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Magdalena Domínguez, Daniel Montolio

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Postal Address:

Institut d'Economia de Barcelona

Facultat d'Economia i Empresa

Universitat de Barcelona

C/ John M. Keynes, 1-11

(08034) Barcelona, Spain

Tel.: + 34 93 403 46 46

ieb@ub.edu

<http://www.ieb.ub.edu>

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BOLSTERING COMMUNITY TIES AS A MEANS OF REDUCING CRIME *

Magdalena Domínguez, Daniel Montolio

ABSTRACT: Recent evidence indicates that alternative policies based in the community reduce crime, especially in disadvantaged neighborhoods. In this paper we study the effects of bolstering community ties on local crime rates. To do so, we take advantage of the quasi-random deployment of a community health policy. The program Barcelona Salut als Barris (BSaB) aims to improve health outcomes and reduce inequalities in the most disadvantaged neighborhoods through community-based initiatives and empowerment of citizens. To test if BSaB reduces crime, we follow a staggered Difference-in-Differences approach. Data comes from administrative records of the Local Police and the City Hall. We find that BSaB significantly reduces intimate crimes but only in the short term. We also find an effect on drug crimes in the long term. Regarding offenders, a reduction for young individuals takes place. Evidence is suggestive that is due to tighter knit communities. These results provide evidence in favor of non-traditional crime preventive policies.

JEL Codes: C23, I18, J18, K42

Keywords: Crime, community ties, Difference-in-Differences, public policy

Magdalena Domínguez
Universitat de Barcelona &
Institut d'Economia de Barcelona (IEB)
E-mail: m.dominguez@ub.edu

Daniel Montolio
Universitat de Barcelona &
Institut d'Economia de Barcelona (IEB)
E-mail: montolio@ub.edu

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1 Introduction

Urban economics has studied innumerable aspects of differences between and within cities, among which growth and inequality have occupied the most attention. However, as already pointed out by Glaeser et al. (1996) and Glaeser and Sacerdote (1999), the proliferation of contrasts related to crime is also striking and their findings are of particular relevance to both individual and overall welfare. The recent literature suggests that individual choices concerning participation in crime may be significantly affected by existing norms and networks (Glaeser et al. 1996; Patacchini and Zenou 2009).

Regarding the relationship between crime and social networks, recent work emphasizes that more tightly knit social networks can raise aggregate crime levels due to the sharing of know-how among criminals (Calvó-Armengol and Zenou 2004) or imitation of peer behavior (Glaeser et al. 1996; Calvó-Armengol et al. 2009). However, they also increase the opportunity cost of committing a crime. Such a possibility is closely related to the concept of social capital, defined by Guiso et al. (2011) as a set of values and beliefs that help cooperation within a community. Indeed, Coleman (1988) already related the strength of social sanction to social network closure. Additionally, systemic models of community organization are built on the notion that well-developed local network structures reduce crime (Flaherty and Brown 2010). This is related to the fact that networks may increase returns on non-criminal activities and raise detection probabilities. On this matter, community-based interventions and initiatives can place a crucial role, particularly in deprived areas.

This research deals with the impact of community ties on crime in an urban context; a line of research that is extremely relevant to the field of economics of crime. Our ultimate goal is to understand better the empirical determinants of criminal activity, how social networks deter or encourage them, and how they interact with socioeconomic factors. Concretely, in this paper we argue that initiatives that bolster community ties in disadvantaged neighborhoods can succeed at reducing local crime rates, especially for crimes that are not driven by a monetary incentive. We test this hypothesis by analyzing a community health policy that was implemented in a quasi-random fashion in the city of Barcelona. *Barcelona Salut als Barris* (BSaB) literally meaning ‘health in the neighborhoods’ was deployed in some of the most disadvantaged neighborhoods of the city with the aim of reducing local disparities. It was ran by the local health center with the participation of local social agents and the community itself. To analyze it, we apply a staggered Difference-in-Differences methodology combined with a battery of socioeconomic controls and time and space fixed effects. Regarding data, we use a unique geocoded criminal offense data-set which is enriched with Barcelona City Hall sociodemographic controls. To the best of our knowledge, no published study has adopted such an approach, with similarly rich data and obtaining causal effects at such small level

of aggregation.

Our estimates suggest that the observed reduction in criminal actions can be attributed to the implementation of BSaB. Specifically, we find that the offense rates for young individuals drop in those neighborhoods that benefit from BSaB. The policy also reduces crimes against the person and those where there is a very close personal link between perpetrator and victim, which we label as intimate crimes. The reduction is close to 25% and only occurs in the short term. We also find there is a reduction in drug crimes but only one year after the policy is implemented. Finally, evidence is suggestive that results are not due to health or unemployment improvements in the participating neighborhoods. Rather, it indicates that the effects are linked to a more robust social fabric. This is supported by an increase in the number of per capita associations.

The novelties of this research reside in many factors. Firstly, the deployment of the policy provides us with an exogenous variation in the drivers of community ties at a very small geographical level, which allows us to determine causal links. Secondly, we make use of a geocoded and highly detailed database that includes data on registered victims, offenders and types of crime. This also adds to the accuracy of our analysis, as we can analyze whether there are differential effects according to crime types, and the demographic characteristics of those involved. Finally, this work contributes to research conducted outside the United States and considers a city in which the residents are heterogeneous in terms of economic and demographic characteristics. Together, these features constitute the external validity of our exercise. Our findings will contribute to academic research and will offer specific guidance for policy-making to deter criminal activity, moving beyond traditional approaches. Furthermore, this case study will benefit other cities, given that the policy recommendations that emerge are applicable to similar urban settings.

The rest of the paper is organized as follows. In Section 2 we analyze the link between community capital and crime. Section 3 describes the institutional framework of the initiative we analyze. Then in Section 4 we present the data we use, and we define our main variables. Section 5 lays out the methodology we follow as well as our empirical model. Then after presenting our main results in Section 6, in Section 7 we offer our conclusions and policy recommendations.

2 Brief review on community ties and public interventions

Crime and social interactions have been extensively studied in economics. In their seminal paper on the subject, Glaeser et al. (1996) (and also Glaeser et al. 2002) detected a large number of social interactions in criminal behavior. These authors present a model in

which social interactions explain the high cross-city variation in crime rates in the US. Additionally, their model provides an index of social interactions, namely the proportion of potential criminals who respond to social influences. The index suggests that the number of social interactions is highest in petty crimes, moderate in more serious crimes, and almost negligible in murder and rape.

Crime economics is a field in which there is room for interdisciplinary contributions. In such framework there are different approaches towards crime prevention, and measures to fight crime can largely be split in either hard or soft policies. While the first one advocates for heavy policing and sturdy prosecuting measures, the second one focuses on reducing crime-triggering disparities. In that respect, contributions to the literature have shown that in many circumstances “tough-on-crime” measures can lead to a worsening of the initial situation and imposing a high cost to society, both in monetary and welfare terms. As an alternative, innovative strategies to prevent crime have been carried out, in which new societal agents play a key role. Lewis and Salem (1981) indicate that programs with a social control perspective strengthen the capacity of the local community to exert social control. Cozens (2008) argues that crime prevention through Environmental Design has potential benefits for public health and in delivering safer environments.

