Governing by contract as a way to reduce crime? An impact evaluation of the large-scale policy of security pacts

Corresponding author:
Marco Calaresu
mcalaresu@uniss.it
00393331357233

Author Details:
Author 1 Name: **Marco Calaresu**
Department: **Department of Law**
University: **University of Sassari**
City and Country: **Sassari, Italy**
Address: **Viale Mancini, 5, 07100**
Email: **mcalaresu@uniss.it**

Author 2 Name: **Moris Triventi**
Department: **Department of Sociology and Social Research**
University: **University of Trento**
City and Country: **Trento, Italy**
Address: **Via Verdi, 26, 38122**
Email: **moris.triventi@unitn.it**

Abstract

In the last decades, governing by contracts, and in particular security contracts and pacts, have been increasingly promoted as a principal means of advancing crime prevention and governing security issues. Security pacts are a form of contract in which various institutional actors declare publicly to approve a common line of action, take mutual commitments and actions to prevent and control crime and disorder. Unfortunately, rigorous impact evaluations of the policy outcomes of security pacts are lacking. In this article, we aim to provide the first rigorous evaluation of the impact of a large-scale policy based on security pacts (involving around 12 million people) on various types of crimes in Italy. We built an *ad hoc* macro-level panel dataset of the 103 Italian provinces, with indicators covering a period spanning between 2004 and 2013. By exploiting variation in the time and place in which the policy was adopted, generalized difference-in-difference models indicate that, overall, security pacts had a limited impact on crime, significantly reducing thefts (and, partially, micro-criminality), but not robbery and homicide rates. We also found evidence of heterogeneous effects along province population size, with largest effects in the larger provinces

and null effects in the smaller ones. These findings are robust to a number of different sensitivity checks.

Keywords

Difference in Differences; Policy evaluation; Crime; Security pacts; Heterogeneous effects.

Competing interest

The authors declare that they have no competing interest.

Authors' information

Marco Calaresu is Assistant Professor at the University of Sassari, where he teaches Political Science and Public Policy Analysis. He earned a PhD in Political Science at Scuola Normale Superiore. He was Visiting Scholar at New York University, and Erasmus+ Visiting Professor at Cardiff University and University of Essex. His research interests include security policies and the quality of democracy. Among his recent publications: Security Pacts: the Italian Experience (*Eleven International Publishing*, 2017); Policing and Urban Control in Rome and Milan. A View From the Southern Edge of Europe, (with R. Selmini, in A. Edwards, E. Devroe, P. Ponsaers, *Policing European Metropolises,* Routledge, 2017).

Moris Triventi is Senior Assistant Professor at the University of Trento, where he teaches Social Research Methods and Sociology of Education. He earned a PhD in Applied Sociology at the University of Milan-Bicocca and was Research Fellow at the European University Institute. He was Visiting Scholar at the University of Amsterdam, University of Cologne and Trinity College Dublin. His research interests include social stratification and inequality, education, labor market, crime, impact policy evaluation and quantitative methods for social research. He published several international edited books and peer-reviewed articles in journals such as *European Sociological*

Review, *Social Science Research*, *Work Employment & Society*, and *Economics of Education Review*.

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INTRODUCTION

From the 1970s onwards, in the political market of the "full-grown democracies" (Hughes et al. 2002), security has become an "obsession" (Curbet 2008), that citizens have charged to their elected representatives (Garland 1996; 2001; Edwards and Hughes 2002), demanding guarantees on personal safety, protection against terrorism, and a reduction of fear and insecurity (Boutellier 2004; Bauman 2005). On the political offer side, at the same time, several policy platforms have been developed to meet that demand at all tiers of government (Taylor 1999; Crawford 2002b). In the last fifteen years, in particular, many European countries have witnessed an expansion in their crime prevention policies (Van Swaaningen 1999; Crawford 2002a; Edwards and Hughes 2005).

As a matter of fact, aside the traditional goal of pre-empting the commission of crime, crime prevention policies have included goals that have usually pertained to other legal systems (Rodger 2008), such as the reduction of fear and insecurity and the enhancement of people's quality of life (Crawford 2009; Di Ronco 2016). Furthermore, new ways of doing crime prevention as reflected in security policies – such as nodal governance and multi-agency partnership and coordination (Virta 2002; Shearing and Wood 2003; Shearing 2005; Terpstra 2008; Edwards and Hughes 2012) – have attracted great attention by criminologists and political scientists (Author's own 2017a). Among the different instruments used to inform such policies, security contracts and pacts (Gaudin 1999; Wacquant 1999; Lascoumes and Le Galés 2004; Le Goff 2004) have been a principal means of advancing crime prevention. In particular, security pacts are a form of contract in which the public actors involved declare to approve a project or a line of action, or taking mutual commitments,

making their resources (not necessarily financial resources) available for common action, agreeing how and when to act (Bobbio 2000). Through the signing of pacts, local authorities on a different scale of government, communities and even individuals have all been given a responsibility for crime prevention (Garland 2001), alongside agencies of formal control that have traditionally been entrusted with crime prevention functions. Using security pacts as a governance instrument, that is "governing by contract" (Gaudin 1999; Cooper and Cooper 2002; Lascoumes and Le Galès 2004), would be a response to the need of reconfiguring the relationships – and the responsibilities – between the national and the local level of government inside a state (Crawford 1999; Johnston and Shearing 2003). In this perspective, the "spread of contractual practices" has been considered "one of the most significant processes of political-administrative change" (Bobbio 2000, 112) in the last fifteen years.

Garland makes it clear that this process is not simply a downloading of responsibilities and tasks, but a much more complex "new form of governing at a distance" and "new mode of exercising power" (1996, 454). Yet, can security pacts be considered in practice as a good solution (at least in terms of their efficacy, intended as the capacity or power to produce a desired effect and results) for reducing crime? There is growing consensus on the need to establish "what works, what doesn't, what's promising" (Sherman et al. 1997) about new security policies, on the basis of solid scientific knowledge and empirical evidence (Hughes et al. 2002; Cook and Ludwig 2006). A number of policy evaluation studies have examined the so-called *formal dimension* of security pacts, in terms of *policy outputs*,[1] namely by focusing on their diffusion, distribution, and contents (Crawford and Evans 2012). By contrast, much less empirical evidence is available on the so-called *substantial dimension* of

[1] As referred by Anderson (2010, 8-9), "Policy outputs are the direct result of the decision-making process which usually involves the adoption of a certain programme, law or regulation. They are defined by the content of a public policy, as it is fixed in legal or administrative documents, and can encompass both substantive and procedural aspects".

security pacts, in terms of *policy outcomes and policy impacts*,[2] namely by assessing their effectiveness and/or efficiency (Howlett 2009 and Howlett et al. 2009).

Although the importance of impact evaluation[3] of the new security policies is increasingly recognized, much work still remains to put this knowledge into practice (Welsh 2007). E.g., taken for granted that the crime rates (especially violent crimes) have been decreasing in most western countries at a steady rate, it remains to be clarifying what effects can be actually linked to specific policies (or measures) for specific crimes in specific years/countries (Ekblom and Pease 1995, 595).

Furthermore, much of the empirical evidence on the policy outcomes of the new security policies, with a special focus on the effectiveness of crime-related policies, is still based on small-scale programs at the school, family or community level (Sherman et al. 1997; Cozens, Saville and Hillier 2005; Welsh and Farrington 2009; Braga, Welsh and Schnell 2015), while attempts to evaluate the policy outcomes with regards to the effectiveness of large-scale crime-related policies are rather scarce. Moreover, while most of the studies on the policy outcomes of crime-related policies have been conducted in the United States, much less empirical evidence is available on Continental and Southern European countries (Author's own 2017b).

This article aims to fill this gap in the impact evaluation literature, namely by establishing if security pacts – a particular kind of security policy promoted since 2007 in Italy as a way of preventing and controlling crime and disorder – were capable (and to what extent) to reach

[2] As stated by Knill and Tosun (2012, 28-29) "Policy outcomes are closely related to the stages of policy implementation and evaluation, [while] policy impacts focus on the extent to which a policy decision and its subsequent implementation have actually brought about the expected results, indicating that they are mainly assessed at the evaluation stage"

[3] We use here the definition of "impact evaluation" as proposed by Knill and Tosun (2012, 175), which affirm, "Impact evaluation concerns the establishing of a causal link between the policy and its effects, which can be either policy outcomes or impacts".

their ultimate goal of reducing crime. We believe that focussing on the Italian case is worthwhile for a number of reasons. Italy can be in fact considered a "most-likely case", according to the definition of George and Bennet (2005) and Levy (2008),[4] since governing by contract was explicitly adopted by the national government as a "good solution" in order to change a previous model of governance that was characterized by various problems associated with a series of sub-optimal policy outcomes (see below for a more detailed discussion). It is also worth noticing that in comparative perspective Italy displays a rather high level of crime rates among the OECD countries. For instance, it is ranked 9th in cases of robbery per 100,000 population, just below the United States (UNODC 2011).

Furthermore, the policy has had a strong quantitative and symbolic relevance in Italy and Europe. On the quantitative side, such a large-scale policy affected more than 3 thousand municipalities and around 12 million people in the 4th most populated country among the 28 countries of the European Union (Eurostat 2018). On the symbolic side, the security issue became strategically important in political and electoral competition, consensus building and legitimation of politics (Battistelli and Lucianetti 2010). As a matter of fact, shortly after the introduction of the policy, the then Italian Minister of Interior, Giuliano Amato, published a press release arguing: "Crime cannot always be prevented, but now we have suitable instruments to deal with it. […] After the adoption of last year's security pacts, crime in the cities, as is well known, has declined dramatically" (Ministry of Interior 2008a). The confidence of the national government in the efficacy of this large-scale policy has recently been renewed. Indeed, in 2017, the then Italian Minister of Interior, Marco Minniti addressed security pacts as one of the most important policies not only to prevent predatory crimes and petty crimes but also to fight these types of crime at the local level (Ministry of Interior

[4] The study of a crucial case of this type should present the requirements of prominence, significance and representativeness, which justify the choice of a specific case to be empirically tested (George and Bennett 2005), according to the logic of "if I cannot make it there, I cannot make it anywhere" (Levy 2008, 12).

2017ab). Unfortunately, such political declarations are not sustained by any rigorous study trying to evaluate the outcomes of the security pacts policy, taking into account the fact that the policy was not randomly applied. Indeed, the specific way in which the policy has been adopted – in different geographical contexts at different times and with a different length – represents a unique opportunity to implement a quasi-experimental design (generalized difference-in-difference) to provide a credible estimate of the causal effect of the security pacts adopted in Italy on various types of crimes, including thefts, robberies, homicides, and micro-criminality in the cities (see below).

