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The Ineffectiveness of 'Observe and Report' Patrols on Crime

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Abstract: The deterrence effect of police on crime has been well established using modern quasi-experimental micro-econometric methods. Although the results from these studies uniformly suggest that police spending is cost justified, it is worth exploring whether police-like alternatives can deter crime even more cheaply. Unarmed private security personnel that conspicuously patrol a neighborhood have the potential to cheaply leverage the ability of police to be informed of crimes while also providing direct deterrence on their own. In the Fall of 2013, a neighborhood in Oakland, CA mounted a campaign to provide observe and report security patrols to augment the publicly provided policing in the area. While the initial effect of the additional security was a drop in crime, it quickly evaporated, calling into question the value of security forces that do not have the ability to apprehend criminals directly.

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Introduction

The empirical micro literature demonstrates that police have a large deterrent effect on crime. Although Levitt's (1997) important contribution was later shown to be in error (McCrary 2002; Levitt 2002), subsequent credible work finds that elasticities of crime with respect to police are likely substantial at least over very short periods (Di Tella and Schargrodsky 2004; Klick and Tabarrok 2005; and Draca, Machin, and Witt 2011). These short-term elasticities are largely borne out over the longer term in designs that, while perhaps not sharing as dramatic a shock for identification purposes as the short-term studies, likely involve more policy relevant settings (Evans and Owens 2007; MacDonald, Klick, and Grunwald 2016). In a benefit cost framework, these estimates imply that police provide considerable social value with Chalfin and McCrary (2018) suggesting that an additional dollar spent on police leads to more than \$1.60 in social benefit, implying that many jurisdictions may be substantially under-policed.

That said, depending on the mechanism by which police generate deterrence, it might be possible to generate these benefits at substantially lower cost. If the mere presence of a conspicuous monitor generates deterrence, uniformed security guards are likely to be much cheaper than police due to reduced training costs as well as the possibility of hiring personnel outside of the civil service and/or police union framework. Security guards acting in an observe and report capacity might also effectively leverage police manpower, serving systematically as additional eyes and ears on the street.

The work on Business Improvement Districts (BIDs) at least hints in this direction (Brooks 2008; Cook and MacDonald 2011). In BIDs, neighborhoods voluntarily pay more in taxes to supply additional public goods, including increased security. These analyses have generally found that the spending leads to reductions in crime, but it is not generally possible to sort out the effects of each individual portion of the spending, which in addition to supplying security guards also generally includes spending on sanitation projects and security cameras. Also, the degree of community support required for the creation of the BIDs may raise some endogeneity concerns due to changes in unobservable civic engagement.

To focus on a plausibly exogenous shock that arguably isolates the effect of observe and report type security patrols alone, we examine the introduction of unarmed patrols in the Lower Rockridge neighborhood of Oakland in late 2013. These patrols were financed by a crowd funding campaign after a resident was the victim of an armed robbery at a carpool station in the neighborhood. Using geo-coded crime data from 2007 to 2016, we examine the effect of these patrols on crime using difference in difference and synthetic control methods. Although the initial effect of these patrols was to lower crime, this decline disappeared within six months.

These results suggest that conspicuous monitors are insufficient to generate the deterrent effect associated with police. Presumably, as criminals learn that the monitors do little more than is done by regular residents alone, their criminal behavior resumes unimpeded. The hope that lower cost observe and report security patrols might prove to be lower cost substitutes for police officers is not borne out. For private security to generate comparable deterrence, it appears as though something like the armed patrols with arrest powers studied in MacDonald, Klick, and Grunwald (2016) might be necessary.

In the following sections, we discuss the value of estimating the deterrence effect of alternative security patrols as substitutes for police patrols. We also provide background for the private security patrol intervention in Oakland, describe the data, research design, results, and conclusions.

The Potential Value of Non-Police Security Patrols

As discussed in the introduction, the best estimates suggest that police generate significant deterrence. This stands in contrast to many of the other crime policy levers available (Chalfin and McCrary 2017), and the value of that deterrence more than offsets the budgetary costs associated with employing the police officers. Using an annual fully-loaded cost estimate of \$130,000 per officer, Chalfin and McCrary (2018) find that if one uses the mean estimate for the value of a statistical life from the labor economics literature to value police-deterred homicides and basic valuations for other averted crimes, a dollar spent on police generates a return of more than 60 percent. Basic public finance principles then would suggest that many more police should be hired.

However, political constraints and recruiting² and training realities might limit expanding police forces substantially at least in the short run. Further, if other police-like personnel can generate comparable deterrence at lower cost, they may represent a superior investment. The campus police studied by MacDonald, Klick, and Grunwald (2016) generate similar deterrence levels as those associated with publicly provided police studied in the literature in general, but their cost levels are comparable, most likely since they receive similar training which is necessitated by the fact that they carry guns and have arrest powers within their campus jurisdiction.

Lower level security personnel, which will generally not be armed, nor will they have arrest power, are available at significantly lower costs. Unarmed security guards might cost as little as \$10-\$20 per hour.³ BLS estimates the annual mean income of security guards as less than \$31,000 per year, as compared to the \$65,000 figure for police officers in 2017. The cost gap may be even larger than is suggested by these income figures since benefit costs for police are substantial and civil service protections will make it costly to dismiss an officer. These ancillary costs will be substantially lower for security personnel.