The last set of strategies mentioned is of particular importance in deprived areas, as social interventions are most needed, and a strong police presence may have a disruptive effect. Crowley (2013) states that policy makers wishing to install effective and efficient developmental crime programs should invest in interventions that deliver prevention programs as well as engage innovative mechanisms for investing in crime prevention efforts. Lawless (2006) analyzes The New Deal for Communities program, an English area-based initiative which aims to transform deprived neighbourhoods. While outcomes indicate modest changes against benchmarks, the author concludes that working with other agencies helps change; and having the community at the heart of the initiative enhances outcomes.

Meanwhile, there has been extensive debate in the literature regarding social capital: what it actually is and how it can be measured. Putnam et al. (1994) set the stage for such considerations when analyzing the effects of social engagement. Ever since, social capital has been defined and measured in several different ways by economic researchers. On this matter, Jackson (2017) considers seven forms of social capital. He then defines community capital as the ability within a community to sustain cooperative behavior in transacting, the running of institutions, the provision of public goods, the handling of commons and externalities, or collective action. This last definition is the one that serves as a reference for this paper.

Most certainly is the case that community capital can play an important role in many economic spheres. The economics of crime is a very important one. On this, a number of papers have focused on social capital as a driver of crime at the local geographical

level, as Hirschfield and Bowers 1997, Lederman et al. 2002, Buonanno et al. 2009 and Akçomak and Ter Weel 2012. However, the results do not present clear conclusions. While Buonanno et al. (2009) find that associational networks have a negative and significant effect on property crimes, Lederman et al. (2002) state that trust has a significant and negative effect on violent crime rates, and Akçomak and Ter Weel (2012) find a negative correlation between social capital and crime rates. Importantly, Hirschfield and Bowers (1997) state that there is a significant relationship between social cohesion and crime levels in disadvantaged areas.

Such evidence is also backed up by sociological studies. In fact, a growing number of studies support the link between low social capital and high crime rates. Warner and Rountree (1997) analyze the role of local social ties in mediating between structural conditions and crime rates, and find that the extent to which friendship networks decrease crime depends on the racial makeup of the neighborhood. Meanwhile Kawachi et al. (1999) argue that two sets of societal characteristics influence the level of crime: the relative degree of deprivation, and the degree of cohesion in the social relations. Additionally, Takagi et al. (2012) find that generalized trust, reciprocity, supportive networks, and social capital within a neighborhood are inversely associated with the probability of becoming a victim of crime.

3 Institutional Setup: Salut als Barris in Barcelona

In the framework of public policy analysis, the community component plays an important role. On this matter, the Local Government Association of the United Kingdom (LGA) defines community action as ‘any activity that increases the understanding, engagement, and empowerment of communities in the design and delivery of local services’ (Local Government Association 2016). Even though the activities may differ, greater engagement of local citizens is key in the planning, design, and delivery of local services. According to the LGA, such action can help to build a community and social capacity by creating social networks. Among its many benefits, improving community cohesion and safety are mentioned. Moreover, the local authorities in Barcelona define community action as ‘a process of stimulating cooperative social relationships between members of a community, a human collective that shares a space and a sense of belonging that results in mutual links and support, and that motivates members to become central agents in the improvement of their own reality’ (Ajuntament de Barcelona 2005). Therefore, the objective of community action is to improve social well-being by promoting active participation. Community action requires the empowerment of citizens to drive change and improvements beyond their own individual spheres.

In 2005, local health authorities in the city of Barcelona (*Agència de Salut Pública*

de Barcelona (ASPB)¹ and *Consorti Sanitari de Barcelona* (CSB)), jointly with different actors from the 10 districts of the city, started developing the community health program ‘Health in the Neighborhoods’ (*Barcelona Salut als Barris*, BSaB). The program aimed to improve health outcomes and reduce inequality between the disadvantaged neighborhoods and the rest of the city. The program has continued to develop uninterrupted since it began in 2008.²

BSaB is implemented through community-based interventions, and it targets areas where income is below 90% of the city median. All potentially participating areas were already benefiting from a previous location-based policy, the ‘Neighborhood Act’ (*Llei de Barris*, LDB, Generalitat de Catalunya 2004), which was passed in the whole of the region of Catalonia to improve living conditions in the most disadvantaged neighborhoods. However, and importantly for us, the LDB was a project focused on improving infrastructure and affected all areas across the BSaB setting.³ Therefore, it will not cause any confounding effects for our study.

Some analysis of BSaB has already been performed by local authorities. While Díez et al. (2012) describe the experience, achievements, lessons, and challenges of the implementation of BSaB, Sánchez-Ledesma et al. (2017) characterize the BSaB prioritization procedure. These last authors state that the community perspective of health stimulates and empowers the community, encourages mutual support and promotes their importance by making them responsible for improving their own reality. Additionally, Barbieri et al. (2018) state the need to identify key indicators for measuring and characterizing community action for health. Based on research into 49 neighborhoods in Barcelona, Barbieri et al. (2018) devise an index to measure and characterize community action for health. However, this literature on BSaB is mostly related to descriptive analysis. Hence, causal analysis of the policy has yet to be undertaken.

3.1 Description of the program

BSaB was deployed between 2008 and 2014 in 12 of the 49 neighborhoods potentially participating, out of the 73 in the city of Barcelona. The 49 neighborhoods potentially

¹All these acronyms are from the original Catalan.

²The program has been kept running even though there have been changes in the party in power in local government, both at the regional and city level. While in 2005 the center-left ‘Socialist Party’ was in power both in the region of Catalonia (since 2003) and in the city of Barcelona (since 1979), it was ousted by the center-right ‘*Convergència i Unió*’ coalition in 2010 and 2011 respectively. Currently (since 2015), Barcelona City Council (*Ajuntament de Barcelona*) is run by *Barcelona en Comú*, a left-leaning party. BSaB is still running.

³In all LDB funding programs, 143 of the 450 applications received were granted approval. Up until 2014, just 8% of the LDB projects were carried out in the city of Barcelona, all of them belonging to disadvantaged areas. Examples of the improvements in infrastructure for public spaces and facilities can be seen in González-Pampillón et al. (2019). As much as 80% of the funds were spent on public spaces and facilities for public use, while an additional 10% was devoted to renovating the existing stock of apartment buildings.

included were those in which average income was below 90% of the city median and where some ‘Neighborhood Act’ activity was taking place. The 12 neighborhoods included in BSaB represent around 15% of the city population and 25% of the potentially participating population.⁴ A key feature for our identification strategy is that the progressive roll-out of BSaB in the territory did not follow any specific pattern with regard to socio-economic or demographic characteristics. This allows it to be regarded as a quasi-random experiment.⁵ The deployment and timing of BSaB are presented in Table 1 and Figure 1.

Table 1: BSaB deployment by neighborhoods

Neighborhood	Start Date	First Priority
Roquetes	Jun-2008	Mental Health
Poble Sec	Jun-2008	Mental Health
St. Pere, Santa Caterina i la Ribera	Jun-2009	Drugs
Torre Baró	Jun-2009	Sexual Health
Ciutat Meridiana	Jun-2009	Sexual Health
Vallbona	Jun-2009	Sexual Health
Barceloneta	Jul-2010	Drugs
Baró de Viver	Mar-2011	Drugs
Bon Pastor	Mar-2011	Drugs
Raval	Oct-2011	Sexual Health
El Besòs i el Maresme	Oct-2013	Drugs
Verneda i La Pau	Nov-2014	Mental Health

Notes: The table presents the 12 treated neighborhoods by the BSaB policy in the city of Barcelona from 2008 to 2014, ordered chronologically. It also displays the start date of the program on each of them, as well as its main content. Source: Agència de Salut Pública de Barcelona (ASPB).

