



# The deterrence of crime through private security efforts: Theory and evidence



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## ABSTRACT

Private individuals and entities invest in a wide variety of market-provisioned self-protection devices or services to mitigate their probability of victimization to crime. However, evaluating the effect of such private security measures remains understudied in the economics of crime literature. Unlike most previous studies, the present analysis considers four separate measures of private security: security guards, detectives and investigators, security system installers, and locksmiths. The effects of laws allowing the concealed carrying of weapons are also evaluated. As private security efforts are potentially endogenous to crime rates, dynamic GMM panel data models are estimated in addition to structural (non-instrumented) regressions. The empirical results suggest that the impact of private security efforts generally varies across crime types, though there appears to be a robust negative relationship between the employment of security system installers and the rate of property offenses.

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## 1. Introduction

In addition to fiscally supporting police, courts, prisons, and other publicly provided methods for deterring crime, private individuals or entities may also invest in market-provisioned self-protection devices or services to mitigate their probability of victimization. Common examples of such 'private security' efforts include, but are not limited to, the installation of residential burglar alarms, the use of closed circuit television (CCTV) cameras by convenience stores, and the purchase of firearms and mace.

Private security investments reflect society's implicit 'derived demand' for offenses in the 'market model' of crime (Ehrlich, 1981, 1982, 1996; Ehrlich & Saito, 2010).<sup>1</sup> There has been a steady increase in the both the level and rate of private security investments over the past several decades. For instance, in the 1970s there were 1.4 public police officers for every private security officer, but this proportion fell to just 0.33 in the 1990s (Blackstone & Hakim, 2010). Private expenditures on self-protection are now larger than public expenditures used toward maintaining the criminal justice system. Philipson and Posner (1996) estimate

that annual expenditures on private protection amount to \$300 billion, although this figure does not include the opportunity cost of some private security measures (e.g., avoiding travel through crime-prone areas) (Ayres & Levitt, 1998).

It is well understood that private security efforts could either deter or displace crime depending on whether the investment is observable to potential offenders. Unobservable investments, such as carrying concealed handguns or installing hidden theft recovery systems in cars, might lower the probability of victimization for unprotected targets (a positive externality) since criminals cannot readily ascertain which potential victims are using the device. Overall crime rates may therefore fall in response to unobservable victim precautions. Conversely, observable precautions, such as employing uniformed security guards or installing a security system along with a sign indicating its presence, might simply displace crime to unprotected targets (a negative externality) assuming that the uptake of such investments is not sufficiently broad. The concomitant effect on the overall crime rate may be negligible or even positive.<sup>2</sup>

Despite the likely importance of self-protection in reducing victimization risks and the associated externalities, empirical evaluations of private security efforts (either observable or

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<sup>1</sup> The market model of crime is comprised of a 'derived demand for offenses' schedule and a 'supply of offenses' schedule. The intersection of the two schedules determines the equilibrium net return to offending ('price') and the equilibrium rate of offending ('quantity'). See Ehrlich (1996, pp. 46–49) for further details on the model.

<sup>2</sup> Lacroix and Marceau (1995) present a theoretical analysis wherein the adoption of an observable private security measure may even induce crime at the protected location by signaling the presence of something valuable.

unobservable) remain sparse in the economics of crime literature. Several studies rely on cross-sectional survey data for a *specific* locale (such as a city, community, or transportation system) and often evaluate only a single type of precaution.<sup>3</sup> Another body of literature employs aggregate-level data and uses panel data methods to estimate the effect of private security on crime—in particular, ‘shall-issue’ laws requiring law enforcement officials to issue concealed carry permits to qualified applicants.<sup>4</sup> These studies also tend to consider only a single type of private precaution and, therefore, may also suffer from omitted variable bias if various types of self-protection are correlated with each other (Benson & Mast, 2001).

This paper contributes to and expands upon the previous empirical literature on estimating the deterrent effect of private security measures by employing a rich, public dataset that has not been exploited in previous work. The present analysis considers four separate measures of private security efforts: security guards, detectives and investigators, security system installers, and locksmiths—as proxied by the employment levels in each group. The effects of laws allowing the concealed carrying of weapons (an unobservable precaution) are also evaluated. Since private security could be jointly determined with (endogenous to) crime, the analysis employs dynamic panel data methods to derive putatively consistent parameter estimates of the effect of self-protection measures.

The empirical results suggest that some types of private security efforts impact some types of crime, but few results generalize across crime categories. The employment of private security guards may help to reduce murder rates. Property offenses are negatively correlated with the employment rates of security system installers. There is also some evidence suggesting that security guards may deter larcenies, while private detectives and investigators may deter auto thefts. The passage of shall issue concealed handgun laws is predicted to either increase crimes by a small amount or, after instrumenting, to have no statistically significant impact.

The paper proceeds as follows. Section 2 discusses various circumstances under which even observable precautions of the type considered in the empirical analysis may impart general deterrence effects. Section 3 reviews the data while Section 4 discusses the empirical methodology for estimating the supply-of-crime regressions. Sections 5 and 6 respectively present structural and instrumental variable estimates of the effects of private security efforts on individual violent and property offense categories. Section 7 provides concluding remarks.

<sup>3</sup> Hakim and Shachmurove (1996) provide evidence that burglar alarms deter commercial burglaries. DiTella, Galiani, and Schargrodsky (2006) (Argentinean data) find that private security guards deter the incidence of residential burglaries. Armitage and Smithson (2007) (British data) conclude that gating residential back alleys reduces crime. See Benson (1997, 1998) for cites to and discussion of earlier studies.

<sup>4</sup> See, e.g., Lott and Mustard (1997), Ayres and Donohue (2003), Lott (2010), Aneja et al. (2012). Ayres and Levitt (1998) conclude that the (unobservable) Lojack anti-theft system deters auto thefts. Gonzalez-Navarro (2008) (Mexican data) reaches a similar conclusion. Benson and Mast (2001) find that some crimes are negatively correlated with the level of (possibly observable) security establishments or security personnel (in addition to concealed carry laws). Priks (2009) (Swedish data) finds that the installation of (observable) surveillance cameras reduced crimes in subway stations with some displacement to surrounding areas. Cook and MacDonald (2010) report that the establishment of business improvement districts in Los Angeles correlates with fewer crimes and arrests without any displacement effects to adjacent areas. Vollard and van Ours (2010) (Dutch data) find that a national law requiring the installation of burglary-proof windows and doors in new residential houses lowered the incidence of burglary with ambiguous displacement effects.

