RODRIGO R. SOARES
Columbia University and
São Paulo School of Economics-FGV

IGOR VIVEIROS
Federal University of Ouro Preto

Organization and Information in the Fight against Crime: The Integration of Police Forces in the State of Minas Gerais, Brazil

ABSTRACT This paper analyzes the experience of information sharing, coordination, and integration of actions of the civil and military police forces in the state of Minas Gerais, Brazil, in the context of the IGESP program. The IGESP was based on the introduction of information management systems and organizational changes akin to those associated with the CompStat system originally developed in New York City. The evidence points to a causal effect of the IGESP on crime. The most conservative estimates indicate a reduction of 23 percent in violent property crimes due to the introduction of the program. There is also evidence that the IGESP is associated with improved police response, measured by apprehension of weapons and clearance rates. We present one of the few estimates available with a clear identification strategy of the impact of CompStat-like programs. The results suggest that coordination of actions and efficient use of information may constitute first-order factors in the fight against crime.

JEL Classifications: H11, K00, K42

Keywords: crime, police, Brazil, IGESP, CompStat

Police systems with multiple forces are common in many countries. In some cases, a militarized and uniformed police is responsible for visible patrolling, while a judiciary police is responsible for investigations.[1] In

ACKNOWLEDGMENTS The authors gratefully acknowledge support from Fundação João Pinheiro during the development of this project. They also thank Afonso Henriques Borges Ferreira, Cláudio Santiago, Ricardo Candea de Sá Barreto, Délio Cunha, Rútila Gazzinelli, Sálvio Lemos, Valeska Drummond, Betânia Peixoto Lemos, and Marcus Vinicius Gonçalves da Cruz for comments and help throughout the process. The paper also benefited from suggestions from Luis Garicano, Paul Heaton, Ben Vollaard, and Ernesto Schargrodsky. Indispensable help and support were provided by the evaluation unit and the Secretariat for Social Defense of the State of Minas Gerais, particularly by Florence Fiuza de Carvalho, Helger Marra Lopes, Eder Sá Alves Campos, and Murillo Marschner Alves de Brito.

1. See Bayley (1985).

Brazil, this system manifests itself in the existence and almost total independence of the military and civil police forces. These two police forces have different attributes, hierarchical structures, and geographic organizations, maintain separate systems of information, and answer to distinct state agencies. The problems of coordination, information exchange, and trust between organizations generated by this dual structure have been identified as barriers to the effectiveness and even democratization of police action, both internationally and in the particular case of Brazil.[2] Still, despite the widely held belief that the unification of police forces or the integration of operations would lead to increased efficacy and reductions in crime and violence, there is no statistically robust evidence supporting this view or estimating the degree of inefficiency generated by the dual system.

This paper explores the experience of information sharing, coordination, and integration of actions of the civil and military police forces in the state of Minas Gerais, Brazil, in the context of the Program of Integration and Management in Public Safety (IGESP). The IGESP was inspired by the CompStat system, implemented originally in New York City and later adopted in slightly modified forms by various cities worldwide. The model is based on modern technologies of information monitoring and targeted policing, using dynamic updating and constant evaluation of strategies and actions by police organizations. Analyzing municipal-level data and exploring the staggered adoption process of the IGESP, we provide one of the first pieces of causal evidence on the effect of the integration of dual police forces on crime and police performance. From a broader perspective, the paper also illustrates how public sector productivity may be adversely affected by lack of communication and coordination among state agencies with overlapping or interacting jurisdictions.

Starting in the 1990s, the state of Minas Gerais experienced a seemingly explosive increase in crime. Violent crime, for example, increased by 500 percent in the short interval between 1990 and the early 2000s, from around 100 to above 500 per 100,000 inhabitants. In this context, violence figured as one of the main public policy issues for the government that took office in 2003. The first action of the new government in the area of public safety was the creation of the Secretariat of Social Defense, consolidating the two previously existing Secretariats of Justice and of Public Safety. The IGESP was then introduced as part of a process of change in state administration that focused on the definition of targets, monitoring, and evaluation of the state's

2. On Brazil, see Beato (1999); Chesnais (1999); Bicudo (2000); for an international context, see Bayley (1999).

performance in various sectors. In the area of public safety, this idea was put into practice through a policy of integration of the entire system, so that the objectives of different actors could be aligned and information exchanged. This strategy was based on two main points: a governance model of joint decisionmaking and integrated actions; and the implementation of an information system allowing data to be shared among the different actors.[3]

In the case of Minas Gerais, the CompStat-like features of the program were coupled with the additional objective of creating a single unit of decisionmaking and action from the two independent bodies of police forces. This implied a redesign of the geographic organization of the two police forces and some definition of the authority relationship between their independent hierarchical structures. The IGESP was then implemented at the most disaggregated level within the new geographic organization of the public security system. The program implied the adoption of a unified information system and police management strategies based on the CompStat model, together with the institutionalization of periodic meetings between the civil and military police forces (in conjunction with other agents of the public security and justice systems, such as district attorneys, municipal secretariats of public safety, and so forth).

The encompassing package of changes represented by the IGESP brought several new elements to police management in Brazil. We do not have adequate data, and do not believe that there is enough independent variation along these various dimensions, to analyze the separate effect of each one. We therefore analyze the overall impact of the implementation of the IGESP. The program was initially adopted by the state capital (Belo Horizonte) in 2005 as a pilot project and subsequently expanded to fifty-six municipalities by 2008. We use data from 2000 to 2008 on the universe of municipalities in Minas Gerais (853) and, exploiting the staggered process of program expansion, apply a difference-in-differences strategy to identify the effects of the IGESP on crime rates and police performance. Our most conservative estimates suggest that implementation of the IGESP reduced property crimes by 23 percent. There is also some evidence of a reduction in personal crimes and homicides, but these effects are estimated less precisely and seem to be partly associated with the experience of the state capital. Robustness exercises suggest that the main results are not related to other policies implemented at the municipal level, to changes in socioeconomic conditions, or to preexisting differential trends in violence. In addition, we present evidence that implementation was

3. Rocha, Barreto, and Gontijo (2008).

associated with improved police performance, through increased apprehension of weapons and clearance rates.

Another initiative to integrate the civil and military police forces took place in the Brazilian state of Ceará in the early 1990s.[4] The experience of Minas Gerais retains particular importance, however, given the depth of the change implemented, its persistency through time, and its consistent geographic expansion. Case studies and anecdotal evidence on the experiences of both Ceará and Minas Gerais support the success of the strategy in reducing crime and increasing police efficiency.[5] Still, there are no econometrically sound analyses of the impact of these programs currently available.

Moreover, there is no empirical literature on the integration of dual police systems or on similar types of public sector reorganizations, whether in other countries or in other areas outside the scope of public security. The literature most closely related to this paper is on the impact of the CompStat system. The information and management tools implicit in CompStat, and the reorganization of operations implied by them, are closely related to the technological and organizational change represented by the IGESP. Numerous papers describe the expansion of the CompStat system in the United States, its logic, and its potential limitations.[6] There are also various case studies and time series analyses of the impact of CompStat in specific contexts.[7] But, again, none of the available studies use a clear identification strategy and a representative sample.

The one exception is a paper by Garicano and Heaton, who are interested in the impact of information technologies on organization and productivity.[8] They use a large sample of U.S. police departments from a law enforcement survey and estimate, with panel data, the impact of information technologies and what they define as modern policing techniques. The results indicate that information technology investments, when linked to particular organizational and management practices similar to those associated with CompStat, tend to increase police productivity. Still, due to data limitations, they can only conduct explicit analyses of the impact of CompStat in a single cross section.

From this perspective, our paper can also be seen as a contribution to the literature on the evaluation of CompStat-like programs. In contrast with other papers on the topic, we use a panel and adopt a strategy from the impact

4. See Brasil and Abreu (2002).

5. See, for example, Brasil and Abreu (2002); Beato and others (2007).

6. Walsh (2001); Brown and Brudney (2003); Weisburd and others (2004).

7. For example, Manning (2001); Willis, Mastrofski, and Weisburd (2003); Rosenfeld, Fornango, and Baumer (2005); Mazerolle, Rombouts, and McBroom (2007).

8. Garicano and Heaton (2010).

evaluation literature. We use data on the universe of affected municipalities to explore a singular episode of centralized decisionmaking on program implementation and expansion, which warrants some degree of exogeneity in adoption. We have a clear identification assumption and test its validity in the data. Finally, we find robust evidence on the effect of CompStat-like interventions on crime and police performance, though in our context these responses are likely to be magnified by the dual police structure that existed previously.

The remainder of the paper is organized as follows. The next section describes the institutional background of police organization in Brazil and discusses the main features of the IGESP. The subsequent section describes the data used in our empirical exercise. We then discuss the empirical strategy and its limitations and present our main results and robustness tests. A final section concludes the paper.