⁴See Table A1 in Appendix for population and income data of all 73 neighborhoods in Barcelona in 2007 and 2014.

⁵The quasi-random deployment of BSaB was confirmed by the public authorities running the program. Importantly, they reported that crime levels were not considered when deciding BSaB implementation and deployment. This is statistically assessed in later sections.

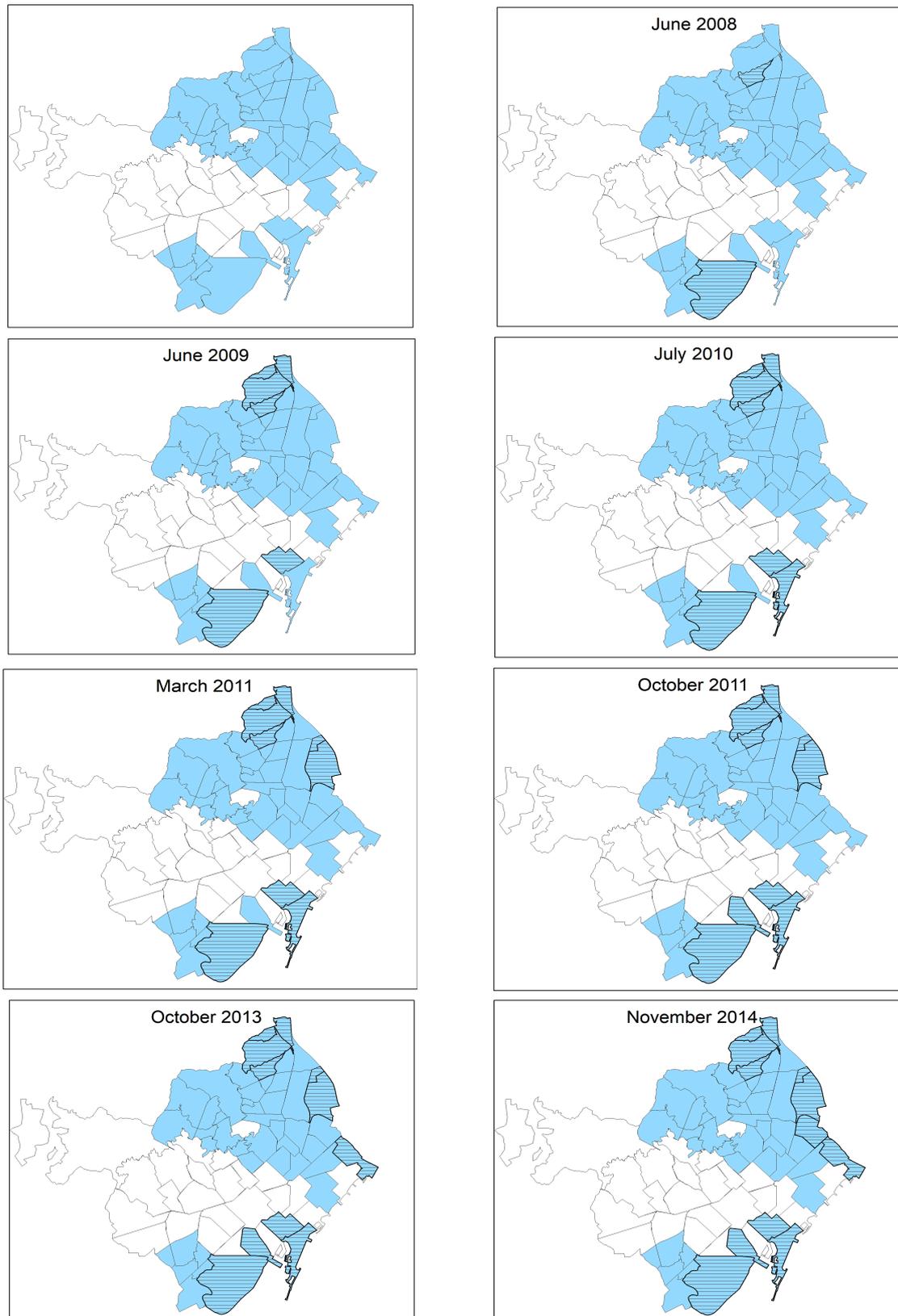


Figure 1: Deployment of BSaB interventions in the city of Barcelona.

Notes: These maps show the geographical deployment of the BSaB policy across the city of Barcelona. The colored neighborhoods are those that were potentially included in BSaB due to their income characteristics (49 neighborhoods). Those that in addition have hatching were those that actually participated (12 neighborhoods by 2014).

As explained in Díez et al. (2012), in the implementation of BSaB, plurality, participation, sustainability, evidence and evaluation were applied in the following phases:

1. Establishment of political alliances and a steering group to facilitate interventions (3 months' time, pre-intervention).
2. Construct qualitative and quantitative community knowledge to list perceived problems (1 to 3 months' time, pre-intervention).
3. Prioritization of problems and interventions by the local community and authorities (1 day, pre-intervention).
4. Drawing up of an intervention plan for previously defined lines of action. Intervention starts.
5. Evaluation of the implementation of the overall plan and of each intervention (1 to 3 years, post-intervention).
6. Maintenance of the working group on health, after the intensive phase (3 to 4 years, post-intervention).

The interventions are intended to facilitate non-competitive physical activity, social relationships, healthy recreation, health literacy, and sexual health. Examples of interventions are attention for young consumers of addictive substances, integration of the drug-dependent population, training and job placement, sexual and reproductive health advice, parenting skills programs, mental healthcare, and healthy leisure workshops (see Díez et al. 2012; Generalitat de Catalunya 2014; Comissionat de Salut 2016).⁶ Table 1 indicates which of these intervention fields were seen by residents as priorities in each neighborhood. However, each neighborhood had a unique combination of interventions, making a heterogeneous analysis by intervention type unfeasible.

For example, in Ciutat Meridiana, one of the activities was named 'Alternative Fridays'. Targeted at adolescents aged 14-18, it aimed to provide healthy leisure activities. In its first edition, over 200 individuals participated, of whom 73% were men and around 60% were foreigners. In satisfaction surveys, respondents were very satisfied and a quarter of participants stated that the activities should be more frequent. Another example is the 'Syrian' program at Bon Pastor. This was aimed at increasing knowledge and awareness of contraception, reproductive health and the offer of public services available in the neighborhood, especially for the immigrant population. The program reached 745 individuals and according to a survey of participants, satisfaction was very high (median of 9/10).

Another important factor in the policy roll-out is that these interventions were mostly managed and run in each neighborhood by the local health center (CAP) alongside a community group that included representatives of civic entities and community associations, as well as social workers. There are almost 70 of these centers throughout the city and

⁶See Table A2 in the Appendix for a full list of activities run in the framework of BSaB.

most of them are related exclusively to a specific neighborhood.⁷ Because each CAP has a certain area and population under its responsibility that is set by the administration, our identification strategy strengthens as spillovers from one neighborhood to another are highly unlikely.⁸ However, and very importantly, all of these interventions were run from the beginning under a community perspective, involving the steering group, the local community, and the authorities. In fact, it is this communal component of BSaB that leads us to hypothesize that BSaB boosted community ties and consequently reduced local crime rates.