The article is then organized as follows. In the second section, we provide a description of the institutional context, including some basic information about the Italian political and administrative structure, and the main characteristics of the security pacts in Italy. In the third section, we elaborate on some hypotheses guiding the empirical analysis. The fourth section presents the data, variables and methods used to assess the outcomes of security pacts. The fifth section presents the empirical results and the last section concludes.

THE INSTITUTIONAL CONTEXT

The Italian Political and Administrative Structure

With the aim of describing the institutional context within which the large-scale policy of security pacts was adopted, as a first step, we provide some basic information about the Italian political and administrative structure.

The Italian Republic can be defined as a "multi-level polity" (Longo and Mobilio 2016, 2) which, according to the art. 114 of the Constitution, is currently made up of municipalities

(*Comuni*), provinces (*Province*), metropolitan cities (*Città metropolitane*), regions (*Regioni*) and the state (Baccetti 2008).[5]

The regions (*Regioni*) are the first sub-national unit of division of the Country, constituting Italy's second Nomenclature of Territorial Units for Statistics (NUTS)[6] administrative level (Eurostat 2011). As prescribed by the Constitution (art. 123), every region has its own statute, adopted through constitutional laws, determining the form of government and the fundamental principles of the organization and the functioning of the region itself. There are currently 20 regions in Italy, of which five (*Regioni a Statuto Speciale*) are constitutionally given an extended autonomy granted by special statutes, with respect to the regions with the ordinary statute (*Regioni a Statuto Ordinario*) (Bolgherini 2014, 198). The legislative powers of both special and ordinary regions are subject to certain constitutional limitations, the most important of which is that regional acts may not conflict with national interests.

Each region in Italy, except for the Aosta Valley,[7] is further divided into provinces (*Province*), forming the intermediate or second-tier territorial level, and constituting Italy's third NUTS administrative level (Eurostat 2011). The provinces in Italy, at least before the reform of 2014 (Law No. 56 of 2014) and the following introduction of the metropolitan cities,[8] represented "the middle layer in the government structure, both in terms of political

[5] It worth noticing, as referred by Baccetti (2011, 164) that "the Italian words 'Regione', 'Provincia' and 'Comune', unlike their English equivalent, refer to both the relevant geographical area and to the local government body associated with that geographical area."

[6] NUTS provides a uniform, consistent breakdown of territorial units for the production of regional statistics for the European Union (Eurostat 2011).

[7] The Region of Aosta Valley is the sole exception: it is not subdivided into provinces, and provincial functions are exercised by the region.

[8] Although is beyond the scope of this work, it can be said that the recently introduced metropolitan cities in Italy are hybrid administrative entities within the Italian local government system, representing both a metropolitan community, coinciding with the old provincial one, and also the various municipalities located within the boundary of the metropolitan area (see Longo and Mobilio 2016).

devolution and administrative decentralisation" (Baccetti 2011, 165). There are currently 110 provinces in Italy (Bolgherini 2014, 198). The province has statutory autonomy within the limits set by the laws of the state and of the region of belonging, with major responsibilities for environment and town and country planning (Baccetti 2011, 175).[9] In each province, there is also a Prefect (*Prefetto*) leading a prefecture (*Prefettura*), who is the representative of the central state authority for public safety and for enforcing the law.

The municipalities (*Comuni*) are the basic sub-national unit of division of the Country, roughly equivalent to a township or a city, and constituting Italy's second Local Administrative Units level (LAU) (Eurostat 2011). There are over 8,000 municipalities in Italy (Bolgherini 2014, 198). They have the power to levy and collect limited local taxes, and they have their own police force for handling public safety and urban security (*Polizia Municipale e Locale*), although their powers are much inferior to those exercised by the national police (*Polizia di Stato*, *Arma dei Carabinieri*, and *Guardia di Finanza*).

The large-scale policy of security pacts

The large-scale policy of security pacts started in Italy on 20th March 2007 (based on the Law No. 296 of 2006 – 2007 Financial Act): at that time prefects, resident in each province of Italy, were granted the power to sign agreements with municipalities, provinces and regions (excluding private stakeholders and citizens) for the initiation of "special programmes" on urban security, such as "coordinated plans of territorial control." In the same year, the Minister of Interior and the President of the National Association of Italian Municipalities (ANCI), following the provision of the 2007 Financial Act, signed a guideline

[9] The South Tyrol and Trentino-Alto Adige are autonomous provinces: unlike all other provinces in Italy they have the same legislative powers as regions and are not subordinated to the region they are part of, namely the region of Trentino-Alto Adige/Südtirol.

agreement with the mayors of the capital cities of metropolitan areas[10] to initiate – within sixty days – specific agreements called "security pacts." Successively, in 2008, the application of the "coordinated plans of territorial control" was extended to small and medium-sized municipalities (Legislative Decree No. 92 and Law No. 125 of 2008).

The pacts pursue the main goal of collaboration between central (the state) and local governments (municipalities, provinces, regions), through the development of an "integrated security system" (Ministry of Interior 2007; 2008b), aimed at increasing safety in the cities and decreasing the perception of insecurity of citizens. In particular, local administrations were granted the capability to acquire more "exclusive powers" at the local level, and also more "shared responsibility" in relation to national government, with particular regards to predatory crimes and petty crimes (Author's own 2012).

In principle, security pacts provided municipalities, provinces and regions with greater decision-making powers in the fields of urban security and crime prevention (Author's own 2017a). In an attempt to start new forms of cooperation between the central and local governments, the pacts provide a two-fold additional function. On the one hand, they bind prefects in each province to promote a more intensive exchange of information and technological cooperation with municipalities, provinces and regions through the establishment of local and territorial integrated security systems, and the creation of shared information and communication systems.

On the other hand, security pacts redistribute responsibilities related to local and situational activities, allowing local police (*Polizia Municipale e Locale*) to cooperate in the fields of prevention and repression of crime with national-level law enforcement agencies, also including joint training activities between national (with special reference to *Polizia di Stato*, *Arma dei Carabinieri*, and *Guardia di Finanza*), and local police forces.

[10] These include the following cities: Bari, Bologna, Cagliari, Catania, Florence, Genoa, Milan, Naples, Palermo, Rome and Turin.

Looking at the institutional configurations of security pacts signed in the 2007-2009 period, previous research (Author's own 2015) found that the prefecture is the only actor that is always involved, signing from time to time the security pacts with the municipalities, the provinces, and the regions (or a combination of such public actors). The municipalities are the main actors involved in signing the pacts with the prefectures. The synergy between the municipality and the province is the most widely used (40% of the pacts), higher than the agreements signed by a single municipality or a group of municipalities with the prefecture (30%). The full institutional configuration, involving local authorities at every level of government (region, province(s) and municipality(s)) is the least common. The contractual commitment varies from one to three years in duration. Nearly 60% of the pacts were undertaken for a total duration of two years, while over 20% of them did not explicitly specify a maximum duration.

Security pacts not only were expected by the central government to be a new organizational and operational model in preventing crime, but also an instrument able to generate, with specific, but sometimes heterogeneous actions (Author's own 2017a), a direct and almost immediate *effect* on crime rates.

The most popular measures put in place by security pacts included, but were not limited to:

- creating new and strengthening the already existing forms of formal and natural surveillance, like i.e., video surveillance systems in the cities (with special reference to the CCTV programmes in the public transport services, and in the historical city centres), and street lighting (with particular reference to the stops of public transport services, and to the peripheral areas);

- adopting neighborhood police services and strengthening law enforcement and community policing through, among others, the project of the *Vigile di quartiere* (a local

municipal police officer that operates in the streets exclusively on foot or by bicycle, e.g., with functions of monitoring public places and nightclubs, collecting requests directly from citizens, and of fighting illegal itinerant trade and counterfeiting of goods);

- establishing the opening and closing hours of pubs and bars in the historical city centres, relocating sales outlets in the peripheral areas, checking/withdrawing/suspending licences of commercial activities through crime mapping analysis, and then through the adoption of specific plans for the redevelopment of different urban areas with different needs;

- establishing new "intermediate bodies" and "working groups" for collaboration between the local governments (municipalities, and/or provinces and regions) and the prefects in each province to make coordination and cooperation easier and smoother, to develop a better "processing task and criticality analysis" about crime prevention, and to adopt future agreements and forms of partnership among local and national police forces.

- recovering the "degraded urban areas" with the adoption and implementation of shared and/or social redevelopment plans;

- instituting a specific financial contribution provided by the local governments for pursuing the goals of the local policy agenda for policing, often associated with the provision of a "special fund" directly handled by the prefects in every province.

RESEARCH HYPOTHESES

The empirical analysis is guided by three main research hypotheses, which refer to our expectations regarding, respectively, 1) the overall effect of security pacts on crime, and 2) on specific types of crime; and 3) the heterogeneous effect of security pacts across provinces of different population size.

To elaborate the first hypothesis we rely on some theoretical consideration from the literature on "governing by contract" (Garland 1996; Gaudin 1999; Lascoumes and Le Galès

2004), applied to the specific features of the Italian institutional and political context. In the 1990s and in the early 2000s', the Italian case was characterized by a weak degree of institutionalization and a low degree of centralization of the model of security governance (Selmini 2005; 2017). Italy was indeed associated with a series of sub-optimal policy outcomes (Author's own 2017; Selmini 2017), including a low tendency to the integration of, and cooperation between, the security actors, and a low degree of responsiveness (namely the ability of the policy to respond to social issues with a pre-emptive policy style), which mostly depended on the high number of non-representative pressure and interests groups, which influenced the policy.

The year 2007, with the adoption of the large-scale policy of security pacts, may be seen as a turning point, as the original model of security governance was questioned. Since some of the actions enforced by the security pacts were actually dedicated to improve the coordination between the actors of security, and to provide a better allocation of funding for crime prevention, this could have led to a reduction of crime rates. This guides us to the first hypothesis:

We expect that the adoption of security pacts brought a reduction in crime rates at the province level (hypothesis 1).

However, we have seen that most of the actions enforced by the security pacts were devoted to an improved control of the territory and neighborhoods, such as the installation of video surveillance systems, the creation of neighborhood police services and the promotion of interventions controlling incivilities. A number of meta-analyses of research evaluating the impact of small-scale interventions such as formal and natural forms of surveillance (Cozens, Saville and Hillier 2005; Welsh and Farrington 2009), and of policing disorder strategies

(Braga, Welsh and Schnell 2015) indicate that these tools are associated with an overall statistically significant – albeit modest in magnitude – crime reduction effect.[11] These systematic reviews also point to the fact that these strategies of crime prevention were more effective in reducing specific types of crime, for instance, property rather than violent crimes. Following these studies, and considering that these activities were prominent in many security pacts, we postulate that the level of efficacy – defined as the capacity or power to produce a desired effect – of security pacts in reducing crime depends on the type of crime itself.