The Program of Integration and Management in Public Safety (IGESP)

The integration of operations and actions of the civil and military police forces in the state of Minas Gerais took place within the Program of Integration and Management in Public Safety (IGESP). The main goals of the program were to allow the free and immediate flow of information between the two police forces and the coordination of integrated planning.[9] In an unprecedented initiative in Brazil, the methodology was developed by the Center for Studies of Criminality and Public Safety of the Federal University of Minas Gerais (CRISP/UFMG), in close cooperation with the military police and the state administration. The program was largely inspired by the CompStat system in New York and its subsequent adaptation to the Colombian city of Bogotá. The model is based on modern technologies of information monitoring and targeted policing, using dynamic updating and a constant evaluation of strategies and actions by the police organizations. More specifically, the IGESP has two objectives: the consolidation and systematization of data and intelligence information gathered by different agents within the state public safety community, generating a common and updated database that is shared among the different parties; and the coordination of strategic actions involving the various parties.

9. See Beato and others (2007). For a more detailed history of the Brazilian police and a contextualized discussion of the changes brought about by the IGESP, see Soares and Viveiros (2013).

An important step in the consolidation of information was the implementation of the Integrated Information System for Social Defense (SIDS), which is the mechanism for sharing information across the military and civil police forces, the judiciary, the public defender's office, and the penitentiary system. The SIDS was a precondition for the implementation of the IGESP, and it was officially instituted in April 2004.[10] Operationally, the SIDS allows the unified management of information, whether related to police investigations, crime registries, judicial prosecutions, or enforcement of criminal court orders.

The coordination of actions and information sharing within the IGESP takes place through strategic meetings involving police forces and other authorities. The meetings are supposed to lead to a deeper understanding of the criminal phenomenon, through discussions focused on the identification of hot spots, key individuals in local crime, and potential underlying causes for changes in the local dynamics of crime and violence.

The team of researchers that worked with the police to design the IGESP identifies the following points as its main goals: (i) to promote interaction and integration of the civil and military police forces; (ii) to allow the exchange of information in the area of public safety; (iii) to give agility to police procedures and actions; (iv) to improve the performance of police activities; (v) to monitor the behavior of criminal activity; (vi) to promote the engagement of the community in the fight against crime; (vii) to focus police actions on high-risk areas; and (viii) to decentralize strategic decisions in the fight against crime.[11] A very important step in this process was the definition of geographic areas of action common to the two police forces. Prior to the IGESP, the civil and military police forces had different, nonoverlapping geographic organizations, so that coordination of actions was very difficult. This change was achieved through the definition of integrated areas, constituting a predetermined geographic region subject to the joint action of specific units of each police force.

The new geographic division took into consideration socioeconomic, cultural, and criminal characteristics of the areas, but the major concern was to make it compatible with the different hierarchies of the civil and military police forces and their chains of command. Therefore, the entire state area was divided into progressively smaller districts, each corresponding to a different level of decisionmaking within the state public safety system: (i) the state was divided into Integrated Regions of Public Safety (RISPs), each with a regional division of the military police and a civil police department; (ii) these

10. State Decree No. 43,778 of 12 April 2004.
11. Beato and others (2007).

TABLE 1. Descriptive Statistics: Average, 2000–08

RISP	*Municipal seat*	*Municipal identification*	*Population*	*Area (Km²)*	*Population density*
1	Belo Horizonte	1	2,338,766	332	7,043.0
2	Contagem	17	1,646,941	4,380	376.0
3	Vespasiano	22	918,785	8,427	109.0
4	Juiz de Fora	86	1,499,313	21,493	69.8
5	Uberaba	32	671,728	41,915	16.0
6	Lavras	143	2,232,245	47,019	47.5
7	Divinópolis	84	1,529,809	49,144	31.1
8	Governador Valadares	50	639,232	21,053	30.4
9	Uberlândia	14	893,694	26,413	33.8
10	Patos de Minas	25	498,299	32,095	15.5
11	Montes Claros	76	1,365,527	100,451	13.6
12	Ipatinga	101	1,682,115	32,893	51.1
13	Barbacena	56	770,807	16,389	47.0
14	Curvelo	68	1,004,653	70,322	14.3
15	Teófilo Otoni	61	861,395	56,580	15.2
16	Unaí	17	331,562	59,471	5.6

Source: João Pinheiro Foundation; State Secretariat of Social Defense; State Single Health System database (DATASUS).

were then subdivided into Areas of Coordinated Integration in Public Safety (ACISPs), corresponding to a regional civil police station and a military police battalion; and (iii) these were further subdivided into the smallest units, the Integrated Areas of Public Safety (AISPs), defining the areas of action of a civil police station and a military police company. The actual implementation of the IGESP took place at the most disaggregated level, the AISP.

The first RISP was the pilot project developed in the capital, Belo Horizonte, in 2005, immediately before the implementation of the IGESP. Additional RISPs were then designed in the metropolitan area of the capital (Contagem and Vespasiano) and Uberaba. In subsequent years, RISPs were created with headquarters in the municipalities of Uberlândia and Montes Claros, until the entire state was subdivided into sixteen RISPs in 2008.[12] Table 1 lists the different RISPs and gives some basic characteristics of the areas.

12. Although the implementation of the design of integrated areas started in 2003, it was only made official through state regulation in early 2008 (Joint Resolution No. 51 of 15 February 2008). For our purposes, the relevant aspect is the actual implementation of the project. In 2010, there was a reorganization of some of the RISPs, which increased the total number from sixteen to eighteen (two new RISPs were created with municipalities that originally belonged to the sixth and seventh RISPs). Our sample period is characterized by the original organization with sixteen RISPs.

The coordination of the entire system of integrated areas is the responsibility of the Executive Secretariat, composed of members of the State Secretariat of Social Defense and representatives of the civil and military police forces. This Executive Secretariat coordinates the actions of the different agents across all levels of the integrated areas. The specific responsibilities and authority of each of these spheres in the decisionmaking process can be summarized as follows.

The Executive Secretariat is responsible for monitoring the actions of the RISPs, ACISPs, and AISPs and for the performance of the police forces in the state. It is responsible for the expansion and implementation of the IGESP in Minas Gerais, for maintaining the information flow between the civil and military police forces, and for managing the state's crime database. It also trains the manpower responsible for the actual implementation of the IGESP in a given AISP.

The Integrated Region of Public Safety (RISP) is responsible for the strategic planning of its ACISPs and AISPs. This planning encompasses exchanging information and designing strategies for fighting crime based on the coordination of actions between the civil and military police forces. The RISP schedules meetings for monitoring and evaluating joint actions, establishes timelines for meetings at the ACISPs and AISPs levels, and produces yearly assessment reports. Each RISP has an Evaluation and Support Group (or Regional Executive Secretariat), which is responsible for the continuous evaluation of the IGESP, as well as for the provision of support and advice for the ACISPs and AISPs. This group is also responsible for the organization and record-keeping of the strategic meetings scheduled by the RISP.

The Integrated Area of Coordination of Public Safety (ACISP) is responsible for the tactical planning of the AISPs, through constant monitoring of the material conditions and performance of the joint actions of the civil and military police forces. It contains a support group for the integration process, which is responsible for taking the demands of each AISP to the RISP, as well as for managing the implementation of the IGESP on the ground and for preparing its evaluation reports.[13]

13. Through 2008, the state of Minas Gerais had thirty-one operational ACISPs, responsible for 123 AISPs in fifty-six municipalities. Some AISPs encompass more than one municipality, while others are smaller than a single municipality (depending on the size of the municipality). In principle, there is not a one-to-one match between municipalities and AISPs, but in practice, all AISPs implemented thus far do not incorporate more than one municipality and, in cases where the same municipality has more than one operating AISP, all of them were implemented in the same year. Thus, for the purpose of our empirical exercise, we keep the municipality as the unit of analysis.

The Integrated Area of Public Safety (AISP) is the geographic unit where the IGESP is actually implemented. It is responsible for operational planning and facilitates dialogue between civil and military police forces on a routine basis. It discusses and analyzes the goals and actions established at RISP and ACISP meetings. An internal data analysis group is responsible for feeding, receiving, and extracting data from the civil and military police information system. With this information in hand, the data analysis group generates the indicators that guide the actions at the AISP level. The group also organizes weekly meetings between civil and military police forces, where the specific goals and actions to be implemented at each moment in time are determined.

The main goal of the IGESP is to promote dialogue among the state agencies in the area of public safety. This is most clearly materialized in the integration of planning and actions between civil and military police forces, but it is not restricted to those agencies. In higher-level meetings, the process includes coordination with the judiciary, public defender's office, and the penitentiary system. The final goal of this coordination is to allow the development of more effective policies and actions in the fight against crime.

On the information side, the objective is to produce, gather, and systematize intelligence information, which should be precise, up to date, and available for immediate use by public safety personnel. This information is passed along to police forces through the strategic meetings. It generates a better understanding of the criminal phenomenon, based on the precise location of critical points and the identification of likely proximate causes. With this information in hand, police forces are better equipped to design tactics and strategies to fight crime, with fast, synchronized, and focused allocation of resources.

On the organizational side, a key role is played by the weekly meetings held at the AISP level. In these meetings, managers and operational personnel from both civil and military police forces exchange experiences, share information, and discuss potential solutions. Typically, the meetings also present the crime statistics, focusing on the main types of incidents and the most violent areas. Broader meetings are held on a monthly basis, including district representatives from the civil police and battalion commanders from the military police; the objective of these meetings is to advise on and provide support for the actions being taken at the AISP level.