3.2 Potential mechanisms: the community component

Theoretically the BSaB policy may affect criminal activity via different pathways. Initially, the most obvious may be the health channel, by which the improved health status of the affected population reduces criminal activity. In these lines, Bondurant et al. (2018) estimated the effects of expanding access to substance-abuse treatment on local crime for US counties. They indeed find that it reduces violent and financially motivated crimes in a certain area, but not immediately.

However, due to the characteristics of BSaB we argue and later show that improvements in health are not the main outcome driver. Instead, we claim that a mechanism of community ties operates.⁹ As previously mentioned, a body of research has documented the association between community capital and becoming a victim of crime. The theoretical pathways via which community capital leads to crime prevention include both formal and informal mechanisms. Sampson and Laub (1995) state that communities with strong social capital are able to exert informal social control and bolster the capacity to obtain services from public agencies and formal institutions. Due to the high degree of involvement that BSaB requires from neighbors, it is expected that closer links are built up within the neighborhood. As a result, informal social control may also arise, increasing the probability of getting caught, potentially leading to a fall in the crime rate in the area.

Several findings can help disentangle the mechanisms that lead to our results. Firstly, we estimate the timing of the results in criminal activity through an event study exercise. We claim that if the response of the crime rate to the policy is relatively fast, it is harder to attribute the reaction to improved health of the population. If health was actually the mechanism behind the effects of BSaB on crime, the results would take

⁷Every resident in Barcelona is assigned to a CAP according to their home address. In a sense, their area of influence (called the Basic Health Area) can very much be seen as that of a school district in the US. Basic Health Areas coincide to a large degree with neighborhoods.

⁸This was also confirmed by the authorities running the BSaB program.

⁹We also test as possible mechanism whether the program improved local labour perspectives by analysing if unemployment figures were significantly affected in treated areas as opposed to non-treated areas. Results are shown in following sections.

some time to materialize (as in Bondurant et al. (2018)). Secondly, we assess if BSaB impacts per capita local associations. Thirdly, we examine whether there have been any changes in the health status of participants and non-participants. Additionally, we analyse if there have been changes in unemployment after the policy implementation, as some of the workshops had such objective, and can affect engagement in criminal behavior. Consequently, if we observe (1) a change in crime rates within a short interval of time after policy implementation, (2) a significant predictive power of the BSaB policy onto local associations and (3) if no effect is found nor in health nor in unemployment, potential impacts on crime are likely to be due to the community feature of the policy and more robust community ties. Moreover, if we find that the effects are homogeneous across neighborhoods, irrespective of the content or priorities set, our hypothesis that community ties lead the result is even stronger.

4 Data and variables

The main data source for this project is a geocoded administrative dataset of all recorded crime in Catalonia from 2007 to 2014. These data is provided by the Local Police (*Mossos d'Esquadra*). It comprises all registered crime with information on the exact time and place of the crime as well as the type of crime. In total it contains over 1.5 million entries. Such detail in our main dataset allows us to estimate our coefficients of interest at a relatively high time frequency (such as a month) and a very low geographical level (such as a neighborhood) while maintaining the robustness of the results. Moreover, this source provides information on the offenders who committed the crimes, as well as on the victims, when available. As BSaB is aimed at specific populations through different interventions, it is possible to evaluate whether the targeted groups are more or less likely to become victims of crime.

Additional data sources come from the Catalan Health Department (ICS) and from the Public Policy and Government Institute (IGOP), a research group at the Universitat Autònoma de Barcelona (UAB; Barbieri et al. 2018). These data sources provide information on the neighborhoods potentially targeted, those actually treated, the timing of the policy in each neighborhood, and details of the activities that formed part of each intervention. This information allows us to understand our setting in detail, build our main explanatory variable and justify the quasi-random nature of the roll-out of the policy

What also have at our disposal a set of socioeconomic variables that will act as controls for our main estimations. All of them are available at the neighborhood-month level. First of all, we have information on the registered local associations (registration date and aims), which allows us to understand the importance of the associations, a proxy for social ties. This information was provided by the Catalan regional authorities (*Generalitat de Catalunya*). Moreover, and related to business cycles, we have information of registered

unemployment rates, as well as housing market prices per square meter.¹⁰ Finally, we also account for a proxy for tourism pressure in per capita terms.¹¹ This last variable accounts for potential confounders resulting from the related economic activity, which is of great relevance in a city highly exposed to such inflows. These last three variables (registered unemployment, house prices, and tourism pressure) were built from information provided by Barcelona City Hall (*Ajuntament de Barcelona*). While associations, house prices and registered unemployment are considered at the neighborhood level, the tourism pressure index is taken at the district level, as a neighborhood may be too small of an influence area. A description of the main variables of this analysis is shown in Table A3 of the Appendix. Our final crime database is comprised of 4,704 observations, resulting from the product of the 12 months in 8 years (2007-2014) in the 49 neighborhoods potentially targeted. For each observation (neighborhood-year-month) crime, offense and victim rates per 1,000 inhabitants were determined, as well as the socioeconomic variables previously mentioned.

4.1 Creating crime typologies

The database provided by the Catalan police is rich in many aspects, one of which is the way crime is codified. There are over 300 types of crime recorded, which cover more than 190 articles of the Spanish Penal Code. Even though having such a large amount of information can be of great value for research, this codification is not functional for our analysis. Based on those 300 types, we construct 17 detailed crime categories, which we also group into 3 broad categories. Both categorizations cover the entire range of recorded crime types.¹²

Considering our setting, we understand that further and more specific crime categories should be designed. To this end, we construct two less traditional crime categories that are transverse to those previously defined. First, we created a broad category we named ‘intimate crimes’, which covers the detailed categories of family, sexual and gender violence. The rationale behind this aggregation is that it summarizes all the crimes related to very close personal relationships. Secondly, following the description by Currie and Almond (2011), we defined a broad category we named ‘anger crimes’ that includes the detailed categories of criminal damages, bodily harm, disobedience, and threatening behavior. These are crimes that are not motivated by money or close links, but still have some behavioral or personal component.¹³ Except for criminal damage to property,

¹⁰According to the National Statistics Institute (INE), 76% of all unemployed individuals appear in the unemployment register. Registered unemployment rates and housing prices are only available since 2009.

¹¹We consider the number of tickets sold daily in every public museum in the city. This proxy is highly correlated (0.69) with the total number of tickets sold in every tourist outlet point in the city.

¹²Details of crime classifications are presented in Table A4 of the Appendix.

¹³Currie and Almond (2011) state that temperamental skills are often not mentioned directly in the

all the other categories correspond to crimes against the person. We understand that criminal damage to property still needs to be included in such a category as it may be a result of anger, irritation or rage. In this regard, the richness of the data allows us to depart from traditionally set crime typologies, and analyze new ones that focus on the types of crime we believe the BSaB policy may affect via the community channel. This helps to pinpoint the causal effects of community ties on crime better.

This last classification indicates that intimate and anger crimes account for almost 1 out of every 5 crimes recorded, and that anger crimes are much more frequent than intimate crimes. Even though it may seem that these do not represent an important part of overall crime, we must consider that they inflict a much higher disutility on their victims than other more frequent types of crime. Indeed, Dolan et al. (2005) indicate that while discounted QALY¹⁴ losses resulting from rapes and sexual assaults are 0.561 and 0.160, while for a common assault this figure is just 0.007. This demonstrates the importance of dealing with such offenses.