## 2. Observable private security efforts and the deterrence hypothesis

The recent theoretical (e.g., Helsley & Strange, 1999, 2004; Hui-Wen & Png, 1994; Shavell, 1991) and empirical literatures on private security focus on unobservable protection efforts such as the carrying of concealed weapons or the use of hidden theft recovery systems (e.g., Lojack).<sup>5</sup> These precautions could result in general deterrence because of the positive externality that arises from criminals being unable to determine which potential victims have actually adopted the efforts. Observable private security efforts, such as a sign indicating the presence of a burglar alarm system, may protect the specific target employing them, but criminals might simply divert their efforts toward visibly unprotected targets. As a result, aggregate crime rates, which are the focus of the market model, might not be affected by the adoption of such precautions.

Benson and Mast (2001), however, posit several theoretical avenues by which even highly visible private security efforts may still effectuate general deterrence and, therefore, be evaluated under the market model. *First*, if observable private security efforts result in criminals diverting their efforts between protected and unprotected targets, entrepreneurs may recognize new sales opportunities and offer more security services to potential targets. When such entrepreneurial efforts are successful, the number of protected targets with observable security efforts could expand over time, perhaps to an extent that “the expected cost of searching for targets and/or committing crimes could rise, making potential criminals less likely to become actual criminals.”<sup>6</sup>

*Second*, sellers of private security services may be unable to prevent non-payers from exploiting the private security investments paid for by actual customers. For example, some potential crime victims may be able to take advantage of observable security investments made by, say, retail business establishments, and, if so, then general deterrence effects may arise as a positive externality. As Benson and Mast (p. 730) note:

Firms in a shopping or entertainment area may employ security primarily to prevent shoplifting, vandalism, and employee theft, for instance, but a potential victim of robbery or rape may choose to shop or socialize in that area to take advantage of the security presence. If a substantial portion of potential victims behave this way, robberies and/or rapes could be reduced because the cost for potential criminals of finding an easy target is higher.

*Third*, purchasers of observable security efforts might recognize the presence of the above positive externalities and internalize them, e.g., by incorporating the cost of providing a secure environment into the price charged for the final product, such as a higher ticket price at a movie theater. In this case “[t]he general deterrence impact could still arise, but it would be paid for by those who benefit from it” (Benson & Mast, 2001, p. 730). And *fourth*, some ostensibly ‘observable’ private security efforts, such as plainclothes security guards or detectives, could in fact be difficult for potential criminals to detect. Uncertainty about where such security efforts are deployed could lead to a general deterrence effect.

<sup>5</sup> See *supra* notes 3 and 4 for cites to recent empirical studies.

<sup>6</sup> Benson and Mast (2001) and DiTella et al. (2006) present evidence that suggests the adoption of private security by higher-income households leads to more crime being diverted to lower-income households who tend to adopt private security at a much lower rate. An implication of this finding is that if lower-income households could afford private security (or where provisioned with it through government subsidy), then the displacement would not have occurred, which is consistent with Benson and Mast’s argument. See also Cook and MacDonald (2010, p. 14: “If adoption of effective technology is broad enough, the scope for displacement. . . is limited.”).

Cook and MacDonald (2010) also offer some insights into how observable precautions might induce general deterrence. To the extent that criminals are heterogeneous in their skills, observable precautions may affect unskilled criminals more than skilled ones. For instance, use of a steering wheel lock may lower aggregate auto theft rates by deterring many 'joyriding' thefts (which tend to be committed by younger, less experienced criminals) but have little effect on the behavior of higher-skilled professional car thieves who fence auto body parts. Owners of higher-valued vehicles (or property in general) may also be more likely to adopt security measures, thereby forcing criminals to divert their efforts toward less lucrative unprotected targets. If the return from committing crime against these latter targets is sufficiently low, criminals may be better off seeking out legitimate employment.

In summary, the potential for even visible private security efforts to induce general (as well as specific) deterrence allows even these types of precautions to be analyzed in the general context of the market model. The following section discusses data that can be used to operationalize the market model in order to empirically evaluate the relationship between private security efforts and crime.

### 3. Data

Previous empirical research on private security efforts and crime has relied on both individual-level data (obtained from surveys) and aggregate-level data. While some individual-level studies may consider a number of private security efforts, the data are typically measured at a single point in time. The cross-sectional estimates obtained from these studies may not control for a variety of unobservable factors that may influence both crime rates and the propensity to adopt private security, thereby leading to biased inferences. Furthermore, the extent to which the results from these studies may generalize to other groups or periods (be they proximate or otherwise) is uncertain.

Aggregate-level studies, which typically employ pooled cross-section time series (panel) data, have allowed researchers to better control for omitted variable bias through estimation of fixed effects models. These studies, however, have not controlled for the wide range of private security efforts due to data limitations. Benson and Mast (2001) use county-level data obtained from the US Census Bureau's *County Business Patterns* (CBP) dataset, which reports the number of establishments specializing in 'security and detective services.' However, the CBP dataset does not contain measures of any other forms of private security.

This study employs aggregate, state-level data compiled by the Bureau of Labor Statistics (BLS) on private security efforts that have not been considered in previous studies. The data are collected in the *Occupational Employment Statistics* (OES) series. The OES dataset provides estimates of the number of persons employed (and wage estimates) for approximately 800 occupations based on a series of establishment-level surveys.<sup>7</sup> Unlike the CBP dataset used by Benson and Mast (2001), the OES dataset reflects employment in private security occupations outside of those firms specializing in providing security services. For example, some private retail establishments (such as department stores) may maintain their own security personnel rather than contracting out for such services. Similarly, various government agencies might recruit, train, and

<sup>7</sup> The OES dataset classifies an 'employee' as any full- or part-time worker paid a wage or salary. The dataset does not reflect the self-employed, owners and partners in non-incorporated firms, household workers, or unpaid family workers. See BLS (2010) for further details.

retain their own security personnel rather than rely on a specialized private security firm.<sup>8</sup>

The OES dataset also goes beyond the CBP dataset in that it measures employment in several relevant occupations.<sup>9</sup> The CBP dataset groups together private guard and detective services, which is a potential shortcoming since the duties performed by those two employment groups are quite different. Annual OES employment estimates are separately available for security guards, private detectives and investigators, security and fire alarm system installers, and locksmiths and safe repairers. Employment trends in these occupations should reflect variation over time in the extent those private security efforts are acquired and deployed by end users across states.<sup>10</sup>

Private security services and devices are, of course, employed across a wide range of industries. Table 1 presents national-level employment estimates for the five largest industry employers of each OES private security group as of May 2006. Not surprisingly, the single largest employment industry is 'investigation and security services,' which comprises about 79.7% of the employment in those industries listed in Table 1.<sup>11</sup> 'Government' (including schools) is the second largest, comprising about 9.3%. Other notable industries include 'legal services' in the case of private detectives and investigators and 'hospitals' in the case of locksmiths and safe repairers.