Descriptive analyses have identified the IGESP as a potentially important factor in the recent reduction in crime rates in Minas Gerais. Beato and others, for example, note that violent crimes in the state capital (Belo Horizonte) were reduced by 17 percent within eight months of the introduction of the

IGESP.[14] In Montes Claros, the Secretariat of Social Defense identified a reduction of 45 percent in crime rates in a similar time interval following implementation.

Data

We conduct our evaluation of the IGESP using yearly municipal data covering the period from 2000 to 2008. Crime data are not available at the municipal level before 2000, and anecdotal evidence suggests that, due to a change in political focus on the part of the state government, the program lost some of its grip starting in the late 2000s. We therefore restrict the analysis to this period. The data were obtained from the military police of the State of Minas Gerais, the State Secretariat of Social Defense, the João Pinheiro Foundation (roughly equivalent to the state statistical and evaluation agency), and the Brazilian Geographical and Statistical Institute. These include variables related to the implementation of the IGESP, outcomes, and controls. We discuss the role played by each variable in the next section. They can be broadly classified into the following groups:

—Number of crimes: number of homicides, violent crimes against the person, and violent crimes against property (source: Minas Gerais state military police);

—Presence of the IGESP: a dummy variable indicating whether a municipality is covered by the program in a given year (source: State Secretariat of Public Defense);

—Police-related information: number of personnel in the military police, number of personnel in the civil police, and number of cars in the military police (source: Minas Gerais state military police and State Secretariat of Public Defense);

—Presence of other public safety programs: dummy variables indicating the presence of various concurrent programs (Fica Vivo, Olho Vivo, and the municipal civil guard) in a municipality in a given year (source: State Secretariat of Public Defense);[15]

14. Beato and others (2007).

15. Fica Vivo is a program focused on particularly violent and economically fragile areas within a municipality, mixing police presence and social actions. Olho Vivo is a program that supports the installation of digital cameras in key hot spots within a municipality. The municipal civil guard is an unarmed force supported by the municipality, responsible for visible policing.

—Police outcomes: number of apprehensions of firearms and other weapons, total arrests, and on-view arrests (source: Minas Gerais state military police); and

—Municipal characteristics: population, gross domestic product (GDP) per capita, and enrollment rates in the public school system (source: João Pinheiro Foundation and Brazilian Geographical and Statistical Institute).

We concentrate on homicides, violent crimes against property, and violent crimes against the person because reporting rates are thought to be higher for these categories (relative to thefts and other nonviolent crimes). The choice of control variables, in turn, was guided by the main concerns in our empirical exercise. Variables related to the number of crimes and other police actions (weapon apprehensions and arrests) are used as outcome variables that may be affected by the introduction of the IGESP. Variables related to municipal characteristics and the presence of other public safety programs are used as controls for changes that may be happening simultaneously to the introduction of the IGESP. Finally, variables related to police personnel and resources try to isolate the organizational and informational aspects of the IGESP, given the possibility that program implementation is accompanied by other concurrent changes in allocation of resources and investments. All these concerns are discussed in detail in the next section, when we outline our empirical strategy.

Table 2 presents the evolution of the different types of crime (measured as rates per 100,000 inhabitants and referred to simply as homicides, crimes against the person, and crimes against property) for municipalities that received the IGESP before 2008 and for municipalities that did not. The table also presents data on population and GDP per capita. It highlights one of the main concerns in our empirical approach: municipalities that received the IGESP were different from those that did not, to the extent that they were usually more violent, larger, and wealthier. In relation to crime rates, this was particularly true in the first places where the program was implemented, but became less so as the program expanded. In any case, our main worry is that these municipalities may be intrinsically different and thus may naturally have distinct dynamics of crime.

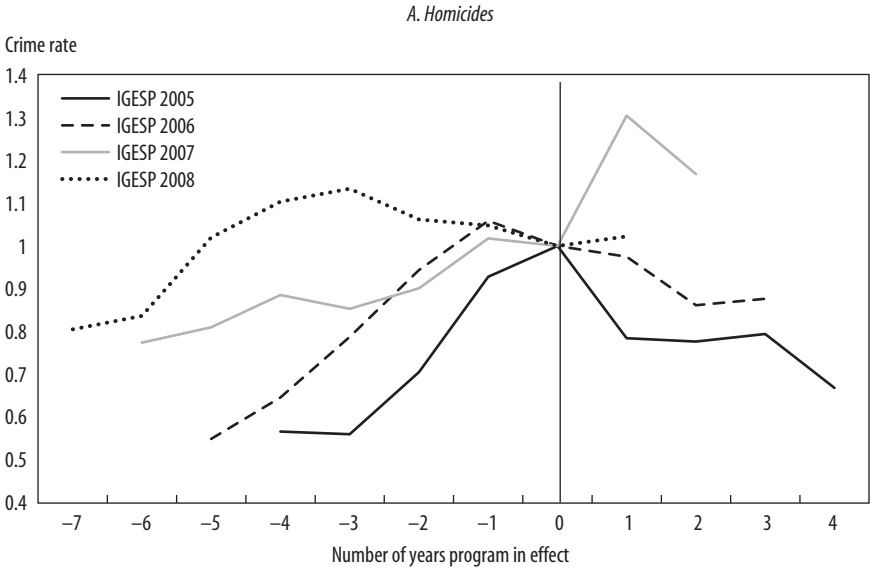
The simple fact that the crime level is different across municipalities receiving and not receiving the IGESP does not constitute a problem on its own. The real issue is whether these distinct crime levels are also associated with different crime dynamics. To explore this possibility, figure 1 plots the crime data from table 2 for the municipalities that received IGESP at some point, but normalizes initial crime levels to one and the year of IGESP implementation

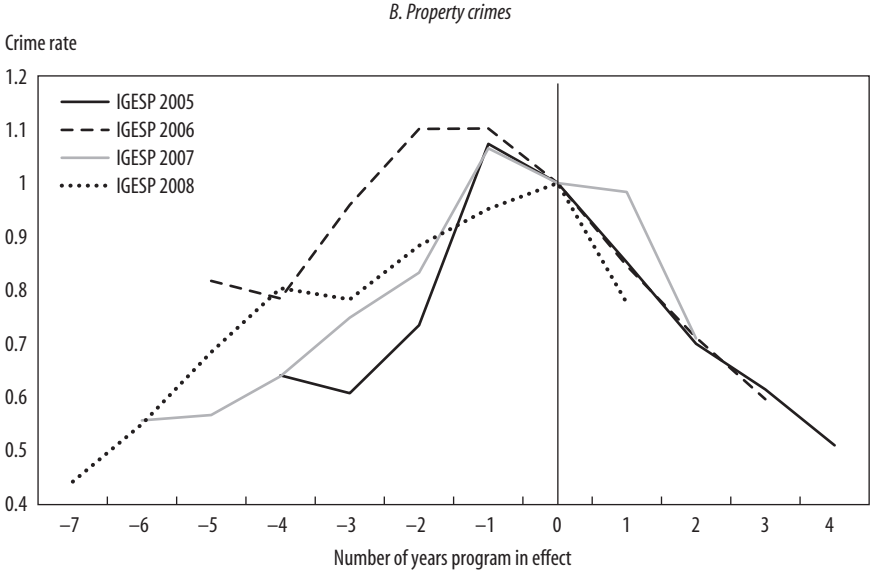
TABLE 2. Evolution of Crime Rates by Year of IGESP Implementation: Municipalities in Minas Gerais, 2000–08[a]

	Municipalities with IGESP in 2008 (N = 56)					*Municipalities without IGESP in 2008 (N = 797)*				
Year	*Homicide*	*Property*	*Personal crime*	*Population*	*GDP p.c.*	*Homicide*	*Property*	*Personal crime*	*Population*	*GDP p.c.*
2000	18.5	650.5	88.8	128,258	8,185	8.9	61.2	44.0	12,936	4,596
2001	20.1	632.1	90.3	134,544	9,138	9.3	71.7	46.9	13,291	4,951
2002	24.6	764.2	98.1	136,842	9,923	10.0	83.9	50.8	13,401	5,855
2003	30.1	991.2	109.7	139,162	11,881	10.5	98.3	49.7	13,501	7,261
2004	33.0	959.4	106.0	144,030	12,567	9.7	101.0	50.7	13,711	8,022
2005	29.3	879.8	100.0	146,724	13,407	12.0	121.7	56.2	13,828	8,685
2006	28.8	752.5	90.9	149,398	14,627	11.9	125.2	57.1	13,944	9,127
2007	27.6	667.4	85.2	152,009	15,604	11.9	128.8	54.5	14,058	10,465
2008	26.0	544.1	68.1	151,942	19,875	11.8	105.3	47.3	14,230	12,892

a. Crime rates are per 100,000 inhabitants. The crimes are homicide, property crimes, and personal crimes. Group averages are weighted by municipality population (for crime rates and GDP per capita).

