The Race Between Deterrence and Displacement: Theory and Evidence from Bank Robberies*

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Abstract

Security measures intended to deter crime may unwittingly displace it to neighboring areas, but evidence of displacement is scarce. We exploit precise information on the timing and locations of all bank robberies and security guard hirings and firings in Italy over a 10-year period to estimate the deterrence and displacement effects of guards. We find that hiring a security guard lowers the likelihood that a bank is robbed by 35-40 percent, though over half of this reduction is immediately displaced to nearby banks that are unguarded. A simple theoretical model of displacement reveals ambiguity in policies to mitigate these spillovers. Our findings suggest that policies that restrict the use of guards in sparse, rural markets and that require the use of guards in dense, urban markets could be socially beneficial.

Keywords: deterrence, displacement, spillover, policing, bank security guards
JEL classification codes: K42

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1 Introduction

Households and firms spend over a trillion dollars annually on security measures to prevent crime.[1] These private expenditures are intended to complement similarly sized public expenditures. Perhaps unsurprisingly, a large literature in the social sciences has emerged to estimate the effectiveness of these measures to *deter* crime.[2] Moreover, the findings of this literature are increasingly used by public and private decisionmakers to evaluate the allocation of law enforcement resources and compare the effectiveness of private security measures.

An important side effect of any criminal deterrent is its potential to *displace* crime. Broadly speaking, displacement is the effect of a security measure in one unit on crime in neighboring units, where "neighbors" may be defined along dimensions of time, space, or crime type, and units may be defined as individuals, firms, or geographic areas. While criminal displacement should not affect an evaluation of the private benefits of deterrence, it may lead to an overestimate of the social benefits of deterrence, prompting a public intervention.

Surveying the criminology literature, Braga (2005) and Bowers et al. (2011) find little evidence of crime displacement and more evidence of a diffusion of benefits, though a central issue in all of these studies is that criminal perceptions are unobservable and, therefore, areas of displacement are likely to be misspecified.[3] The identification of displacement effects, while potentially important, raises multiple endogeneity and measurement issues.

First, because crime is determined in an equilibrium between potential criminals

[1] In the US alone, households and firms spend nearly half a trillion dollars (Chalfin, 2013). The OECD estimates that the US accounts for 40% of spending on security by member states (*The Security Economy, OECD, 2004.*)

[2] See, for example, Chalfin and McCrary (2018), Nagin (1998) and Cameron (1988) for surveys on the empirical literature on criminal deterrence.

[3] The criminology literature has argued that criminal spillovers can be positive or negative. On the one hand, crime displacement reduces the benefits of focused policing. On the other hand, the benefits of crime control may diffuse to nearby locations, generating additional benefits, though this might be interpreted as a broader deterrent effect. Moreover, additional incapacitation generated by crime control in neighboring areas may yield positive spillovers as well. A series of focused policing experiments have analyzed changes in crime levels in neighborhoods that are contiguous to treatment areas (e.g., Braga et al. (1999), Weisburd and Green (1995)).

and potential targets of crime ((e.g., Furlong, 1987)), investments in crime prevention reflect the underlying propensity of crime to occur. This will bias a regression of attempted crime on the security investments of neighbors since they co-exist in a similar environment.[4] Moreover, firms invest in crime prevention in response to changes to the underlying propensity for crime, which introduces issues of reverse causality.

Second, crime is also determined in a strategic equilibrium between potential targets of crime: the vulnerability of one target is generally a function of the vulnerability of alternative targets. Hence investments to increase the security of a particular bank are made both in response to and are reflective of the investment decisions of other banks. To the extent that neighboring firms operate in a similar environment, this has the potential to introduce multicollinearity issues. Furthermore, since banks respond to one another, this may exacerbate simultaneity issues.

Finally, because units may not be well defined *a priori*, identification of displacement effects may suffer from contamination. For instance, determining whether a police patrol on one block displaces crime to a neighboring, unpatrolled block is complicated by the fact that the patrol may indirectly deter crime on the neighboring block.[5] All of these issues are further compounded by the fact that crime data often suffer from measurement error.

The primary innovation of this paper is to identify displacement effects in a unique institutional setting with a geographically detailed data panel data set that allows us to circumvent the empirical issues highlighted above. Specifically, we estimate the extent to which hired security guards in Italian banks displace robberies to neighboring bank branches. This is a rich criminal context, as bank robberies are exceedingly common in Italy (the average bank faces a 7% risk of attempted robbery

[4]Spatial correlation in criminal activity gives rise to what are known as "hot spots," small areas where crime tends to concentrate. For an overview of the criminology literature, see Braga (2001).

[5]As Barr and Pease (1990) point out, it is difficult it to estimate displacement even in a controlled experimental setting. Before starting a trial, researchers must take a stand on the spatial nature of deterrence: if criminals perceive policing to be larger not just in treatment areas but also in control areas, then estimates of displacement will suffer from contamination. Moreover, "some displaced crime will probably fall outside the areas and types of crime being studied or be so dispersed as to be masked by background variation" (p. 293).

in a given year). Using complete information on the robbery histories and installed security measures of all registered Italian banks from 2000-2009, we find that the hiring of a dedicated guard reduces the probability of a bank robbery between 2.7 and 4.4 percentage points (31 to 50 percent). However, this private deterrent effect is substantially offset as robberies are displaced to nearby, unguarded banks: half of the robberies deterred at guarded banks will spillover to a nearby unguarded bank. No spillovers are found to affect nearby, guarded banks.

Since hiring guards generates a substantial negative externality on unguarded banks, one might presume that a policy that dissuades hiring would be welfare improving. However, we show with a simple theoretical model of displacement that it is ambiguous *a priori* whether policy should dissuade or promote the use of criminal deterrents when they displace crime. Underlying this counter-intuitive result is the fact that crime may be displaced differentially across agents depending on their deterrence choices. Because of this, agents may face a coordination game with multiple equilibria when investing in deterrents.

Given this ambiguity, two broad types of regulations could be deployed to combat displacement externalities: price regulations (a tax or subsidy on security investments) or quantity regulations (e.g., requiring security investments in all banks or restricting security investments in all banks). Because we find that crime is displaced entirely to unguarded banks, this suggests that the negative spillovers arise entirely due to a lack of coordination in the hiring decisions of neighboring banks. Hence, quantity regulations that drive investment decisions to a corner solution are well suited to facilitate coordination (and mitigate displacement) as opposed to price regulations that are more effective at interior solutions.

With this in mind, we conduct simulation exercises that are based on our estimated displacement effects to identify banking markets that are attractive candidates for policies that promote the hiring of guards and banking markets that are attractive candidates for policies that dissuade the hiring of guards. We find that hiring guards is unlikely to generate a social surplus in most of the country; however, guard requirements in certain densely populated urban areas may be socially beneficial. Moreover, we show that large multi-branch banks could reduce their exposure to bank robberies by reallocating their guards across different branches.

Although we study the use of private security guards, our results contribute to the broader economic literature that estimates the effect of policing on crime. A number of studies have exploited plausibly exogenous, localized and persistent increases in police guards stemming from terrorist attacks to estimate these effects.[6] Our setting is well suited to the estimation of potential displacement effects, which is often lacking in those analyses that rely on broader shocks. When it comes to public security measures, Di Tella and Schargrodsky (2004) find that car thefts drop on blocks where police officers have been assigned to guard specific buildings, but they find little evidence of an increase in car thefts in unprotected blocks.[7] In our context, private security guards are similarly salient, as they are positioned in uniform in front of bank branches during business hours.

Our paper is perhaps most closely related to the few studies that have tried to estimate displacement effects of private auto-theft deterrents. Ayres and Levitt (1998) show that car GPS-based tracking devices that are unobservable to thieves reduce motor-vehicle thefts across the board. When the devices are observable, as in Mexico, cars that are protected are less likely to be stolen but the attention of car thieves appears to be diverted towards unprotected cars (Gonzalez-Navarro, 2013). Similarly, van Ours and Vollaard (2016) find negative externalities for partially observable car immobilizers.[8]

The remainder of this paper is organized as follows. In Section 2, we present a simple model of crime prevention that describes the strategic relationship between the security investment decisions of different banks and we propose an empirical

[6] see, e.g., Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005) Draca et al. (2011).)

[7] Donohue et al. (2013) reexamine the data, finding some evidence of displacement, though they conclude that for lack of statistical power the inferences are not firm.

[8] There is also evidence of temporal displacement in marine pollution, from the day to the night when planes started to be used to monitor the North Sea for oil spills (Vollaard, 2017), while Vollaard and van Ours (2011) find no evidence of displacement against old homes when burglary-proof windows and doors become compulsory for new ones. But again criminals might simply move farther away than just a few blocks.

approach to identify deterrence and displacement effects that follows from the logic of our model. In Section 3, we describe our unique data set of Italian bank robberies and security investments. In Section 4 we present estimates of these effects. In Section 5, we use our estimates to consider how the reallocation of guards by a social planner or private banks could best lead to reductions in robberies. We conclude in Section 6.

2 A Simple Model of Displacement

We present a model of crime prevention that delineates the roles of deterrence and displacement. It is intentionally simple and stylized since our primary goal is to explore the strategic interactions between banks that arise with displacement. The main contribution of our model is the finding that a coordination game may arise between banks; hence even if the direction of displacement externalities is known to positive or negative, it is ambiguous as to whether security measures should be encouraged or discouraged.[9] The model also provides a conceptual basis for the endogeneity problems in estimating criminal deterrence and displacement effects, so it is a useful starting point for our empirical analysis.

Banks $i = 1, 2, ...N$ operate in a single market, which is defined as the set of banks that are viewed as substitutes from the perspective of potential bank robbers. Each bank chooses whether or not to hire a guard, which we denote as $g_i \in \{0, 1\}$ respectively. The cost of hiring a guard, $c_i > 0$, and the expected loss to i in the event of robbery, $L_i > 0$ may both vary by bank.

Each bank faces a probability of being robbed $p(g_i, g_{-i})$, where g_{-i} is the number of neighboring banks to i that hire guards. This specification of the probability of being robbed is quite flexible, and it accommodates both deterrent and displacement

[9] A theoretical literature on deterrence (and sometimes displacement) incorporates complexities such as dynamic considerations Sah (1991), labor market considerations (Burdett et al., 2004, Clotfelter, 1977) and time inconsistency (Lee and McCrary, 2009). In a more data-driven study Amodio (2019) shows that households' investments in burglary protection depend on the investments of their neighbors.

effects. We posit that

$$p(0, g_{-i}) - p(1, g_{-i}) \geq 0 \tag{1}$$

$$\frac{\partial p}{\partial g_{-i}} \geq 0 \tag{2}$$

$$\left.\frac{\partial p}{\partial g_{-i}}\right|_{g_i=0} - \left.\frac{\partial p}{\partial g_{-i}}\right|_{g_i=1} \geq 0 \tag{3}$$

Equation (1) encapsulates the deterrent effect, and equation (2) encapsulates the displacement effect. Equation (3) reflects the extent to which crime is differentially displaced to unguarded banks versus guarded banks.