While the OES dataset clearly provide several distinct advantages, Abraham and Spletzer (2010) highlight several issues that may hinder its application to any econometric analyses relying on the time series dimension of the data.<sup>12</sup> Before 1996, establishments in specific industries were surveyed on a three-year rotating cycle, but since then all industries are sampled in each year. However, save for the federal and state government 'industries,' the largest establishments within each industry are still surveyed only once every three years.

OES employment estimates for a given year are derived from panels of establishment surveys taken over prior years to ensure

<sup>8</sup> These data have not been widely used in academic studies; the majority of their (few) econometric uses have been in the labor economics literature. See, e.g., Dey, Houseman, and Polivka (2009), Dey and Stewart (2008), and Abraham and Spletzer (2010).

<sup>9</sup> The OES does not release state-level occupation-specific employment estimates by industry (such estimates are available only nationally). Thus, one cannot determine the number of, say, security guards employed within retail establishments at the state level.

<sup>10</sup> Employment levels for private guards and detectives will obviously represent the extent to which those efforts are directly acquired and deployed by the purchasers of those services. The relationship between employment and deployment levels pertaining, e.g., to alarm systems may be somewhat less direct, but changes in employment levels are still likely to reflect changes in the extent to which alarms, security systems, safes, etc. are purchased and utilized by end users (in the fixed-effects framework employed here the estimated effects of private security on crime are identified off of within-state variations in these measures).

<sup>11</sup> This figure is likely biased upward (and all others downwards) since the OES data are effectively capturing counts of employees that are 'directly' employed by each industry. It is almost certain that many of the persons counted in the 'investigation and security services' industry are actually deployed in other industries (e.g., government) through outsourcing (contracting with security firms). Regardless, these data may give a rough approximation of the rank order (in terms of employment intensity) of those industries (outside of 'investigation and security services') that employ and outsource private security services.

<sup>12</sup> Indeed, the BLS does not encourage using OES data for conducting time series analysis and urges researchers that choose to do so "to note the changes in survey procedures and the limits of the methods used with a pooled sample" (BLS, 2010). As discussed herein, it does not appear that these changes have had a large effect on the estimates for the occupational classes considered in this analysis, and the BLS acknowledges that "comparisons of occupations [over time] that are not affected by classification changes may be possible if the methodological assumptions hold." (*Id.*) Furthermore, the econometric methodology employed herein (which relies on panel data methods as opposed to pure time series analysis) should help to account for some of the shortcomings in the OES data (bearing in mind the BLS's warnings regarding these methods).

**Table 1**  
Top five employment industries for various private security services (as of May, 2006).

Security guards		Private detectives and investigators		Security and fire alarm system installers		Locksmiths and safe repairers	
Industry	Employment	Industry	Employment	Industry	Employment	Industry	Employment
Investigation and security services	560,380	Investigation and security services	17,180	Investigation and security services	25,920	Investigation and security services	13,610
Local government	34,630	State government	2300	Building equipment contractors	18,600	Colleges, universities, and professional schools	1070
General medical and surgical hospitals	33,130	Local government	1240	Electrical and electronic goods merchant wholesalers	1040	Elementary and secondary schools	370
Elementary and secondary schools	31,340	Legal Services	1200	Machinery, equipment, and supplies merchant wholesalers	980	State government	340
Traveler accommodation	28,630	Business support services	1030	Miscellaneous durable goods merchant wholesalers	530	General medical and surgical hospitals	280

Notes: Data are from the BLS Occupational Employment Statistics series.

that large establishments are sampled. Prior to 2002, estimates were based on the current year's survey panels plus the previous two years' (each panel consists of about 400,000 establishments across all industries) with establishments assigned an October, November, or December reference date. Beginning in 2002, the OES survey moved to a design using six semi-annual panels (each consisting of about 200,000 establishments) with each establishment assigned a May or November reference date.<sup>13</sup> As such, ensuring that a given year's estimates reflect the largest establishments in a given industry comes at the cost of those estimates not reflecting data pertaining exclusively to that year (which in turn makes it less likely that the data are able to capture actual year-to-year changes). The estimates, however, are benchmarked to the average of the most recent May and November employment levels.

Changes in industry classification schemes used in constructing the OES dataset may be another problem. In 1999, the OES survey changed from its own survey-specific occupational coding system to the Standard Occupational Classification (SOC) system, and in 1999 it changed from the Standard Industrial Classification (SIC) system to the North American Industry Classification System (NAICS). These changes may make it difficult to make meaningful comparisons in the OES employment estimates over time. For example, only about half of all surveyed establishments could be assigned NAICS codes based upon their earlier SIC classification (Abraham & Spletzer, 2010).

This study uses OES data beginning in 1999, which corresponds to the earliest year for which employment estimates for all four of the above-mentioned private security occupations are available, through 2010. Fig. 1 graphs the national-level OES employment levels for the four private security occupation classes over the sample period. Most of the series display a relatively stable pattern over the sample period, suggesting that the changes in the occupational classification schemes underlying the OES survey in 1999 and 2002 did not have a large effect on these employment estimates. The exception is the alarm and security system installer series. In the first three years of the sample (1999–2001) there is a marked peak in the series that occurs in 2000. Variation in the number of state cells containing employment estimates over these years appears to explain some of this movement. Data are available for only 22 states in 1999, but this number increases to 38 states (an increase

of 72%) by 2000. In 2001, however, the number of state-cells with usable observations then falls to 32.