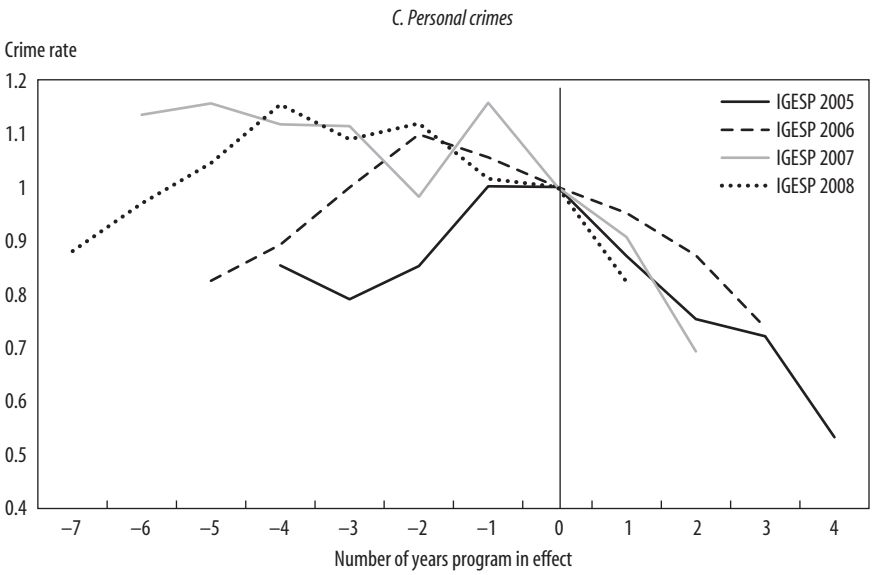
FIGURE 1. Evolution of Crime Rates for Municipalities That Received IGESP at Different Moments in Time: Minas Gerais, 2000–08[a]





(*continued*)

FIGURE 1. Evolution of Crime Rates for Municipalities That Received IGESP at Different Moments in Time: Minas Gerais, 2000–08[a] (*Continued*)



a. Crime rates have been normalized to one and the year of IGESP implementation to zero.

to zero. For each panel in the figure, the vertical line indicates the first year of IGESP implementation for municipalities that received the program. The figure suggests that there is some time series correlation between the introduction of the IGESP and reductions in crime rates. In most cases, the first year of implementation is associated with a reversal in the crime trend, while in others there appears to have been some previous reduction that was intensified by entry into the program. For yet a few others, reductions in crime seem to start the year after initial implementation. More important, the dynamics of crime before IGESP implementation do not seem to follow any particular pattern, so that preexisting trends do not seem to be an issue. In any case, various other changes could be taking place in these municipalities during this period, so figure 1 should be taken simply as suggestive evidence of the potential crime-reducing effects of the IGESP.

Particularly important for our later discussion on identification is the specific decisionmaking process behind the implementation of the IGESP. Sapori and Andrade argue that two main points were essential: the administrative

rearrangement promoted by the creation of the new Secretariat of Social Defense (SEDS); and structure and logistics.[16] The authors argue that the starting point of integration was the creation of the new Secretariat, which was given authority over the operational coordination of the activities of the civil and military police, the fire brigade, the public defender's office, and prison administration. It was up to the SEDS to trace the guidelines for operational integration between the police forces. From a structural and logistical point of view, the integration policy had as main components the sharing of information, the new geographic design of the system, and operational planning. The locations to receive the IGESP were therefore those that met the initial conditions for the implementation of the program, which started naturally at municipalities housing the headquarters of the higher hierarchical levels of the police forces. The decisionmaking process was concentrated in the Secretariat of Social Defense, and municipalities played virtually no role in volunteering for or demanding implementation of the program.

Empirical Strategy

Our main interest lies in the incidence of crime, so our key variables are crime rates (homicides, property crimes, and personal crimes) per 100,000 inhabitants (transformed by the natural logarithm).[17] We also analyze the impact of the IGESP on the response of the public security apparatus, by looking at apprehension of weapons (firearms and other weapons), arrests, and on-view arrests, all normalized by the total number of registered crimes (and transformed by the natural logarithm). The rate of arrests per number of crimes is called the clearance rate.

16. Sapori and Andrade (2009).

17. Several municipalities in the sample are small, with large variance in crime rates, so the variable measuring the number of crimes per 100,000 inhabitants includes a substantial number of zeros. As the natural logarithm is not defined in zero, we add one to the number of crimes before calculating the crime rates and applying the logarithmic transformation. Qualitative results are identical—and quantitative results very similar—if instead we substitute the natural logarithm for crime rates below one by zero (substituting all negative or undefined values of the natural logarithm by zero). If we were dealing with variables with very low means, these procedures might create distortions in terms of estimated effects, but our crime rates have quite high means, so this does not seem to be a problem. (Conditional on positive crime rates, the average rates are 21 for homicides, 87 for property crimes, and 60 for personal crimes.) We also estimate our main specification using a Tobit model and the crime rate without the log transformation, which produces very similar quantitative and qualitative results.

Our specification uses a difference-in-differences strategy and compares municipalities receiving the IGESP to those that did not receive it, before and after intervention. The absence of common support across treated and non-treated municipalities and the staggered nature of the implementation process precludes the use of matching techniques. We discuss this issue extensively, and deal with its potential problems, in our robustness exercises. The benchmark specification is the following:

$$\text{OUTCOME}_{mt} = \alpha + \beta \text{IGESP}_{mt} + \gamma \mathbf{X}_{mt} \theta_m + \mu_t + \varepsilon_{mt}, \quad (1)$$

where OUTCOME_{mt} indicates some variable of interest (crime rates, clearance rates, or weapon apprehension rates) for municipality m in year t; IGESP_{mt} is a dummy variable taking a value of one if municipality m in year t is covered by the program and zero otherwise; $\mathbf{X}_{mt}$ is a vector of municipal characteristics; θ_m is a municipality fixed effect; μ_t is a year fixed effect; ε_{mt} is a random term; and α, β, and γ are parameters. Under the usual assumptions, $E[\varepsilon_{mt}|\text{IGESP}_{mt}, \mathbf{X}_{mt}, \theta_m, \mu_t] = 0$, an ordinary least squares (OLS) estimation of the above equation provides an unbiased estimate of β. The source of variation used to identify the program's effect is the distinct timing of implementation across municipalities, which allows us to compare municipalities that received the program with those that did not receive it. In this hypothetical setting, the random term ε_{mt} is not correlated with the independent variables, so OLS estimates of β indeed provide the parameter of interest: the causal impact of program adoption ($\text{IGESP}_{mt} = 1$) on OUTCOME_{mt}.

In the context of the IGESP, there are two main potential problems with this strategy: omitted variables and dynamic endogeneity. Regarding omitted variables, municipalities that received the program may also have received more resources in the area of public safety or adopted other social and security policies. This might be expected if good local governments adopt good policies in several different areas simultaneously. Assuming that the IGESP is indeed a good policy, this would mean that municipalities receiving the IGESP would also have adopted other successful policies, so that the effect of the program would be confounded with that of other changes taking place at the same time. This concern is lessened by the fact that police forces in Brazil are under state-level control, so actual implementation is not decided at the local level.

We include as controls in the regression above a series of variables related to the allocation of resources to public safety, the adoption of other local programs in the public safety area, and municipal characteristics associated with economic performance and social policy, which are included in the vector $\mathbf{X}_{mt}$.

These can be classified into three groups: police resources (civil and military police personnel and number of cars used by the military police); other local programs in the public safety area (dummy variables indicating the presence of a municipal civil guard, and of the Fica Vivo and Olho Vivo programs); and socioeconomic characteristics (GDP per capita and enrollment rates in public schools).

The second potential problem is that implementation of the IGESP itself may have responded to criminality conditions within a municipality, such that the treatment variable may be endogenous. The use of municipality fixed effects partly helps to deal with this problem, given that it controls for systematic time-invariant differences across municipalities. It does not entirely solve the problem, however, since the possibility of endogeneity persists in its dynamic version: the timing of program adoption may depend on the past evolution of a variable of interest (for example, the program may be implemented sooner in areas with particularly bad shocks in crime rates); and initial conditions associated with program adoption may also be associated with a particular evolution of the dependent variable (for example, if crime rates are converging over time, areas with initially worse conditions may be more likely to receive the program).

We use three strategies to deal with this potential problem. First, we add to the initial specification linear trends at the municipal level, which eliminate concerns related to convergence in crime rates or, more generally, differential trends across locations. In this specification, the treatment variable would capture whether there was a shift in the municipality-specific crime trend when the program was implemented. Second, we assess whether there is evidence of preexisting trends in periods immediately before program implementation. If the program is just capturing the dynamic behavior of crime not modeled explicitly in equation 1, this should also show up as a systematic change in the years immediately before program adoption. Finally, as an initial assessment of how serious the issue of dynamic endogeneity may be, we conduct a hazard estimation of the probability that a given municipality joins the program.[18] Specifically, our dependent variable indicates the presence of the IGESP in a municipality. As soon as municipalities join the program, they leave the sample. We therefore estimate the effect of municipalities' characteristics on the probability of joining the program. Our main interest is how this probability is related to fixed municipal characteristics and to changes in

18. This follows Galiani, Gertler, and Schargrodsky (2005) and Biderman, de Mello, and Schneider (2010).

endogenous variables. Thus, our hazard estimation evaluates the probability that a municipality joins the IGESP as a function of shocks to criminality (differences in crime rates in previous years), changes in other dimensions of public security policy, and a set of variables indicating initial conditions.[19]

In line with anecdotal evidence from state reports, the results show that logistic and administrative considerations were the dominant factors determining IGESP implementation. The state government pushed the program outward from the state capital in a radial fashion, focusing first on municipalities that housed regional civil police stations and military police battalions. Since integration at the higher levels of the hierarchical structure was a necessary condition for integration at the lower levels, this was a first step that followed the logistic organization of the program. In the estimation of determinants of adoption (not shown), distance to the state capital and presence of regional civil police stations and military police battalions are by far the most important determinants of IGESP implementation. Past shocks to dependent variables are statistically significant at the 10 percent level in only one out of eighteen estimated coefficients, and even then with a very modest quantitative effect. Initial property crime levels do seem to be systematically related to program adoption (positively), but this does not constitute a problem since initial conditions are controlled for by municipality fixed effects.