Considering that security pacts were mainly focused on the prevention of predatory crimes and petty crimes at the local level, it is expected that security pacts had a higher level of efficacy in reducing thefts and robbery rates, as well as micro-criminality rather than homicides (hypothesis 2).

The third aspect considered by our evaluation exercise refers to the possible contextual heterogeneity in the effect of security pacts. The conventional approach to policy evaluation focuses on estimating the mean outcomes of programmes. Nonetheless, several scholars underline that implementation processes cannot be defined as "context free" (Maynard-Moody, Musheno and Palumbo 1990). Among others, Berman (1980) argues that due to contextual factors within the implementing environment at the local level (such as the scope of change of the policy, the validity of technology, goal conflict, and institutional setting), policy designers are not unable to control the implementation process. For these reasons, it is likely that public policy interventions are more or less effective in obtaining their chosen

[11] There are many specific causal mechanisms by which these crime prevention actions could reduce crime, but a detailed discussion of them is beyond the scope of this paper. The interested reader could refer, e.g., to the pioneering work of Tilley (1993), about the evaluation of the Safer Cities programme.

outcome according to the pre-existing characteristics of the context on which they intervene and to the characteristics of those who are in charge of implementing the policy. Heterogeneity in the effects of public policies is indeed attracting growing attention, especially in the case of large-scale policies that were implemented in very different contexts at the same time (Heckman, Smith and Clements 1997; Pawson and Tilley 1998; James, Lahti and Hoynes 2006).

In our work, we aim to test whether security pacts were more or less effective in reducing various types of crimes in provinces with different population size. Population size is used here as a general characteristic of the units analysed, which is linked to various contextual conditions that may moderate the effect of security pacts. In particular, provinces with a larger population are likely to have a higher population density and a higher number of medium-large cities. Given the nature of the most widespread interventions promoted within the framework of the security pacts, we may expect that these are the contexts in which the policy might have been more effective. Population size is also related to the baseline level of crime rates at the province level. For instance, in 2006 – the year just before the first pact was signed – the degree of correlation between province population size and various crime indicators amounted to 0.50 in the case of theft rate, 0.64 in the case of robberies and 0.72 for the micro-criminality index. One may expect that the policy has been more effective in reducing crime in those contexts characterized by relatively high criminality, in which there is more room for improvement. Contrary, in contexts with already low crime rates the possibility of further diminishing crime is reduced by *floor effects*.

Following these considerations, we expect that the level of efficacy of security pacts in reducing crime depends on the demographic size of the provinces in which the national policy is adopted. Considering that the most populated regions and provinces tend to be

those with the highest number of large cities and crime rates, thus, it is expected that the magnitude of the effect of security pacts would enlarge as the population size grows (hypothesis 3).

DATA, VARIABLES AND METHODS

Data

In order to assess the effect of security pacts on crime rates, we have built an ad-hoc macro-level dataset, in which Italian provinces (103 cases)[12] are the units of analysis and a number of aggregated indicators are the variables of interest. Each indicator (provinces' attributes) has been repeatedly measured in a time-span ranging from 2004 to 2013 (10 years), in order to cover both a period antecedent and subsequent to the adoption of the policy under analysis.[13]

The outcome indicators and control variables are collected from queries to the database of provinces' indicators provided on the website of the Italian National Institute of Statistics (ISTAT) and on the website of the Department of Public Security of the Italian Ministry of Interior. Additional indicators come from the "Eurostat: Poverty and Social Exclusion Statistics," which are aggregated data from EU statistics on income and living conditions (EU-SILC) survey. Finally, we also relied on data from the "Italian Registry of Local and Regional Administrators," issued by the Department of Internal and Territorial Affairs of the Italian Ministry of Interior.

Our treatment variables are derived from an intensive research of the characteristics of security pacts signed in the period 2007-2009. We focus our analysis on this period since it

[12] See the Appendix for details about our strategy to harmonize the units of analysis over time.

[13] We decided to exclude data previous to 2004 due to an important re-definition of the ways several of our indicators have been collected and reported by the National Institute of Statistics (ISTAT).

includes the vast majority of security pacts and those that are more homogeneous in terms of policy goals. Since each pact could last more than one year, we are able to cover a period of implementation spanning from 2007 to 2012. The analytical sample size involves 919 cases when the first three outcomes (namely, the theft rate, the robbery rate, and the homicide rate, see below for further specification) are analyzed, and 816 when the fourth outcome (namely, the index of micro-criminality, see below for a definition) is investigated (due to missing data for 2012 and 2013 that we decided not to interpolate).

Variables

We relied on four outcome variables, which measure different forms of crime in the period 2004-2013. More precisely, the detected offences included in our four indicators are those reported by the police to the judicial authorities and are available for the provinces with a prefecture.

The first indicator is the *theft rate*, which represents the number of thefts reported per 1,000 inhabitants. The second indicator is the *robbery rate*, which measures the number of robberies reported per 1,000 inhabitants. The third variable is the *homicide rate*: This corresponds to the intentional homicides (which include theft-robbery, mafia-type, and terrorism-related homicides) per 100,000 inhabitants. Finally, the last outcome is the *rate of micro-criminality*, which is computed as the total number of offences related to what the Minister of Interior in Italy calls "micro-criminality." More precisely, this indicator is calculated putting in the numerator the sum of the following crimes that took place in the cities: pick-pocketing, mugging, theft from and of cars; the denominator includes the total number of crimes in the cities. Unfortunately, from 2004 to 2007, ISTAT has released only an aggregated indicator of thefts, so it was not possible to calculate this indicator. For these

years, we have interpolated the missing values of the time-series within each province using a natural cubic spline interpolation (Herriot and Reinsch 1973).[14]

In the multivariate analyses, the four outcomes were introduced with a one-year delay: thus, we measure crime rates the year following the adoption of the security pacts, in order to take into consideration time from the signature of the pacts and their actual implementation.

Our main treatment variable is called *treatment* and is a time-varying dummy variable that assumes value 1 in the combination of province and years affected by a security pact and zero in all the other cases (provinces that never adopted a security pact and years of the *treated* provinces in which the policy was not active).

The main control variables are represented by year-dummies (9) and provinces dummies (102). We have included additional variables in our statistical models, in order to improve the precision of our estimated causal effects. All these variables are time-varying. When information at the province level was not available, we relied on indicators measured at the regional level.

The control variables are: province total population; percentage of foreign resident population on the total province population; percentage of male foreign resident population on the total province population; percentage of unemployed individuals in the province; an indicator measuring the incidence of severe material deprivation[15] (regional level, in %); the

[14] We should be cautious in interpreting the results related to this policy outcome also because summing the absolute values of different crimes might generate an implicit substitution effect.

[15] This indicator measures the share of persons who have living conditions severely constrained by a lack of resources. In particular, they experience at least 4 out of 9 following deprivations items: they cannot afford i) to pay rent or utility bills, ii) keep home adequately warm, iii) face unexpected expenses, v) eat meat, fish or a protein equivalent every second day, v) a week holiday away from home, vi) a car, vii) a washing machine, viii) a color TV, ix) a telephone.

percentage of individuals living in households with very low work intensity[16] (regional level); the percentage of individuals at risk of poverty[17] (time-varying at the regional level).

All these variables were included in the models since they could be related to the adoption of the policy at the province level and, moreover, previous literature suggests that they might be correlated with crime rates. For instance, higher crime rates were found to be related to higher unemployment rates (e.g. Chiricos 1987; Raphael and Winter-Ebmer 2001; Gould, Weinberg and Mustard 2002) and higher poverty levels (e.g. Patterson 1991; Hsieh and Pugh 1993; Ludwig, Duncan and Hirschfield 2001) by a number of studies conducted in different contexts and periods.

Moreover, we have built additional control variables measuring the political orientation of the ruling party or coalition both at the national level and at levels lower than the national government. As for the latter, the first is the political orientation of the ruling party or coalition at the provincial level, and the second is the political orientation of the ruling party or the coalition at the regional level. Both variables are measured using three broad categories: 1) centre-left; 2) centre-right; 3) other (see Appendix).

Methods

The main goal of the empirical analysis is to assess the effect of the security pacts in a condition in which a random allocation of the policy and/or the instrument did not occur. As sketched in the introduction, the Italian authorities were enthusiastic about the reduction of crime in some symbolically medium and large sized cities that adopted the security pacts and

[16] This indicator includes individuals aged 0-59 living in households in where the adults work less than 20% of their total work potential during the last years.

[17] This indicator measures the percentage of the persons in the total population with an equalized disposable income below the risk-of-poverty threshold (which is set at 60% of the national median equalized disposable income).

interpreted this as the effectiveness and/or the efficacy of the policy. Nonetheless, to establish whether governing by contract reached the goal of reducing crime, we need to provide a strict test using appropriate quantitative methods for an ex-post impact evaluation, in terms of policy outcomes (Farrington 2003, Knill and Tosun 2012). The specific method adopted crucially depends on the characteristics of the security pacts and the data availability. In our case, our empirical setting is defined by a policy that affected only a sub-group of Italian provinces and in which the beginning, duration and the end of the security pacts differ across provinces. In this work, we rely on two-way fixed-effects regression model on macro-level panel data, which in our context is equivalent to a generalized difference-in-difference model (Wooldridge 2003; Wing, Simon and Bello-Gomez 2018). By introducing province fixed effects we are able to control for all the time-invariant characteristics of the provinces that may also be correlated to crime rates and to the factors leading to the adoption of security pacts. This allows us to provide an estimate of the causal effect of security pacts that is based on within-provinces temporal variation and not cross-sectional differences, which could have biased our estimates (Angrist and Pischke 2009). Furthermore, including year fixed-effects we are able to account for the trend of crime rates in the whole country, regardless of the adoption of security pacts. We do this including year-dummies, without imposing any arbitrary functional form to the time trends. Also, thanks to the inclusion of additional time-varying control variables, we are able to account for the potential confounding effects of changing contexts. One has to bear in mind that our modeling strategy is more powerful than a standard difference-in-difference model with two points in time and one treatment, because of two reasons. First, our estimated effects take into consideration not only the effect of the adoption of the policy but also what happens to crime rates when the provinces that adopted the security pacts stop to use this policy instrument. Second, given that we observe a pre-treatment period, we are also in the position to test the parallel trends assumption at the basis

of difference-in-difference models, which posits that the average change in the control group represents the counterfactual change in the treatment group if there were no treatment. The estimation was conducted using the command –xtreg– in statistical software Stata 14.1. Standard errors are appropriately estimated considering the non-independence of repeated observations over time (Bertrand, Duflo, and Mullainathan 2004).