There is a noticeable upward trend in the alarm and security system installer series after 2001. One possible explanation for this effect is the adoption of the NAICS system by the OES in 2002. However, this explanation does not seem especially compelling given the smoother year-to-year patterns in the other series. Another possible explanation is that the September 11, 2001 terrorist attacks increased demand for private security, especially the demand for security and alarm systems.<sup>14</sup>

#### 4. Empirical specification

Let  $j$  index the OES private security occupation groups. The supply-of-offenses regression takes the following dynamic, double-logarithmic form:

$$\ln O_{i,t} = \alpha + \beta \ln O_{i,t-1} + \sum_j \gamma^{(j)} (\ln \text{PrivSec}_{i,t}^{(j)}) + \delta \text{Shall}_{i,t} + \Psi \ln D_{i,t-1} + \theta_i + \lambda_t + \tau_{i,t} + \varepsilon_{i,t}, \quad (1)$$

where  $\alpha$  and  $\varepsilon_{i,t}$  denote the constant and random error term, respectively. The subscript  $i = \{1, \dots, 51\}$  indexes states (including the District of Columbia) and  $t = \{1999, \dots, 2010\}$  years. The variables  $\theta_i$  and  $\lambda_t$  denote vectors of state and year indicators, respectively, while  $\tau_{i,t}$  denotes a vector of state-specific time trends.<sup>15</sup> Table 2 presents descriptive statistics for select covariates used in estimating Eq. (1). These variables are discussed further below.

The dependent variable,  $O_{i,t}$ , denotes the reported number of Uniform Crime Reports (UCR) Part I offenses per 100,000 state residents. The variable  $O_{i,t-1}$ , which is employed as a regressor, is the once-lagged value of the dependent variable. Part I offenses consist of both violent (murder, rape, robbery, and aggravated assault) and property (burglary, larceny, and auto theft) crimes. Specifications using total violent or property offenses are not considered since the former is dominated by assaults and the latter by larcenies; there is also little or no reason to weigh the individual crime categories equally.

<sup>13</sup> So, e.g., based on the current OES sampling design, annual estimates pertaining to May, 2008 are derived from data for the 'current panel' (dated May 2008) and the five previous panels (November 2005, May 2006, November 2006, May 2007, and November 2007), for six semi-annual panels in total.

<sup>14</sup> Dain and Brennan (2003) discuss the increasing liability of property owners for failing to provide security to patrons following the September 11th attacks.

<sup>15</sup> Eq. (1) does not include the usual assortment of demographic covariates used in empirical crime models (e.g., population density, percentage urban, separate variables for age groups, etc.) as these measures evolve only slowly through time and are likely to be highly correlated with the included state dummies and state-specific trends.

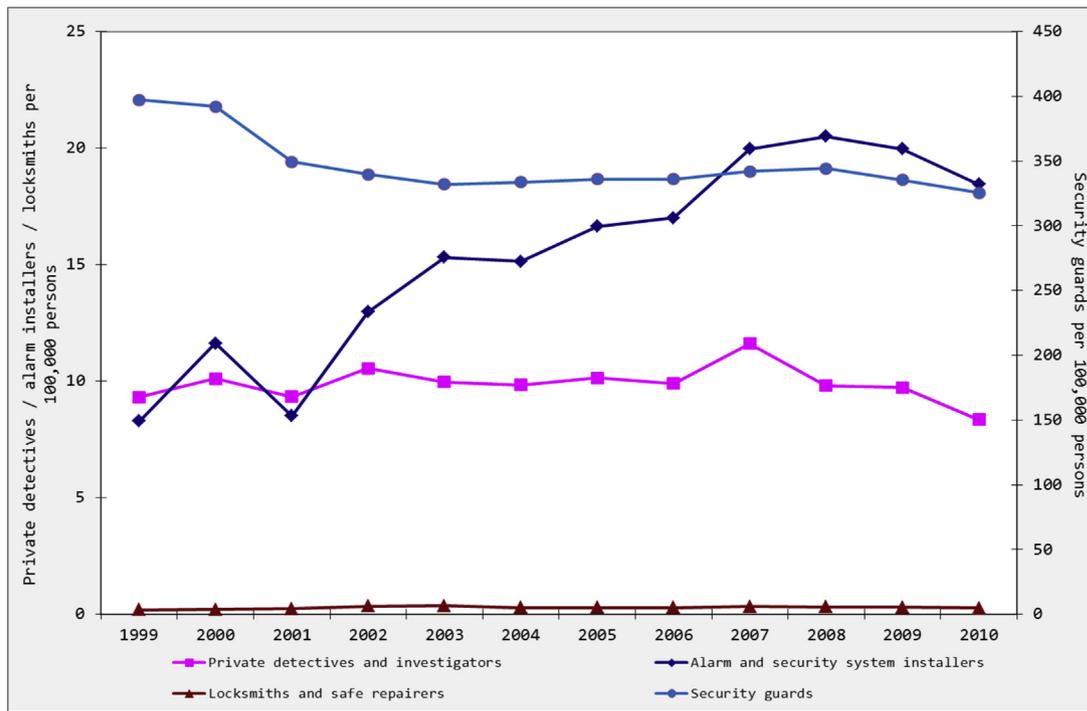


Fig. 1. National-level OES private security occupation employment trends, 1999–2010.

Table 2  
Descriptive statistics and data sources for select variables.

Variable	Mean	Std. dev.	Minimum	Maximum
Murders per 100,000 persons	5.247	5.001	0.456	46.435
Rapes per 100,000 persons	33.395	11.169	11.146	92.939
Robberies per 100,000 persons	116.430	98.446	6.752	763.482
Aggravated assaults per 100,000 persons	270.684	145.736	34.087	963.909
Burglaries per 100,000 persons	698.381	232.854	291.476	1286.887
Larcenies per 100,000 persons	2325.702	544.769	1178.997	4181.310
Auto thefts per 100,000 persons	345.559	219.505	70.452	1715.423
Private security guards per 100,000 persons	340.860	255.546	61.170	2107.763
Private detectives and investigators per 100,000 persons	9.746	5.136	1.711	43.887
Security and fire alarm systems installers per 100,000 persons	16.191	7.675	2.199	45.354
Locksmiths and safe repairers per 100,000 persons	5.934	3.479	0.608	34.361
Shall	0.691	0.462	0.000	1.000
Lagged police per 100,000 persons	284.113	90.176	146.515	831.374
Lagged prisoners per 100,000 persons	425.611	185.040	117.891	1885.023

Notes: Figures represent annual state-level data for the years 1999–2010. The number of observations ranges from 484 to 612. The sources of the data are as follows. Individual violent and property crime rates (and arrest rates, estimates not shown): Federal Bureau of Investigation, *Uniform Crime Reports*, <http://www.fbi.gov/about-us/cjis/ucr/ucr/>. Private security guard; detective and investigator; security and fire alarm installer; and locksmith and safe repairer employment: Bureau of Labor Statistics, *Occupational Employment Survey*, [http://www.bls.gov/oes/oes\\_dl.htm](http://www.bls.gov/oes/oes_dl.htm). Shall-issue laws: data for 1999–2006 from Aneja et al. (2012); data for 2007–2010 compiled by author. Police: US Census Bureau, <http://www.census.gov/govs/apes/> (data reflect police employment by local governments). Prisoners: Bureau of Justice Statistics, <http://bjs.ojp.usdoj.gov/index.cfm?ty=pbse&sid=40> (data reflect persons incarcerated under the jurisdiction of state or federal correctional authorities). State population: US Census Bureau, <http://www.census.gov/popest/index.html>.