We address three additional methodological issues in our estimation. First, given that the variance of crime rates is directly related to population size (homicides, for example, are rare events in small cities), we weight regressions by population size. Second, since difference-in-differences strategies may underestimate standard errors due to autocorrelation in the residuals, we cluster standard errors at the municipal level, allowing for an arbitrary structure of correlation within municipalities over time.[20] Third, because the effect of the IGESP may take time to manifest (to allow for establishing trust between parties, developing the protocol for interactions, learning the new information technologies, and so on), we evaluate whether the treatment effect is heterogeneous over time.

Finally, as mentioned, a traditional concern in the crime literature is the problem of underreporting in official crime data.[21] Our choice of the types of crimes to be analyzed takes this concern into account (reporting rates for homicides and violent crimes are thought to be typically higher than for thefts

19. For a detailed discussion of the specific modeling strategy used in our hazard estimation, see the working paper version of this study (Soares and Viveiros, 2010, appendix A.1).

20. Bertrand, Duflo, and Mullainathan (2004).

21. See Soares (2004).

TABLE 3. The Effect of IGESP on Crime Rates in Municipalities in Minas Gerais, 2000–08: Benchmark Specification[a]

	Type of crime		
Explanatory variable	*Homicide (1)*	*Property (2)*	*Personal (3)*
IGESP	0.00473	–0.453***	–0.193***
	(0.0514)	(0.0508)	(0.0317)
Constant	2.653***	4.848***	4.025***
	(0.0289)	(0.0233)	(0.0197)
Summary statistic			
No. observations	7,677	7,677	7,677
R^2	0.778	0.930	0.773

*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS regressions. The dependent variables are crime rates (per 100,000 inhabitants) expressed in natural logarithms. All regressions include municipality and year dummy variables and are weighted by municipal population. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

and other petty crimes). In addition, we use municipality fixed-effects and, in some specifications, municipality-specific time trends to control for any systematic difference in reporting rates across locations. To the extent that the IGESP may increase reporting rates, any remaining measurement error is likely to bias our estimates toward positive values.

Results

Table 3 presents our benchmark results, with the basic specification from equation 1 without the inclusion of any control variables. The results indicate a negative and statistically significant relationship between the timing of implementation of the IGESP and property and personal crimes and an insignificant relationship with homicide rates. Since the dependent variables are in logarithmic form, the coefficients can be approximately interpreted as semi-elasticities. The point estimates therefore suggest that IGESP implementation was associated with reductions of 45 percent in property crimes and 19 percent in personal crimes.

The effect of the IGESP may be heterogeneous as time passes, possibly due to the establishment of trust among the state agencies involved in the coordination and integration efforts and to the development of more adequate and efficient operational procedures as different parties learn about the new system. To assess this possibility, table 4 presents the results of regressions

TABLE 4. The Effect of IGESP on Crime Rates in Municipalities in Minas Gerais, 2000–08: Lagged Impacts[a]

	Type of crime					
	Homicide		*Property*		*Personal*	
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
IGESP	0.0198	–0.0132	–0.281***	–0.293***	–0.135***	–0.143***
	(0.0553)	(0.0648)	(0.0550)	(0.0525)	(0.0379)	(0.0382)
$IGESP_{t-1}$	–0.0918***	–0.0764*	–0.271***	–0.200***	–0.0983*	–0.0787*
	(0.0324)	(0.0401)	(0.0436)	(0.0329)	(0.0563)	(0.0466)
$IGESP_{t-2}$		–0.0695		–0.161***		–0.0573
		(0.0484)		(0.0448)		(0.0356)
Constant	2.887***	2.918***	5.312***	5.343***	4.003***	4.017***
	(0.0233)	(0.0252)	(0.0277)	(0.0260)	(0.0222)	(0.0220)
Summary statistic						
No. observations	6,824	5,971	6,824	5,971	6,824	5,971
R^2	0.792	0.806	0.934	0.938	0.782	0.792

* Statistically significant at the 10 percent level.

*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS regressions. The dependent variables are crime rates (per 100,000 inhabitants) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

identical to those in table 3, except that the coefficient on the treatment variable is allowed to vary according to the length of exposure to the IGESP. We include three treatment variables that indicate whether the municipality is covered by the program in the current year, whether the municipality was covered by the program in the previous year ($IGESP_{t-1}$), and whether the municipality was covered by the program for at least two years ($IGESP_{t-2}$). The results indicate that there seems to be some heterogeneity in the effect of the program over time, particularly in the case of homicides. For personal crimes, the results are very similar to those in table 3, and most of the effect is concentrated in the first year of program implementation. In the case of property crimes, most of the effect is still concentrated in the first year of program implementation, but there are also significant lagged effects. Still, in this case, the aggregate effect over time is very close to that presented in table 3: table 4 suggests that roughly 60 percent of the previously estimated effect is due to the simultaneous impact, while 40 percent comes from the increased impact over time.

For homicides, the effect of the IGESP was not significant in table 3. Now, the strongest effect appears in the first lag, that is, the year after the initial implementation of the program. As in the case of property crimes, column 2

suggests that the effect of the program on homicides also tends to increase over time, since the coefficient on $IGESP_{t-2}$ is negative and statistically significant. This result seems reasonable, since the determinants of homicides are more complex in nature and, therefore, should not respond immediately. This would be the case, for example, if more investigative effort were required to reduce homicide rates.

The results discussed thus far suggest that the effect of the IGESP is strongest for property crimes. This is standard in most of the crime literature, in the context of other interventions: economically motivated crimes tend to respond more to programs for crime prevention and control. For these crimes, criminals are more likely to make a rational cost-benefit analysis of potential gains involved in a certain action. According to this logic, crimes with a more personal and emotional character, including most homicides and personal crimes, are likely to be less responsive in the short run to improvements in police action. The pattern of results obtained, therefore, is consistent with what should be expected from the technology of intervention represented by the IGESP.

In light of the results from tables 3 and 4, but trying to keep the specifications as simple as possible and not to lose time-series variation when not strictly necessary, we use the first lag of the IGESP (whether the municipality already had the program in the previous year) as the treatment variable for homicides. For property and personal crimes, we keep the simultaneous presence of the IGESP as the treatment variable.

In tables 5, 6, and 7, we include our municipal controls in the benchmark specification. The central issue tackled in this series of tables is whether municipalities that received the IGESP were also experiencing other relevant changes simultaneously with the introduction of the program. There are at least three circumstances in which this would seem reasonable and even expected. First, the IGESP may bring with it other changes in the area of public safety, particularly related to greater effort and resources allocated to fighting crime. Second, if municipalities receiving the IGESP are particularly concerned with crime, they may also be adopting other programs to reduce violence. Finally, municipalities receiving the IGESP may also be experiencing positive shocks in other socioeconomic dimensions.

With that perspective in mind, we introduce control variables that account for differences across municipalities in terms of police resources, other programs targeting crime, and local socioeconomic conditions and policies. The tables shows that our previous results remain virtually unaltered when each of these sets of controls is included separately and when all of them are included

TABLE 5. Controlling for Confounding Factors: Homicide Rates[a]

Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*
$IGESP_{t-1}$	−0.0810*	−0.102**	−0.0903**	−0.105**
	(0.0424)	(0.0494)	(0.0445)	(0.0485)
Military police personnel	−7.194			−9.237
	(42.82)			(45.79)
Military police cars	101.3			71.08
	(96.38)			(95.34)
Civil police personnel	148.1*			147.5*
	(87.98)			(89.49)
Municipal guard		0.0928		0.0895
		(0.0827)		(0.0860)
Fica Vivo program		−0.0354		−0.0344
		(0.0530)		(0.0547)
Olho Vivo program		0.0577		0.0363
		(0.0738)		(0.0699)
GDP per capita			−0.00317	−0.000915
			(0.0915)	(0.0916)
School enrollment			−0.0819	−0.0512
			(0.0681)	(0.0891)

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.
*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS estimates of the effect of the IGESP on crime rates in municipalities in Minas Gerais in 2000–08. The dependent variable is the homicide rate (per 100,000 inhabitants) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population. Control variables are as follows: the number of military and civil police personnel and the number of military police cars (per capita), for police resources and personnel; dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo, for other public safety programs; and GDP per capita and the public school enrollment rate, for socioeconomic variables. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

simultaneously. Considering the specification that includes all controls at the same time (column 4 in tables 5, 6, and 7), the results suggest that IGESP adoption is associated with reductions in the incidence of crimes on the order of 45 percent for property crimes, 17 percent for personal crimes, and, with a lag of one year, 10 percent for homicides.[22]