The first model we have estimated has the following specification:

$$Y_{pt} = \alpha + \beta_1 \cdot T_{pt} + \sum_{p=1}^{102} \varphi_p \cdot \boldsymbol{PROV} + \sum_{t=1}^{7} \gamma_t \cdot \boldsymbol{YEAR} + \sum_{z=1}^{9} \delta_z \cdot \boldsymbol{Z} + \varepsilon \qquad (1)$$

where *Y* is the outcome variable, *T* is the treatment dummy variable, *PROV* is a set of dummy variables indicating the provinces (province fixed-effects), *YEAR* are the dummies indicating the years (years fixed-effects), Z is a vector of control variables and ε is the error term. The subscript index *p* and *t* denote the level at which each variable varies, indicating respectively the provinces and the years; z indicates the number of control variables included in the models. The parameter of interest is β_1, which represents the average effect of the security pacts on crime rates.[18]

The second set of models has the aim of investigating whether the effect of security pacts was homogeneous or heterogeneous across provinces of different population size. In order to answer this question, we estimated a set of models similar to (1) but adding an interaction term between the dummy variable designing the adoption of the security pacts and the (time-varying) province population size. The model has this form:

[18] It is worth noticing that, since not all municipalities have taken part to the security pacts within the same policy, this parameter might resemble more an estimate of an Intention-To-Treat (ITT) parameter in randomized controlled trials with non-compliers than an Average Treatment on the Treated (ATT).

$$Y_{pt} = \alpha + \beta_1 \cdot T_{pt} + \beta_2 \cdot PopSize_{pt} + \beta_3 \cdot (T_{pt} \times PopSize) + \sum_{p=1}^{102} \varphi_p \cdot \boldsymbol{PROV} +$$

$$+ \sum_{t=1}^{7} \gamma_t \cdot \boldsymbol{YEAR} + \sum_{z=1}^{9} \delta_t \cdot \boldsymbol{Z} + \varepsilon \qquad (2)$$

As suggested by Brambor, Clark and Golder (2006), we report meaningful quantities from the interaction using either tables or graphical representation indicating how the marginal effect of the treatment variable varies according to the values of the interacted variables.

EMPIRICAL RESULTS

Descriptives

We begin the empirical section by describing the main features of the security pacts and the trends of various crime indicators between 2004 and 2013. In the period under scrutiny 35 provinces were affected by security pacts, which corresponds to approximately 34% of the provinces included in our data (103). As depicted in Figure 1, the policy was unevenly distributed across geographical areas, with a much higher incidence in the North West (46%), North East (41%) and Centre (38%) compared to the South (17%) and the Islands (23%). The first security pact was signed in the year 2007 and the last one in 2011. The average duration of security pacts was 697 days, the minimum was 365 days, whereas the maximum length was 1251 days (3.4 years).

Figure 1

Among the provinces that adopted at least one security pact, the average number of participating municipalities was 64, ranging from a minimum of 7 to a maximum of 315.

Among the provinces that adopted security pacts, the average number of citizens affected by the policy amounted to around 506 thousand, the minimum was slightly more than 12 thousand and the maximum exceeded 2.7 millions of people. According to our dataset, security pacts involved more than 3 thousand municipalities and affected nearly 12 million inhabitants.

In Table 1 we see that, in the period under scrutiny, there was an average of 21 thefts over 1,000 inhabitants, with a minimum of 2 and a maximum of 61. The overall mean of robbery rate amounted to 0.43; this rate ranges from 0.03 to 4.59. The average homicide rate amounted to 1 out of 100,000 inhabitants with a quite large variation (from 0 to 83). Finally, the index of micro-criminality rate had an average value across the period and provinces of 5.5, a minimum value of 0.5 and a maximum value of 23. Additional descriptive statistics regarding the time-varying characteristics of the provinces used as control variables in our analyses are provided in Table A1 in the Appendix.

Table 1

Figure 2 reports the overall trends of the four outcome variables in the period from 2004 to 2013. The two dashed grey lines indicate the year of beginning and the end of the adopted security pacts. Looking at thefts, the overall level at the beginning and the end of our series seems quite similar; however, some relevant changes occurred during this period. From 2004 to 2007 the theft rate increased, while in the following two years a steady decrease took place. From 2010, a new, approximately linear, upsurge in thefts occurred. A similar, but less pronounced, trend over time is found when robberies and the index of micro-criminality in

the cities are considered. The rate of intentional homicides instead shows a distinct pattern of slow but constant decline from 2004 and 2012, with a sudden increase in 2013.[19]

Figure 2

In Figure 3 we plot the same statistics but distinguishing between the group provinces that never participated in any security pact and those that, instead, adopted the policy at some point. First of all, the figure suggests that the provinces adopting security pacts had higher baseline levels of thefts, robberies and micro-criminality in the cities, whereas a lower level of homicide rates. Looking at trends in theft rates, we observe that the pre-policy trends between the two groups are very similar; then, after the adoption of the first set of security pacts in 2007, there was a reduction of thefts in both groups but much stronger among the treated provinces.

When considering the second outcome, the trends in the treated and non-treated provinces are similar between 2004 and 2006, while in 2007 a divergence occurred: robbery rate continued to increase in the provinces that afterwards have adopted the policy whereas a reduction occurred in the other provinces. Interestingly, between 2007 and 2008 a decline in the robbery rate occurred but this was more pronounced in the provinces that were involved in the security pacts.

Figure 3

[19] The total number of intentional homicides in Italy in the year 2013 is of 868 compared to 524 in 2012. This increase is largely due to the (controversial) classification as intentional homicides in the Province of Agrigento (which passes from 2 intentional homicides to the total number of 372) of the deaths of 366 African migrants in a shipwreck a quarter-mile away from the coast of Lampedusa, an Italian island barely 70 miles from northern Africa.

The third graph in Figure 3 shows that the declining trends in homicide rates took place in both groups of provinces, but with opposite short-term variations around the year of the first adoption of the security pacts. Finally, the level of micro-criminality in the cities expanded from 2004 to 2007 in both groups of provinces. Afterwards, it reduced only to a small extent among provinces that did not adopt any security pact, while the decrease was more pronounced among the treated provinces.

To sum up, descriptive statistics seem to suggest that a decline in some kinds of crimes occurred around the year of adoption of security pacts. However, the simple examination of trends over time is not enough to provide conclusive evidence on this aspect. We will address such issue more rigorously in the next section.

The outcomes of security pacts

In this section, we discuss empirical evidence about whether the security pacts have been effective in reducing crime in Italian provinces. To address this issue, we estimated a fixed-effects regression model that analyses how changes in crime rates are related to the adoption of the policy in various provinces and years. In this model, we are able to control for all the time-invariant characteristics of the provinces that may also be correlated to crime rates and to the factors leading to the adoption of a security pact. Furthermore, we are able to account for the trend of crime rates in the whole country, regardless of the adoption of security pacts. We do this including year-dummies, without imposing any arbitrary functional form on the time trends. Finally, we are able to take into account a number of time-varying socio-demographic and political features of Italian provinces that may be also linked to crime trends and the adoption of the policy.

Table 2 presents the main results of our analysis. In the columns, the four outcomes of interest are reported: thefts, robberies, homicide rates and the index of micro-criminality in

the cities. On the rows, we report the main effect of the security pacts, using – as the treatment variable – a dummy indicating the provinces and years in which the policy was active. Looking at the results from the first set of models, we see that the security pacts produced a statistically significant reduction of –1.1 in the rate of thefts in the year following their adoption, which corresponds to –1,437 thefts in a year in absolute terms and –5.3% in the thefts rate in relative terms.

Looking at the point estimates we see that the sign of the effect is also in the expected direction (negative) when looking at robberies and micro-criminality rate, but no statistically significant effects are detected. In the first case, the negative effect is very small in substantial terms, while in the second it is larger but with high uncertainty due to a large variation around the estimate. A non-significant effect is also found for homicide rates. It is useful to say that including our time-varying control variables does not alter substantially the estimated effect size, but it is helpful to slightly reduce their standard errors.

Table 2

As a second step, we investigated the existence of heterogeneous effects of the security pacts. As anticipated, while there is a wide array of potential variables that may moderate the outcomes of the policy, we decided to focus attention on one of the most basic variables: the province population size. Indeed, it could be that the pact has been more or less effective in crime prevention in contexts characterized by differential structural characteristics and levels of crime rates, which are related to population size. The results from the interactive dynamic models are presented in Figure 4 in terms of the partial effects of the treatment (participating in the security pacts) along province population size. The shadow grey area indicates the 95% confidence intervals as a measure of the uncertainty around the estimates. If the confidence

interval overlaps the zero line, we do not have enough empirical material to reject the null hypothesis of no effect of security pacts (Gelman and Hill 2007). Thus, the graphs can help in understanding in which contexts the security pacts have been more or less effective in reducing various types of crime rates.

Figure 4

We have seen before that, security pacts did not have any significant effect, on average, on robbery and homicide rates. We can add to this finding that such nonsignificant effect is observed across the whole range of population sizes, without any notable distinction. On the other hand, the first and the last graphs show a different pattern. The policy was not effective in provinces of small dimensions while its effect increases as the population size grow. More precisely, the security pacts did not have any effect on the theft rate in provinces below 500 thousand inhabitants, whereas their effect was around –2.5% (95% CI: –3.3; –1.6) in provinces of around 1,650,000 residents to arrive at –5.5% (95% CI: –7.6; –3.4) in provinces that exceed 3.5 million inhabitants.

ROBUSTNESS CHECKS

At the end, in line with widespread practice in the field of policy evaluation, we have performed a number of robustness checks to provide indirect evidence on whether the assumptions at the basis of the generalized difference in difference approach could be satisfied in our context (Wing, Simon and Bello-Gomez 2018), and to what extent our findings depend on specific methodological choices (Muñoz and Young 2017; Young and

Holsteen 2017). The results of the robustness checks that we will discuss briefly in this section are reported in the Appendix.