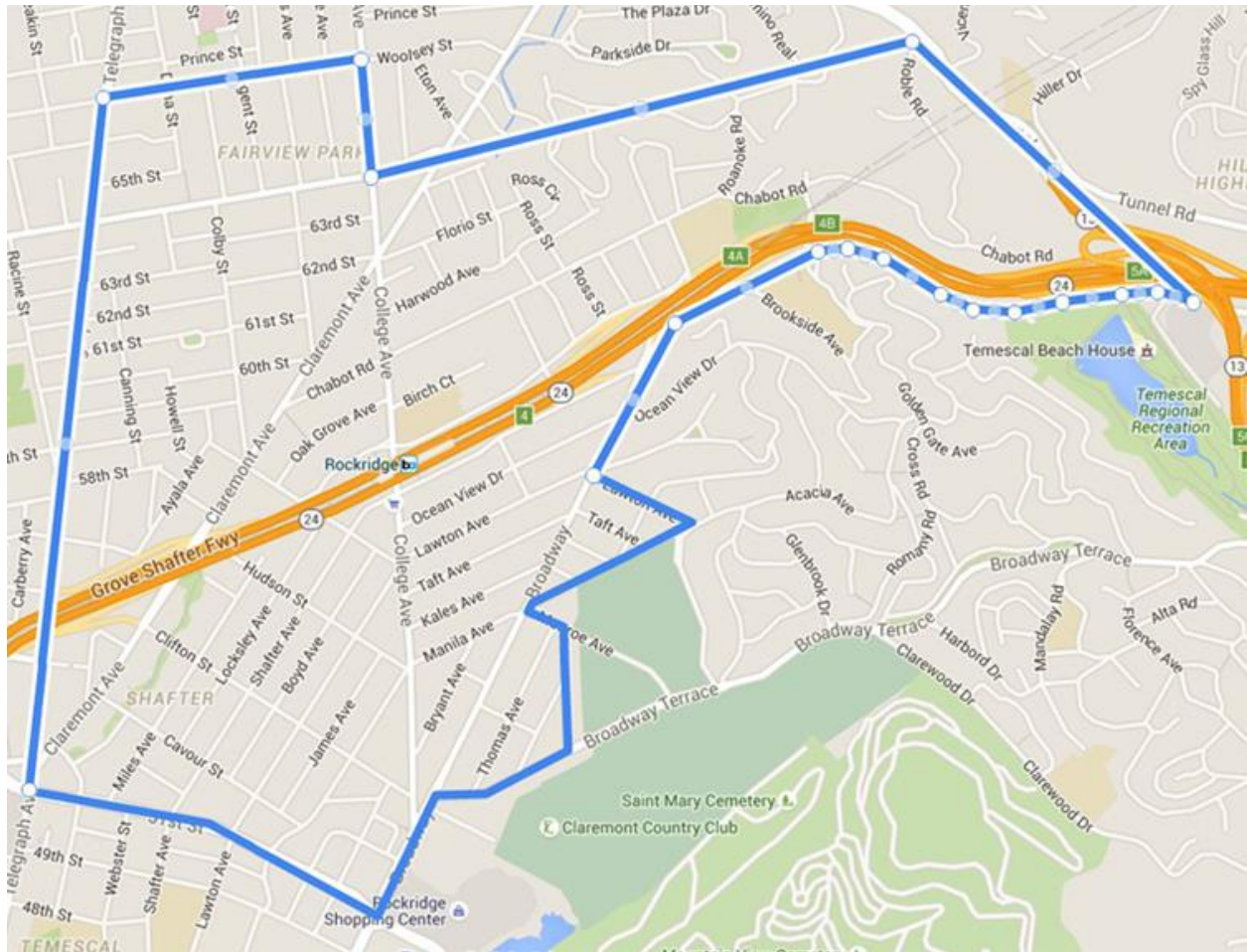
The Safer Rockridge Initiative

On September 2013 a resident of Lower Rockridge, a neighborhood in the city of Oakland, California, launched a crowd-funding campaign for a private security patrol after experiencing an armed robbery at a carpool station. The specific area proposed for the patrol was the area north of Highway 24, west of College Ave, east of Telegraph Ave., and south of Woolsey St (Lower Rockridge North/West in Figure 1).

Figure 1:

² When Washington, D.C. significantly increased its police force in 1989-1990, it ended up hiring gang members and other candidates who might not have been optimally placed as police officers. See Keith A. Harriston and Mary Pat Flaherty, D.C. Police Paying for Hiring Binge, The Washington Post, August 28, 1994, page A1.

³ <https://unitedamericansecurity.com/how-much-do-security-guards-cost/>



After the announcement of this crowd-funding campaign, two other campaigns were formed in immediately adjacent neighborhoods (Lower Rockridge South and Lower Rockridge North/East). Each of the crowd-funding campaigns successfully funded. Subsequently, the patrol for Lower Rockridge North/East was combined with North/West (forming Lower Rockridge North) and the boundary for Lower Rockridge South was slightly extended. Patrol services began on November 4, 2013.⁴ The area covered by Safer Rockridge patrols is reported in Figure 1. During the period covered by the analysis, Safer Rockridge patrolling activity varied between 35 and 45 weekly hours and involved one or two security officers at time patrolling the area by car or by foot. The patrol officers employed by Safer Rockridge are unarmed.

The mission of the Safer Rockridge organization is to “increase the safety of Lower Rockridge by providing unarmed eyes and ears only private security patrols to the neighborhood.”⁵ The security patrol officers are trained to observe and report criminal activity, and their first response to any criminal or safety concern will be to contact the Oakland Police Department. “The primary purpose of the patrols is to create a visual deterrent to crime, to operate as our eyes and ears during peak crime hours,

⁴ A crowdfunding campaign named “CrowdTilt” was used for the first four months of the patrols, through February 2014. Subsequent funding has come from community contributions to a new non-profit organization, Safer Rockridge, that was created by the organizers of the original CrowdTilt campaigns.

