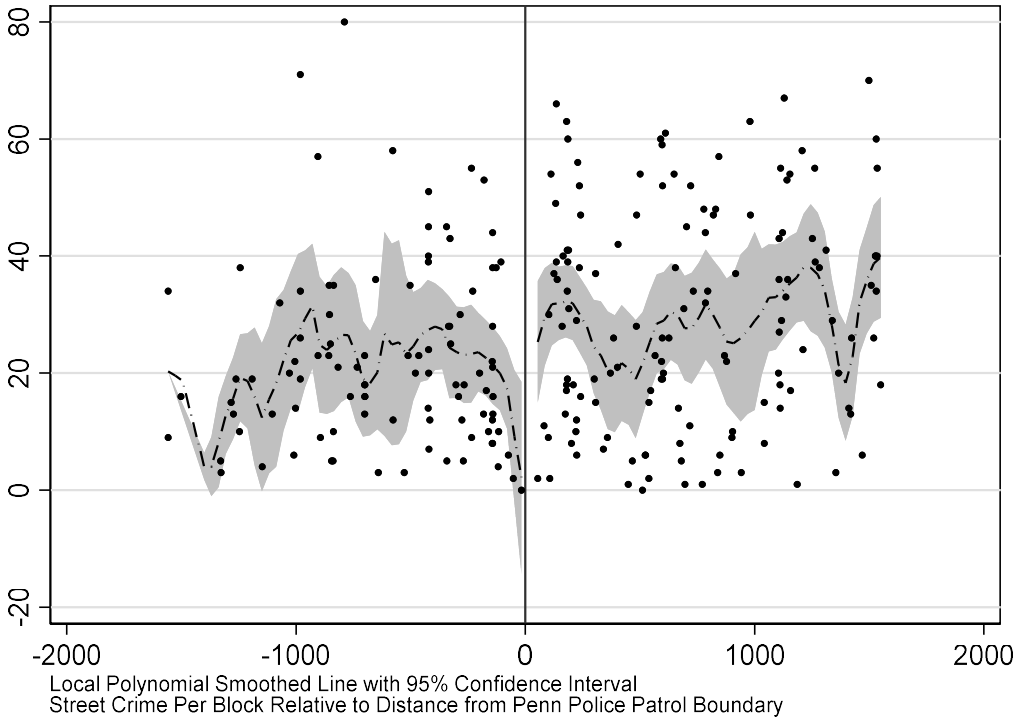
Figure 3: Violent Crime as a Function of Distance to Penn Boundary (in ft.)



While the discontinuity is evident for the property crime, the violent crime discontinuity is well within the confidence intervals on either side.

Figure 4 provides a more ad hoc grouping of crimes that we dub "street crimes" and includes assaults, burglaries, purse snatching, robberies and theft from vehicles to try to capture the opportunistic nature of these crimes and their potential greater sensitivity to police presence which is largely borne out in the graph.

Figure 4: Street Crime as a Function of Distance to Penn Boundary (in ft)



The more formal regression discontinuity estimates suggest that there is an elasticity of total crime with respect to police on the order of -0.3 with the elasticity ranging from -0.2 for property crime and -0.7 for violent crime with both estimates being statistically significant, although there is variation in statistical significance for specific individual crime categories. The differences in counts of each category means that one cannot directly compare the point estimates from these elasticities. However, the bottom line is that the differences when estimated by a formal regression are all statistically and substantively significant.

To avoid the possibility that there are other unobservable changes at the campus boundary, we examined both the issuance of parking tickets, which is handled by a non-police entity both on Penn's campus and outside of it, and occurrence of traffic accidents. We examined these falsifications in case there is a hard to quantify difference in risk-taking propensity or the fastidiousness of rule-following on either side of the boundary as these attributes could also influence crime. In neither case did we observe a statistically significant discontinuity.

Figure 5: Parking Tickets as a Function of Distance to the Penn Boundary (in ft.)