First of all, we have investigated the existence of a common pre-trend in the outcome variables as an indirect way to assess the plausibility of the parallel trends assumption underlying the difference in difference strategy (Kahn-Lang and Lang 2018). The results suggest that after adjusting for socio-demographic, political and economic characteristics of provinces, the trends in the crime rates before the beginning of the policy follow a very similar pattern in both treated and non-treated provinces.[20] Additionally, we have estimated a linear probability model to predict the adoption of security pacts as a function of the level of crime rates and other province characteristics in the year before the first adoption of the security pact (2006). As shown in figure A3, we did not find evidence of selection into the treatment on the basis of the observed variables. Analysing selection into treatment in a dynamic way by using population average models (Rabe-Hesketh and Skrondal 2008) leads to the same reassuring conclusion (see figure A4).

Second, we have conducted a number of robustness checks to assess how much our findings change if we use different estimation methods and model specifications. We have estimated the same two-way fixed effect models but also including province-specific linear trends, as suggested by recent reviews on the difference in difference strategy (Wing, Simon and Bello-Gomez 2018). Table A3 indicates that the results are pretty much in line with those reported in the manuscript and the estimated effect of the security pacts on thefts rate is even slightly larger in this 'stricter' specification.

We also used a different estimation technique compared to the standard linear fixed effect estimator. More specifically, we applied fractional response model with province and year

[20] See figure A2 for an example on the thefts rate, the only one in which we find the main effect of security pacts. Additional tests in line with Angrist and Pitschke (2009, 238-239) suggests that the assumption of no anticipatory effects cannot be rejected. This provides additional evidence for the robustness of our finding.

fixed effects to properly account for the fact that our outcome variables are percentages/proportion and therefore bounded in a specific interval (Papke and Wooldridge 1996; 2008). Figure A5 shows that the results are qualitatively very similar to those obtained with the linear fixed effects specification. The only exception refers to the effect of security pacts on the micro-criminality rate, which is now statistically significant at the 95% confidence level, even if substantially small.

Finally, we applied computational multimodel analysis (Muñoz and Young 2017; Young and Holsteen 2017), a recently developed method to assess in a systematic way to what extent our results are dependent on specific choices in model specification. More specifically, we run 2,916 models with all the possible combinations of the available control variables (different sets of variables), and by allowing for both linear and quadratic effects of the quantitative control variables. The results of this exercise show that our estimate of the effect of security pacts on theft rates is very robust (see figure A6): i) in all the model specifications we detected a consistently negative effect (100% sign stability); ii) in all the model specifications the estimated effects are statistically significant at the 95% confidence level (100% significance stability); iii) our preferred estimate is conservative compared to the estimated effects under alternative model specifications, which provide larger estimated effects.

CONCLUSIONS

The aim of this work was to provide empirical evidence about the policy outcomes of a large-scale security policy adopted in Italy, at the end of the 2000s, to prevent and control crime and disorder at the local level.

The policy consisted in the adoption of the so-called security pacts, a form of contract in which the public actors involved declare to approve a project or a line of action, or taking mutual commitments, making their resources (not necessarily financial resources) available for common action, agreeing how and when to act. Through the signing of security pacts in Italy, local authorities, on a different scale of government (*Regioni*, *Province* and *Comuni*), have been attributed competences in the field of crime prevention, aside agencies of formal control (e.g. *Polizia di Stato*, *Arma dei Carabinieri*, and *Guardia di Finanza*). Thus, security pacts not only were expected by the policy designer at the central level of government to be a new organizational and operational model in preventing crime, but also an instrument able to generate, with specific actions (e.g. creating new forms of formal and natural surveillance, and adopting neighborhood police services), a direct and almost immediate effect on crime rates.

While the Italian authorities made enthusiastic declarations both on the efficacy and the effectiveness of governing by contract in the year immediately after the adoption of the policy, such confidence was not based on any strong empirical evidence and/or any evidence-based evaluation effort, but rather on basic descriptive statistics from *ad hoc* medium and large-sized cities. So far, also due to the absence of better available data a rigorous and comprehensive evaluation of the outcomes of the security pacts in Italy – but also in other European countries in which governing by contract has been developed – is lacking. It would be then interesting to see studies on the effectiveness of security pacts and crime-related large-scale policies in other European countries, in order to promote evidence-based policy evaluation at various levels, from micro-level interventions to large-scale policies.

This work was a first attempt to provide empirical evidence to establish whether and to what extent governing by contract, through security pacts, reached the goal of reducing crime. To pursue this aim: i) we conducted an intensive search on the characteristics of

various security pacts adopted in the period 2007-2009 (the period that includes the vast majority of security pacts and those that are more homogeneous in terms of policy goals), since these pieces of information were fragmented and/or not available; ii) we built an *ad hoc* dataset in which the Italian provinces (103) were observed in a ten year age-span (2004-2013). Our analytical strategy based on generalized difference-in-difference models was able to take into account that the policy was adopted in different years across provinces, leaving out all time-invariant differences across provinces, the overall trends in various crime rates at the national level, and also a number of time-varying socio-economic and demographic characteristics of our units of analysis. In doing so, we should have limited the sources of unobserved heterogeneity that could introduce bias in our estimates of the causal effects of the security pacts, in terms of policy outcomes.

The results of our analyses supported to some extent the three proposed research hypotheses. Our first hypothesis was that the adoption of security pacts brought a reduction in crime rates at the provincial level, due to fact that the original model of security governance (characterized by a weak degree of institutionalization and a low degree of centralization) was questioned. Our first hypothesis is only partially corroborated by the data, since we have not found a generalized reduction of all types of crime produced by the adoption of the security pacts. Differently, security pacts appear to have been effective only on some types of crime and in specific contexts.

Our second hypothesis was that – given the nature of the interventions promoted within the framework of security pacts – the policy should have been able to reduce only specific types of crime, in particular, predatory crimes and petty crimes. Our hypothesis was partially supported since we found out that security pacts significantly reduced thefts, but not other crimes such as homicides. However, contrary to our expectations, the trends in robbery rates seem not to be affected by the pacts.

Our analysis also pointed out the importance of considering potential heterogeneous effects when looking at the outcomes of such a large-scale policy. In our case, in line with the expectations of the third hypothesis, we found out that security pacts were more effective in reducing both thefts and micro-criminality in the provinces with a larger population size. We argued that this could be explained by the fact that more densely populated areas and those with a higher number of cities were those with also higher baseline crime rates before the intervention. It could be that security pacts worked particularly well in urban contexts characterized by severe problems of crime and disorder, in which a set of interventions such as those promoted by the policy have more room to be effective. Therefore, our strategy – while looking only indirectly at the mechanisms that can make a policy intervention to be more or less effective – could be regarded as a first attempt to focus attention on the issue of "why" an initiative might work (Pawson and Tilley 1998).

As a conclusion, it is important to stress limitations of the current study and venues for future directions in this area of research. By relying on the provinces as a unit of analysis – coherently with the policy design – we took into account an important institutional actor in the signing of security pacts and for their implementation. However, not all the municipalities – as another fundamental institutional level involved in the signing and implementation of security pacts at the local level – in the treated provinces were directly affected by the pacts. The use of the data at the municipality level represents a more fine-grained level of detail that would have allowed us a more precise definition of our treatment variable; and, as a consequence, of the estimated size of the policy outcomes. Unfortunately, detailed data on crime rates at the municipality level are not available in Italy at the moment.

A second limitation regards the outcome variables. While the outcomes we relied on are important for the policy under scrutiny, other could be of interest as well. In particular, security pacts were not only intended to reduce crime but also incivilities and citizens' fear of

crime. Unfortunately, at the moment indicators measuring these phenomena are not available at the level necessary to be included in our analytical strategy. Future studies should address this issue when additional data will become available. Moreover, it was not possible to rely on outcomes that did not come from official statistics, because information from victimization surveys (Hope 2017) is not available at a detailed geographical level and for all the relevant years before and after the security pacts.

Finally, it is worth noting that we have analyzed the effectiveness of a complex policy, which involved a package of interventions, varying across contexts. This means that we are not able to disentangle the effects of specific elements of the policy (e.g. the different security pacts and/or a specific action inside a pact, and/or the failure/success of the implementation of the actions contained in the pacts). While it is quite common for the intervention literature to analyze the effects of broad reforms (e.g. Rodriguez-Planas 2012, in the educational field), future studies might be interested in understanding the effectiveness of single measures from such multifaceted interventions. Moreover, it could also be that the effect of the policy differs across the time points and provinces (Strezhnev 2017); future studies could pursue this direction, in order to provide a more detailed picture on the heterogeneity in the outcomes and/or impacts of the policy.

Despite such limitations, we offered an example of how it is possible to assess the effect of large-scale policies, in a "most-likely" case, since governing by contract was explicitly adopted in Italy as a "good solution" to change a previous model of governance of security issues associated with sub-optimal policy outcomes. Two main lessons can be drawn from our analysis of the Italian experience, in terms of general implications when the central government uses security pacts – among a variety of possible laws and measures – to prevent and control crime and disorder at the local level.

First, differently from what the democratic governments are frequently promising, it looks that governing by contracts is by no means a "silver bullet" policy capable of solving in one fell swoop the challenges of security, and ultimately capable of reducing crime under any condition. Instead, heterogeneity in the outcome of public policies should be expected, especially in the case of large-scale policies simultaneously adopted and eventually implemented in very different contexts at the same time. As a matter of fact, our research found a remarkable variance in the capacity of governing by contract to reduce crime at the local level: e.g., security pacts were more effective in larger provinces and much less effective in less densely populated ones.

As a second lesson, related to the first one, it could be argued that our quasi-experimental research design demonstrated that the adoption and implementation processes and their outcomes cannot be defined as "context-free". As a consequence, security pacts should not be adopted in any context at the same time to avoid they boil down easily to simply "symbolic statements" among actors involved in the governance of security. We suggest that, in the future, security policies should move away from a general adoption and implementation model (namely, applicable to any context), without first passing through a pilot project phase and a subsequent robust monitoring and evidence-based evaluation of its outcomes.