⁵ <http://www.rockridgehome.com/charitable-mission.html>

and when appropriate, to communicate and coordinate with the police.”⁶ Patrol members are recognizable by their service uniforms and their primary duty consists of observing and reporting suspicious activities. Active intervention to help anyone in need “when it is safe to do so.”⁷

The service offered by the security officers is independent from an individual's contribution to the Safer Rockridge organization. Therefore, an officer will not engage in more frequent patrolling of any specific area within the stated boundaries, even if the location hosts residents who contributed a larger share of the Safer Rockridge revenue. Similarly, if a specific street within the patrolling area does not include contributors to Safer Rockridge, the service remains the same as in the rest of the patrolling area.

The Effect of the Safer Rock Program on Crime

We collapsed the data⁸ to the monthly beat level starting with data from January 2007 running through October 2016. In Table 1, we provide descriptive statistics on crime levels both in the Safer Rock area and outside of it. We break down the data both before the Safer Rock guards were employed and after.

| Table 1: | | | | |
|--------------------------------------|-----------------|---------------|-------------------------|---------------|
| Mean Monthly Crime Levels | | | | |
| (Standard Deviations in Parentheses) | | | | |
| | Safer Rock Area | | Outside Safer Rock Area | |
| | Before 11/2013 | After 11/2013 | Before 11/2013 | After 11/2013 |
| Total Crime | 100 (30) | 66 (30) | 51 (71) | 24 (38) |
| Property Crime | 84 (26) | 59 (29) | 37 (52) | 18 (29) |
| Violent Crime | 16 (8) | 7 (5) | 14 (20) | 6 (11) |

In our first examination, we estimate a standard difference in difference model with beat level fixed effects and period fixed effects, clustering our standard errors at the beat level. In Table 2, we examine total crime, property crime, and violent crime.

| Table 2: | | | |
|---|-------------------|-------------------|-----------------|
| Effect of Safer Rock Patrols Difference-in-Difference Model | | | |
| (Standard Errors Clustered by Beat) | | | |
| | Total Crime | Property Crime | Violent Crime |
| Safer Rock Intervention | -6.62** (3.23) | -5.47** (2.37) | -1.15 (0.89) |
| Beat Fixed Effects | Yes | Yes | Yes |

⁶ <http://saferrockridge.org/about-us/our-mission>

⁷ <http://saferrockridge.org/about-us/about-the-patrols>

⁸ Data are available at <http://data.openoakland.org/dataset/crime-reports>

| | | | |
|----------------------|-----|-----|-----|
| Period Fixed Effects | Yes | Yes | Yes |
|----------------------|-----|-----|-----|

In the basic difference-in-difference model, the Safer Rock intervention generates a statistically significant reduction in total crime and property crime, but the reduction in violent crime is not statistically significant. Proportionately, the overall crime reduction is about 7 percent as is the drop in property crime.

If we examine a shorter window, namely six months before and six months after the intervention, the effects are substantially larger as seen in Table 3.

| Table 3: | | | |
|---|---------------------|---------------------|----------------|
| Effect of Safer Rock Patrols Difference-in-Difference Model – 6 Months Before and After | | | |
| (Standard Errors Clustered by Beat) | | | |
| | Total Crime | Property Crime | Violent Crime |
| Safer Rock Intervention | -20.68*** (1.86) | -20.72*** (1.34) | 0.05 (0.64) |
| Beat Fixed Effects | Yes | Yes | Yes |
| Period Fixed Effects | Yes | Yes | Yes |

In Table 4, we allow for a baseline shift effect of the interaction and then additional individual period treatment effects.

| Table 4: | | | |
|--|---------------------|---------------------|--------------------|
| Effect of Safer Rock Patrols Difference-in-Difference Model – Changing Over Time | | | |
| (Standard Errors Clustered by Beat) | | | |
| | Total Crime | Property Crime | Violent Crime |
| Safer Rock Intervention | -27.08*** (1.78) | -31.32*** (1.45) | 4.24*** (0.44) |
| Additional Effects of Intervention | | | |
| Months 4-6 | 3.82** (1.66) | 10.01*** (1.29) | -6.19*** (0.46) |
| Months 7-9 | 11.18*** (1.65) | 18.63*** (1.36) | -7.45*** (0.43) |
| Months 10-12 | 8.98*** (1.65) | 11.65*** (1.36) | -2.68*** (0.42) |
| Months 13-15 | 39.32*** (1.52) | 45.04*** (1.20) | -5.73*** (0.49) |
| Months 16-18 | 75.27*** (1.63) | 82.23*** (1.38) | -6.95*** (0.49) |
| Months 19-21 | 52.72*** (2.03) | 58.34*** (1.45) | -5.62*** (0.72) |
| Months 22-24 | 7.43* (3.91) | 13.98*** (2.61) | -6.55*** (1.34) |
| Months 25-27 | 6.96** | 12.68*** | -5.73*** |

| | | | |
|----------------------|--------------------|--------------------|--------------------|
| | (2.73) | (1.95) | (0.85) |
| Months 28-30 | 13.80*** (1.87) | 19.46*** (1.43) | -5.66*** (0.54) |
| Months 31-33 | 5.00*** (1.74) | 10.70*** (1.35) | -5.69*** (0.52) |
| Months 34-35 | 21.10*** (1.94) | 27.50*** (1.42) | -6.41*** (0.60) |
| | | | |
| Beat Fixed Effects | Yes | Yes | Yes |
| Period Fixed Effects | Yes | Yes | Yes |
| | | | |

Table 4 makes it clear that for total crime and property crime, the initial reduction in crime associated with the Safer Rock intervention dissipates substantially after month six and in many subsequent periods the joint effect (baseline effect plus specific period effect) is not significantly different from zero. For violent crime, there appears to be no deterrence effect of the Safer Rock intervention.

One concern with the foregoing analysis might be that the difference-in-difference approach is inapt as there are substantial differences in crime levels and trajectories across police beats in Oakland. If the regressions from Tables 2-4 are re-estimated allowing for differential period fixed effects for the Safer Rock area in the pre-intervention period, for each of the three outcomes, the Safe Rock specific pre-period fixed effects are jointly significant (in each case $p < 0.01$). This draws into question whether the difference-in-difference approach can credibly isolate the causal effect of the Safer Rock intervention.