Table 2: Crime distribution by typology and location

	% Crime	% Residence	% Street	% Other
All crime	100	10	45	46
Intimate	3.0	62	25	13
<i>Family</i>	0.7	68	19	13
<i>Gender</i>	2.0	64	26	10
<i>Sexual</i>	0.3	36	31	32
Anger	15.9	21	45	35
<i>Criminal damages</i>	8.5	21	41	38
<i>Bodily harm</i>	3.0	11	52	38
<i>Disobedience</i>	1.8	8	67	25
<i>Threatening behavior</i>	2.5	43	31	26
Drugs	0.7	3	87	10

Notes: This table presents the composition of the crime categories labeled as ‘intimate’ and ‘anger’, as well as its contribution to overall crime. It also indicates where these crimes took place, considering a residence, the street or other locations. Source: Own construction from Catalan Police data.

Additionally, Table 2 shows how crime types are distributed by location. It is clear that there are some typologies with location patterns that are particularly attached to an address. These are indeed those which we already included in the intimate crime category. Also, some others, such as threatening behavior (included in the anger broad category), also present a high share of being committed at a residence. Because of this location pattern, and its relevance in light of the characteristics of the BSaB policy, our analysis will mainly focus on intimate and anger crimes. We will also pay particular

literature but are proxied by psychological traits, social skills, and behavioral issues.

¹⁴Quality-Adjusted Life Years.

attention to drug offenses, as they are closely related to the initiatives carried out as part of BSaB.

Tables A5 to A8 in the Appendix show summary statistics for our dependent variables and controls. Results are shown both for the entire city of Barcelona (all 73 neighborhoods) and for the area potentially included in BSaB (49 neighborhoods).

5 Empirical Strategy

To evaluate the impact of BSaB on local crime rates we adopt a staggered Difference-in-Differences approach (sDiD), where our observational unit is a neighborhood-month pair. The ‘staggered’ term comes from the fact that treatment was implemented over a different periods of time for the different observational units. This method quantifies the impact of a given program (in this case, BSaB) as the difference of outcome changes (post- vs. pre-intervention) between participants and non-participants. In this case, and in order to have comparable treatment and control units, the spatial units of analysis are the neighborhoods in Barcelona where some LDB activity was taking place and whose income was below 90% of the city median (those colored blue in Figure 1; the white areas are not part of our analysis). We quantify the impact of the BSaB policy as the difference in crime before and after the implementation of BSaB for neighborhoods where BSaB took place (blue and with hatching in Figure 1) and those where it did not (blue but without hatching in Figure 1).

$$sDiD = E[Crime(after) - Crime(before)|BSaB = 1] - E[Crime(after) - Crime(before)|BSaB = 0] \quad (1)$$

Since the implementation of BSaB was staggered across neighborhoods, the before and after periods differ across treatment observations.

It should be noted that the artificial nature of the geographical boundaries may introduce the problem of potentially capturing spillover effects across neighborhoods. This problem is a general concern in the urban economics literature when dealing with geographically small treatment and control units. In order to address this issue, researchers can either choose some types of crime that follow a more geographically concentrated pattern (such as Warner and Rountree 1997) or construct a unique exposure to the treatment measure (as in Takagi et al. (2012), where a weighting matrix assigned each unit a level of ‘exposure’ to treatment). In this analysis, we focus on types of crime with a clear location pattern, such as those that mostly take place in residences, which above all are those we classify as intimate crimes. We also consider drug-related and anger crimes due to the nature of the policy. Restricting the study in such a way dispells potential spillover concerns. This is also supported by the functioning of the policy itself, that was

ran by local health centers that only deliver to the specific neighborhood they are in.

Taking the previous points into consideration, our first set of estimations tests the impact of BSaB on criminal activity as follows:

$$Crime_{it} = \alpha + \beta_1 T_{it} + \beta_2 BSaB_i + \beta_3 (T_{it} \cdot BSaB_i) + \theta X_{it} + \gamma_t + \delta_i + \varepsilon_{it} \quad (2)$$

where the dependent variable is the victim/offense/crime rate per 1,000 inhabitants, i is the neighborhood, t is the time period, $BSaB_i = 1$ for participants, $T_{it} = 1$ for the post-treatment period (different for each treatment unit), γ_t and δ_i are time and space fixed effects, X_{it} is a vector of socioeconomic controls and ε_{it} is the error term.

Our main results also include interaction terms between baseline neighborhood characteristics and a time trend. Additionally, observations are weighted by population size. In the case of victims and offenders, we consider as dependent variables specific victim/offense rates per 1,000 individuals, considering the characteristics of the victims/offenders in terms of gender and age. In all cases, the sDiD estimator of the policy effect is given by β_3 .

Finally, responses over time are studied following an event study approach. We perform fixed-effects regressions of the following type:

$$Crime_{it} = \alpha + \beta_1 T_{it} + \beta_2 BSaB_i + B_\tau (D_{it} \cdot BSaB_i) + \theta X_{it} + \gamma_t + \delta_i + \varepsilon_{it} \quad (3)$$

where the dependent variable is the victim/offense/crime rate per 1,000 inhabitants, i is the neighborhood, t is the time period, $BSaB_i = 1$ for participants, $D_{it} = 1$ are distance to treatment dummies (different for each treatment unit), γ_t and δ_i are time and space fixed effects, X_{it} is a vector of socioeconomic controls and ε_{it} is the error term.

We estimate $BSaB_i \cdot T_{it}$ interactions, leaving $D_{it} = -1$ as the reference period. Each of the ϕ_d coefficients quantifies the criminal activity difference between the $BSaB$ neighborhoods and the control group relative to the period -1. While coefficients $\{-M, \dots, -1\}$ identify anticipation effects, coefficients $\{0, \dots, M\}$ identify dynamic treatment effects. First of all, this allows us to test the existing of pre-trends. Secondly, it helps us to determine the speed at which the policy may affect criminal activity (if at all), potentially leading to heterogeneous results among typologies. Also importantly, it may helps us disentangle potential mechanisms behind the results.

6 Results

6.1 Baseline specifications

In order to tackle possible endogeneity issues of treatment status, in Table 3 we present a set of t-tests performed on differences between treatment and control units, previous to the intervention (in 2007). These indicate that there are no significant differences between treatment and non-treatment units in a set of observable socioeconomic and demographic characteristics. Regarding crime rates, differences appear at the level, but not in growth rates.

Table 3: t-tests on pre-existing crime rates and sociodemographics

	Mean	Std. Err.	t-value
Sociodemographics			
Population	749.11	4666.67	0.874
Men	-207.26	2381.53	0.932
Women	956.37	2300.37	0.682
Teenagers	27.41	179.51	0.880
Spanish	1316.47	1398.09	0.357
Foreign	-2577.23	1921.53	0.205
Mortality rate	-57.25	76.40	0.457
Fecundity	-4.52	2.26	0.062
Housing prices	-1.62	1.86	0.402
Retirement	-45.68	41.95	0.297
Associations per 1000 inh.	0.04	0.045	0.440
Crime Rates			
All	-3.48	1.07	0.001
Property	-3.04	0.95	0.002
Person	-0.31	0.10	0.003
Intimate	-0.16	0.04	0.000
Anger	-0.19	0.17	0.275
Drugs	-0.04	0.02	0.026
Crime Growth			
All	0.01	0.03	0.702
Property	-0.03	0.04	0.525
Person	0.10	0.06	0.103
Intimate	0.15	0.13	0.275
Anger	0.03	0.06	0.592
Drugs	-0.49	0.13	0.000

Notes: This table presents balancing tests for sociodemographic (panel a) and criminal characteristics (panels b and c) between treated and control units in 2007, before the BSaB policy was deployed. Source: Own construction from Barcelona City Hall and Catalan Police data.