22. There is a well-known problem of endogeneity of police allocation in this type of regression (see, for example, Levitt, 1997, 2002; and Schargrodsky and di Tella, 2004). Police may be allocated to a certain area because crime rates are high, in which case a simple regression analysis may end up revealing a positive correlation between police and crime. This problem is no doubt present in our case, and some of the coefficients on the police variables are positive and statistically significant. In addition, our variables for civil police personnel and the number of military police cars seem to be measured with a lot of error (in that there are enormous variations in the series from year to year). None of these problems affect the basic results in tables 5, 6, and 7 or any of the results presented in the following sections. If we exclude these three variables altogether, the estimated coefficients remain virtually identical. In unreported regressions, we went one step further, by eliminating the variables on military police cars and civil police

TABLE 6. Controlling for Confounding Factors: Property Crime Rates[a]

Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*
IGESP	−0.458***	−0.432***	−0.469***	−0.445***
	(0.0509)	(0.0628)	(0.0578)	(0.0637)
Military police personnel	−68.77			−61.16
	(46.95)			(48.29)
Military police cars	179.6			182.4
	(142.9)			(142.0)
Civil police personnel	390.1***			374.8***
	(130.9)			(129.7)
Municipal guard		0.0481		0.0448
		(0.0827)		(0.0853)
Fica Vivo program		−0.0509		−0.0331
		(0.0881)		(0.0777)
Olho Vivo program		−0.0282		−0.0618
		(0.108)		(0.0949)
GDP per capita			0.0323	0.0189
			(0.102)	(0.105)
School enrollment			−0.0914	−0.0631
			(0.0830)	(0.0813)

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.
*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS estimates of the effect of the IGESP on crime rates in municipalities in Minas Gerais in 2000–08. The dependent variable is the property crime rate (per 100,000 inhabitants) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population. Control variables are as follows: the number of military and civil police personnel and the number of military police cars (per capita), for police resources and personnel; dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo, for other public safety programs; and GDP per capita and the public school enrollment rate, for socioeconomic variables. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

personnel and instrumenting for the variable indicating the number of military police per capita. Our instrument was the interaction of battalion and year dummy variables, which is based on the idea that there are two steps in the allocation of military police to different areas: the number of policemen per battalion is first determined following bureaucratic and administrative guidelines, and the number of policemen within a battalion is then allocated to different areas according to the commander's discretion. The key identifying assumption is that a battalion commander has more freedom to allocate policemen to different areas under his or her command than to increase or reduce the total number of policemen under his or her command (exchanges with the State Secretariat of Social Defense suggested that this is a roughly good approximation for the allocation mechanism). In other words, variation of policemen is more exogenous at the battalion level than within battalions. Indeed, the first stage in this strategy is extremely strong, with an F statistic for the joint hypothesis that the coefficients on the instruments are zero (battalion-year interaction dummy variables) on the order of 1.7×108. In the second stage of this estimation, the estimated impacts of the IGESP are virtually unchanged. Moreover, the instruments for military police allocation seem to do at least part of the job: the effect of policemen per capita is negative and significant for property crimes, negative but insignificant for homicides, and positive but small in magnitude and very insignificant for personal crimes. In any case, the coefficient on the treatment variable shows that the previous results are in no way related to potential endogeneity problems or measurement error in police-related variables.

TABLE 7. Controlling for Confounding Factors: Personal Crime Rates[a]

Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*
IGESP	−0.207***	−0.167***	−0.183***	−0.166***
	(0.0385)	(0.0447)	(0.0293)	(0.0435)
Military police personnel	92.38***			95.59***
	(33.22)			(34.48)
Military police cars	101.9			118.5
	(81.86)			(85.26)
Civil police personnel	26.98			19.78
	(64.46)			(66.71)
Municipal guard		0.0287		0.0224
		(0.0406)		(0.0423)
Fica Vivo program		−0.0400		−0.0383
		(0.0400)		(0.0404)
Olho Vivo program		−0.0437		−0.0679
		(0.0420)		(0.0438)
GDP per capita			0.100	0.0832
			(0.0648)	(0.0645)
School enrollment			0.0397	0.0196
			(0.0530)	(0.0613)

*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS estimates of the effect of the IGESP on crime rates in municipalities in Minas Gerais in 2000–08. The dependent variable is the personal crime rate (per 100,000 inhabitants) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population. Control variables are as follows: the number of military and civil police personnel and the number of military police cars (per capita), for police resources and personnel; dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo, for other public safety programs; and GDP per capita and the public school enrollment rate, for socioeconomic variables. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

We also estimated the regressions for some nonviolent crimes, which we do not analyze here due to concerns related to underreporting and because the IGESP is not likely to have much effect on these crimes. As expected, we found no significant effect of the IGESP on misdemeanors and nonviolent personal crime and only a small effect on nonviolent property crime (less than half of the analogous estimate for violent property crime in table 6). This pattern supports our decision to focus on violent crimes.

Robustness

This section deals with two potential problems not addressed in our benchmark specification: the comparability of municipalities in the sample and the functional form of the estimating equation. One of the main problems with our empirical approach involves the possibility of a differential dynamic behavior of crime rates across municipalities, potentially associated with endogeneity in program adoption. This phenomenon may occur, for example, when there

is convergence or mean reversion in crime rates across regions and when municipalities with initially higher crime rates are more likely to receive the IGESP. Tables 8, 9, and 10 address this issue for homicides, property crimes, and personal crimes, respectively.

In the first column of the tables, we include a linear trend for each municipality in addition to the variables included in the previous specification. In other words, we let each municipality have its own specific evolution in crime rates and ask whether, even in this setting, the adoption of the IGESP was associated with deviations from this trend. This is very demanding of the data, since we include 853 independent time trends in the specification. In the remaining columns, we include different combinations of pretreatment dummy variables (equal to one in some specific period prior to IGESP implementation). For each type of crime, we consider four specifications with different placebos: one with three dummy variables corresponding to the first, second, and third years immediately prior to implementation, respectively; one with only the dummy variable corresponding to the first year; one with a single dummy variable corresponding to the first two years; and one with a single dummy variable corresponding to the first three years.

Column 1 of tables 8, 9, and 10 shows that the estimated effect of the IGESP remains negative and statistically significant for all types of crime even when a municipality-specific linear trend is added to the specification. For homicides (table 8), the estimated coefficient increases relative to table 5. For property crimes (table 9), the estimated coefficient is reduced in magnitude, but remains strongly significant. For personal crimes (table 10), it remains similar. This specification can be seen as a very conservative and extreme test, since the convergence in crime rates itself could be partly determined by the implementation of the IGESP.

When we include the placebos capturing preexisting trends, the results on the treatment variables remain very similar to those from the previous series of tables, although the homicide results are slightly weaker. More importantly, the placebo variables vary between positive and negative across types of crime and specifications, and they are statistically significant at 10 percent in only one out of eighteen cases (with a positive sign). Independently of the specific way we model the preexisting trend, there is no evidence of statistically significant differential behavior of crime rates across locations prior to the implementation of the IGESP. In short, our main results are not affected when we explicitly deal with the possibility of differential trends, and there is no evidence of dynamic endogeneity problems. Indeed, the estimated coefficients seem to reflect a causal effect of the IGESP on crime rates.

TABLE 8. Municipality-Specific Linear Trends and Preexisting Trends: Homicide Rates[a]

	Municipality-specific linear trend	*Pretreatment placebos*			
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
$IGESP_{t-1}$	−0.196***	−0.0589	−0.0726*	−0.0627	−0.0680*
	(0.0744)	(0.0379)	(0.0384)	(0.0397)	(0.0399)
Municipality-specific linear trend?	X				
Pretreat dummy variable:					
Pretreatment year 1		0.117	0.0919		
		(0.0855)	(0.0805)		
Pretreatment year 2		0.118			
		(0.0764)			
Pretreatment year 3		0.0290			
		(0.0459)			
Pretreatment years 1 and 2				0.110*	
				(0.0666)	
Pretreatment years 1, 2, and 3					0.0872
					(0.0576)
Summary statistic					
No. observations	6,823	6,823	6,823	6,823	6,823
R^2	0.839	0.794	0.793	0.794	0.794

* Statistically significant at the 10 percent level.

*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS estimates of the effect of the IGESP on crime rates in municipalities in Minas Gerais in 2000–08. The dependent variable is the homicide rate (per 100,000 inhabitants) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population. The following control variables are included (not shown): the number of military and civil police personnel and the number of military police cars (per capita); dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo; and GDP per capita and the public school enrollment rate. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

Another potential concern suggested by the numbers in table 2 is that municipalities that did not receive the IGESP are so different from those that did that they cannot constitute a good comparison group. A related concern is that the number of treated municipalities is relatively small (fifty-six), so the previous results may depend on the presence of specific outliers. Municipalities that received the IGESP are indeed much larger, wealthier, and more violent than municipalities that did not receive it, so these are legitimate concerns. To address these and other issues, we reestimate the most complete specification from tables 5–7 on alternative samples and check the sensitivity of the results to the changes. This exercise also helps shed light on the source of the variation identified in the estimated coefficients.