To search for a suitable comparison beat, we re-ran the original Table 2 difference-in-difference specification using the Safe Rock data and iteratively using each other beat as a single comparison jurisdiction, including the Safe Rock specific pre-intervention period fixed effects. Although many potential comparators led to seemingly suitable comparisons (with Safe Rock specific pre-intervention period fixed effects being jointly insignificant), the treatment effects were all over the map for each outcome, as seen in Figures 2, 3, and 4 below. These figures show kernel densities for the estimated treatment effects from the regressions in which the Safe Rock specific pre-intervention period fixed effects generate F statistics with p values greater than 0.1.

Figure 2:
Treatment Effects Using Each Different Comparator

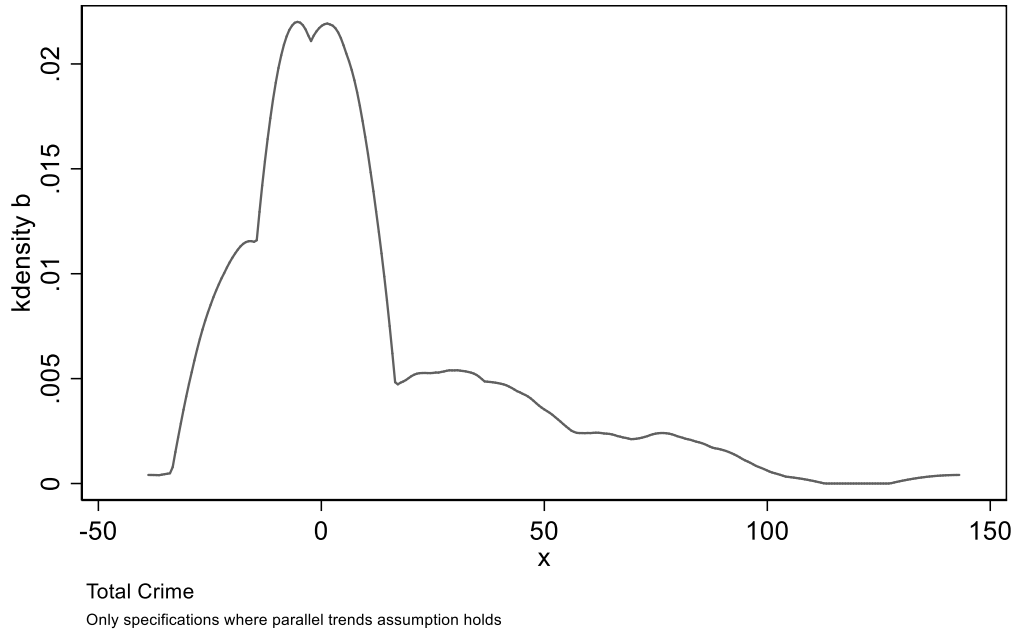


Figure 3:
Treatment Effects Using Each Different Comparator

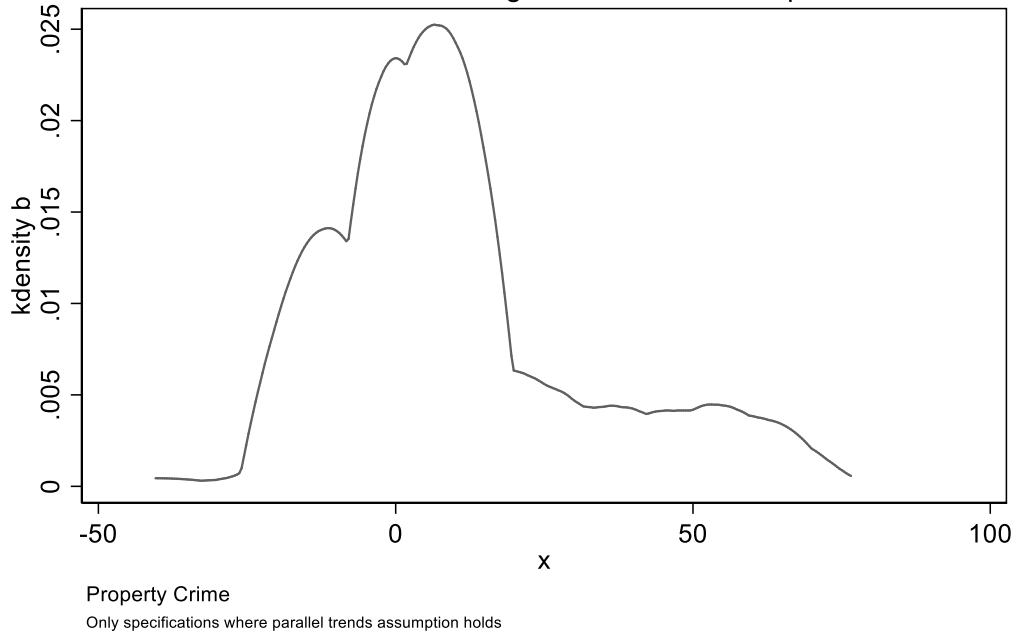
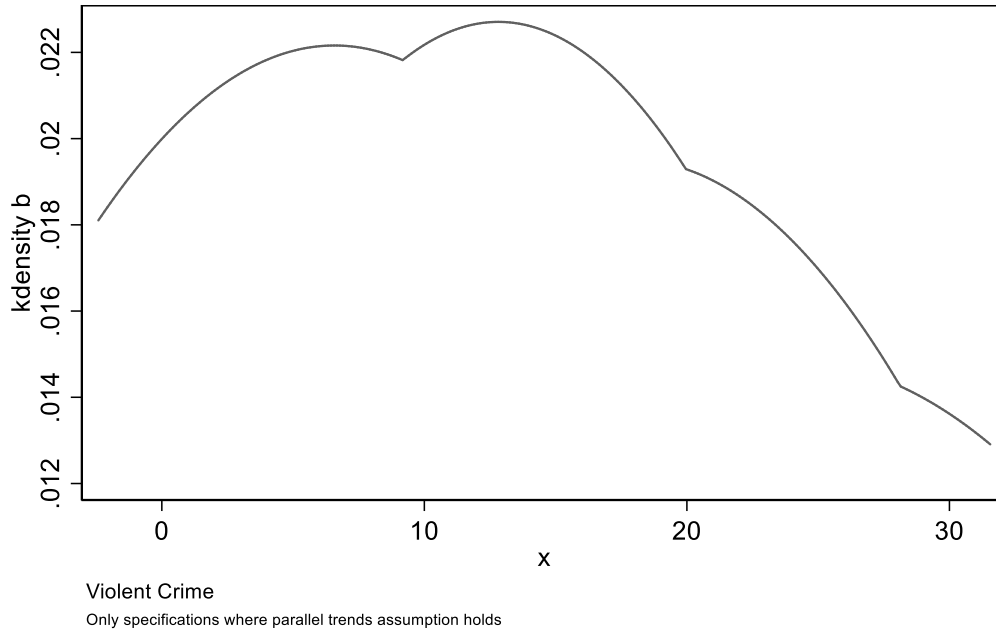