Putting this all together, bank i will hire a guard if its expected loss with a guard, including the hiring cost, is less than its expected loss without a guard, or

$$p(1, g_{-i})L_i + c_i < p(0, g_{-i})L_i \tag{4}$$

We can rewrite this hiring condition to better highlight the strategic interactions of banks as

$$\underbrace{p(0, g_{-i}) - p(1, g_{-i})}_{\pi(g_{-i})} > \underbrace{\frac{c_i}{L_i}}_{\lambda_i} \tag{5}$$

The left hand side can be thought of as the marginal benefit of hiring a guard in units of expected robberies. We refer to this as the *guard premium*, which can be specified as a function of a single argument $\pi(g_{-i})$ and is equivalent to the ability of a guard to deter crime, given market conditions (see equation (1)). The right hand side can be thought of as the marginal cost of hiring a guard expressed in units of expected robberies, which we specify with a single parameter λ_i. Note that the guard premium does not directly vary with i but rather only indirectly with market level conditions (through $-i$) whereas the marginal cost of hiring does vary directly with i. For this reason, we can order banks by their propensity to hire a guard without loss of generality as $0 < \lambda_1 \leq \lambda_2 \leq, ..., \leq \lambda_N$.

Within this simple framework, we derive the equilibrium decisions of all banks summarized in Proposition 1.[10] We define an equilibrium as a set of hiring decisions by all banks such that no bank would benefit from unilaterally deviating.

Proposition 1. *Suppose p satisfies equations (1)-(3) and exhibits a given positive level of deterrence. Define $\lambda_{N+1} = \infty$. Then*

a (No Displacement) If equation (2) holds with equality, $\pi(g_{-i}) \equiv \pi(0)$ is a constant function, and e_0 banks in the market will hire guards in equilibrium, where e_0 uniquely satisfies $\lambda_{e_0} < \pi(0)$ and $\lambda_{e_0+1} \geq \pi(0)$.

b (Existence) In equilibrium, $e \geq e_0$ banks in the market will hire guards for any e that satisfies $\lambda_e < \pi(e-1)$ and $\lambda_{e+1} \geq \pi(e)$.

c (Uniqueness) Let i be the smallest positive number such that $\pi(i-1) \leq \lambda_i$ for some i. For all $j > i$ such that $\lambda_j < \pi(j-1)$ then $i-1$ banks may hire guards or *j banks may hire guards in equilibrium.*

Proof. See Appendix. □

The proof of Proposition 1 immediately follows from the fact that a bank i hires a guard only if all banks $j < i$ hire guards as well. This introduces an ordering into banks' strategies and allows equilibrium to be determined by the marginal bank that would hire a guard. The marginal bank can simply be recovered by comparing the relative positions of λ_i and guard premia. If multiple λ_i are positioned between the relevant guard premia, then displacement may allow for the existence of multiple equilibria.

We provide intuition for the results of Proposition 1 in a series of diagrams. In panel (a), there is a deterrent effect but no displacement, so the guard premium for each bank does not vary with other banks' hiring decisions. Hence, those banks whose costs are below the fixed guard premium (equal to $\pi(0)$) will hire guards (as shown in black) and those banks whose costs exceed it will not hire guards (as shown in gray).

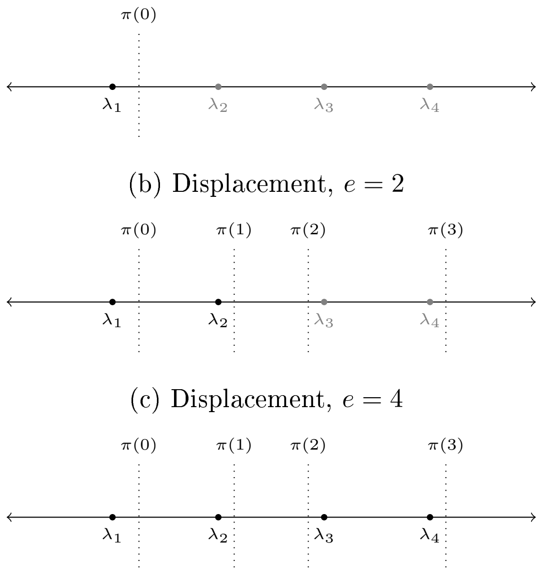
[10] We ignore the trivial case where there is no deterrent effect, as no bank would hire a guard ($c_i > 0$).

Figure 1: Equilibrium in Guard Hiring

(a) No Displacement, $e_0 = 1$

(b) Displacement, $e = 2$

(c) Displacement, $e = 4$



Note: Black dots represent banks who hire guards and grey dots represent banks who do not.

In panel (b), we introduce displacement. This generates variation in the guard premium. As more guards populate the market, the guard premium increases, so now two banks find it optimal to hire guards. However, this is not the unique equilibrium: because λ_3 and λ_4 are positioned between $\pi(2)$ and $\pi(3)$, a coordination game has emerged between banks 3 and 4. In panel (c), we show a second equilibrium in which four banks now find it optimal to hire guards. While it is profitable for neither of these banks to hire a guard or for both of them to hire a guard, it is never profitable for only 3 to hire a guard. Finally, the degree of differential displacement does not qualitatively affect these results. Greater differential displacement will only increase the distances between $\pi(i)$ and $\pi(j)$ (keeping the position of $\pi(0)$ unchanged).

Because displacement is an externality, it is useful to compare the competitive equilibrium described in Proposition 1 with the socially optimal allocation of guards under displacement. Displacement is a negative externality that is ignored by banks in the competitive equilibrium, so basic intuition would suggest that an unregulated market would feature too many guards. This intuition, however, is flawed since displacement may create coordination games amongst banks. Consider the case of panels (b) and (c) in Figure 1, and suppose that the socially optimal number of guards in this market is 3.[11] The multiplicity of competitive equilibria implies that one equilibrium will feature too many guards while the other will feature too few. We summarize this in the following proposition:

Proposition 2. *The socially optimal number of guards in a market may be higher or lower than the number of guards that would be hired in a competitive equilibrium.*

An immediate policy implication of Proposition 2 is that it is not obvious whether regulation should encourage or discourage the hiring of guards, despite the fact that they generate negative externalities. A multiplicity of equilibria arises because consecutive λ_i lie between the respective guard premia – intuitively, as banks become more homogeneous (i.e., the distances between their λ_i diminish). Indeed, we might expect this to occur quite frequently since banks hire guards from a common local market (reducing variation in c_i) and nearby branches, catering to similar customer bases, may hold a similar amount in reserves (reducing variation in L_i). Hence policy ambiguity may be the rule rather than the exception. We capture this intuition in the proposition below.

Proposition 3. *Complete Coordination.*

a If $\pi(0) < \lambda_1$ then an equilibrium exists in which no banks hire guards.

b If $\pi(N) > \lambda_N$ then an equilibrium exists in which all banks hire guards.

Proposition 3 states that a complete coordination game among banks will arise when banks are similar (λ_1 is not too different from λ_N), deterrence is relatively low ($\pi(0)$ is small) and displacement is relatively high ($\pi(N)$ is very different from λ_N). We use intentionally vague terms to describe these conditions because many combinations of market characteristics may sustain multiple equilibria and policy ambiguity.

Finally, we should note that standard policies that are used to correct externalities may offer very different performance in this setting. Price regulations, such

[11] It is straightforward to see that this can be supported by some combination of c_i's and L_i's, as the number of free parameters (8) exceeds the number of constraints that pin down this set up (5).

as Pigouvian taxes or subsidies, can be easily incorporated into the model as they operate entirely through c_i. For instance, a tax will shift the locations of all λ_i to the left. While that effectively decreases the "value proposition" of deterrence by strengthening the first condition of Proposition 3, it weakens the second condition of Proposition 3 and only increases the dispersion of the λ_i to the extent that the L_i vary. Hence, taxes may be ill suited to "fix" the conditions underlying coordination problems between banks. On the other hand, quantity regulations, such as guard requirements or restrictions can eliminate the coordination problem entirely by forcing all banks to a particular equilibrium. Of course these may be less attractive when a market does not suffer from complete coordination problems.

3 Data

We have been granted access to the yearly Census of Bank Branches collected by the Italian Banking Association (*Associazione Bancaria Italiana*) between 2000 and 2009. We observe the precise location (latitude and longitude) of each bank, which allows us to assign them to markets of varying size. Branch managers whose bank has signed up an agreement with ABI about bank robberies are required to inform the ABI's research center on crime against banks (OSSIF[12]) whenever their branch is victim of a crime.[13] For each branch, we observe a full history of all attempted robberies. The Census also contains a full history of investments in 37 distinct security measures. These include most importantly the hiring of guards in addition to the installation of deterrents such as bulletproof glass, security vestibules, time locks, etc.

As shown in Figure 2, the spatial distribution of banks in Italy generally follows the spatial distribution of population and economic activity. Distinct clusters correspond to major metropolitan areas, and there is greater bank density in the wealthier North. Robberies are also clustered in major cities though they occur throughout

[12]Website: www.OSSIF.it.

[13]The number of agreements increases over time. Overall our dataset covers 71 percent of robberies. Between 2004 and 2009 the percentage goes up to 83 percent.