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Figures

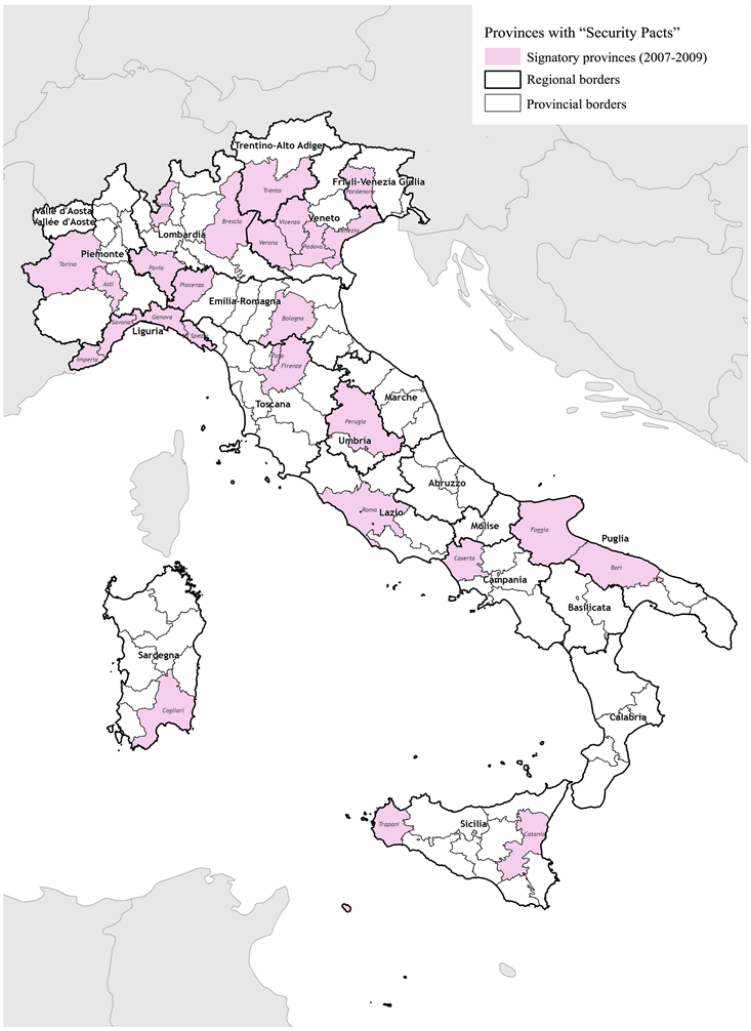

Figure 1 - Provinces that adopted security pacts (2007-2009).

Source: Own data processing. Image: ArcGis.

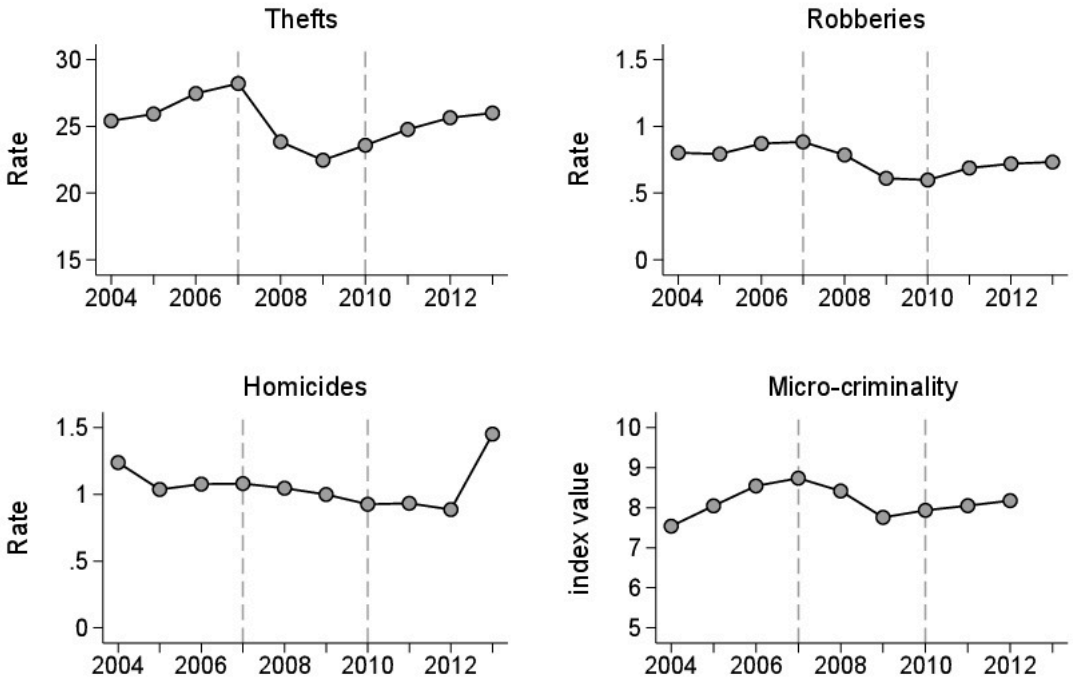

Figure 2 - Crime incidence in Italy over time (2004-2013).

Note: the estimates are weighted for each time-varying province population size

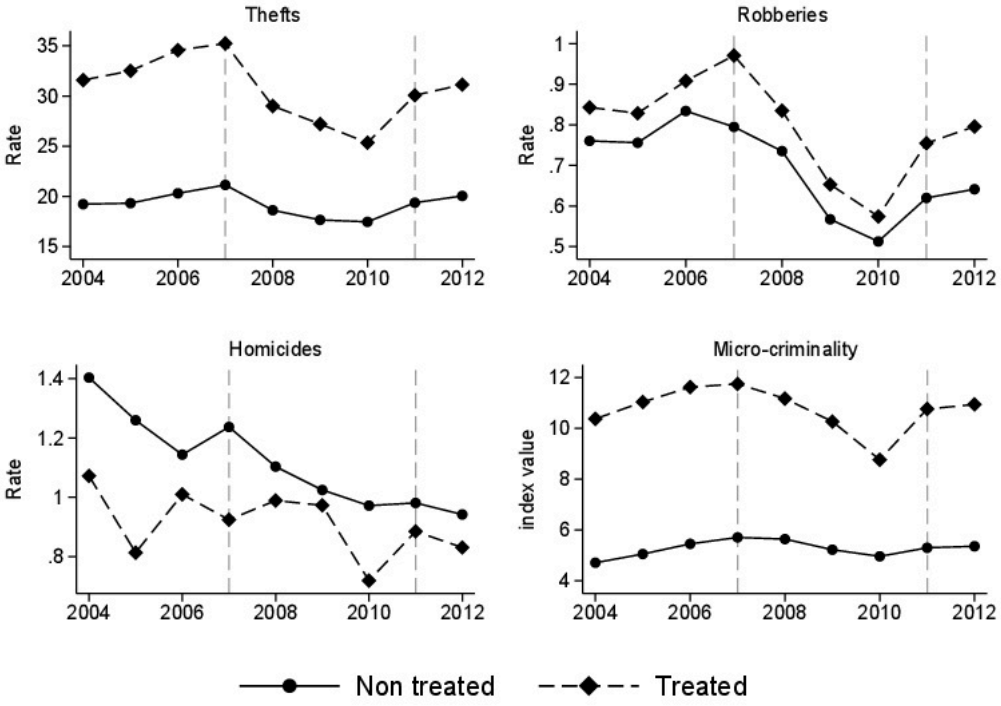

Figure 3 - Crime incidence in the provinces that adopted and not adopted the security pacts in Italy (2004-2013).

Note: the estimates are weighted for each time-varying province population size

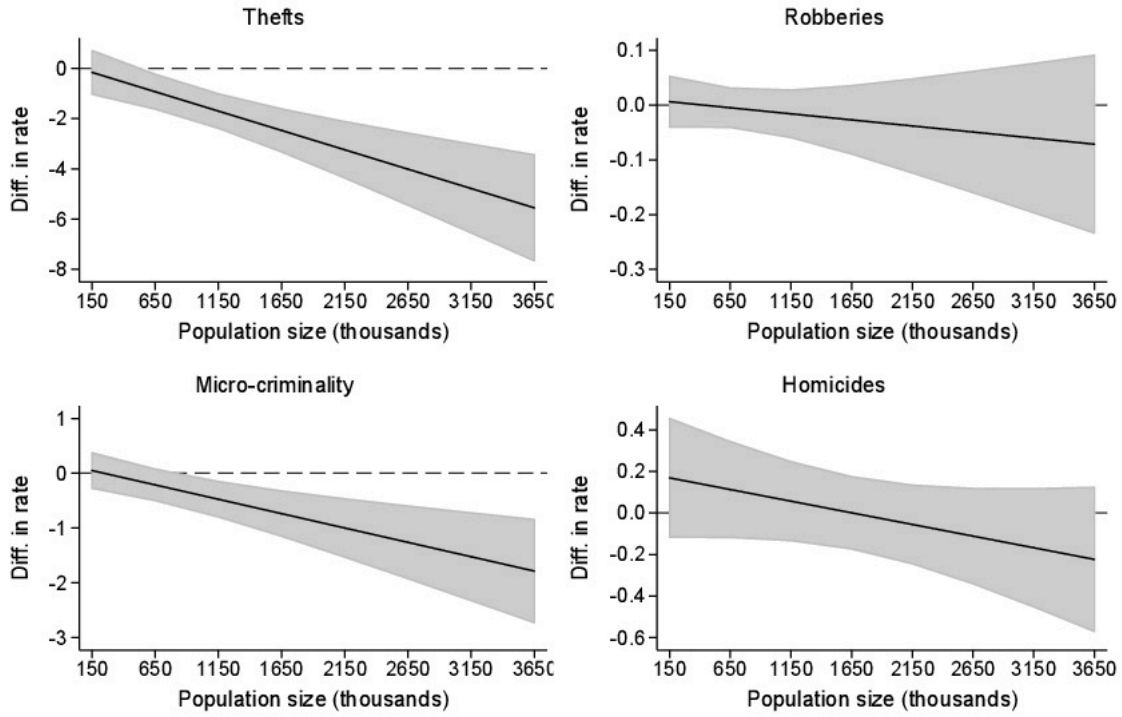

Figure 4 - Impact of security pacts on four crime indicators according to province population size in Italy.

Tables

Table 1 – Descriptive statistics of the outcome variables

Variable		Mean	Std. Dev.	Min	Max	Observations
Thefts	Overall	20.54	8.82	2.30	60.70	N = 1030
	Between		8.54	6.84	50.57	n = 103
	Within		2.34	6.67	34.61	T = 10
Robberies	Overall	0.43	0.44	0.00	4.60	N = 1030
	Between		0.42	0.08	3.34	n = 103
	Within		0.14	-0.61	1.69	T = 10
Homicides	Overall	1.05	2.78	0.00	83.10	N = 1030
	Between		1.24	0.18	9.61	n = 103
	Within		2.50	-8.16	74.54	T = 10
Micro−criminality	Overall	5.44	3.50	0.50	22.70	N = 816
	Between		3.45	1.13	20.43	n = 103
	Within		0.80	1.96	9.02	T = 8

Table 2 - Treatment effects of security pacts on crime rates one and two years after the adoption of the policy: partial effects, standard errors and statistical significance level (* p<0.05; ** p<0.01; *** p<0.001).