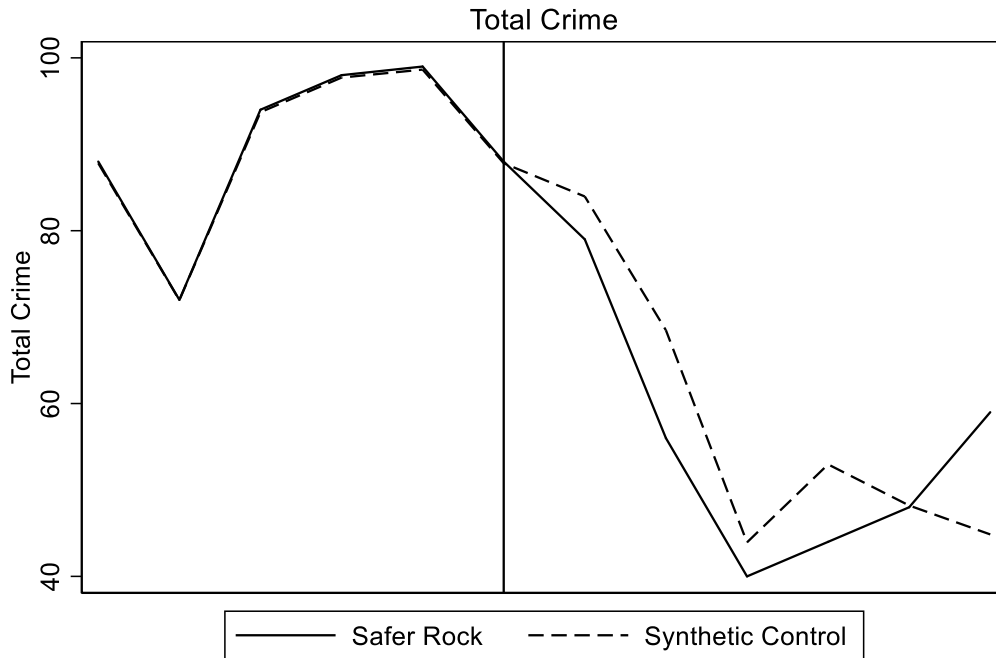
Figure 4:
Treatment Effects Using Each Different Comparator



Synthetic Controls

Because of the absence of an obvious comparison beat, we implement the synthetic control approach of Abadie, Diamond, and Hainmueller (2010) to find the weighted combination of comparator beats that provides the best pre-period match for Safer Rock.

Figure 5: Synthetic Control -- 6 Months Before & After



As suggested in the difference-in-difference estimates, there is a decline in crime immediately after the Safer Rock intervention, but it dissipates within 6 months as seen in Figure 5. However, as distinct from the earlier estimates, the synthetic control estimates provide confidence that Safer Rock and the comparison beats are plausible comparators at least in the pre-period, making them presumptively good counterfactual comparisons in the post intervention periods. When the estimated effects for the first five periods after the intervention are examined, only the first three are statistically significant, and even then, only at the 10 percent level.

Figure 6 tells the same story for property crime.