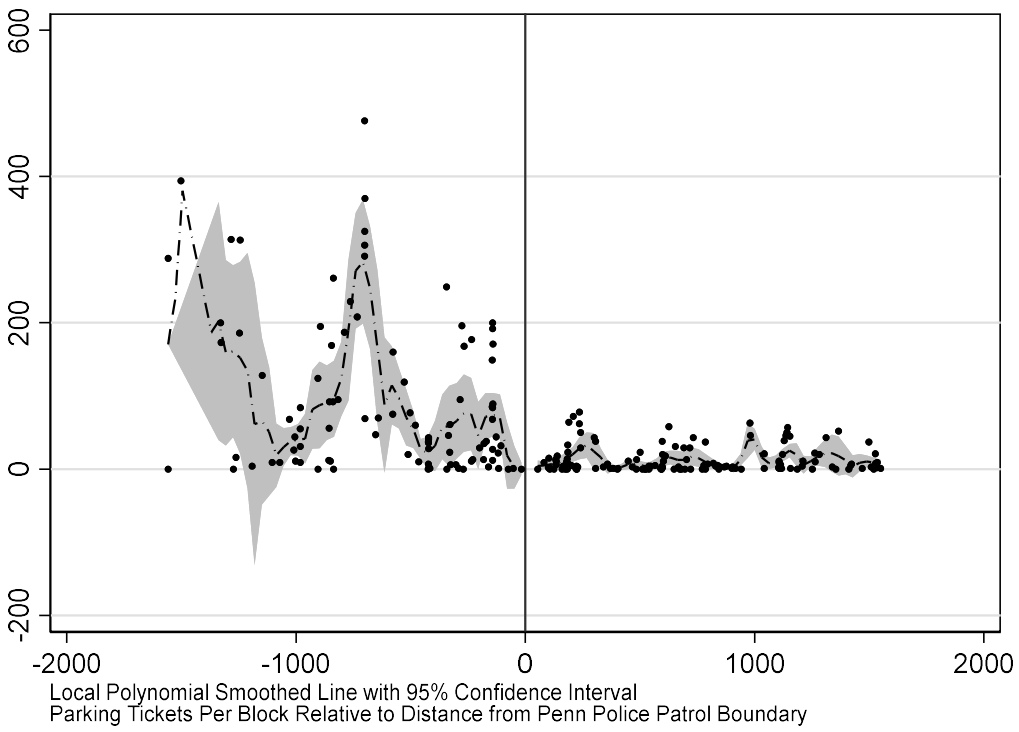
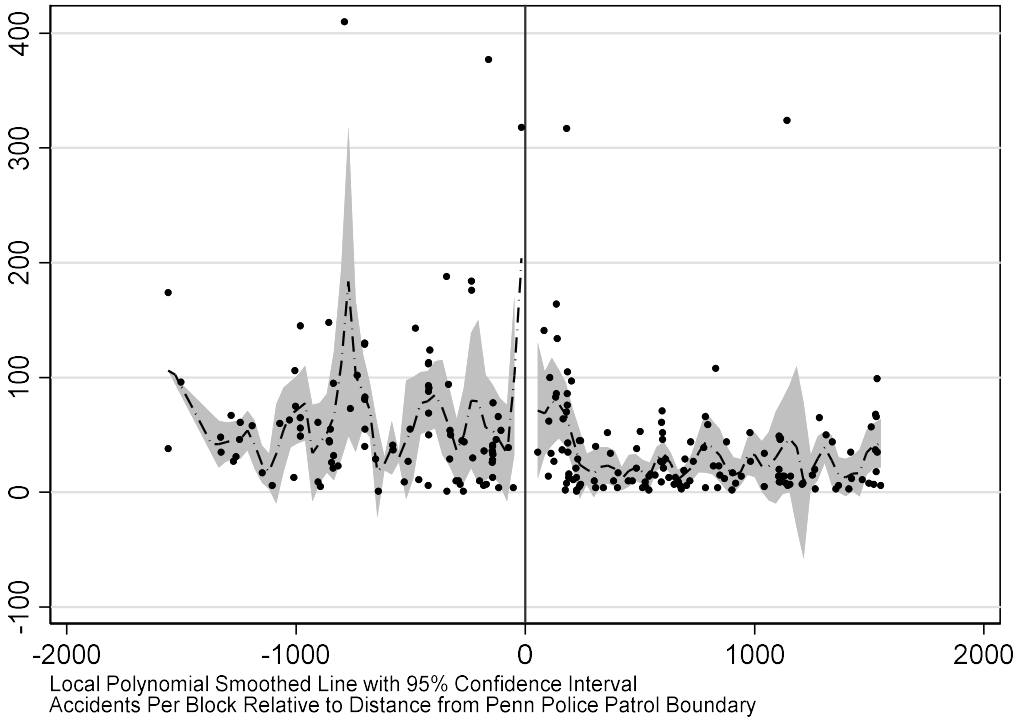


Figure 6: Traffic Accidents as a Function of Distance to the Penn Boundary



The falsifications give us no reason to believe our crime estimates are not causal. Given that they are in the range of previous estimates from the literature and that Penn's fully loaded police

salary is comparable to the average salary used by Chalfin and McCrary (2018), the net welfare generated by these private police is comparable, perhaps with the benefit that wrongdoing by Penn's private police is more easily remedied and disciplined than it is with public police. The basic design and results of our Penn study were replicated in Heaton, Hunt, MacDonald, and Saunders (2016) for the University of Chicago's campus police.