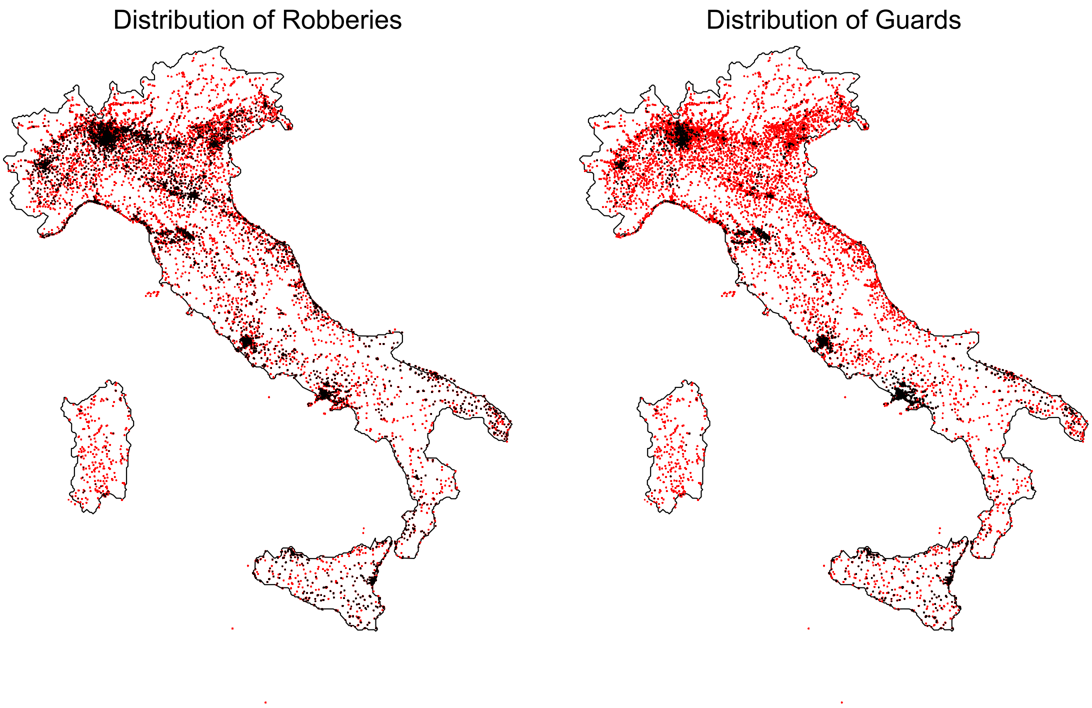


Figure 2: Geographic Distribution of Banks, Guards, and Robberies

Notes: Each red dot represents a bank branch. The black dots in the left panel represent banks that have been victimized from 2000 and 2009 and in the right panel represent banks with security guards.

the country. The distribution of security guards mimics the distribution of robberies, which portends a number of the endogeneity issues in identifying displacement that we previously raised.

Summary statistics of our sample are presented in Table 1.

Table 1: Summary statistics

	Mean	Std. Dev.	Min	Max
Panel A: Whole Sample, N=245,712				
Number of Robberies	0.07	0.30	0	5
Guard	0.08	0.27	0	1
Number of Security Devices	7.48	4.94	1	36
No substitute branches in the 500m markets	0.41	0.49	0	1
No substitute branches in the 50km markets	0.00	0.02	0	1
Panel B: Provinces with Below Median Robberies, N=125,401				
Number of Robberies	0.04	0.22	0	4
Guard	0.04	0.19	0	1
Number of Security Devices	7.59	5.22	1	36
No substitute branches in the 500m markets	0.46	0.50	0	1
No substitute branches in the 50km markets	0.00	0.02	0	1
Panel C: Provinces with Above Median Robberies, N=120,311				
Number of Robberies	0.10	0.36	0	5
Guard	0.13	0.33	0	1
Number of Security Devices	7.36	4.61	1	34
No substitute branches in the 500m markets	0.36	0.48	0	1
No substitute branches in the 50km markets	0.00	0.02	0	1

On average, bank branches risk 0.07 robberies per year, and 8 percent of them hire security guards. When focusing on provinces with a below-median number of attempted robberies the numbers drop to 0.04 and 0.04, respectively, while they increase to 0.10 and 0.13 in provinces with an above-median number of attempted robberies. When assigning bank branches to 500m by 500m squares, about 40 percent have no neighboring banks. When the market size increases to 50km by 50km, almost all bank branches have neighboring banks. Bank robberies were stable for most of our sample period, though there has been a secular decline in the total number of robberies beginning starts in 2008 (see Appendix Figure A1).[14]

[14]Previous research has shown that a 2007 spike in robberies was driven by a collective pardon that freed about 20,000 inmates in the second half of 2006 (Barbarino and Mastrobuoni, 2014). Interestingly, the trend in bank robberies for the US is surprisingly similar to the Italian one after 2007. According to Marco Iaconis, head of the Security Office of the Italian Banking Association, this is driven by the increased use of vaults with time-locks, which severely limit the amount of cash that is quickly available to the tellers.

The spatial distribution of the hiring and firing of security guards that gives rise to longitudinal variation in the use of security guards is shown in Figure 3. It is fairly clear that firings are more common than hirings, which is consistent with the banks trying to disinvest in security guards.

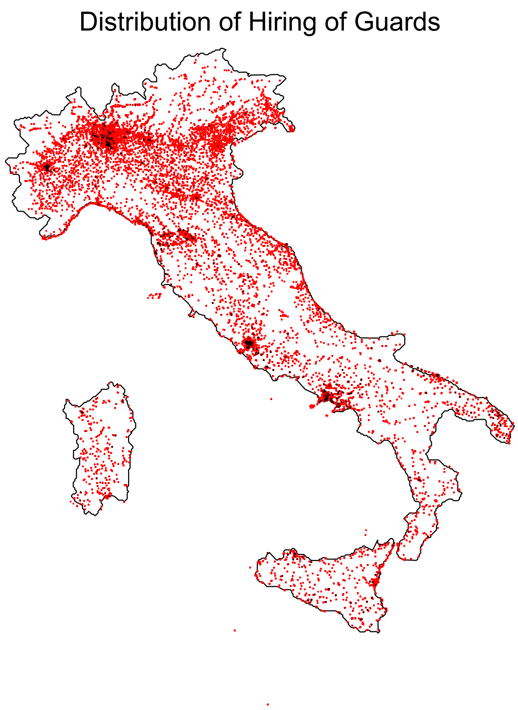


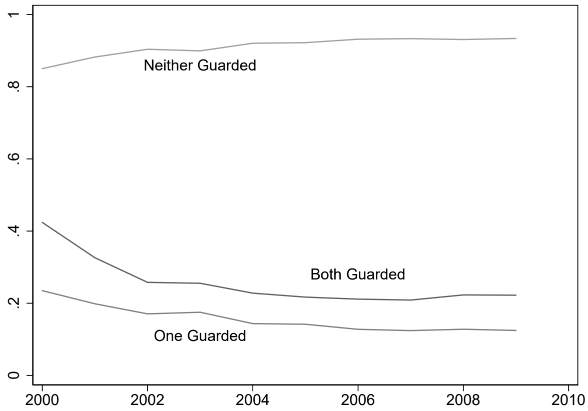
Figure 3: Geographic Distribution of Hiring and Firing of Security Guards

Notes: Each red dot represents a bank branch. The black dots in the left panel represent the hiring of security guards, the ones in the right panel represent the firing of security guards.

Finally, we present raw evidence that banks' security investments are highly correlated and increasingly so over time. In Figure 4, we compare each bank with its nearest neighbor. The least common configuration has one bank with a guard and one bank without a guard, and such pairs have become rarer over time. Of course, this observed correlation might simply be an artifact of random chance as opposed to coordination. As such, we compute the baseline probability that two neighboring banks would have made the same investment decision by chance.[15] In panel (a) of Figure 5, we present the fraction of bank pairs that have made the same

[15] If p_t is the fraction of banks with guards in a given year in a given province, this number is $p_t^2 + (1 - p_t)^2$ for all banks within that province.

Figure 4: Bank Pairs by Guard Status



Notes: Each bank is compared with its nearest neighbor.

investment decisions in each year alongside this baseline. Banks tend to behave similarly, and this behavior is increasing over time. While these facts are consistent with displacement effects generating coordination games between banks, they may simply reflect the fact that neighboring banks share a common environment. Hence, we should not conclude that displacement effects exist from this observation alone.

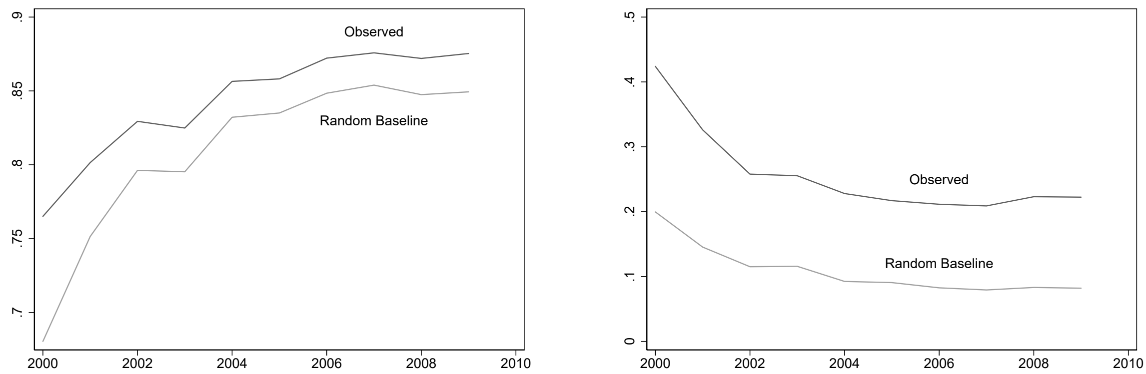
In panel (b) of Figure 5, we restrict our attention to bank pairs in which at least one of the banks has hired a guard. In 2000, over 40% of all bank pairs featured both banks with guards. Given that 20% percent of banks in 2000 hired a guard, we would expect only 20% of bank pairs to both hire guards if hiring was truly random. Although the use of guards declined over the sample period, the gap between observed coordination and a random baseline remained large (approximately twenty percentage points) and persistent. This is also suggestive, though not dispositive, of coordinated behavior.

4 Empirical Approach and Results

Following equations (1)-(3), deterrence and displacement are features of the function p. Our data presents a unique opportunity to estimate this function directly. For

Figure 5: Coordinated Hiring and Firing of Guards

Frac. of Bank Pairs with Same Guard Status   Propensity of Bank Pairs to Coordinate with Guards



Notes: Each bank is compared with its nearest neighbor. In each panel, we compare coordination with a corresponding baseline that we would expect if guards were randomly assigned to banks within provinces.

bank i in market j observed in year t, we specify the regression equation

$$r_{ijt} = \beta_1 g_{ijt} + \beta_2 g_{-ijt} + \beta_3 g_{ijt} g_{-ijt} + \epsilon_{ijt} \tag{6}$$

where r_{ijt} is a dummy variable equal to 1 if a robbery attempt was made on bank i in year t, g_{ijt} is a dummy variable equal to 1 if bank i had a guard in period t, and g_{-ijt} is equal to the fraction of banks in market j (other than i) that were guarded in period t.[16] It follows that β_1 can be interpreted as the deterrent effect, β_2 can be interpreted as the displacement effect, and β_3 can be interpreted as the degree of differential displacement between guarded and unguarded banks.

Estimation of these effects is complicated by the fact that unobservable determinants of robbery in the error term, ϵ_{ijt}, are certainly correlated to the hiring decision of bank i. Indeed, the guard hiring condition (equation (4)) features the probability of being robbed p prominently. Moreover, these unobservables should be correlated to the hiring decisions of other banks in the market.