Figure 6: Synthetic Control -- 6 Months Before & After
Property Crime

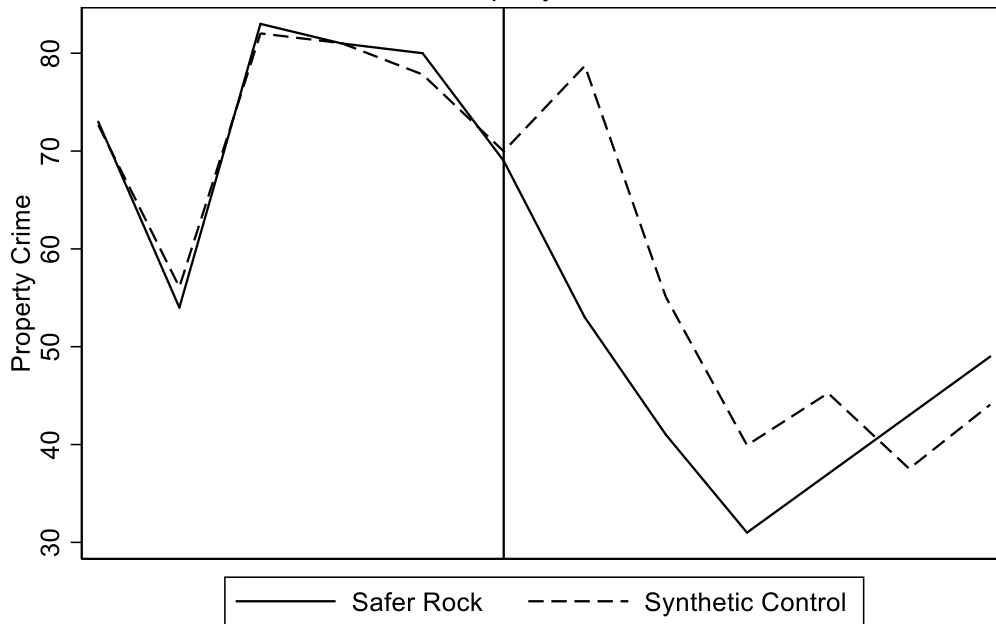
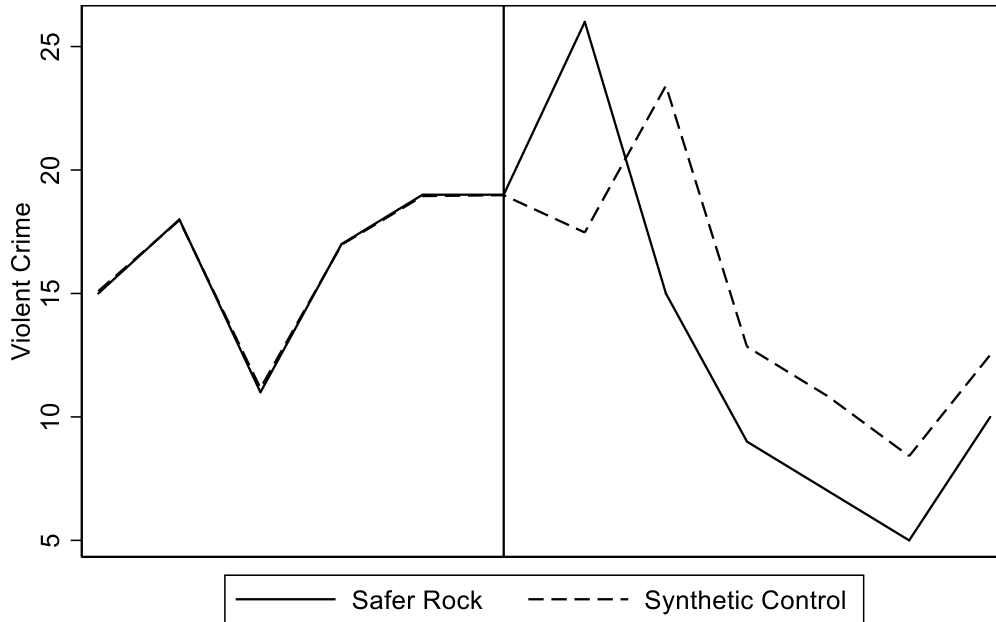


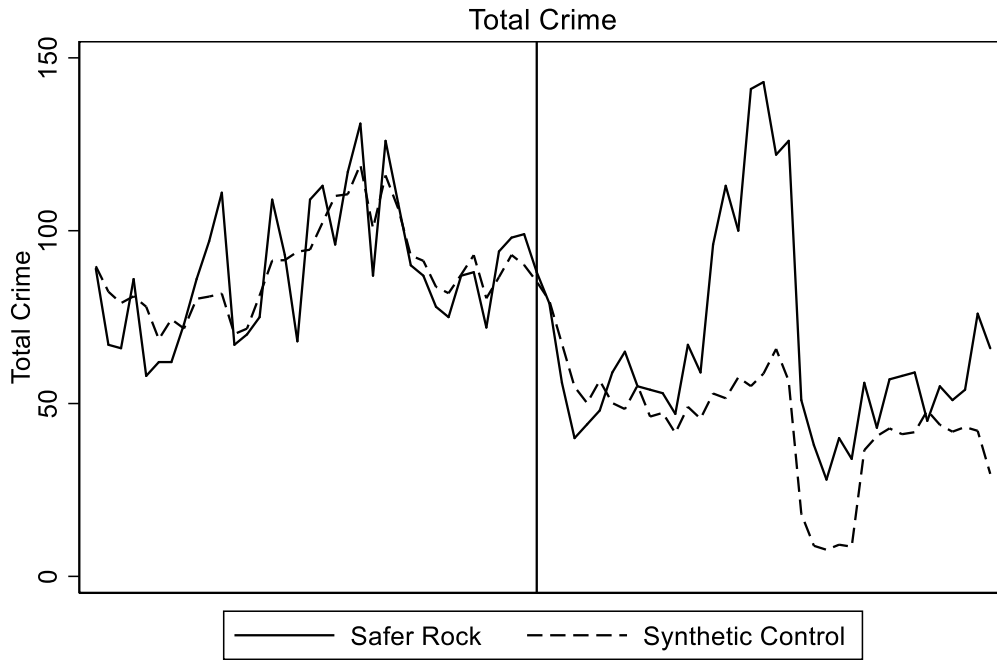
Figure 7 indicates a slightly different pattern for violent crime. Although the immediate response after the intervention is for violent crime to increase in Safer Rock, violent crime then declines relative to the synthetic comparator. These effects (including the initial increase) are statistically significant at the 10 percent level, but not at the 5 percent level.