The variable  $D_{i,t-1}$  denotes a vector of ‘public’ deterrence variables, which—according to the ‘market for offenses’ model—are expected to influence the impact of private security. These measures include police employment and prisoners per 100,000 state residents as well as offense-specific arrest rates. These variables are lagged one year to help mitigate simultaneity bias. All other Greek letters denote (vectors of) coefficients to be estimated.

The variable  $PrivSec_{i,t}^{(j)}$  denotes employment in the  $j$ th OES private security group, expressed on a per-capita basis. The  $\beta^{(j)}$  coefficients represent the associated crime rate elasticities measured with respect to the  $j$ th employment class. An estimate of  $\beta^{(j)}$  that is negative and statistically significant is interpreted as evidence of a (general) deterrent effect of that private security effort.

Another private precaution taken against crime is the ownership of weapons, particularly handguns. Allowing private citizens

to carry concealed (unobservable) handguns may induce general deterrence. A large and contentious empirical literature debates the efficacy of shall-issue concealed carry laws in reducing crimes, with some studies finding large negative effects (consistent with deterrence) and others finding no effects or even positive effects.<sup>16</sup> Following this literature, Eq. (1) includes a dummy variable  $Shall_{i,t}$  reflecting the presence of a shall-issue law allowing citizens to carry concealed handguns. This variable takes a value of one in the first full year following the legal adoption of the law and in each subsequent year the law is in effect.

<sup>16</sup> See Lott (2010) for citations to and critical discussion of these various studies.

## 5. Newey–West estimations

Eq. (1) is first estimated *via* Newey–West (hereafter ‘N–W’) regression taking the private security (and all other) variables as conditionally exogenous. The N–W estimator provides test statistics that are robust to heteroskedasticity and autocorrelation of arbitrary form.<sup>17</sup> In implementing the estimator, the maximum order of significant autocorrelation in the error terms is set equal to one.

Table 3 presents the estimation results. In each specification the covariates are jointly statistically significant. Although not reported, *F*-statistics computed separately for the year dummies, state dummies, and state-specific time trends indicate that each set of controls was highly statistically significant. The Arellano–Bond test fails to reject the null hypothesis of no (first-order) serial correlation in the residuals.

The point estimate of the security guards elasticity is negative and statistically significant in the murder and auto theft specifications. A one percent increase in per-capita private security guards is associated with a 0.25% decrease in per-capita murders and a 0.12% decrease in per-capita auto thefts. The estimated elasticity on private detectives is negative in only three of the seven specifications and never statistically significant at conventional levels.

The effect of security and fire alarm installers is negative in five of the seven crime models and statistically significant for the property offenses of burglary, larceny, and auto theft. Within these latter results, the estimated elasticity ranges from  $-0.03$  to  $-0.02$ . The estimated elasticities pertaining to locksmiths and safe repairers takes a negative sign only in the rape and auto theft regressions, with the estimated elasticity being statistically significant in the former and equal to  $-0.03$ . The elasticities on this measure are positive and statistically significant in the murder and robbery regressions, though again these findings might reflect the influence of endogeneity.

The shall-issue coefficient takes a positive sign in all regressions save for the rape model and is statistically significant in the murder, robbery, assault, burglary, and larceny models. These latter findings may imply that the passage of shall-issue laws increases the propensity for crime, as some recent research (e.g., Aneja, Donohue, & Zhang, 2012) has suggested. However, as the shall-issue law impact is being identified from only eight state changes in the data, it is difficult to give any strong causal interpretation to these estimates.

The lagged dependent variable is positively and significantly correlated with crime in all specifications except murder. The other coefficient estimates are generally consistent with expectations and the deterrence hypothesis. The estimated arrest elasticities are negative in five of the seven specifications and statistically significant in the murder and larceny regressions. A 1% increase in the arrest rate for murder (larceny) correlates with .04 (0.01)% fewer murders (larcenies). The estimated (lagged) per-capita police employment elasticity is negative in five cases but statistically significant only in the murder regression. Specifically, a 1% increase in the (lagged) police rate is associated with a 0.39% decrease in per-capita murders. The estimated effect of increased incarceration rates is negative and statistically significant in the murder and robbery regressions. The associated elasticity is estimated at  $-0.60$  in the former case and  $-0.23$  in the latter.

## 6. Dynamic-GMM estimations

The above N–W estimates of private security effects could be inconsistent as they do not address the potential underlying bias from the predetermined or endogenous variables. Determining the uncontaminated causal effect of private security effects on crime therefore necessitates ‘breaking’ the simultaneity between crime and private security.

While many studies in the empirical economics of crime literature address endogeneity concerns (whether they be in regard to deterrence, labor market, or other factors), an ongoing difficulty with these efforts is the identification of appropriate instrumental variables. In this regard, recent studies in this literature (e.g., Moody & Marvell, 2008; Saridakis & Spengler, 2009) employ so-called ‘dynamic GMM’ estimators developed, *inter alia*, by Holtz-Eakin, Newey, and Rosen (1998), Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998). These estimators rely on so-called ‘internal’ instruments (which are derived as the lagged levels or lagged differences of the endogenous regressors themselves), thereby obviating the need for the researcher to otherwise find suitable ‘external’ instruments (assuming any exist). Statistical tests can then be conducted to explore the performance of the full set of employed instruments in terms of their validity and relevance.

Dynamic GMM estimators are also specifically designed to address a number of econometric and data issues relevant to this study<sup>18</sup>—in particular, the dynamic bias in fixed effects models with lagged dependent variables (Nickell, 1981). The potential for dynamic bias is particularly relevant to panels with a large number of groups relative to periods, as is the case here. Dynamic GMM estimators account for this bias by also instrumenting the (endogenous) lagged dependent variable.