5, **Private Security Guards as an Alternative**

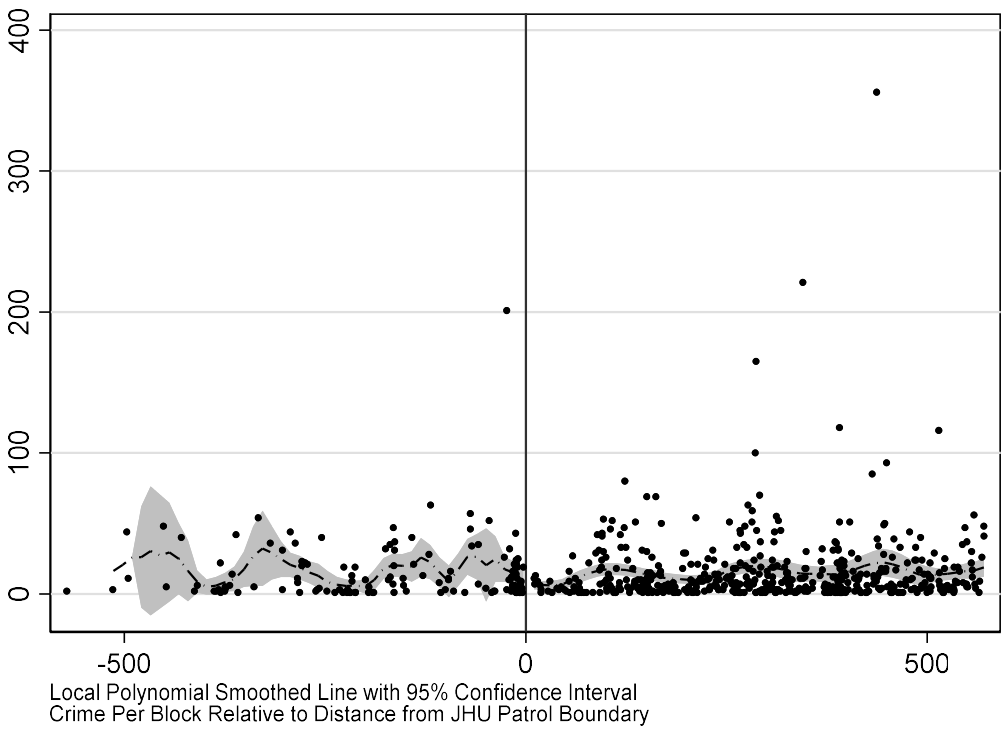
We duplicated our Penn study using data from Johns Hopkins University (main Homewood campus[14]) for the period 2012-2018. Johns Hopkins is interesting since it does not have private police officers but instead has many private security guards that are not state certified law enforcement officers, are unarmed, and have no arrest powers, largely because, at the time, the state of Maryland barred private entities from providing their own police. Like Penn, however, it is an academically rigorous school located in a relatively high crime city.

Studying security guards is useful for several reasons. In the context of this chapter, security guards provide a much cheaper option, with fully loaded salaries well less than half that of their police counterparts, as well as the soft benefits of potentially being able to address wrongdoing more easily and quickly. Again, we aggregate crime to the physical city block level over the sample period and we used the distance to the campus boundary as the forcing variable in our regression discontinuity design.

As seen in Figure 7, we find no discontinuity in total crime at the campus boundary.

Figure 7: Total Crime as a Function of Distance to the JHU Boundary

[14] https://publicsafety.jhu.edu/assets/uploads/sites/9/2022/08/Homewood-Campus-Boundary.pdf



Further examination of finer crime categories continued to yield no discontinuity at the campus boundary.

This finding suggests that cheaper private security options that are not publicly certified law enforcement officers do not yield the same (or any) benefits as do private or public police.[15] While having limited power, unarmed security might seem attractive in a debate focused on police misconduct, especially in instances where violence escalates, there is no deterrence and so it is unlikely that private security guards can be justified on a crime basis. These results correspond with those of Fabbri and Klick (2021) who likewise find that unarmed, limited authority security guards in a residential neighborhood in Oakland had no enduring effect on crime.

6, **Conclusions**

The debate regarding the effect of police on crime has been clouded by a misreading or unwillingness to acknowledge the research evidence on the deterrent effects of the police on crime. Bindler and Hjalmarsson (2021) use digitized records of historic crime data and police deployment and find that the creation of the London Metropolitan Police in 1829 led to a significant decline in crime relative to adjacent municipalities, suggesting that the deterrent effect of the police on crime is a historical and contemporary reality. Less is known about the

[15] These results bolster the causal interpretation of the Penn police study. If one thought there was something unobservable about behavior on a university campus that was correlated with crime but was not driven by police, we should have expected to find a discontinuity on the Johns Hopkins campus even though they had no police on duty.

deterrent potential of private police or private security, options that take on increasing importance in an era where calls for police reform are occurring and the US is experiencing a significant rise in gun violence in major cities (MacDonald, Mohler, & Brantingham, 2022) that is spreading across neighborhoods at a faster rate than cross sectional differences in places (Brantingham et al., 2021). In this chapter we show that there is a growing body of evidence that private police are also effective at reducing crime around universities, whereas private security appear to have minimal effect. Importantly, the costs of police are a relative bargain compared to the costs of crime victimization they avert in society. Private police may even be more cost effective if they are able to innovate and are held more accountable to the public they serve, though this point is speculation and not one that we can settle with empirical evidence at this point. Future work should investigate the optimal set of institutional arrangements that make for cost effect private police that remain accountable to the public.

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