The fact that banks strategically make decisions in a common environment in-

[16]We specify g_{-ijt} as a fraction instead of number in order to estimate a displacement effect that is invariant to market size. This is advantageous because the size of markets is unknown *a priori*, which leads us to compare estimates across many different market definitions.

troduces yet another source of endogeneity into equation (6). Because g_{-i} enters directly into equation (4), each bank's hiring condition is implicitly a function of its neighbors' hiring conditions as well. Hence, not only are unobserved environmental factors subsumed in L_i and p_i potential sources of endogeneity, but those factors subsumed in L_{-i} and p_{-i} are as well. In the language of Manski (1993), the displacement effects β_2 and β_3 correspond to correlated effects. These effects are difficult to disentangle from the factors that led that competitor to hire the guard in the first place, as i's expectations over these factors enter into i's strategic hiring decision. We attempt to identify these effects by exploiting the panel structure of our data along three dimensions: across banks, across markets, and over time.

First, we note that banks are clearly located in markets of varying sizes (see Figure 2), yet there is no *a priori* correct definition of a market. By properly defining a market and controlling for market specific characteristics, we may be able to control for confounders related to the common environment shared by banks. To do so, we group banks into markets indexed by j, where markets are defined by subdividing Italy into squares of equal area bounded by latitude and longitude. We take no prior stance on the size of a market and instead conduct our analysis on squares of varying dimensions.[17]

Now, note that while the identification of the simple deterrent effect of a guard (β_1) is subject to the same concerns as the identification of displacement effects it should not be a affected by the size of the market in which a bank operates. This suggests an empirically driven approach to assessing whether we are able to control for common environmental confounders with fixed effects that capture smaller and smaller markets. In the limiting case of a market with just a single bank branch we are only exploiting within variation over time, which we later discuss introduces misclassification problems around the timing of the hiring and firing of guards.

Consider the following deterrence regression equation:

[17] In order to make sure that our results were not affected by effects at the boundaries of markets, we re-estimated all of our results by shifting the "grid" of markets by various amounts and found no systematic differences in our estimates. Specifically, if markets were defined as $k$km by $k$km, then we reestimated all of our results by shifting the grid of markets by $k(1+\delta)$ to the North and East for $\delta = 0.1k, 0.2k, ..., 0.9k$.

$$r_{ijt} = \beta_1 g_{ijt} + \lambda_j + \lambda_t + \epsilon_{ijt} \tag{7}$$

where β_1 now represents a simple deterrent effect, λ_j is a market fixed effect and λ_t is a flexible yearly time trend. As this equation is specified under successively smaller market definitions, the set of confounders encapsulated in ϵ_{ijt} shrinks, but the simple deterrent effect should be unaffected. Hence, if estimates of β_1 are largely unaffected by any choice of j below a certain threshold, then we might conclude that our fixed effects can successfully control for environmental confounders related to deterrence (e.g., the local propensity for crime, local labor market conditions, etc.)

We present the results of this exercise in Table 2.[18] In specification (1), we include no market fixed effects and obtain a small and insignificant estimate of deterrence. This is because the deterrence and endogenous investments presumably cancel each other. As we begin to control for local conditions in specifications (2)-(6), we obtain statistically significant and increasing estimates of deterrence effects. The one but last column presents the number of market fixed effects, that is the number of squares used to cover the country. The coefficients become more negative as the number goes up from 6 regions to 651 regions. In specifications (7)-(12), with market sizes between 25km by 25km and 500m by 500m we obtain stable and statistically significant deterrence estimates of roughly 4 percentage points. This suggests that unobserved local conditions would bias our estimates of deterrence downward (i.e., in a more positive direction), which is consistent with our model since a higher propensity for robbery would induce banks to hire guards.

The "limiting" case of this exercise is the inclusion of bank-fixed effects (λ_i) presented in specification (14). These estimates reflect a tradeoff between reverse causality due to measurement error and a greater ability to absorb environmental confounders.[19] Because we only observe whether banks hired a guard by the end of

[18] In all results presented, we estimate robust standard errors clustered by 50km squares. The statistical significance of all of our results is essentially unchanged if we instead cluster at the market j level.

[19] This is related to an important point raised by Chalfin and McCrary (2018) in their estimation of the effect of police on crime with aggregate crime regressions. Fixed effects regressions may seriously exacerbate measurement error bias when police staffing is measured with some error.

the year, we may mismeasure whether a robbery was attempted on a guarded bank versus an unguarded bank, leading to downward biased estimates of deterrence. In specification (14), β_1 is identified only off of variation in hiring within banks over time, so this problem may be particularly acute as the misclassification is amplified. Indeed, we estimate roughly half as strong a deterrence effect. In specification (15), we attempt to mitigate this tradeoff by omitting all observations in which a guard was just hired or fired (i.e., $g_{ijt} \neq g_{ijt-1}$). Doing so delivers an estimate of deterrence in line with what we estimated using market fixed effects.[20]

Table 2: Simple Estimates of Deterrence in Successively Smaller Markets

		Dependent Variable: Number of Robberies				
	Market FE	Deterrent effect	SE	Obs	# Spatial FE	R-squared
(1)	None	-0.0043	(0.0041)	245,712	0	0.0051
(2)	800km	-0.0068*	(0.0040)	245,712	6	0.0073
(3)	400km	-0.0142***	(0.0037)	245,712	12	0.0109
(4)	200km	-0.0193***	(0.0036)	245,712	28	0.0134
(5)	100km	-0.0275***	(0.0035)	245,712	74	0.0183
(6)	50km	-0.0345***	(0.0036)	245,712	211	0.0239
(7)	25km	-0.0402***	(0.0037)	245,711	651	0.0303
(8)	10km	-0.0426***	(0.0038)	245,707	2773	0.0418
(9)	5km	-0.0441***	(0.0038)	245,695	5644	0.0568
(10)	2km	-0.0430***	(0.0041)	245,670	9509	0.0829
(11)	1km	-0.0421***	(0.0040)	245,643	12748	0.1061
(12)	500m	-0.0386***	(0.0042)	245,612	16775	0.1329
(13)	250m	-0.0338***	(0.0044)	245,577	21059	0.1590
	Bank FE					
(14)	All years	-0.0157***	(0.0040)	244,742	33672	0.2174
(15)	Excluding switching years	-0.0366***	(0.0064)	203,696	31077	0.2276

Notes: All regressions include year fixed effects. Robust standard errors clustered by 50km squares in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Importantly, all estimates in specifications (6)-(13) and (15) are precisely measured and not statistically significantly different from one another. We take this as evidence that these fixed effects plausibly control for environmental confounders due to deterrence. This implies that the inclusion of these fixed effects in the full

[20] In Appendix Table A1, we re-estimate deterrence effects for specifications (1)-(13) on the restricted subsample that omits all observations in which a guard was just hired on fired. Our estimates are essentially unchanged, except for the smallest market size (250m by 250m). This suggests that misclassification error in hiring/firing is not an issue in regressions with market fixed effects. Note that we are unable to exclude observations in which a neighboring bank in the market just hired a guard because it would dramatically reduce our estimation sample. Hence, this is an imperfect solution.

regression equation

$$r_{ijt} = \beta_1 g_{ijt} + \beta_2 g_{-ijt} + \beta_3 g_{ijt} g_{-ijt} + \lambda_j + \lambda_t + \epsilon_{ijt} \tag{8}$$

will yield estimates of displacement (β_2 and β_3) that could be biased only by confounders that (1) vary over time within markets, (2) vary across markets within a year, and most importantly (3) are uncorrelated to confounders that also influence the deterrent effect of a guard.

We estimate equation (8) defining markets from 50km squares down to 250m squares and present our results in Table 3. In all specifications, our estimates of deterrence ($\beta_1 + \beta_3 * \overline{g}_{-ijt}$) are nearly identical to our estimates in Table 2, which confirms the extent to which this research design addresses the potential endogeneity due to shared environments of competitor banks.[21] The effect of any potential confounder that varies by both time and by market will generally change as we define markets differently. The fact that all deterrence estimates are roughly constant across specifications suggests that endogeneity related to market definition, which by construction includes most confounders that release contextual effects, is controlled for.

Table 3: Estimates of Deterrence and Displacement Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable: Number of Robberies							
Guard (β_1)	-0.0348***	-0.0373***	-0.0391***	-0.0407***	-0.0382***	-0.0369***	-0.0318***	-0.0289***
	(0.0053)	(0.0054)	(0.0051)	(0.0048)	(0.0048)	(0.0044)	(0.0045)	(0.0051)
% Neighbors with Guards (β_2)	0.0020	0.0063	0.0058	-0.0001	0.0109	0.0181**	0.0169**	0.0147*
	(0.0271)	(0.0236)	(0.0149)	(0.0118)	(0.0091)	(0.0082)	(0.0078)	(0.0081)
Guard × % Neighbors with Guards (β_3)	0.0014	-0.0148	-0.0171	-0.0171	-0.0231	-0.0200	-0.0336***	-0.0260**
	(0.0286)	(0.0271)	(0.0231)	(0.0192)	(0.0145)	(0.0130)	(0.0119)	(0.0131)
No substitutes	-0.0315	-0.0178	0.0053	0.0064	0.0062*	0.0075**	0.0085***	0.0073**
	(0.0321)	(0.0204)	(0.0064)	(0.0044)	(0.0035)	(0.0033)	(0.0031)	(0.0031)
Square fixed effects	50km	25km	10km	5km	2km	1km	500m	250m
Year fixed effects	√	√	√	√	√	√	√	√
Observations	245,712	245,711	245,707	245,695	245,670	245,643	245,612	245,577
R-squared	0.0239	0.0304	0.0418	0.0568	0.0830	0.1062	0.1330	0.1590
$\beta_1 + \beta_3 \overline{g_{-ijt}}$	-0.0347	-0.0385	-0.0405	-0.0420	-0.0398	-0.0382	-0.0335	-0.0298
p-value ($\beta_2 + \beta_3 = 0$)	0.920	0.781	0.615	0.351	0.339	0.863	0.113	0.348
Average n. of neighboring branches	357	170	65.62	32.83	10.96	4.989	2.464	1.539

Note: Robust standard errors clustered by 50km squares in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

[21] In order to use those observations for which a bank has no neighbors to estimate deterrence effects, we flag them with a dummy variable equal to 1 and present the estimated coefficient.