Figure 7: Synthetic Control -- 6 Months Before & After
Violent Crime



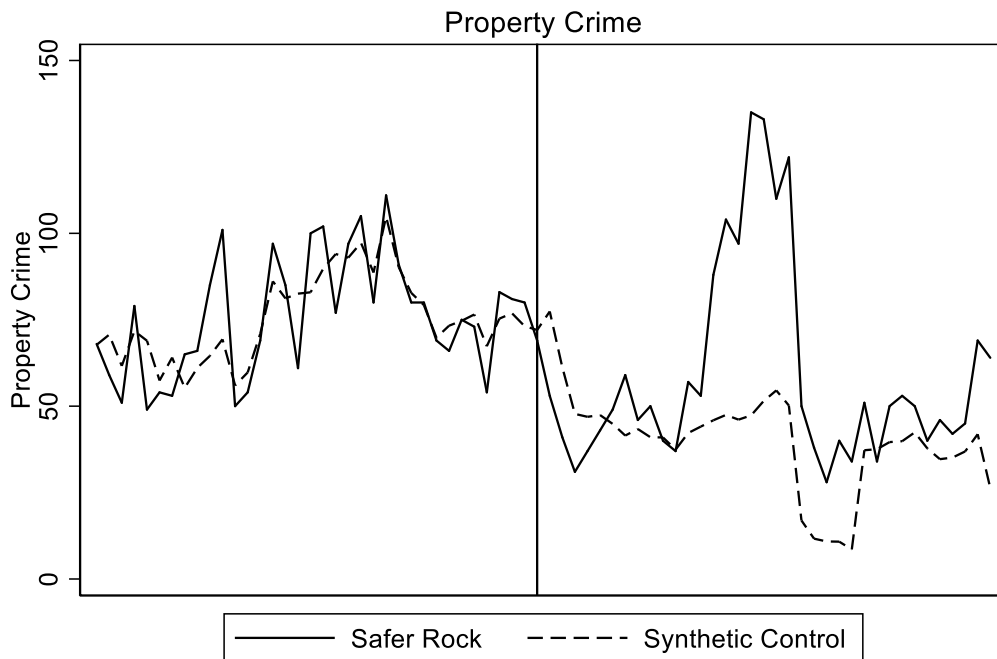
To examine the longer term effects, we again use the synthetic control approach, this time using 36 months before and after the intervention (i.e., all of our post intervention data and an equal number of pre-treatment periods). In the longer time period, as seen in Figure 8, total crime reacts as before. That is, while there is a very short term, statistically insignificant, decrease in crime, it disappears before the sixth month after the introduction of private security patrols in Safer Rock.

Figure 8: Synthetic Control -- 36 Months Before & After



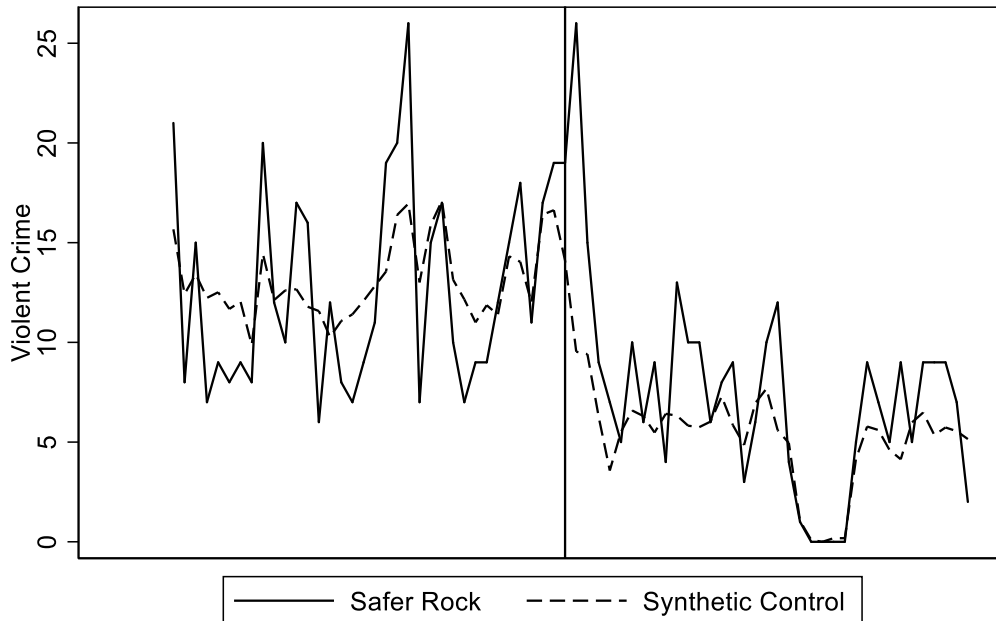
The same is true with respect to property crime, as seen in Figure 9.

Figure 9: Synthetic Control -- 36 Months Before & After



The analysis of the longer violent crime period, presented in Figure 10, indicates that the earlier finding of the Safer Rock intervention being associated with a potential decline in violent crime is not robust.