We look at six alternative samples: (i) we exclude the state capital, which is almost four times larger than the second-largest municipality; (ii) we restrict the sample to municipalities with more than 50,000 inhabitants, to create a

TABLE 9. Municipality-Specific Linear Trends and Preexisting Trends: Property Crime Rates[a]

	Municipality-specific linear trend	*Pretreatment placebos*			
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
IGESP	−0.267***	−0.481***	−0.497***	−0.446***	−0.447***
	(0.0471)	(0.110)	(0.0977)	(0.0958)	(0.0901)
Municipality-specific linear trend?	X				
Pretreat dummy variable:					
Pretreatment year 1		−0.0791	−0.0960		
		(0.0983)	(0.0864)		
Pretreatment year 2		0.0602			
		(0.0602)			
Pretreatment year 3		−0.00351			
		(0.0377)			
Pretreatment years 1 and 2				−0.00043	
				(0.0671)	
Pretreatment years 1, 2, and 3					−0.00239
					(0.0495)
Summary statistic					
No. observations	7,676	7,676	7,676	7,676	7,676
R^2	0.952	0.932	0.932	0.932	0.932

*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS estimates of the effect of the IGESP on crime rates in municipalities in Minas Gerais in 2000–08. The dependent variable is the property crime rate (per 100,000 inhabitants) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population. The following control variables are included (not shown): the number of military and civil police personnel and the number of military police cars (per capita); dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo; and GDP per capita and the public school enrollment rate. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

control group more similar to the municipalities that received the IGESP; (iii) we restrict the sample to 2004–08, to reduce the weight of a long baseline period without IGESP implementation (the first implementation takes place in 2005); (iv) we reestimate the model without population weights; (v) we restrict the sample to municipalities that eventually received the IGESP (that is, to municipalities that eventually entered the treatment group); and (vi) we combine three of the above criteria, restricting the sample to municipalities that eventually received the IGESP, excluding the state capital, and restricting the time period to 2004–08. The results are presented in table 11.

When we exclude the state capital from the estimation (column 1), the results related to property and personal crimes remain very similar, while the lagged effect on homicide rates is reduced in magnitude and becomes insignificant. When the sample is restricted to municipalities with a population above 50,000 (column 2) or to the period 2004–08 (column 3), the qualitative results again remain very similar, and there are only small quantitative changes: the

TABLE 10. Municipality-Specific Linear Trends and Preexisting Trends: Personal Crime Rates[a]

	Municipality-specific linear trend	*Pretreatment placebos*			
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
IGESP	−0.145***	−0.153**	−0.164***	−0.143***	−0.155***
	(0.0427)	(0.0642)	(0.0566)	(0.0525)	(0.0588)
Municipality-specific linear trend?	X				
Pretreat dummy variable:					
Pretreatment year 1		0.0144	0.00318		
		(0.0668)	(0.0640)		
Pretreatment year 2		0.0448			
		(0.0384)			
Pretreatment year 3		−0.00820			
		(0.0373)			
Pretreatment years 1 and 2				0.0342	
				(0.0411)	
Pretreatment years 1, 2, and 3					0.0146
					(0.0342)
Summary statistic					
No. observations	7,676	7,676	7,676	7,676	7,676
R^2	0.825	0.776	0.776	0.776	0.776

** Statistically significant at the 5 percent level.

*** Statistically significant at the 1 percent level.

a. All columns report difference-in-differences OLS estimates of the effect of the IGESP on crime rates in municipalities in Minas Gerais in 2000–08. The dependent variable is the personal crime rate (per 100,000 inhabitants) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population. The following control variables are included (not shown): the number of military and civil police personnel and the number of military police cars (per capita); dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo; and GDP per capita and the public school enrollment rate. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

coefficients on homicides are slightly larger, while those on property crimes are slightly smaller. When we run the original specification without population weights (column 4), only the effect on property crimes remains statistically significant. Very similar results are obtained when we restrict the sample to municipalities that eventually received the IGESP (columns 5 and 6), except that the coefficients on homicides are similar to those estimated in table 5, but only statistically significant in column 5 (and only at 10 percent). The effect on property crimes is statistically significant and quantitatively large in every single specification presented thus far.

Our final robustness test is related to the functional form of the estimating equation. To deal with the issue of crime rates equal to zero in some small municipalities, we added one to the number of crimes before calculating the crime rate and taking the logarithmic transformation. This procedure may quantitatively affect the estimated coefficients when the mean of

TABLE 11. Alternative Samples

Dependent and explanatory variable	*Excluding state capital* (1)	*Restricting population to > 50,000* (2)	*Restricting period to 2004–08* (3)	*Not weighted by population* (4)	*Only IGESP municipalities* (5)	*Only IGESP municipalities, excluding capital, 2004–08* (6)
Homicide rate						
$IGESP_{t-1}$	−0.0537	−0.173***	−0.146***	0.0997	−0.0906*	−0.0902
	(0.0547)	(0.0471)	(0.0445)	(0.0785)	(0.0489)	(0.0850)
No. observations	6,815	511	4,264	6,823	448	275
R^2	0.767	0.908	0.835	0.667	0.914	0.936
Property crime rate						
IGESP	−0.427***	−0.377***	−0.360***	−0.272***	−0.242***	−0.232***
	(0.0795)	(0.0725)	(0.0445)	(0.0641)	(0.0611)	(0.0664)
No. observations	7,667	570	4,264	7,676	504	275
R^2	0.906	0.943	0.945	0.697	0.923	0.940
Personal crime rate						
IGESP	−0.126**	−0.162***	−0.164***	−0.00959	−0.0300	−0.0553
	(0.0489)	(0.0452)	(0.0395)	(0.0622)	(0.0470)	(0.0669)
No. observations	7,667	570	4,264	7,676	504	275
R^2	0.746	0.910	0.815	0.578	0.887	0.913

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.
*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS estimates of the effect of the IGESP on crime rates in municipalities in Minas Gerais in 2000–08 (unless a different period is specified). The dependent variable is the homicide, property, or personal crime rate (per 100,000 inhabitants) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population (unless otherwise noted). The following control variables are included (not shown): the number of military and civil police personnel and the number of military police cars (per capita); dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo; and GDP per capita and the public school enrollment rate. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

the dependent variable is small, thereby biasing the estimation of the causal effect of implementation of the program. To address this concern, we estimate regressions identical to the complete specification from tables 5–7, but use Tobit models instead of OLS and use the crime rate (without the logarithmic transformation) as the dependent variable. Despite the problem of incidental parameters in nonlinear models with fixed effects, we use Greene's Monte Carlo results from (2004) to estimate our Tobit models with fixed effects by brute force.[23] Greene presents evidence that bias in Tobit models with fixed effects is very small for $T \geq 5$ and depends chiefly on the fraction of censored

23. Greene (2004).

TABLE 12. Tobit Models[a]

	Homicide rate	*Property crime rate*	*Personal crime rate*
Explanatory variable	*(1)*	*(2)*	*(3)*
IGESP		−230.0***	−14.64***
		(6.508)	(1.164)
$IGESP_{t-1}$	−4.170***		
	(0.490)		
No. observations	6,823	7,676	7,676

*** Statistically significant at the 1 percent level.

a. The table reports Tobit estimates of the effect of the IGESP on crime rates in municipalities in Minas Gerais in 2000–08. The dependent variables are crime rates (per 100,000 inhabitants). All regressions include a constant and municipality and year dummy variables and are weighted by municipal population. The following control variables are included (not shown): the number of military and civil police personnel and the number of military police cars (per capita); dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo; and GDP per capita and the public school enrollment rate. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

observations in the sample. Since our sample has $T = 9$, we draw on his results and explicitly discuss the likely bias when presenting the results.

The qualitative results are again identical to those obtained previously (see table 12). Quantitatively, the marginal effect presented in the table must be divided by the means of the dependent variables to yield a proportional change comparable to the coefficients estimated with the logarithmic version. Making this calculation using the 2002 crime rates for municipalities that would eventually receive the IGESP as reference points, the results from table 12 imply that the implementation of the IGESP would be associated with reductions of 17 percent in homicides (with a lag of one year), 30 percent in property crimes, and 15 percent in personal crimes. In relative terms, the estimated effects are of the same order of magnitude of those reported above. In any case, we prefer the specifications using the dependent variables with the logarithmic transformation, as it seems more natural to think that the absolute effect of the IGESP would depend on the initial level of crime.

Given the number of years in our sample and the fraction of censored observations in our dependent variables (from 46 percent for homicide rates to 6 percent for personal crimes), Greene's results suggest that the bias in estimated marginal effects and standard errors should be no greater than 15 percent.[24] In light of the estimates in table 12, such adjustments to coefficients and standard errors would change neither the qualitative results nor the order of magnitude of the quantitative results.

24. Greene (2004).

Channels

As a final exercise, we evaluate the impact of the IGESP on variables that try to capture dimensions of police efficiency. These are variables related to the apprehension of firearms and other weapons and to total and on-view arrests. The main question here is how police action responds to a certain level of violence. Therefore, we normalize these variables by the total number of crimes, so that we assess the effect of the IGESP on apprehension and clearance rates (transformed by the natural logarithm).