Furthermore, we estimate a logit model where the dependent variable was the treatment indicator. We also estimate a panel logit, where the timing of the treatment was

considered. The results of these two exercises (in Tables A9 and A10 of the Appendix) show that our explanatory variables do not seem to explain either the fact of being included in the intervention nor its timing. These results provide evidence that the parallel trends assumption holds in this setting.¹⁵

Table 4 presents results based on the estimation of Eq.(2) for crime rates while Table 5 present the results for offense and victim rates, in both cases clustering standard errors at the neighborhood level (Cameron and Miller 2015) and weighting each observation by population size. Each column indicates a different specification, each one being more stringent than the previous one. Our preferred specification is that in column 4. By including neighborhood specific time trends, it is a stringent specification.

Overall, the results for the estimated impact of BSaB on local crime rates indicate that there was indeed a negative and significant impact in crime. Even if we do not see a decrease in criminal activity across all its different aspects studied after the policy implementation, we do see important reductions in aspects of key relevance in light of BSaB.

In the broad crime categories, reductions in crimes against the person and other crimes are observed. For the case of other crimes, the effect is driven by crimes labeled as disobedience to agents of the law. Somewhat related to the reduction in crimes against the person, BSaB had a negative impact on intimate crime rates: BSaB reduces intimate crime rates by 0.07, which implies a decrease of 25%. For this category, the results are mainly derived from gender crimes. It must be noted that crime rates for intimate crimes are much lower than for other criminal typologies, making percentage decreases of higher magnitude. Regarding drug crimes, which represent another important result considering the policy under study, we no direct effect of BSaB is found. For anger crimes, no results are found either.

On the matter of offenders and victims, Table 5 evidences a reduction in criminal outcomes of significant sets of the population. Even if no widespread significant reduction of offenses is found, we do see a significant reduction in the offense rates of those individuals under 18 years of age. Regarding victimization, we do not find any significant impact. When analyzing these results by age and gender (see Table A11 in the Appendix), we observe that for offenders the results are led by those of female offenders under 18, and we also observe a significant reduction for male offenders aged 18-25.

¹⁵Such a feature was later confirmed informally by anecdotal evidence provided by the authorities running BSaB in the Barcelona Public Health Agency (ASPB). At informal meetings we learned that the assignment of neighborhoods to the intervention did not follow any rule-based procedure and it was rather a quasi-random decision.

Table 4: Difference-in-Differences Estimates

	(1)	(2)	(3)	(4)
Against property	7.897** (3.315)	0.804 (1.174)	0.691 (1.108)	0.186 (0.715)
Against person	0.373*** (0.115)	-0.087 (0.074)	-0.089 (0.071)	-0.088 (0.056)
Other	0.290** (0.115)	-0.194 (0.164)	-0.206 (0.153)	-0.095* (0.051)
Intimate	0.057** (0.026)	-0.101*** (0.032)	-0.075** (0.031)	-0.066*** (0.024)
Anger	0.663*** (0.186)	-0.089 (0.107)	-0.063 (0.093)	-0.098 (0.090)
Drugs	0.110* (0.055)	-0.013 (0.052)	-0.017 (0.052)	-0.018 (0.018)
Observations	4,702	4,702	4,702	3,264
Neighborhood FE		Y	Y	Y
Year-Month FE			Y	Y
Neighborhood-Time trends				Y

Notes: This table reports the results of the Difference-in-Differences estimation following Eq. (2) for the 2008-2014 period. Each column presents a different specification according to the controls added, being each more demanding than the previous one. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units. The coefficient showed is that of interest in a Difference-in-Differences setting, being $Treated \cdot Post$. Confidence intervals are based on standard errors clustered at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Difference-in-Differences Estimates

	(1)	(2)	(3)	(4)
Off. U18	0.488*** (0.161)	-0.583** (0.249)	-0.513** (0.228)	-0.428*** (0.154)
Off. 18-25	8.917** (3.421)	-0.801 (2.105)	-1.133 (1.991)	-1.632 (1.403)
Off. 25-35	2.729** (1.165)	0.125 (0.805)	-0.105 (0.760)	-0.587 (0.350)
Off. 35-45	2.464** (0.968)	0.755** (0.333)	0.320 (0.313)	-0.166 (0.267)
Vict. U18	1.066* (0.582)	0.278 (0.199)	0.099 (0.192)	0.209 (0.128)
Vict. 18-25	25.812* (13.458)	5.346 (5.369)	6.237 (5.389)	5.805 (4.495)
Vict. 25-35	6.680** (2.979)	0.034 (0.656)	0.736 (0.573)	1.018 (0.814)
Vict. 35-45	5.593** (2.277)	0.848* (0.451)	0.635 (0.385)	0.896 (0.722)
Observations	4,702	4,702	4,702	3,264
Neighborhood FE		Y	Y	Y
Year-Month FE			Y	Y
Neighborhood-Time trends				Y

Notes: This table reports the results of the Difference-in-Differences estimation following Eq. (2) for the 2008-2014 period. Each column presents a different specification according to the controls added, being each more demanding than the previous one. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units. The coefficient showed is that of interest in a Difference-in-Differences setting, being $Treated \cdot Post$. Confidence intervals are based on standard errors clustered at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Furthermore, Table 6 presents several robustness checks for the previous estimates. On them, we cluster standard errors in a more stringent way and we include other sociodemographic controls but at the expense of losing observations. We also perform placebo exercises. We find that the coefficient estimated for BSaB of Table 4 are stable across these alternative specifications. Moreover, and very importantly, our falsification exercise (last column of Table 6), which assigns random treatment in terms of neighborhoods and roll-out, reflects no significant results.

Further consideration is given to column 4 in Table 6. In it, additional controls on tourism and housing prices are included to specification of column 4 in Table 4. For this exercise, all results of Table 4 hold. But additionally, a reduction in anger crime is registered. Concretely, according to this specification, BSaB reduces anger crimes by 0.22, which roughly translates to an important average decrease of 14%. When analyzing its components, we conclude that the anger crime figures are mostly driven by criminal damages.