In contrast, the displacement effects that we estimate vary considerably by market definition. This is not surprising, as not all banks within a given market may be equally substitutable from the perspective of a robber, and this heterogeneity will be more stark in larger markets. In large markets, we find no evidence of displacement. In markets smaller than 1 km^2, we find displacement effects of 1.5-2 percentage points to unguarded banks (β_2). Specifically, if an unguarded bank's neighbors hires guards, the branch is probability of being robbed will increase by roughly 20%. However, we find no statistically significant displacement effects to guarded banks ($\beta_2 + \beta_3$), even in the smallest markets. This suggests that policies that incentivize all banks to make security investments will suffer less from reduced effectiveness due to negative displacement externalities.[22]

Although the specifications in Table 3 are well suited to control for confounders related to the shared environment of banks in a market, they are less well suited to control for confounders related to a particular bank's propensity to be targeted in a robbery attempt. Including bank fixed effects, as in specification (14) of Table 2 might address this problem, but it would also make endogeneity due to measurement error in the timing of guard hiring more acute. Moreover, our prior strategy of dropping observations when guard status switches is inapplicable here since we would not be able to define g_{-ijt} in a consistent manner that excluded this error.

Instead, we attempt to address this potential source of endogeneity by including a richer set of controls related to the timing of robberies. In Table 4, we present four specifications of our main regression with markets defined as 500m by 500m squares. The first specification is a pure replication of our main regression. In specification (2), we add market-specific linear time trends to more flexibly control for time varying unobservables, and our estimates are essentially unchanged. In specification (3), we control for the number of other security devices besides guards

[22]In Appendix Table A3, we re-estimate our baseline results on two subsamples of markets: those with branches that are more similar on the basis of their use of all security device, and those with branches that are less similar. Consistent with our theory, we find that deterrent effects of guards are strongest in markets with less similar branches, whereas displacement effects of guards are strongest in markets with more similar branches.

that banks have in operation, and our estimates remain unchanged.[23] In specification (4) we add market-specific quadratic time trends and, again, the estimates change very little. This is our preferred specification, and we present estimates of $\beta_1 - \beta_3$ with augmented controls (specification (4)) for all market sizes between 250m and 100km graphically in Appendix B.

In columns (2)-(5) of Table 5, we present the results of a number of additional robustness checks. In specification (2), we restrict our estimation to a pre-2008 subsample, when the number of bank robberies was quite stable and obtain broadly similar results. In specification (3), we relax the assumption of linearity in displacement spillovers by specifying the fraction of neighbors with guards quadratically. Our estimates of deterrence are similar, and we still find statistically significant evidence of displacement, though we are unable to precisely estimate differential displacement. In specifications (4) and (5), we include lagged robberies as controls in order to assess the extent to which we have addressed simultaneity issues. Our estimate of β_3 is slightly reduced, and we can no longer precisely estimate a differential displacement effect on this smaller sample.

To summarize, hiring a guard reduces the probability that a bank is robbed in a given year by roughly 40% off of a base of 7 percentage points. If such a bank has neighboring banks within 500m without guards, then roughly half of this reduction will be offset by robberies that are displaced to those banks. However, neighboring banks who already employ guards do not suffer any additional robberies due to displacement.

5 Displacement Policies

Displacement spillovers indicate a role for policy. We focus on spatial displacement, assuming that crime displacement and spatial displacement do not interact. While it is difficult to measure displacement across crime types, Mastrobuoni (forthcom-

[23] We should note that the correlation between the use of a guard and the number of security devices is extremely low (0.02). In first differences, this correlation is even smaller (0.015).

Table 4: Deterrence and Displacement Effects with Additional Controls

	(1)	(2)	(3)	(4)
	Number of Robberies			
Guard	-0.0318***	-0.0321***	-0.0318***	-0.0342***
	(0.0045)	(0.0046)	(0.0045)	(0.0049)
% Neighbors with Guards	0.0169**	0.0170**	0.0169**	0.0190**
	(0.0078)	(0.0078)	(0.0078)	(0.0085)
Guard × % Neighbors with Guards	-0.0336***	-0.0343***	-0.0336***	-0.0245
	(0.0119)	(0.0119)	(0.0119)	(0.0151)
Number of Security Devices		0.0005*		
		(0.0003)		
Neighbors Average Num. of Sec. Devices		-0.0006*		
		(0.0003)		
No substitute bank	0.0085***	0.0045	0.0085***	-0.0048
	(0.0031)	(0.0039)	(0.0031)	(0.0045)
Market (500m) and Year FE	√	√	√	√
Market specific linear time trends			√	√
Market specific quadratic time trends				√
Sample	Full	Full	Full	Full
Observations	245,612	245,612	245,612	245,612
R-squared	0.1330	0.1331	0.1330	0.1941
p-value ($\beta_2 + \beta_3 = 0$)	0.113	0.103	0.114	0.704

Note: Column 2 restricts the analysis to years before 2008. Robust standard errors clustered by 50km squares in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Robustness Regressions for Deterrence and Displacement Effects

	(1)	(2)	(3)	(4)	(5)
	Number of Robberies				
Guard	-0.0342***	-0.0378***	-0.0322***	-0.0323***	-0.0309***
	(0.0049)	(0.0058)	(0.0056)	(0.0060)	(0.0060)
% Neighbors with Guards	0.0190**	0.0162	0.0490**	0.0181**	0.0163*
	(0.0085)	(0.0112)	(0.0216)	(0.0092)	(0.0091)
Guard × % Neighbors with Guards	-0.0245	-0.0324**	-0.0494	-0.0186	-0.0198
	(0.0151)	(0.0160)	(0.0442)	(0.0184)	(0.0191)
% Neighbors with Guards squared			-0.0370		
			(0.0267)		
Guard × % Neighbors with Guards squared			0.0281		
			(0.0478)		
Lagged Number of Robberies				-0.0473***	-0.0538***
				(0.0057)	(0.0057)
Lagged Num. of Robb. against Neighbors					-0.1358***
					(0.0081)
No substitute bank	-0.0048	-0.0093*	-0.0050	0.0048	-0.0020
	(0.0045)	(0.0054)	(0.0046)	(0.0051)	(0.0052)
Observations	245,612	187,897	245,612	210,702	210,702
R-squared	0.1941	0.2366	0.1941	0.2084	0.2142
p-value ($\beta_2 + \beta_3 = 0$)	0.703	0.294	0.992	0.982	0.854

Note: Robust standard errors clustered by 50km squares in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

ing) measures transition probabilities within the broader category of commercial robberies. Since robbers are likely to move to different targets before moving to different crimes altogether, these numbers are indicative of potential displacement across crime types.

Mastrobuoni (forthcoming) shows that bank robbers operating in the city of Milan have a very high degree of specialization. Conditional on robbing a bank, there is a 90 percent chance that a robber's next target is a bank even though banks constitute only 10 percent of victims. Conditional on robbing a business that is not a bank, the chance that a robber's next target is a bank drops to less than 2 percent.

The institutional characteristics of a particular market – the number of banks, likelihood and costs of robbery, and costs of guards – determine whether displacement should be addressed by an increase or a decrease in the use of guards. These characteristics are difficult to observe, but we can use our empirical results in concert with our theoretical model to assess which markets are the most attractive candidates for different types of public and private policies.

Our parameter estimates pin down a fundamental object of our model: the guard premium. Specifically, $\pi(0) = -\beta_1$ and $\pi(N) = -(\beta_1 + \beta_3)$. If hiring costs are constant across banks in a market, we can simply apply Proposition 3 to determine the range of losses (L_i) for which completely coordinated equilibria exist. Assuming an annual cost of €40 thousand for a security guard,[24] it follows that an equilibrium with no guarded banks will exist if $L_1 <$€1.37 million, and an equilibrium with all guarded banks will exist if $L_N >$€662 thousand.[25]

It is likely that some market exists in which every bank will face a loss of less than €1.37 million in the event of a robbery, hence an equilibrium exists in which no banks in Italy hire guards. However, this need not be the socially optimal outcome.

[24] According to the Italian Banking Association, banks follow the wage rules (*Tariffe di Legalita'*) set by the Ministry of Interior. In 2007 the hourly wage of a private security guard set by the Ministry was €24,27. With an average opening time of 7 hours for 5 days a week the yearly cost is close to €44,000.

[25] One may surmise that the use of guards is a proxy for broader security investments which may cost substantially more than €40 thousand per year. However, the extremely low correlation between the use of guards and the use of other security devices in our sample suggests that our estimates reflect the use of guards *per se*.

Indeed, in certain highly urban markets, it is likely that some bank will face a loss of greater than €662 thousand in the event of a robbery; in those markets, an equilibrium with all banks hiring guards also exists.

Without detailed information on c_i and L_i for all banks, we cannot identify which equilibrium generates greater social benefits in a particular market. Instead, we consider four counterfactual scenarios to explore which markets are most likely to benefit from the use of more guards, and which markets are most likely to benefit from the use of fewer guards. We do so from the perspective of a national policymaker with the ability to enact local policies that could increase or decrease the total number of guarded banks. These policies could take the form of extreme quantity restrictions as suggested by theory, or more gentle restrictions that gradually increase or reduce the number of guards in a market.

Scenario 1: Banning Guards

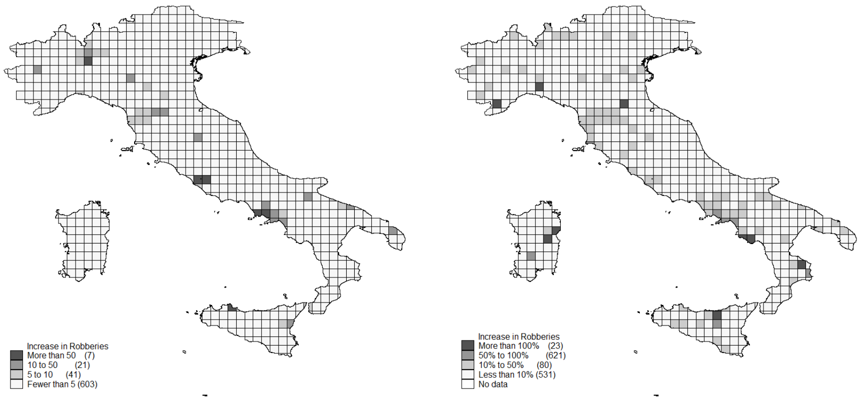
The natural policy response to a negative externality would be to discourage the use of guards. Suppose banks were no longer permitted to hire guards. Then the predicted change in the number of robberies in each market would be given by

$$\Delta r_{ijt} = \sum [\beta_1 g_{ijt} + \beta_2 g_{-ijt} + \beta_3 g_{ijt} g_{-ijt}]$$

For each 500m by 500m market, we simulate the total increase in robberies that would arise from implementing such a policy in 2005 using our preferred specification. We aggregate the changes in these markets into 25km by 25km squares for visual clarity and overlay them on a map of Italy in Figure 6.

Figure 6: Simulated Increase in Robberies from Banning Security Guards

(a) Absolute Increase (b) Percentage Increase



Notes: Changes are simulated in 2005 using our preferred specification (column (4) of Table 4) with markets defined as 500m squares and then aggregated to the 25km by 25km level for visual presentation.