The predominant dynamic GMM estimators are ‘difference-GMM’ and ‘system-GMM’ (Roodman, 2009). The difference-GMM estimator applies lagged levels of the endogenous variables as instruments in a first-differenced fixed effects model. System-GMM combines the first-differenced model with the same model in levels and uses lagged differences of the endogenous variables for the differenced equation. While system-GMM is asymptotically more efficient relative to difference-GMM, the latter is preferable in the present context. The system-GMM estimator requires that the instruments are uncorrelated with unobserved state fixed effects—a condition that is met if the time series is stationary. In principle, panel unit root tests could be employed to determine stationarity, but such tests would be expected to be very low powered with the relatively short panel employed here. Since difference GMM by definition relies on a first-differenced specification, all nonstationary variables are transformed to stationary ones (assuming they follow an  $I(1)$  process), thereby making the estimator less sensitive to initial conditions.

Inference with dynamic GMM estimators is affected by the instrument count (Roodman, 2008, 2009). The number of instruments generated using these methods is increasing in the time (and group) dimension of the panel, and inference in finite samples is biased when the number of instruments becomes large (i.e., approaches the sample size). At the same time, the relative efficiency of system-GMM is achieved in part through the use of

<sup>17</sup> Estimating the same models discussed herein with cluster-robust standard errors (with the clustering correction applied to the state level) produced qualitatively similar results.

<sup>18</sup> Specifically, these estimators are applicable to applications involving: (1) large  $N$ , small  $T$  panels; (2) a linear functional relationship; (3) a single dynamic, left-hand-side variable (which depends on its own past realizations); (4) independent variables that are not strictly exogenous (i.e., correlated with past and possibly current realizations of the error); (5) fixed group effects; and (6) heteroskedasticity and autocorrelation within (but not across) groups (Roodman, 2009). All of these aspects are applicable in the present case.

**Table 3**  
The effect of private security efforts on crime: Newey–West regressions, 1999–2010.

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto theft
ln(lagged dependent variable)	−0.200** (2.356)	0.226*** (2.885)	0.300*** (3.921)	0.439*** (6.747)	0.477*** (7.509)	0.431*** (6.612)	0.583*** (8.307)
ln(security guards per 100,000 persons)	−0.248*** (2.624)	0.039 (0.725)	−0.015 (0.199)	−0.047 (0.885)	−0.076 (1.586)	−0.036 (1.019)	−0.115* (1.747)
ln(private detectives and investigators per 100,000 persons)	−0.001 (0.045)	0.011 (0.861)	−0.005 (0.354)	0.009 (0.657)	0.010 (1.051)	0.005 (0.713)	−0.011 (0.796)
ln(security and fire alarm systems installers per 100,000 persons)	0.009 (0.344)	0.011 (0.724)	−0.004 (0.174)	−1.357E−04 (0.009)	−0.024* (1.678)	−0.026** (2.240)	−0.031* (1.826)
ln(locksmiths and safe repairers per 100,000 persons)	0.056** (2.175)	−0.032** (2.376)	0.030* (1.809)	0.001 (0.077)	0.009 (0.921)	0.003 (0.414)	−0.004 (0.274)
Shall	0.155** (2.300)	−0.011 (0.418)	0.066* (1.935)	0.050** (2.029)	0.062*** (2.621)	0.031** (1.969)	0.042 (1.298)
ln(lagged crime-specific arrest rate)	−0.035* (1.735)	4.742E−04 (0.029)	−0.021 (1.470)	−0.003 (0.198)	−0.020 (1.537)	−0.014* (1.946)	0.012 (0.980)
ln(lagged police per 100,000 persons)	−0.386** (2.036)	0.067 (0.587)	−0.188 (1.355)	−0.056 (0.449)	−0.020 (0.225)	0.041 (0.589)	−0.113 (0.996)
ln(lagged prisoners per 100,000 persons)	−0.603*** (3.310)	−0.064 (0.559)	−0.227* (1.664)	0.098 (0.870)	0.005 (0.054)	0.095 (1.456)	0.143 (1.053)
Constant	9.676*** (5.918)	2.545** (2.541)	5.871*** (4.499)	3.038** (2.941)	3.966*** (4.269)	3.846*** (4.700)	2.758* (2.129)
Observations	371	372	372	372	372	372	372
F-statistic ( $H_0$ : All slopes = 0) ( <i>p</i> -value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Arellano–Bond test ( <i>p</i> -value)( $H_0$ : No first-order (AR(1)) serial correlation in residuals)	0.281	0.232	0.341	0.952	0.821	0.982	0.475

Notes: All regressions reflect annual state-level data and contain full sets of state indicators, year indicators, and state-specific time trends (estimates not shown). The dependent variable in each regression is the natural log (“ln”) of the crime rate per 100,000 persons listed at the top of the column. Absolute value of *t*-statistics reflecting Newey–West heteroskedasticity- and autocorrelation-consistent (HAC) standard errors in parentheses.

\* Statistical significance at the 10% level in a two-tailed test.

\*\* Statistical significance at the 5% level in a two-tailed test.

\*\*\* Statistical significance at the 1% level in a two-tailed test.

additional instruments. Given the time dimension of the panel and a potential maximum of 51 groups, difference-GMM is likely a better option relative to system-GMM for reducing the number of instruments and obtaining valid inference. Nevertheless, as there are still more moment restrictions than groups, there may be some risk of over-fitting the endogenous regressors (Roodman, 2009).<sup>19</sup> Additionally, whether the dynamic GMM estimates identify actual causal effects depends critically on the lagged instruments not suffering from the same sources of endogeneity assumed to affect the private security measures themselves. This may be a strong assumption, and the usual instrumental variable regression diagnostics may be relatively low-powered in their ability to reject any suggested violation of the necessary exclusion restrictions. Therefore, the conjectural nature of the dynamic GMM estimations reported below should be kept in mind when interpreting the results.

Table 4 presents one-step difference-GMM estimates of the relevant analog to Eq. (1).<sup>20</sup> All four private security measures are

<sup>19</sup> In these estimations, the lagged deterrence controls for arrest, police employment, and imprisonment rates may be predetermined, thereby necessitating that they also be instrumented in order to obtain unbiased estimates of their impacts on crime rates. However, this approach is not taken here because doing so would require imposing even more moment restrictions. Furthermore, Arellano (2003) shows that the order of magnitude of the bias for predetermined variables when not instrumenting is smaller (asymptotically) relative to that for endogenous variables in (one-step) GMM estimation.