Table 6: Robustness exercises

	Baseline	Twoway clustering by neigh. and year	Tourism control	Tourism and housing controls	Placebo
Against property	0.186 (0.715)	0.186 (0.487)	0.169 (0.628)	0.113 (0.891)	0.088 (0.085)
Against person	-0.088 (0.056)	-0.088*** (0.030)	-0.088 (0.053)	-0.105* (0.060)	0.007 (0.005)
Other	-0.095* (0.051)	-0.095* (0.047)	-0.095* (0.051)	-0.154*** (0.055)	-0.001 (0.005)
Intimate	-0.066*** (0.024)	-0.066*** (0.015)	-0.066*** (0.023)	-0.073* (0.036)	0.002 (0.003)
Anger	-0.098 (0.090)	-0.098 (0.060)	-0.098 (0.090)	-0.201*** (0.054)	-0.008 (0.009)
Drugs	-0.018 (0.018)	-0.018 (0.025)	-0.018 (0.016)	-0.018 (0.020)	-0.001 (0.001)
Observations	3,264	3,264	3,264	2,377	3,264
Neighborhood FE	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y
Neighborhood-Time trends	Y	Y	Y	Y	Y
Control: Tourism			Y	Y	
Control: Housing				Y	

Notes: This table reports the results of alternative specifications for the Difference-in-Differences estimation following Eq. (2) for the 2008-2014 period. Each column presents a different specification. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units. The coefficient showed is that of interest in a Difference-in-Differences setting, being $Treated \cdot Post$. Confidence intervals are based on standard errors clustered at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Finally, we present results from the Safety and Victim Survey for Catalonia 2007-2014.

In the Survey, individuals are asked about safety and civility in their neighborhood and district, as well as about their experiences of being a victim of crime in the past 12 months. Specifically, individuals were asked whether they feel safety and civility have improved, worsened or stayed the same in their neighborhood in comparison to the previous year. We use this question by running a logistic regression on safety and civility having improved against the presence of BSaB in the neighborhood in that year. Estimates are presented in Table 7. The presence of BSaB raised the probability of perceiving an improvement in safety by approximately 3%. From this result, we conclude that even if local crime rates do not drop for all the categories analyzed, individuals living in the participating neighborhoods feel that safety has improved. However, no significant results were found for perceptions of civility. We believe that the fact that civility is less specific than safety may influence these results. ¹⁶

Table 7: Changes in perception in the neighborhood

	Civility	Security
BSaB	-0.007 (0.004)	0.032*** (0.004)
Observations	21,779	21,779
Wald Chi2	225.98	160.9
Neighborhood FE	Y	Y
Year FE	Y	Y

Notes: This table presents Difference-in-Differences estimates of the BSaB policy in other outcomes besides crime, each presented in a different column. We show average marginal effect from logistic regression with district and year fixed effects and robust standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Overall our results are in line with those of previous studies, while in many ways they represent improvements on some of the approaches previously adopted. Takagi et al. (2012) had already established that support networks and social capital are inversely associated with crime. However, crime was only measured for any kind of victim, making a broader analysis. Our results are also related to those of Buonanno et al. (2009) and Lederman et al. (2002), although our findings differ in some aspects. Buonanno et al. (2009) find a clear effect of social capital on crime, but their dependent variable is property crime. We did not find a significant effect on all property crimes. Moreover, Lederman et al. (2002) state that trust (seen as social capital) has a significant and robust effect on violent crime, proxied by homicide rates.

Our findings are of value in light of the policy evaluation. The type of crime that is reduced is intimate crimes, that most likely will affect women. This is extremely relevant for two reasons. Firstly, many interventions were aimed at empowering women and raising

¹⁶It could be that each respondent has a different concept of civility (as broadly specified in the survey) and it may be more difficult to perceive.

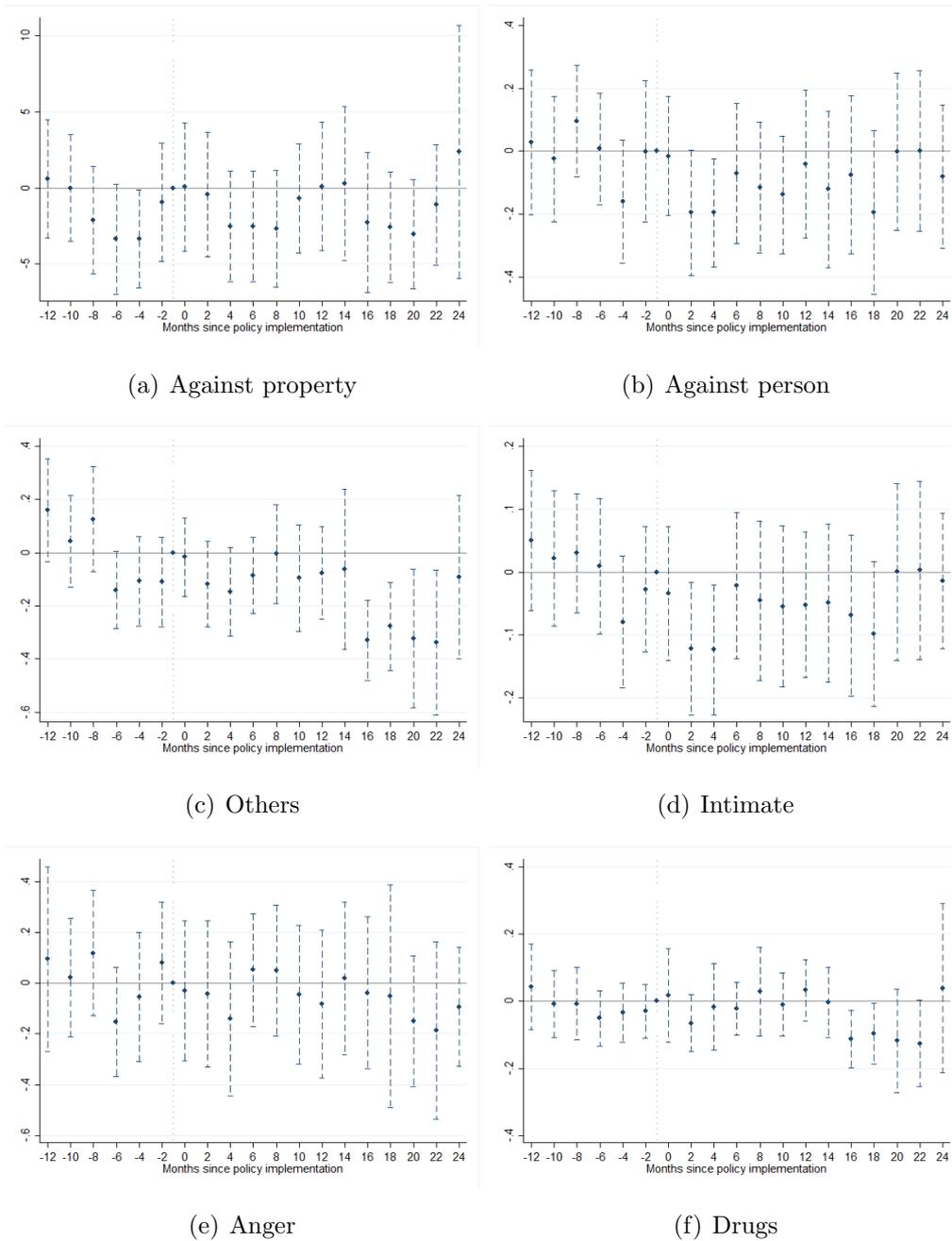
awareness of sexual health and education. Moreover, most of the actions targeted younger population groups, that from our results for offenders seem to be those more positively affected (showing a lower offender rate) as a result of the program. Secondly, it is relevant because our findings indicate that progress was achieved on such an important issue as violence against women. Indeed, according to national statistics, in 2018 there were over 30 thousand cases registered as gender violence in Spain.