The first equations estimated are identical to the most complete specification in tables 5–7. We also adopt a variant, given the distinct nature of the phenomenon. If there is an unusually high stock of weapons and criminals circulating in society, we might expect a temporary effect of the IGESP: the number of weapons apprehended and the number of arrests would rise after the implementation of the program, as police action became more effective and the excess number of criminals and weapons were removed from society, but then decline to their original levels over time as the total number of crimes was also reduced. This perspective is equivalent to the idea that it is easier to increase clearance rates when crime rates are high. To evaluate this possibility, we create treatment variables corresponding only to the first and second years of implementation.

The results, presented in table 13, suggest that implementation of the program increased both the apprehension of weapons and the number of arrests per reported crime. The increase in apprehension rates seems to persist throughout the sample period. In the case of clearance rates, the effect is particularly strong in the first years of the program (especially the second year). These results reinforce the perception that the coefficients estimated above do indeed reflect the causal impact of the IGESP on crime rates. The evidence suggests that part of this reduction in crime is due to a more effective response of the public safety apparatus.

Concluding Remarks

This paper evaluates the impact of the IGESP program on crime rates and the effectiveness of police in the state of Minas Gerais, Brazil. The objective of the IGESP program was to integrate the civil and military police forces, based on the introduction of information management systems and organizational changes akin to those associated with CompStat.

TABLE 13. Police Efficacy: Weapon Apprehension and Clearance Rates[a]

	Apprehension of weapons		*Clearance*		*Apprehension of weapons*		*Clearance*	
Treatment variable	*Firearms* (1)	*Other weapons* (2)	*Total arrests* (3)	*On-view arrests* (4)	*Firearms* (5)	*Other weapons* (6)	*Total arrests* (7)	*On-view arrests* (8)
IGESP	0.325***	0.198**	0.137*	0.170**				
	(0.0587)	(0.0949)	(0.0739)	(0.0743)				
IGESP year 1					0.203***	0.172*	0.130**	0.154***
					(0.0670)	(0.0934)	(0.0649)	(0.0597)
IGESP year 2					0.155***	0.110*	0.187***	0.237***
					(0.0464)	(0.0562)	(0.0463)	(0.0457)
Control variable								
Police resources and personnel	X	X	X	X	X	X	X	X
Other public safety programs	X	X	X	X	X	X	X	X
Socioeconomic characteristics	X	X	X	X	X	X	X	X
Summary statistic								
No. observations	7,229	7,229	6,907	6,907	7,229	7,229	6,907	6,907
R^2	0.818	0.873	0.878	0.874	0.816	0.873	0.878	0.874

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.
*** Statistically significant at the 1 percent level.

a. All columns report difference-in-difference OLS estimates of the effect of the IGESP on police efficacy in municipalities in Minas Gerais in 2000–08. The dependent variables are the weapons apprehensions rate (weapons apprehended over total reported crimes), the clearance rate (total arrests over total reported crimes), and the on-view arrest rate (on-view arrests over total arrests) expressed in natural logarithms. All regressions include a constant and municipality and year dummy variables and are weighted by municipal population (unless otherwise noted). The following control variables are included (not shown): the number of military and civil police personnel and the number of military police cars (per capita); dummy variables indicating the presence of a civil municipal guard, the Fica Vivo program, and the Olho Vivo; and GDP per capita and the public school enrollment rate. The sample covers 853 municipalities. Robust standard errors (clustered at the municipality level) are in parentheses.

The evidence presented points to a causal effect of the IGESP on crime. Our most conservative estimates suggest a reduction of 23 percent in property crimes. There is some evidence of effects on personal crimes and a delayed reduction in homicides, but these estimates are less robust and seem to be partly related to the experience of the state capital. The program is also associated with improved police response to crime, measured by the apprehension of weapons and clearance rates. The estimated coefficients imply that between 2005 and 2008, roughly 50,000 property crimes were avoided due to the implementation of the IGESP.

To our knowledge, this is the first set of econometrically rigorous estimates of the effect of the integration of dual police forces on crime. The results suggest that the coordination and informational gains represented by this change may constitute an important factor in a successful policy for fighting crime in Brazil.

References

Bayley, David H. 1985. *Patterns of Policing: A Comparative International Analysis.* Rutgers University Press.

———. 1999. "The Contemporary Practices of Policing: A Comparative View." In *Civilian Police and Multinational Peacekeeping: A Workshop Series. A Role for Democratic Policing.* U.S. Department of Justice, National Institute of Justice.

Beato, Cláudio. 1999. "Políticas públicas de segurança e a questão policial." *São Paulo em Perspectiva* 13(4): 13–27.

Beato, Cláudio, and others. 2007. "IGESP: Uma solução viável e eficiente na execução da política pública de segurança." *Informativo CRISP* 5(7).

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119(1): 249–75.

Bicudo, Hélio. 2000. "A unificação das polícias." *Estudos Avançados* 14(40): 91–106.

Biderman, Ciro, João M. P. de Mello, and Alexandre Schneider. 2010. "Dry Laws and Homicides: Evidence from the São Paulo Metropolitan Area." *Economic Journal* 120(543): 157–82.

Brasil, Glaucíria, and Domingos Abreu. 2002. "Uma experiência de integração das polícias civil e militar: os distritos-modelo em fortaleza." *Sociologias* 4(8): 318–55.

Brown, Mary M., and Jeffrey L. Brudney. 2003. "Learning Organizations in the Public Sector? A Study of Police Agencies Employing Information and Technology to Advance Knowledge." *Public Administration Review* 63(1): 30–43.

Chesnais, Jean Claude. 1999. "A violência no Brasil: causas e recomendações políticas para a sua prevenção." *Ciência & Saúde Coletiva* 4(1): 53–69.

Galiani, Sebastian, Paul J. Gertler, and Ernesto Schargrodsky. 2005. "Water for Life: The Impact of the Privatization of Water Services on Child Mortality." *Journal of Political Economy* 113(1): 83–120.

Garicano, Luis, and Paul Heaton. 2010. "Information Technology, Organization, and Productivity in the Public Sector: Evidence from Police Departments." *Journal of Labor Economics* 28(1): 167–201.

Greene, William. 2004. "Fixed Effects and Bias due to Incidental Parameters Problem in the Tobit Model." *Econometric Reviews* 23(2): 125–47.

Levitt, Steven D. 1997. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime." *American Economic Review* 87(3): 270–90.

———. 2002. "Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime: Reply." *American Economic Review* 92(4): 1244–50.

Manning, P. K. 2001. "Technology's Way: Information Technology, Crime Analysis, and the Rationalizing of Policing." *Criminology and Criminal Justice* 1(1): 83–103.

Mazerolle, Lorraine, Sacha Rombouts, and James McBroom. 2007. "The Impact of CompStat on Reported Crime in Queensland." *Policing: An International Journal of Police Strategies and Management* 30(2): 237–56.

Rocha, Geórgia R., Jésus T. Barreto Jr., and Ricardo F. Gontijo. 2008. "Modelo de gestão integrada do sistema de defesa social de Minas Gerais." Brasília-DF.: I Congresso Consad de Gestão Pública.

Rosenfeld, Richard, Robert Fornango, and Eric Baumer. 2005. "Did Ceasefire, CompStat, and Exile Reduce Homicide?" *Criminology and Public Policy* 4(3): 419–50.

Sapori, Luís Flávio, and Scheilla Cardoso P. de Andrade. 2009. "Integração policial em Minas Gerais: desafios da governança da política de segurança pública." *Civitas-Revista de Ciências Sociais* 8(3): 428–53.

Schargrodsky, Ernesto, and Rafael di Tella. 2004. "Do Police Reduce Crime? Estimates Using the Allocation of Police Forces after a Terrorist Attack." *American Economic Review* 94(1): 115–33.

Soares, Rodrigo R. 2004. "Crime Reporting as a Measure of Institutional Development." *Economic Development and Cultural Change* 52(4): 851–71.

Soares, Rodrigo R., and Igor Viveiros. 2010. "Organization and Information in the Fight against Crime: An Evaluation of the Integration of Police Forces in the State of Minas Gerais, Brazil." IZA Discussion Paper 5270. Bonn: Institute of Labor Economics (IZA).

———. 2013. "The Program of Integration and Management in Public Safety in Minas Gerais, Brazil: A Descriptive Analysis." In *Lessons from the Economics of Crime: What Reduces Offending?,* edited by Philip J. Cook and others, pp. 205–32. MIT Press.

Walsh, William F. 2001. "CompStat: An Analysis of an Emerging Police Managerial Paradigm." *Policing: An International Journal of Police Strategy and Management* 24(3): 347–62.

Weisburd, David, and others. 2004. "Reforming to Preserve: CompStat and Strategic Problem Solving in American Policing." *Criminology and Public Policy* 2(3): 421–56.

Willis, James J., Stephen D. Mastrofski, and David Weisburd. 2003. "CompStat in Practice: An In-Depth Analysis of Three Cities." Washington, DC.: Police Foundation.