In the first panel, we present the absolute effects of this policy. In much of the country, banning guards would lead to no more than 5 additional robberies. However, in metropolitan areas, we might find much greater increases. For instance, Rome, Naples, Milan and Palermo would experience more than 50 additional robberies. Because this policy would mechanically have a greater effect on large population centers, we present the relative effects of this policy in percentage terms in the second panel. As before, certain more heavily populated areas (Genova, Florence, Bologna, Rome, Naples) would tend to experience greater increases in robberies.

This result is consistent with the intuition of Proposition 3. Large, urban markets will tend to have more banks, and hence greater scope for heterogeneity among banks. This should increase the likelihood that a coordination game would arise that would generate at least one equilibrium in which there would be no negative externality as too few banks would hire guards.

Scenario 2: Requiring Guards

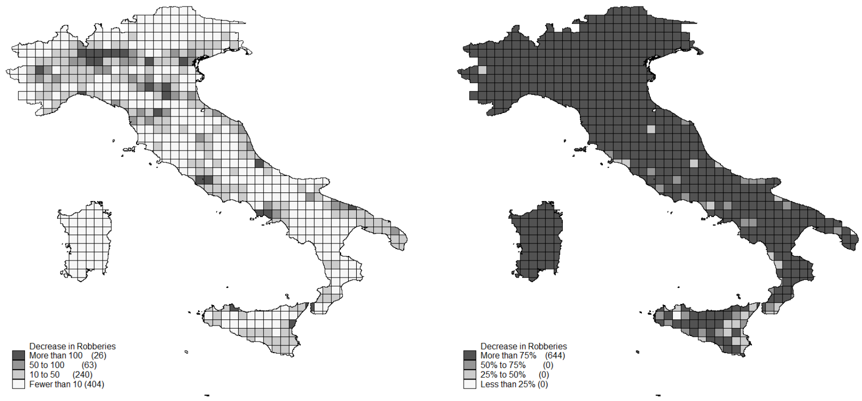
If instead all banks were required by law to hire guards, the predicted change in the number of robberies in each market would be given by

$$\Delta r_{ijt} = \sum [\beta_1(g_{ijt} - 1) + \beta_2(g_{-ijt} - 1) + \beta_3(g_{ijt}g_{-ijt} - 1)]$$

We present the effects of this policy in Figure 7. As before, we present the absolute increase in robberies from guard requirements in the first panel. Not surprisingly, the greatest reductions in robberies are concentrated in the most densely populated areas that feature the greatest number of potential targets. These include the relatively wealthy Po' river valley in the north (which includes Milan, Turin, Bologna and Venice) along with the major cities of Rome, Naples, Bari and Florence, all of which are covered by the darkest squares. In the second panel, we instead look at the relative effects of guard requirements. In pretty much the entire country, robberies would decrease by over 75%. Of course, this does not imply that universally requiring guards is the optimal policy since hiring comes at some cost.

Figure 7: Simulated Decrease in Robberies from Requiring Security Guards

(a) Absolute Decrease (b) Percentage Decrease



Notes: Changes are simulated in 2005 using our preferred specification (column (4) of Table 4) with markets defined as 500m squares and then aggregated to the 25km by 25km level for visual presentation.

Scenario 3: Gradual Removal of Guards

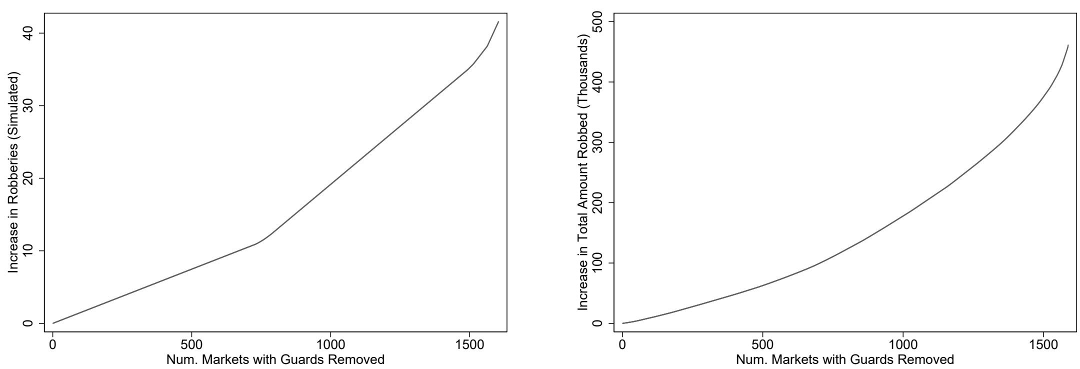
In the third scenario we consider a less extreme counterfactual in which we determine the net number of additional attempted robberies that would we expect if we optimally removed a single guard from a single market taking into the account that this might displace crime to other neighboring banks. We then repeat this exercise by optimally choosing a second market from which we remove a guard, then a third market, and so on.[26]

We present the results of this exercise in the first panel of Figure 8. As shown in the first panel, the benefits of removing guards at the margin are small – the 500 least effective guards in Italy deter fewer than 10 annual robberies altogether. However, these marginal effects do increase since successive removals creates more newly unguarded banks that are susceptible to crime that is displaced from still-guarded banks.

If we instead select markets for guard removal on the basis of losing the least expected amount to robbery instead of simply allowing the fewest number of additional robberies, we arrive at similar results. We estimate the expected cost of a robbery at a bank as the average amount stolen from all attempted robberies in that bank's province (*provincia*) in a given year. As shown in the second panel of Figure 8, each removed guard increases the expected amount lost to robbers by approximately €250, though this does increases to close to €1000 at the margin.

Figure 8: Simulated Marginal Effects of Removing Guards

(a) Increase in Robberies (b) Increase in Amount Stolen



Notes: Changes are simulated using our preferred specification on data from 2005. All amounts robbed are denominated in 2005 €.

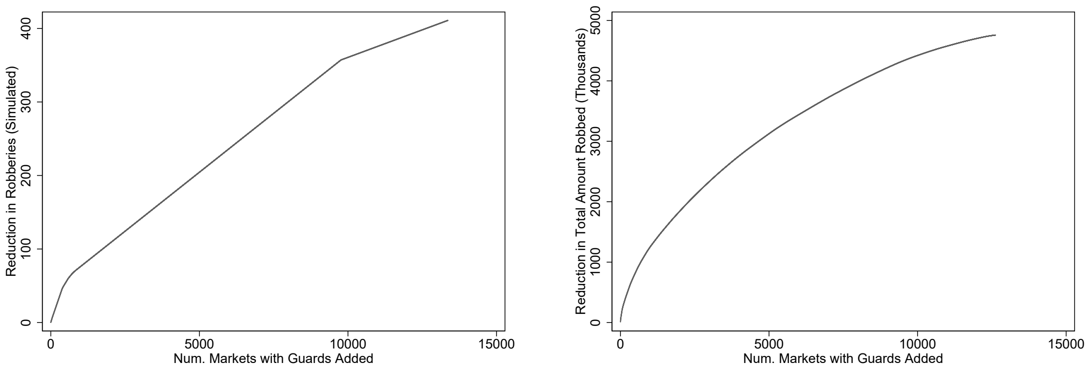
[26] In all of our simulation exercises, we restrict ourselves to a single change per market to avoid the computational burden of an exponentially more complicated dynamic programming problem. Despite the fact that this does not necessarily yield the globally optimal reallocation of guards, we believe that it does provide useful benchmarks on the marginal values of the second, third, and so on guards who are added or subtracted.

Scenario 4: Gradual Addition of Guards

In the fourth scenario, we consider the analogous counterfactual in which we incrementally add guards to unguarded banks. As shown in Figure 9, adding guards has a small effect, as each additional guard deters approximately 0.6 robberies in expectation. Each added guard reduces the expected amount lost to robbers by approximately €1000, though this eventually declines to approximately €400.

Figure 9: Simulated Marginal Effects of Adding Guards

(a) Decrease in Robberies (b) Decrease in Amount Stolen



Notes: Changes are simulated using our preferred specification on data from 2005. All amounts robbed are denominated in 2005 €.

Although the monetary values of a marginal guard implied by these exercises suggests that guards will not justify their salaries, we must caution that our analysis fails to account for other external costs of robberies beyond the robbers' haul. In particular, the perception of the added safety from guards may be valued quite highly by banks, their employees and their customers. Without knowledge of the private costs of exposure to robbery risk and the cost savings from not hiring guards, we cannot definitively identify optimal regional policies for security investments at banks.

Nevertheless, our analysis does suggest that banks in sparsely populated areas should be discouraged from hiring guards – of the small number of robberies that are deterred, a relatively large proportion will be displaced to nearby banks that are likely to be unguarded. On the other hand, large cities may want to consider encouraging the use of guards in local banks. Given the preponderance of targets and the relatively high exposure to robbery, encouraging the use of guards might generate meaningful deterrence that would not be displaced if other nearby banks were also guarded.[27]

5.1 A Bank-level Approach

In practice, the decisions to hire and fire guards belong to individual banks. We accordingly consider an alternative counterfactual in which banks optimally relocate a guard from one of their branches to another and then compute the simulated change in robberies that would result from such a decision. While banks do not consider the spillover effects of their decisions in our simulation, the change that we simulate covers all banks and hence includes these spillover effects.