Figure 10: Synthetic Control -- 36 Months Before & After
Violent Crime

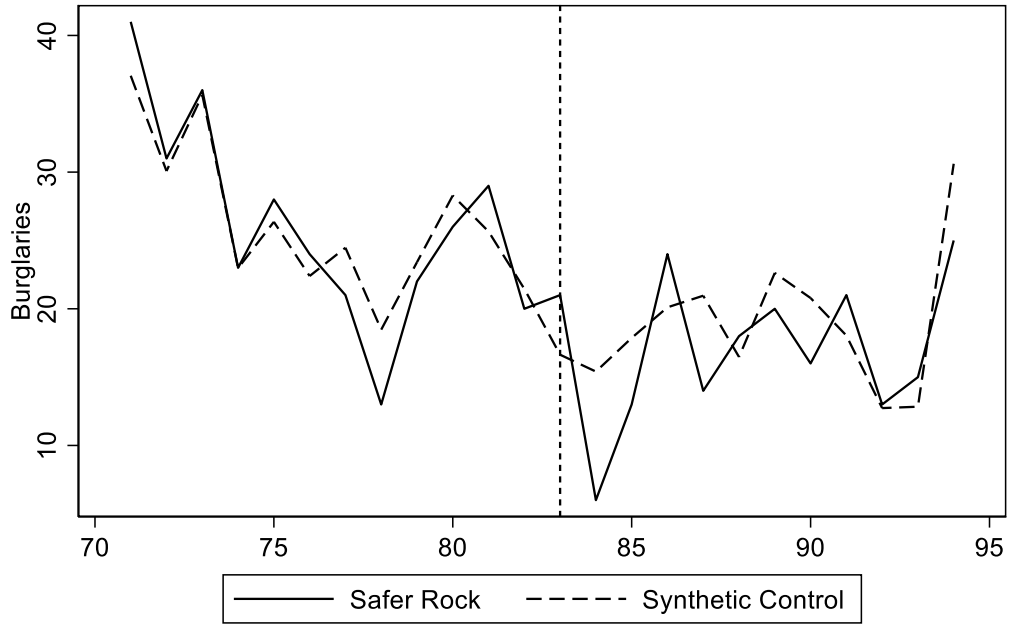


Burglaries and Robberies

The Safer Rockridge neighborhood organization reports a more positive analysis of the security guard intervention.⁹ Using difference-in-difference and Bayesian state-space models, Liu (2014) focuses on burglaries and robberies finding a decline in these crimes of almost a quarter through October 2016. As suggested above, the difference-in-difference approach is problematic here given that the rest of Oakland on average is not a particularly good comparator for the Safer Rock area. Figure 11 presents an analysis of burglaries 12 months before and after the Safer Rock intervention using the synthetic control method.

⁹ See <http://saferrockridge.org/faqs>, "Are the patrols effective?"

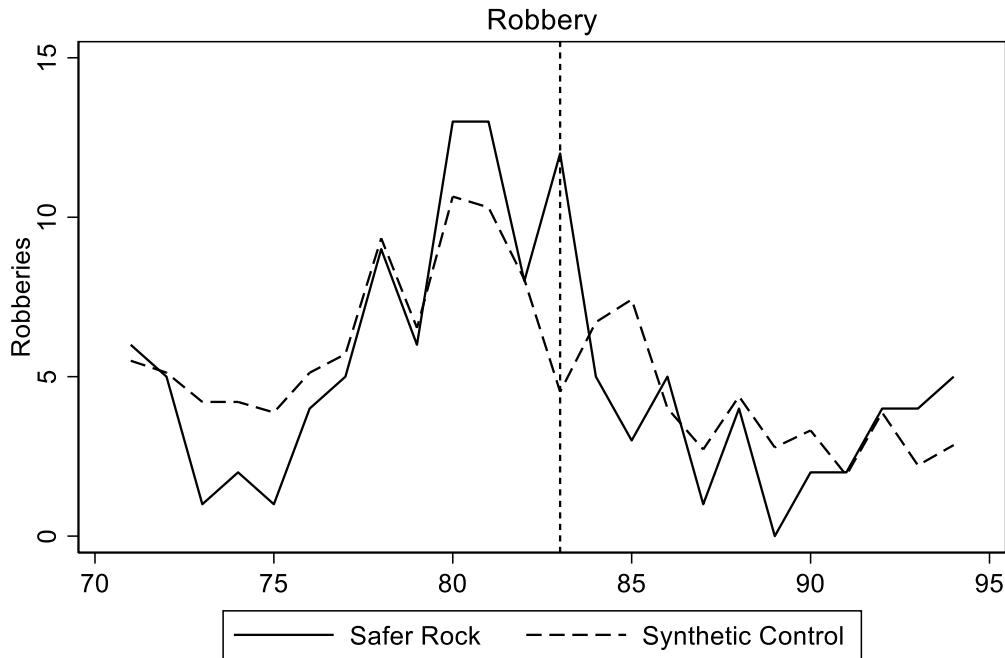
Figure 11: Synthetic Control -- 12 Months Before & After
Burglary



Much like the foregoing analysis, any effect of the Safer Rock intervention is short lived even with respect to burglaries specifically.

Figure 12 examines robberies.

Figure 12: Synthetic Control -- 12 Months Before & After



Admittedly, the pre-period match for robbery is not as good, but there is no consistent effect of the Safer Rock intervention on robberies. Based on these analyses, it appears as though the positive conclusions drawn by the Safer Rock organization are driven by inapt comparisons between their neighborhood and all of the other police beats in Oakland, which do not provide a suitable basis for comparison.

Conclusion

Although it is tempting to hope that relatively low-cost security guards will provide comparable deterrence effects to police. Substituting toward security guards could represent a large cost savings without giving up the crime reduction benefits that have now been well-demonstrated with respect to police. Unfortunately, our analysis suggests that any deterrence benefits of security guards are short-lived at best. Using difference-in-difference and synthetic control methods, we find that deploying security guards in the Safer Rock area of Oakland generated small crime reductions that lasted no longer than a few months.

Our finding, in some sense, is consistent with a rational actor model of crime augmented with learning effects. Because the Safer Rock security guards could not do anything more than generic citizens always could do (i.e., call the police to report a crime), they should not have an enduring effect on crime if criminals are rational. While it may take some time for criminals to understand that security guards do not represent a meaningful change in the likelihood of punishment, our results suggest that criminals learn this lesson relatively quickly. The question of the relative effectiveness of security guards as

compared to police officers is an important one for evaluating self-help expenditures like those undertaken by the Safer Rock community and by other private actors such as universities.¹⁰

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¹⁰ See the analysis done by Johns Hopkins University where it compares expanding its existing security guard staffing with implementing a campus police force (<https://publicsafetyinitiatives.jhu.edu/interim-study-report/>).