<sup>20</sup> The covariance matrix of dynamic GMM estimators can be obtained through the ‘one-step’ or ‘two-step’ option (see Roodman, 2009 for further details), the latter being asymptotically more efficient. Arellano and Bond (1991) use Monte Carlo methods to show that two-step estimation of the difference-GMM model results in severely downward biased standard error estimates. Windmeijer (2005) offers a correction, but in the instant case this approach resulted in some models failing to estimate, thereby forcing reliance on the one-step estimator. Blundell and Bond (1998), however, show that one-step standard errors are virtually unbiased for moderately sized samples. Furthermore, the estimated standard errors on all the difference-GMM estimates presented herein are obtained from a robust estimator of

treated as endogenous variables. In addition, the lagged dependent variable is taken as predetermined, so this variable is instrumented as well.

The ‘GMM-style’ instruments applied to these measures are their respective lagged levels. The Sargan test of overidentifying restrictions is used as a baseline in selecting the number of lags used for the GMM-style instruments, starting with a baseline lag-order of three and adding more lags until the test becomes satisfied.<sup>21</sup> In most instances, this latter condition was met (thereby indicating that the instruments were exogenous and that the system was correctly specified) across models with the use of three lags,<sup>22</sup> but in two cases (assault and auto theft) higher-order lags were needed to obtain an insignificant Sargan test. Following standard practice, all other covariates used in estimating Eq. (1) are used as additional, non-excluded instruments.<sup>23</sup>

the covariance matrix, which results in standard error estimates that are consistent in the presence of heteroskedasticity and autocorrelation of arbitrary form within panels.

<sup>21</sup> While either the Sargan or Hansen test can be used to evaluate the overidentifying restrictions, the former is likely better suited for the dataset and models considered here. The Hansen test is severely weakened (will fail to reject a false null hypothesis) when the instrument count is large relative to the number of groups. According to Roodman (2009), a Hansen-test *p*-value of even 0.25 with a relatively large instrument set could be indicative of weak test. In the present case, the estimated *p*-values for the Hansen test were well above this level even when the Sargan tests were satisfied. It is thus unlikely that the Hansen test can be relied upon for these estimations. And although the Sargan test (unlike the Hansen test) is not robust to heteroskedasticity, this problem is assumed to be of relatively less importance here, and as such, the computed Sargan test is reported for all estimations.

<sup>22</sup> Another way to reduce the number of instruments is to ‘collapse’ them (Roodman, 2009). However, simply restricting the lag length to a single order as done here resulted in a smaller set of instruments.

<sup>23</sup> Note that state fixed effects are netted out due to differencing, and as such, the state dummies are not directly employed in the various difference-GMM models.

**Table 4**

The effect of private security efforts on crime: one-step difference-GMM regressions, 1999–2010.

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto theft
ln(lagged dependent variable)	−0.528*** (2.706)	−0.122 (0.906)	0.085 (0.513)	0.472*** (4.098)	0.476** (2.586)	0.313** (2.602)	0.509** (2.560)
ln(security guards per 100,000 persons)	−0.870** (2.173)	−0.240 (1.191)	−0.288 (1.010)	−0.026 (0.111)	0.002 (0.009)	−0.212* (1.702)	−0.375 (1.610)
ln(private detectives and investigators per 100,000 persons)	0.059 (0.893)	0.026 (0.697)	−0.033 (0.862)	−0.018 (0.431)	0.015 (0.459)	0.028 (1.379)	−0.080** (2.228)
ln(security and fire alarm systems installers per 100,000 persons)	0.057 (0.478)	−0.029 (0.593)	−0.098** (2.144)	0.124 (1.641)	−0.073* (1.715)	−0.072** (2.275)	−0.129** (2.296)
ln(locksmiths and safe repairers per 100,000 persons)	0.008 (0.095)	−0.037 (0.858)	−0.028 (0.668)	0.005 (0.093)	0.068 (1.553)	0.030 (1.296)	0.017 (0.346)
Shall	0.041 (0.204)	0.021 (0.236)	0.161 (1.313)	−0.004 (0.066)	−0.040 (0.436)	0.004 (0.074)	0.067 (0.644)
ln(lagged crime-specific arrest rate)	−0.006 (0.170)	0.016 (1.118)	−0.011 (1.211)	0.005 (0.364)	−0.013 (0.926)	−0.010 (1.318)	1.998E−04 (0.011)
ln(lagged police per 100,000 persons)	−0.327 (1.514)	0.073 (0.606)	−0.122 (1.014)	−0.009 (0.049)	0.086 (0.579)	0.005 (0.053)	−0.032 (0.204)
ln(lagged prisoners per 100,000 persons)	−0.798*** (2.883)	−0.026 (0.140)	−0.667*** (3.510)	0.133 (0.632)	−0.167 (0.898)	−0.033 (0.268)	−0.002 (0.007)
Observations	282	282	282	282	282	282	282
F-statistic ( $H_0$ : All slopes = 0) (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Lag-order of GMM-style instruments	3	3	3	5	3	3	6
<i>IV regression diagnostics (p-values)</i>							
Sargan statistic ( $H_0$ : overidentifying restrictions are valid)	0.378	0.177	0.476	0.541	0.169	0.144	0.182
AR(1) test ( $H_0$ : No first-order autocorrelation in first-differences)	0.264	0.098	0.080	0.038	0.018	0.004	0.003
AR(2) test ( $H_0$ : No second-order autocorrelation in first-differences)	0.118	0.100	0.031	0.026	0.923	0.611	0.504
<i>Weak instruments test</i>							
Within-group (lower bound) autoregressive coefficient	−0.535	−0.276	−0.073	0.024	−0.059	−0.073	−0.017
Pooled OLS (upper bound) autoregressive coefficient	0.086	0.678	0.510	0.673	0.699	0.700	0.731

Notes: All regressions reflect annual state-level data and contain full sets of year indicators and state-specific time trends (estimates not shown). The dependent variable is the (first-differenced) natural log ("ln") of the crime rate per 100,000 persons listed at the top of each column. All regressions treat the private security covariates (including shall-issue laws) as conditionally endogenous. In addition to the GMM-style instruments, all models employ the lagged arrest, police, and incarceration rates, as well as the full sets of year dummies and state-specific time trends, as "excluded" instruments. Absolute value of *t*-statistics reflecting heteroskedasticity- and autocorrelation-consistent (HAC) standard errors in parentheses.