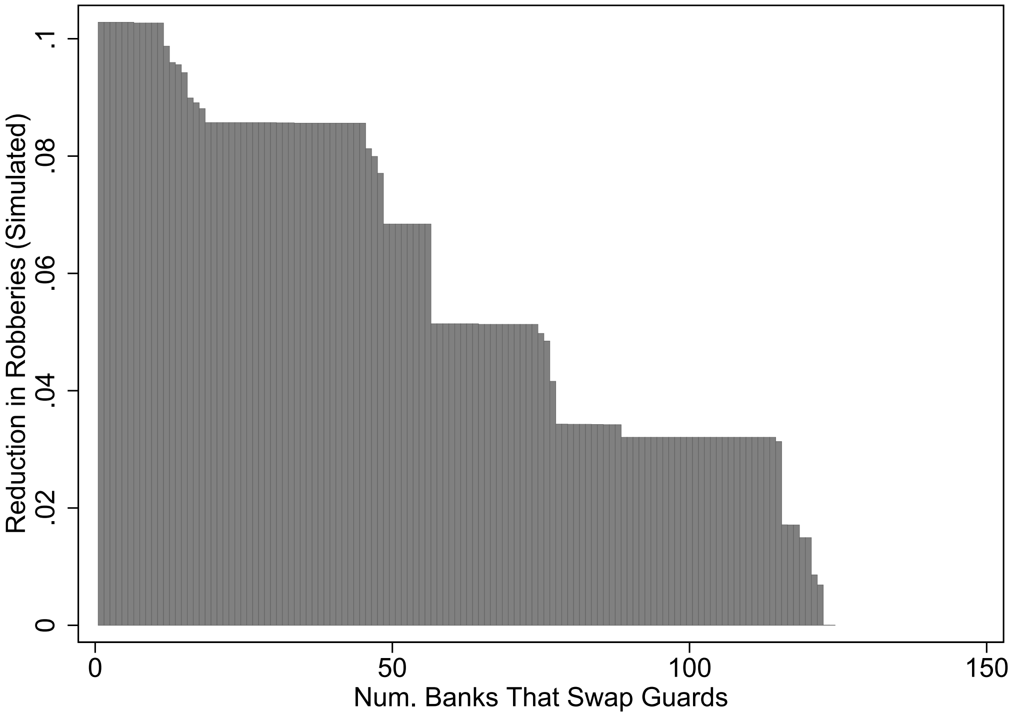
We present the results of this simulation in Figure 10. If roughly 20 banks swapped guards, each of these swaps would eliminate approximately 0.06 robberies. This reduction is primarily driven by the movement of guards from markets with many unguarded branches to markets with few unguarded branches. These moves will displace fewer robberies to unguarded branches. Of course, such markets may be rare, hence many banks with branches in fewer markets (or markets with less heterogeneous guard allocations) would generate much smaller reductions in robberies from these swaps.

6 Conclusion

Understanding whether visible security measures displace crime or extend deterrence to nearby areas is crucial for the design of intelligent law enforcement strategies. Unfortunately, the empirical challenges in identifying and estimating such effects are considerable. Based on a series of randomized control trials that increase policing in some well-defined areas, criminologists have embraced the idea that displacement

[27] A fundamentally different type of policy might involve restrictions on L_i, perhaps through cash holding regulations. Such regulations might compress variation in λ_i, and hence, following the logic of Proposition 3, potentially increase the likelihood of a socially suboptimal coordinated equilibrium in which no banks hire guards.

Figure 10: Simulated Changes in Robberies from Moving Guards Across Bank Branches



Notes: Changes are simulated using our preferred specification (column (3) of Table 4) with markets defined as 500m squares. Simulations are performed with data from 2005.

is at most limited and that benefits from increased policing diffuse to nearby areas (see Bowers et al., 2011, Braga, 2005). However, these studies must all contend with the inescapable fact that criminal perceptions are unobservable, which requires researchers to take a stand on how criminals perceive the spatial distribution of police changes. This is critically important from an empirical perspective, as misspecifying these perceptions can easily contaminate any analysis in favor of finding diffused benefits of deterrence as opposed to displacement of crime (Barr and Pease, 1990). Meanwhile, when economists have attempted to estimate deterrence effects of police patrols in quasi-experimental settings (Di Tella and Schargrodsky, 2004, Draca et al., 2011, Klick and Tabarrok, 2005), they have suffered from insufficient statistical power to measure potential displacement.

In this study we estimate deterrent and displacement effects of highly visible private security guards of commercial banks. In line with a game-theoretic model where banks' strategically invest in security measures, we find robust evidence that banks respond to the hiring and firing of guards operated by nearby banks. Our unique institutional setting allows us to circumvent numerous identification threats inherent to the measurement of displacement: we observe all potential targets of crime (and hence all potential units that could experience displacement), their precise spatial relationships with each other, all relevant attempted crimes, and all strategic responses of banks to one another.

Consistent with the existing economic literature, we find that visible guards act as a substantial deterrent to potential criminals. Unlike previous studies, we find that much of this reduction in crime is deflected towards nearby bank branches: about half of attempted robberies that are deterred by a security guard are displaced to nearby banks, but only to those that are unguarded.

Each year Italian banks spend about €200 million on security guards (Mastrobuoni and Rivers, 2019) to combat an epidemic of robberies. Our findings have immediate policy implications. The displacement effects that we estimate indicate a important role for the coordination of security investments by neighboring banks.

Indeed, we find strong evidence that certain banks overinvest in security guards in an uncoordinated fashion. Policies that promote coordination, either by encouraging all banks to hire guards or by encouraging all banks to fire guards, could efficiently reduce the victimization of banks in the aggregate. Given Italy's indubitable status as an outlier in robbery risk, such policies have the potential to generate substantial benefits to banks, consumers and law enforcement.

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A Proofs

Lemma 1. *Bank i will hire a guard only if all banks $j < i$ hire guards.*

Proof. We proceed by induction. Let k be the smallest number such that $g_k = 0$. By construction, $k-1$ banks hire guards, and because $g_k = 0$, $\pi(k-1) < \lambda_k$. Therefore $\pi(k-1) < \lambda_{k+1}$, hence $g_{k+1} = 0$. By induction, no bank $k' > k$ will hire a guard □

Proof. Proof of Proposition 1.

1. If equation (2) holds with equality, then $\pi(0, g_{-i}) = \pi(1, g_{-i})$ for all values ofg_{-i}. Without loss of generality, we can call this $\pi(0)$. By inequality (5) A bank will hire a guard if and only if $\lambda_i < \pi(0)$. The claim follows from the fact that the λ_i are weakly increasing.

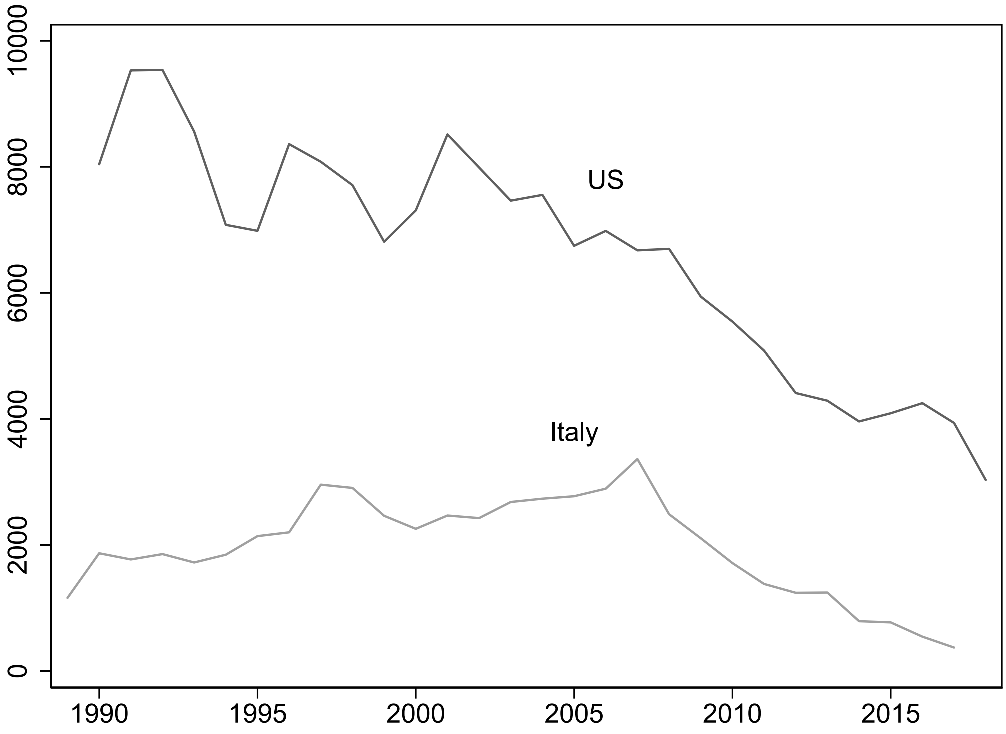
2. Equation (2) implies that π is a weakly increasing function in g_{-i}. The claim follows immediately from Lemma 1 and Proposition 1.1.

3. Since $\pi(i-2) > \lambda_{i-1}$ and $\pi(i-1) \leq \lambda_i$ by assumption, an equilibrium exists in which banks $1, ..., i-1$ hire guards. Since $\lambda_j < \pi(j-1)$, by Lemma 1 an equilibrium also exists in which banks $1, ..., j$ hire guards.

□

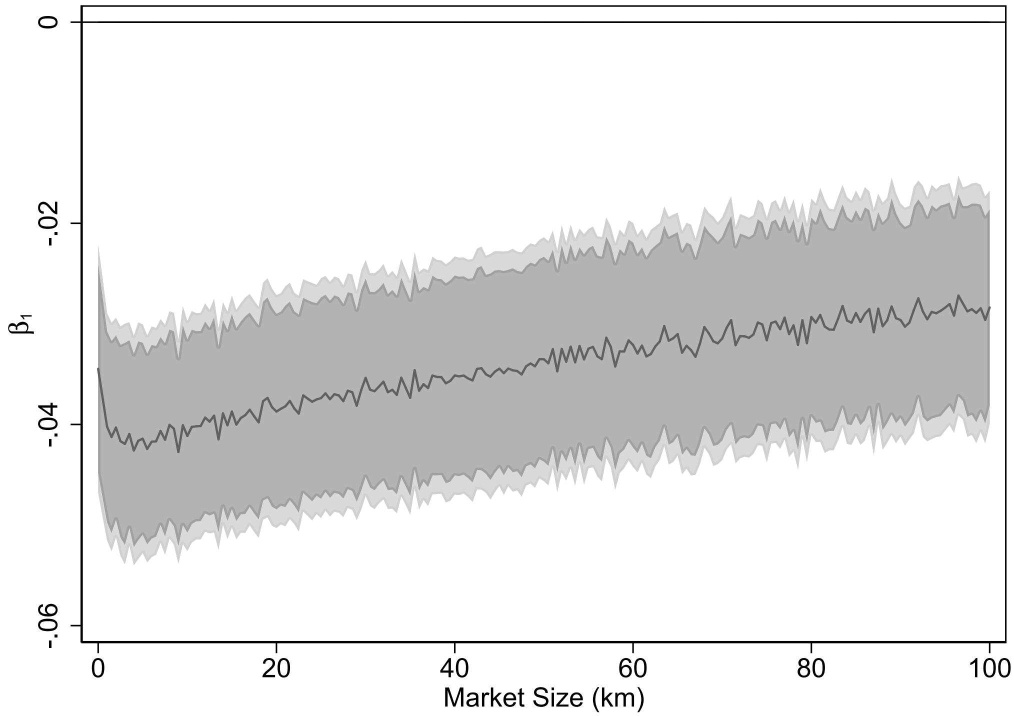
B Additional Figures

Figure A1: Bank Robberies, 1990-2016



Notes: Italian statistics are obtained from the Italian from Banking Association, and US Statistics are obtained from the Federal Bureau of Investigation.