Note: N=1,030, with the exception of the model on the index of micro−criminality where N=816.

	One year after treatment		Two years after treatment	
Outcome	Effect	Standard Error	Effect	Standard Error
Thefts	−1.094**	0.379	−1.052**	0.381
Robberies	−0.006	0.020	−0.041*	0.018
Homicides	0.080	0.112	0.085	0.129
Micro−Criminality	−0.265	0.156	−0.370*	0.176

APPENDIX

Definition of the provinces over time

In the years covered by the study, some exceptional re-arrangements in the definition of Italian provinces occurred. In few cases new provinces were introduced, in other cases some municipalities have changed their province of affiliation, joining a new-born or a pre-existing province. Our empirical strategy needs to rely on the same set of provinces over time. To achieve this goal and – at the same time – dealing with the changes in some administrative units, we adopted the following adjustments. For the years in which new provinces were observed (usually 2010-2013), we computed a revised version of each indicator using the following expression:

$$RevI_{oldprov} = \frac{(I_{oldprov} \times P_{oldprov}) + (I_{newprov} \times P_{newprov})}{(P_{oldprov} \times P_{newprov})}$$

The revised indicators (*RevI*), thus, are computed as the average of the values from the old province/s ($I_{oldprov}$) and the new province/s ($I_{newprov}$) weighted for the respective population size (P). When a new province took municipalities from two "old" provinces, we have attributed the values of its correspondent indicator to both old provinces in order to reflect the number of municipalities that have changed the province of affiliation.

Additional tables and figures

Table A1 - Descriptive statistics of time-varying control variables.

Variable		Mean	Std. Dev.	Min	Max
Population size	overall	563936	617957	87057	4180529
	between		628313	88112	3917376
	within		20073	405282	827090
Proportion of migrants	overall	6.02	3.38	0.58	15.93
	between		3.10	1.08	12.32
	within		1.37	1.66	9.63
Proportion of male migrants	overall	2.92	1.70	0.24	7.826725
	between		1.60	0.41	6.306808
	within		0.59	0.92	4.579785
Unemployment rate	overall	8.42	4.69	1.90	26.8
	between		4.15	3.03	18.13
	within		2.22	2.48	18.97932
Material deprivation	overall	8.24	6.76	0.50	35.9
	between		5.83	2.22	21.32
	within		3.47	2.04	25.42505
Low work intensity	overall	18.39	11.02	5.00	44.6
	between		10.97	7.94	40.49
	within		1.46	13.86	24.16806
Social exclusion	overall	10.27	5.05	2.80	25.1
	between		4.85	4.28	19.82
	within		1.49	6.52	17.31854
Region: Center-Left	overall	0.515	0.500	0.000	1.000
	between		0.399	0.000	1.000
	within		0.304	-0.385	1.415
Region: Center-Right	overall	0.456	0.498	0.000	1.000
	between		0.397	0.000	1.000
	within		0.304	-0.444	1.356
Region: Other	overall	0.029	0.168	0.000	1.000
	between		0.169	0.000	1.000
	within		0.000	0.029	0.029

Province: Center-Left	overall	0.547	0.498	0.000	1.000
	between		0.393	0.000	1.000
	within		0.308	-0.353	1.347
Province: Center-Right	overall	0.364	0.481	0.000	1.000
	between		0.368	0.000	1.000
	within		0.313	-0.536	1.264
Province: Other	overall	0.089	0.285	0.000	1.000
	between		0.178	0.000	1.000
	within		0.224	-0.611	0.989

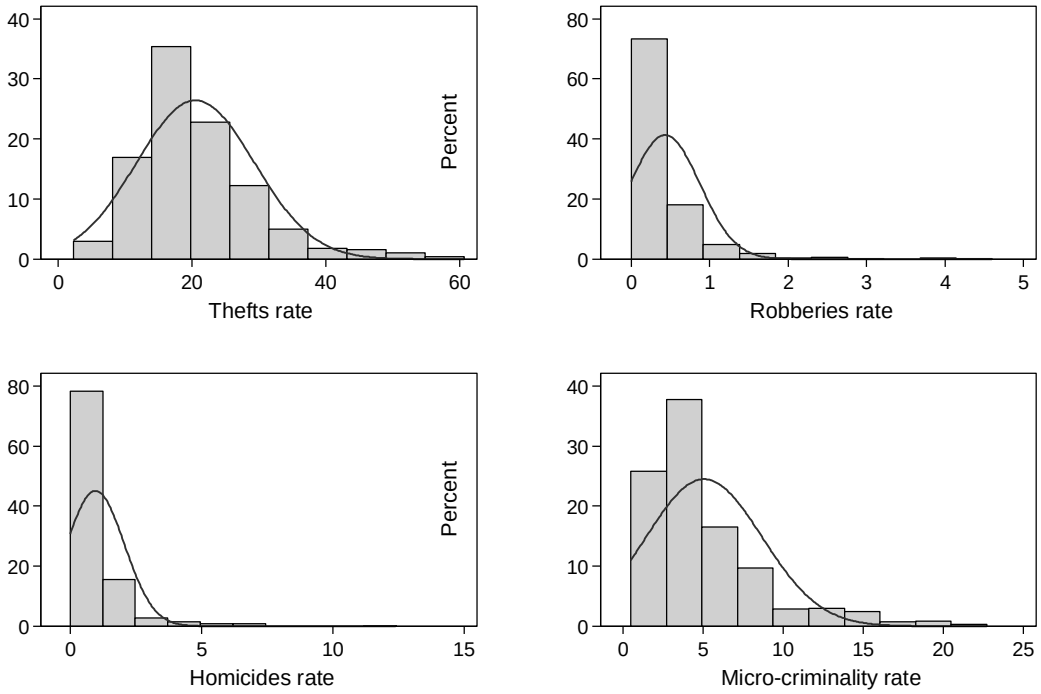

Figure A1 - Histogram of the distribution of the four outcome variables and added normal density.

Note: outliers excluded from the distribution of the homicides rate.

Full models with different specifications

Table A2 - Generalized difference-in-difference model with stepwise inclusion of covariates: regression coefficients, standard errors in parentheses and level of statistical significance (* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

	Thefts			Robberies			Homicides			Micro−Criminality		
	M1	M2	M3	M1	M2	M3	M1	M2	M3	M1	M2	M3
Security pact adoption	−1.116**	−1.084**	−1.094**	0.000	−0.005	−0.006	0.179	0.114	0.080	−0.236	−0.267	−0.265
	(0.371)	(0.375)	(0.379)	(0.020)	(0.018)	(0.020)	(0.159)	(0.112)	(0.112)	(0.161)	(0.161)	(0.156)
Year (Ref.=2004)												
2005	1.013***	1.127***	1.030***	0.028*	0.013	0.012	−0.032	−0.017	−0.239	0.504***	0.434***	0.556***
	(0.173)	(0.191)	(0.273)	(0.011)	(0.013)	(0.014)	(0.085)	(0.102)	(0.186)	(0.056)	(0.063)	(0.084)
2006	1.942***	2.060***	1.951***	0.056***	0.023	0.017	−0.076	−0.055	−0.315	0.816***	0.660***	0.824***
	(0.207)	(0.253)	(0.282)	(0.013)	(0.025)	(0.024)	(0.078)	(0.131)	(0.178)	(0.106)	(0.119)	(0.123)
2007	−1.076***	−0.788*	−0.943*	0.006	−0.059	−0.064	0.045	0.094	−0.428	0.789***	0.491**	0.750***
	(0.291)	(0.343)	(0.380)	(0.015)	(0.045)	(0.040)	(0.113)	(0.218)	(0.353)	(0.135)	(0.182)	(0.171)
2008	−1.949***	−1.485**	−1.659***	−0.080***	−0.172*	−0.178**	−0.149	−0.076	−0.623	0.376*	−0.046	0.161
	(0.309)	(0.441)	(0.454)	(0.022)	(0.068)	(0.063)	(0.092)	(0.313)	(0.331)	(0.160)	(0.243)	(0.229)
2009	−1.760***	−1.178*	−1.519**	−0.096**	−0.208*	−0.205*	−0.080	0.060	−0.479	0.260	−0.259	−0.124
	(0.453)	(0.547)	(0.570)	(0.029)	(0.087)	(0.081)	(0.098)	(0.428)	(0.295)	(0.174)	(0.300)	(0.286)
2010	−0.274	0.400	0.079	−0.015	−0.149	−0.149	−0.067	0.159	−0.342	0.440**	−0.185	−0.172
	(0.350)	(0.609)	(0.651)	(0.026)	(0.096)	(0.090)	(0.101)	(0.594)	(0.302)	(0.154)	(0.330)	(0.320)
2011	0.345	0.894	0.366	0.010	−0.103	−0.123	−0.143	0.379	−1.055	0.437**	−0.089	−0.143
	(0.331)	(0.581)	(0.715)	(0.025)	(0.088)	(0.093)	(0.088)	(0.862)	(0.704)	(0.152)	(0.302)	(0.364)
2012	0.611	1.265*	0.690	0.005	−0.125	−0.161	0.623	1.109	−0.507	.	.	.
	(0.325)	(0.625)	(0.834)	(0.024)	(0.097)	(0.111)	(0.794)	(1.659)	(0.349)	.	.	.
Population size		−0.000**	−0.000**		−0.000	−0.000		−0.000	−0.000		−0.000	−0.000*
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Proportion of migrants		0.020	0.257		0.086	0.098		−1.251	−0.245		0.414	0.458
		(0.661)	(0.781)		(0.067)	(0.075)		(1.616)	(0.598)		(0.337)	(0.370)
Proportion of male migrants		−0.352	−0.766		−0.100	−0.123		2.709	0.656		−0.491	−0.619
		(1.326)	(1.599)		(0.107)	(0.129)		(3.279)	(1.194)		(0.643)	(0.711)
Unemployment rate			−0.014			−0.004			−0.088			0.042
			(0.066)			(0.003)			(0.098)			(0.026)
Material deprivation			0.027			0.007			0.191			0.011
			(0.034)			(0.004)			(0.186)			(0.023)
Social exclusion			0.049			−0.009			0.021			−0.026
			(0.060)			(0.006)			(0.045)			(0.026)