\* Statistical significance at the 10% level in a two-tailed test.

\*\* Statistical significance at the 5% level in a two-tailed test.

\*\*\* Statistical significance at the 1% level in a two-tailed test.

### 6.1. Evaluating the instruments

Before proceeding to the point estimates, consider the regression diagnostics given at the bottom of Table 4. All seven models are statistically significant (as indicated by the *F*-statistics).

Arellano and Bond (1991) note that serial correlation in the idiosyncratic error term  $\varepsilon_{i,t}$  may cause some of the lagged instruments to be rendered invalid. The authors develop a test for serial correlation that is applied to the differenced residuals. First-order (AR(1)) serial correlation is expected in the differenced residuals, and this result is borne out in the murder and larceny regressions. The AR(1) test, however, does not speak to the validity of the instruments because validity depends on whether there is serial correlation in the levels of the residuals. This latter source of serial correlation can be evaluated by testing for AR(2) serial correlation in the differenced residuals. This latter test is insignificant in all cases except in the robbery and assault regressions, and as such, the point estimates on the private security measures in these two models may still be biased.

Instruments must be relevant in addition to being valid. Bobba and Coviello (2007, p. 303) note that:

In a multivariate panel data framework it is not clear how to test for weak instruments, hence we use the known bias in Difference GMM by comparing its sample performances with alternative estimators with known properties in dynamic panel data. . .

Specifically, following a procedure suggested by Bond, Hoeffler, and Temple (2001), Bobba and Coviello test for weak instruments

in difference-GMM by comparing its estimated autoregressive coefficient (*i.e.*, the coefficient estimate on the lagged dependent variable) with corresponding within-group (lower bound) and pooled (upper bound) estimates. If the difference-GMM autoregressive coefficient is smaller than the within-group estimate, then the difference-GMM estimator is likely to be downward biased. This result would suggest that the instruments are only weakly correlated with the endogenous regressors (Bond et al., 2001, p. 7).

The difference-GMM autoregressive coefficients in Table 4 all lie between their respective lower and upper bounds, suggesting that those estimates do not suffer from finite sample bias due to weak instruments. The following subsection further evaluates the difference-GMM estimates.

### 6.2. Difference-GMM estimates

The top portion of Table 4 presents the difference-GMM estimates. Again, all reported *t*-statistics reflect standard errors adjusted for generalized heteroskedasticity and autocorrelation. First consider the murder regression. Relative to the N–W results, the instrumented private detectives and investigators elasticity remains negative but is approximately 3.5 times larger in magnitude. As suggested above, the N–W estimate might suffer from endogeneity bias that operates in a positive direction (*e.g.*, higher murder rates may result in greater employment of private detectives and investigators). Instrumenting also causes the positive N–W elasticity estimate on private detectives/investigators to turn negative, though the effect remains statistically insignificant. Instrumenting still results in positive estimated security/fire alarm

and locksmith/safe repairer elasticities, though both estimates are smaller than their N–W counterparts.

Recall that only the locksmith/safe repairer elasticity was found to be statistically significant in the N–W rape regression. Instrumenting causes the magnitude of this elasticity to become somewhat larger in magnitude (*i.e.*, 'more negative'), but the effect is no longer statistically significant. The positive and significant N–W estimate of the locksmiths/safe repairer elasticity in the robbery regression turns negative and statistically insignificant when using difference-GMM. However, the security/fire alarm elasticity becomes more negative after instrumenting and turns statistically significant. The estimated value of this latter elasticity implies that a 1.0% increase in security/fire alarm installers is associated with 0.1% fewer robberies. None of the private security measures was found to be statistically significant in the N–W assault regression, and these same results are borne out when using difference-GMM. Note that the results obtained from the robbery and assault models can only be regarded as tentative given the failure of the AR(2) test in those cases.

Turning to the property crime regressions, the difference-GMM estimate of the security/fire alarm system elasticity is negative and statistically significant across all offense types, taking a value of between  $-0.13$  and  $-0.07$ . In each case, the magnitude of the estimated effect is larger (more negative) relative to the non-instrumented N–W estimate. Additionally, the point estimate on the security guards elasticity is negative, statistically significant, and larger than the corresponding N–W estimate in the larceny regression, as is the point estimate on the private detectives/investigators elasticity in the auto theft regression.

Finally, the N–W estimates of the impact of shall-issue laws generally suggested a positive effect of such laws on crime rates. However, after instrumenting, while most the individual coefficient estimates on the shall issue dummy remain positive, none are statistically insignificant at conventional levels.

## 7. Concluding remarks

This study estimates the impacts of various private security efforts on crime rates using both structural and dynamic-GMM estimation techniques. Instrumenting tends to make deterrence effects associated with private precautions larger in magnitude, a result that is consistent with the market model of offenses. Some of the results suggest that private security may exert appreciable impacts on crime rates. For example, the instrumented elasticity on security guard employment is larger than that associated with the incarceration rate in the case of murder.<sup>24</sup> At the same time, the impact of most private security efforts appears to vary both within and across violent and property offense categories. One result that appears to generalize with regard to property offenses is the negative impact of security system/alarm installer employment rates.

A number of topics not addressed in this study would benefit from further investigation. Whether private security and public protection (*e.g.*, policing) efforts are substitutes or complements in demand may have important implications for the optimal allocation of law enforcement efforts. Combining the OES private security data considered here with data on employment rates for publicly

<sup>24</sup> Note also that across crime categories the largest private security guard elasticity is found in the murder specification. To the extent that one might expect the influence of private guards to be related to the impact of (public) police on crime in general and murder in specific, this latter result is consistent with much of the literature on the deterrent effect of police. See in particular Chaffin and McCrary (2013) (reporting a consistently larger impact of police on murder relative to all other crime categories) and the references cited therein. On the other hand, some authors have questioned whether police can plausibly exert such a large effect on murder; see, *e.g.*, Klick and Tabarrok (2005, note 24).

employed police from the same or other sources could allow for exploration of this important issue.

Another interesting question is what determines the demand for and supply of private security efforts over time. There are a number of potential determinants besides crime and policing rates that may be relevant, including (but not limited to) the industrial organization of the security industry and laws that regulate the licensing of security firms or professionals. Finally, there are number of other ways private entities can reduce their victimization risk, such as not walking alone, leaving the lights on at night, and organizing community watch programs. More granular data might allow for further study of such private precautions.

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