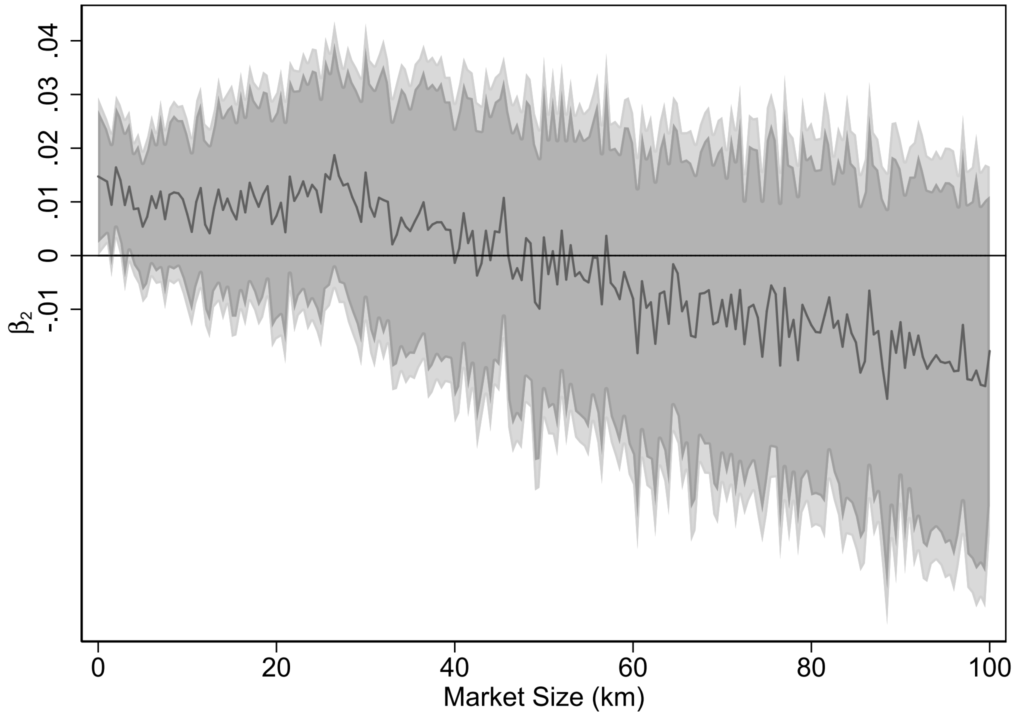
Figure A2: Estimates of β_1 Under Various Market Size Definitions



Notes: All controls from specification (4) of Table 4 are included. 95% confidence intervals are calculated with robust standard errors clustered by 50km squares.

C Additional Tables

Figure A3: Estimates of β_2 Under Various Market Size Definitions



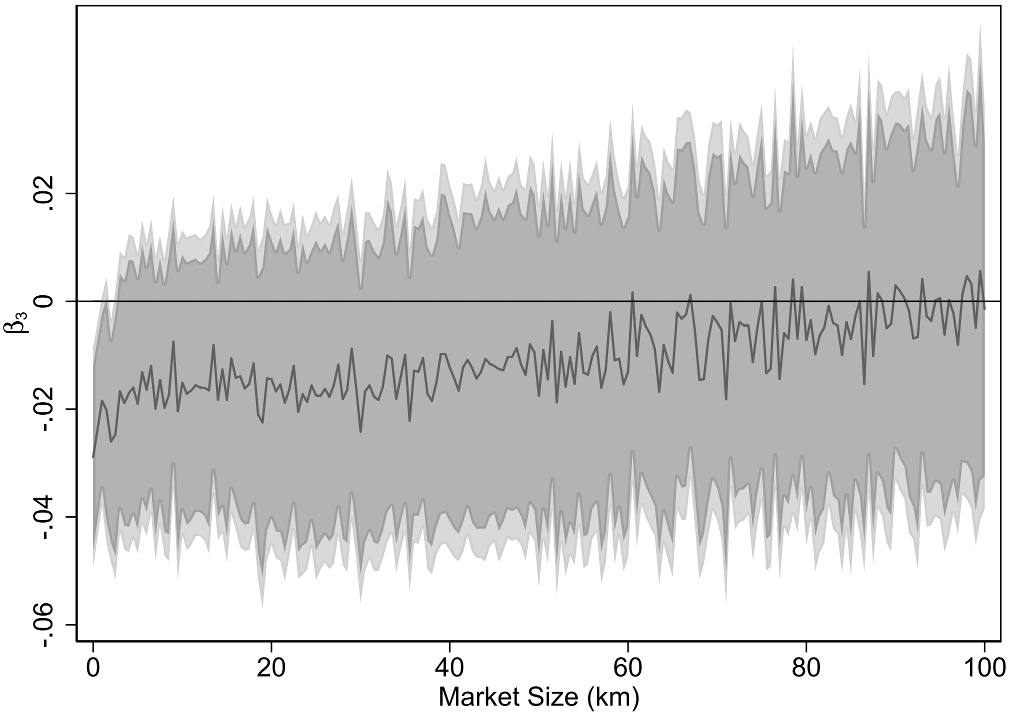
Notes: All controls from specification (4) of Table 4 are included. 95% confidence intervals are calculated with robust standard errors clustered by 50km squares.

Table A1: Estimates of Deterrence With and Without Switching Years

		All Years			Excluding Switching Years		
		Deterrence	SE	Observations	Deterrence	SE	Observations
(1)	None	-0.0043	(0.0041)	245,712	-0.0081*	(0.0047)	206,185
(2)	800km	-0.0068*	(0.0040)	245,712	-0.0097**	(0.0047)	206,185
(3)	400km	-0.0142***	(0.0037)	245,712	-0.0162***	(0.0044)	206,185
(4)	200km	-0.0193***	(0.0036)	245,712	-0.0212***	(0.0043)	206,185
(5)	100km	-0.0275***	(0.0035)	245,712	-0.0290***	(0.0041)	206,184
(6)	50km	-0.0345***	(0.0036)	245,712	-0.0364***	(0.0042)	206,184
(7)	25km	-0.0402***	(0.0037)	245,711	-0.0427***	(0.0041)	206,184
(8)	10km	-0.0426***	(0.0038)	245,707	-0.0453***	(0.0043)	206,166
(9)	5km	-0.0441***	(0.0038)	245,695	-0.0470***	(0.0043)	206,112
(10)	2km	-0.0430***	(0.0041)	245,670	-0.0468***	(0.0047)	206,020
(11)	1km	-0.0421***	(0.0040)	245,643	-0.0479***	(0.0045)	205,909
(12)	500m	-0.0386***	(0.0042)	245,612	-0.0445***	(0.0045)	205,730
(13)	250m	-0.0338***	(0.0044)	245,577	-0.0427***	(0.0049)	205,510

Notes: All regressions include province-year fixed effects. Robust standard errors clustered by 50km squares in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Figure A4: Estimates of β_3 Under Various Market Size Definitions



Notes: All controls from specification (4) of Table 4 are included. 95% confidence intervals are calculated with robust standard errors clustered by 50km squares.

Table A2: Estimates of Deterrence and Displacement Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable: Number of Robberies							
Guard (β_1)	-0.0489***	-0.0451***	-0.0498***	-0.0492***	-0.0488***	-0.0441***	-0.0399***	-0.0371***
	(0.0083)	(0.0084)	(0.0082)	(0.0077)	(0.0074)	(0.0065)	(0.0064)	(0.0073)
% Neighbors with Guards (β_2)	-0.0167	-0.0023	-0.0085	-0.0148	0.0082	0.0256**	0.0227**	0.0174
	(0.0363)	(0.0337)	(0.0263)	(0.0192)	(0.0138)	(0.0121)	(0.0109)	(0.0110)
Guard × % Neighbors with Guards (β_3)	0.0401	0.0041	0.0126	0.0023	-0.0009	-0.0157	-0.0277*	-0.0235
	(0.0362)	(0.0355)	(0.0331)	(0.0268)	(0.0203)	(0.0167)	(0.0150)	(0.0163)
No substitutes	-	-0.0964	0.0261*	0.0066	0.0091	0.0066	0.0097*	0.0108**
	-	(0.0745)	(0.0155)	(0.0083)	(0.0061)	(0.0053)	(0.0050)	(0.0049)
Square fixed effects	50km	25km	10km	5km	2km	1km	500m	250m
Year fixed effects	√	√	√	√	√	√	√	√
Observations	126,265	126,265	126,263	126,259	126,250	126,242	126,227	126,208
R-squared	0.0243	0.0284	0.0366	0.0490	0.0752	0.0997	0.1283	0.1582
$\beta_1 + \beta_3 \overline{g_{-ijt}}$	-0.0450	-0.0447	-0.0485	-0.0490	-0.0489	-0.0453	-0.0418	-0.0382
p-value ($\beta_2 + \beta_3 = 0$)	0.584	0.963	0.899	0.608	0.647	0.413	0.688	0.664
Average n. of Neighboring Branches	466.4	261.8	108.4	53.86	16.42	6.902	3.233	2.07

Note: The sample is restricted to bank branches that are based in 27 provinces (out of 110) whose corresponding metropolitan area contains the largest number of bank branches. Robust standard errors clustered by 50km squares in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Deterrence and Displacement Effects on Subsamples of Markets

	(1)	(2)	(3)
	Number of Robberies		
	Baseline	More similar branches	Less similar branches
Guard	-0.0321***	-0.0199***	-0.0462***
	(0.0045)	(0.0071)	(0.0070)
% Neighbors with Guards	0.0171**	0.0219	0.0145
	(0.0078)	(0.0139)	(0.0107)
Guard × % Neighbors with Guards	-0.0343***	-0.0514***	-0.0016
	(0.0119)	(0.0167)	(0.0211)
Number of Security Devices	0.0005*	0.0006	0.0015***
	(0.0003)	(0.0005)	(0.0004)
Neighbors Average Num. of Sec. Devices	-0.0006*	0.0005	0.0002
	(0.0003)	(0.0005)	(0.0005)
No substitute bank	0.0045	0.0031	0.0090
	(0.0039)	(0.0058)	(0.0071)
Observations	245,612	120,012	124,665
R-squared	0.1330	0.1872	0.1541
p-value ($\beta_2 + \beta_3 = 0$)	0.104	0.0462	0.530

Note: Market similarity is defined on the basis of the use of all security devices. For each of the 44 devices that we observe, we compute the standard deviation of their use in the market. Markets for which the sum of these standard deviations is below the median are classified as more similar, and markets for which the sum of these standard deviations is above the median are classified as less similar. Robust standard errors clustered by 50km squares in parentheses: *** p<0.01, ** p<0.05, * p<0.1.