Low work intensity			0.027			0.009*			−0.090			0.087**
			(0.068)			(0.004)			(0.077)			(0.027)
Region political orientation (Ref.: Center−Left)												
Center−Right			−0.374			−0.042*			−0.106			−0.034
			(0.336)			(0.020)			(0.136)			(0.142)
Province political orientation (Ref.: Center−Left)												
Center−Right			0.902*			0.004			−0.364			0.542**
			(0.390)			(0.041)			(0.335)			(0.164)
Other			0.670			0.039			−0.819			0.461
			(0.666)			(0.053)			(0.791)			(0.347)
Province fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	20.734***	32.427***	31.799***	0.439***	0.759**	0.785**	1.010***	1.946*	2.537*	5.004***	6.611***	5.725***
	(0.229)	(3.725)	(3.901)	(0.015)	(0.231)	(0.238)	(0.097)	(0.773)	(1.041)	(0.111)	(1.222)	(1.222)
N	919	919	919	919	919	919	919	919	919	816	816	816
R^2*(within)*	0.322	0.346	0.358	0.117	0.148	0.183	0.007	0.011	0.036	0.105	0.122	0.167

Plausibility of the parallel trends assumption

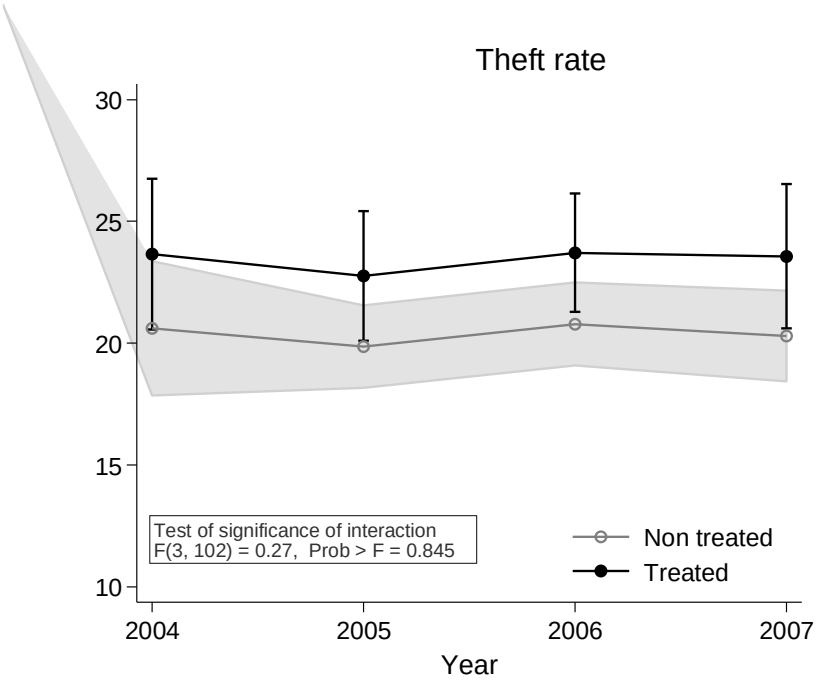

Fig A2 - Test of parallel trend assumption: OLS linear regression to compare adjusted predicted trends in the outcome variable (thefts rate) across treated and non-treated provinces before the beginning of the policy.

Note: the trends show the same pattern for the other three outcomes.

Checks for selection into the treatment

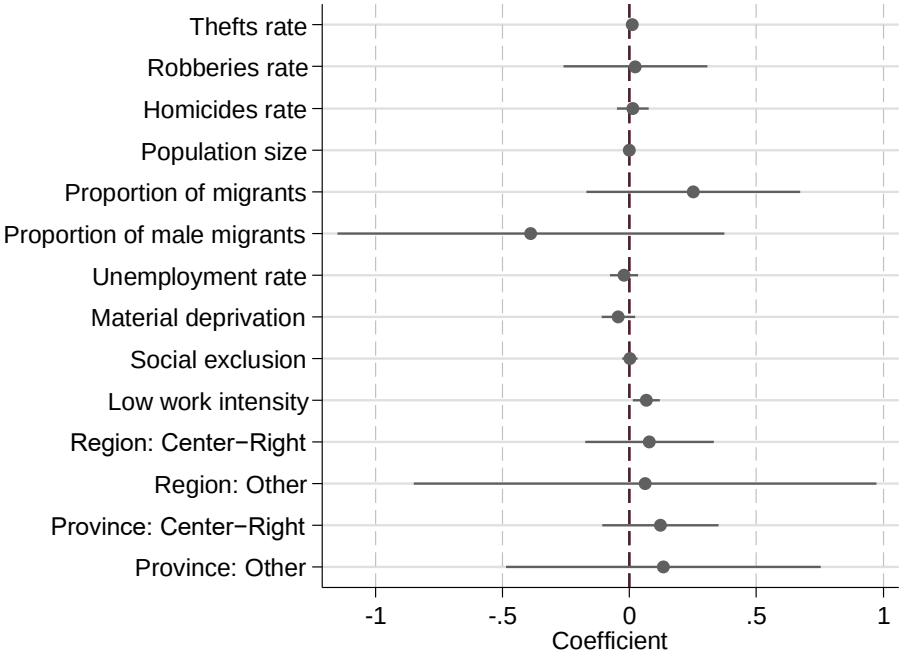

Fig A3 – Linear probability model to predict the adoption of security pacts as a function of covariates measured in 2006: regression coefficients and 95% confidence intervals.

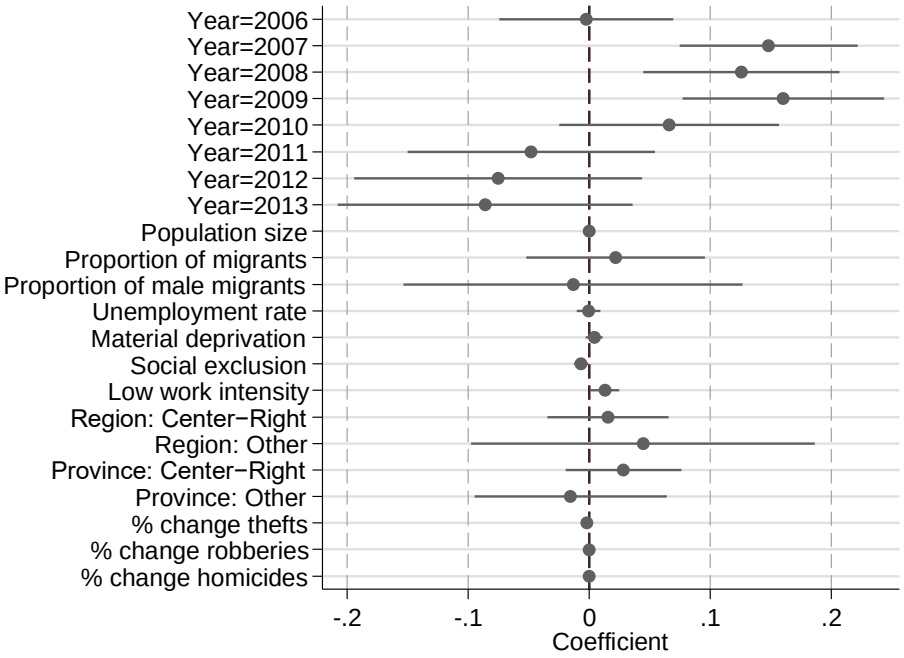

Fig A4 - Population average linear probability model to predict dynamically the adoption of security pacts: regression coefficients and 95% confidence intervals.

Alternative modelling strategies

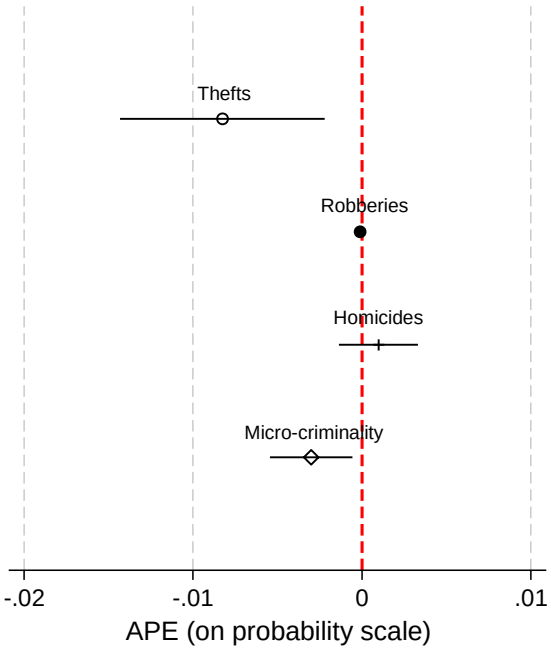

Figure A5 - Fractional regression model with fixed effects: average partial effects on the four outcomes (probability scale 0-1) and 95% confidence intervals.

Table A3 - Fixed effects model with added province-specific linear trends: Treatment effects of security pacts on crime rates: partial effects, standard errors and statistical significance level (* p<0.05; ** p<0.01; *** p<0.001).

Outcome	Effect on crime rate	Standard Error
Thefts	−1.168**	0.421
Robberies	−0.008	0.019
Homicides	0.088	0.135
Micro-Criminality	−0.239	0.168

Model robustness

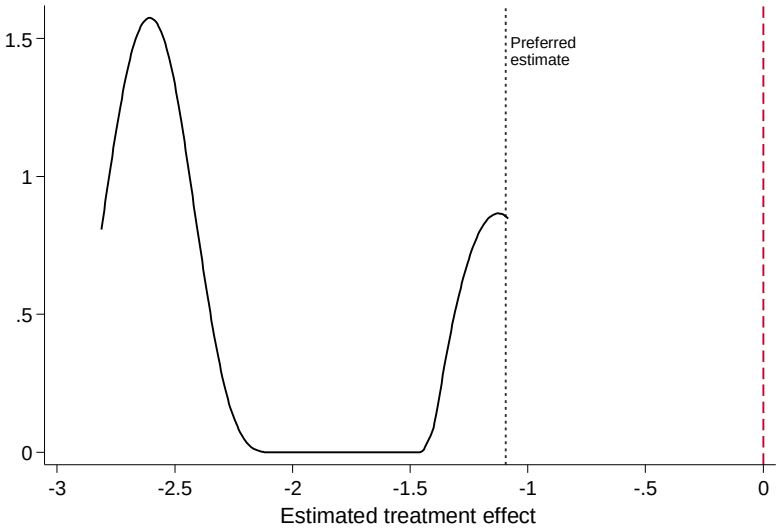

Fig A6 - Modelling distribution of the effect of the adoption of security pacts on thefts rate: Kernel density graph of estimates from 2,916 models with different combinations of control variables (different sets of variables and functional forms).

Note: Vertical short-dashed line indicates the preferred estimate of −1.094 as reported in Table 2. More information on the procedure followed to perform the analysis is provided in Young and Holsteen (2017).