6.2 Event study analysis

We present estimations from Eq. (3), where we analyze dynamic treatment effects of the policy. We interact the treatment indicator with distance to treatment dummies, which are neighborhood-specific. For this analysis, period -1 is taken as point of reference. We perform a binning of effect window endpoints as in Schmidheiny and Siegloch (2019). On this matter, authors show that this exercise is key for identification of dynamic treatment effects. In this case we bin periods 12 months before and 24 months after BSaB interventions. The results are presented in Figure 2 for the typologies previously analyzed.

The first feature to highlight in Figure 2 is that in all subfigures there do not seem to be any anticipatory effects of BSaB on crime. This strengthens the evidence found in Table 3 on the parallel trend assumption holding in this context. The second analysis corresponds to the dynamic treatment effects. As Figure 2 reflects, the impact of BSaB is different over time across crime rates. No dynamic treatment effects are found for crimes against property, for crimes against the person there is an effect in the very short term (months 2-4) and for other crimes an effect is found in the medium-long term (months 16 and on). This pattern reflects what happens in the detailed crime categories that are of interest in this paper. Related to crimes against the person, the effect of BSaB on intimate crime rates occurs in the very short run. The impact is quite immediate, showing a significant decrease 2 months after policy implementation. However, Figure 2 also shows that the impact is quite ephemeral, as by month 6, the effect had already become diluted. A very different picture is found for anger crime rates. In this case, no dynamic treatment effects are found. Yet, even if confidence intervals are higher and point estimates are not significant, we consistently see negative coefficients from the second semester onwards. Finally, for the case of drug crimes a medium-long term is found, even if no significant effect was found in the Difference-in-Differences estimates. For this crime category, BSaB takes longer to affect local crime rates, as significant and negative effects are found 16 months after deployment.

Figure 2: Event study exercise on the effects of BSaB on crime



Notes: This graph reports the results of an event study exercise derived from the baseline Difference-in-Differences estimation for the 2008-2014 period for crimes against property, against person, other crimes, intimate crimes, anger crimes and drug crimes. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units. Confidence intervals are based on standard errors clustered at the neighborhood level.

These results are also supported by the evidence show in Table 8. In such table we present joint significance tests for all lag and lead coefficients. Results for both intimate and drug crimes indicate that we cannot reject the hypothesis that all anticipatory effects are equal to zero, while it is possible to reject it for all dynamic treatment effects.

Table 8: Joint significance tests for anticipatory and dynamic effects

	F-stat anticipatory F(11,3093)	Prob > F	F-stat dynamic F(25,3093)	Prob > F
Intimate	1.28	0.229	1.39	0.093
Anger	1.18	0.296	0.84	0.697
Drugs	1.00	0.441	2.39	0.000

Notes: This table reports the results of joint significance test of the pre and post coefficients of the event study exercises shown in Figure 2. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units. F-stats columns present the statistic realization for the test that either all lag coefficients or all lead coefficients are jointly different from zero.

6.3 Disentangling potential mechanisms

Our main hypothesis is that the BSaB policy reduces criminal activity at the local level through its community component. One way to test this hypothesis is to link local associations to crime via BSaB. In other words, we assess if BSaB increased local associations and then if this increase further translated into lower crime rates.¹⁷ As already mentioned, information on the registered local associations accounts for registration date, aims and place of action. This information was provided by the Catalan regional authorities (*Generalitat de Catalunya*).

Moreover, to completely rule out other potential mechanisms, we carry out further analysis on whether health and unemployment outcomes acted as channels on the lowering on crime rates. For the first one we use microdata from the Barcelona Health Survey (Encuesta de Salud de Barcelona, ESB) for the 2001-2016 period. Specifically, we use the ‘health status’ question, which is based on self-perception. Answers range from 1 (very bad) to 5 (very good). We then compared the answers of individuals in treatment and control neighborhoods in 2006 (just before BSaB) and in 2016 (after BSaB). We also perform a similar analysis for a mental health indicator, derived from the Goldberg scale GHQ-12. In the Goldberg Scale, a higher number (1 to 12) indicates a higher risk of bad mental health. For the second one, we use information from Barcelona City Hall on the registered unemployment rate by neighborhood.

¹⁷We set up the exercise as two-stage least square regression where we use the exogenous deployment of BSaB as an instrument for the number of local associations per 1,000 inhabitants, that can be endogenous to local crime rates:

$$\begin{aligned}
 Crime_{it} &= \alpha_2 + \beta_2 \widehat{assoc_pc}_{it} + \theta_2 X_{it} + \gamma_t + \delta_i + \varepsilon_{it} \\
 assoc_pc_{it} &= \alpha_1 + \beta_1 BSaB_{it} + \theta_1 X_{it} + \gamma_t + \delta_i + \varepsilon_{it}
 \end{aligned}
 \tag{4}$$

Regarding the validity of the instrument, it must hold that (1) BSaB highly correlates with per capita local associations (relevance), and (2) BSaB is exogenous to local crime (exogeneity). The first one is tested by a regression of BSaB on local associations. The second one is backed up by a logit regressing the probability of being treated on several sociodemographic variables including crime (Table 3) and a pretrends analysis (Figure 2).

Table (9) presents the results for the impact of the BSaB on local associations and on other potential mechanisms. Results reported in Table (9) show, first, a positive and statistically significant estimate for per capita local associations.¹⁸ Second, there are no significant differences in the means of health and mental status between individuals in treatment and control neighborhoods before and after BSaB implementation. In line with these results, Palència et al. (2018) find no evolution of self-rated health for men and women in treatment and control neighborhoods. Furthermore, results indicate no statistically significant impact of BSaB on local unemployment.

Table 9: Potential mechanisms

	Per capita Associations	Unemployment	Health Status	Mental Health
BSaB	0.504*** (0.171)	-0.003 (0.003)	-0.087 (0.081)	-0.064 (0.157)
Observations	3,264	3,264	3,716	3,653
Neighborhood FE	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y
Neighborhood-Time trends	Y	Y	Y	Y

Notes: This table presents Difference-in-Differences estimates of the BSaB policy in other outcomes besides crime, each presented in a different column. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units and the specification is the same that in our baseline specification for crime. Confidence intervals are based on standard errors clustered at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Given BSaB positively affects per capita local associations we perform an Instrumental Variables exercise. In it, we use BSaB as an instrument for local associations to then study its impact on local crime rates. We compare the results of this exercise to our baseline estimates (that of column 4 in Table 4). Results are shown in Table (10). The Instrumental Variables exercise provides evidence in the same direction than our reduced form estimates of Table 4. Still, the F-stat reported for the first stage of this exercise shows a somehow weak instrument. For this reason, we take the results of this exercise with caution and do not rely on them for our main analysis. Nonetheless, we take results of Table 9 and Table 10 as suggestive evidence that the mechanism behind the effectiveness of BSaB towards crime is likely to be community ties.

¹⁸Also see Figure A2 for the event study exercise on the impact of BSaB on per capita local associations.

Table 10: Instrumental Variables estimations on crime types

	Reduced Form	Instrumental Variables
Against Property	0.186 (0.715)	0.368 (1.344)
Against Person	-0.088 (0.056)	-0.174 (0.125)
Other	-0.095* (0.051)	-0.189* (0.098)
Intimate	-0.066*** (0.024)	-0.131** (0.061)
Anger	-0.098 (0.090)	-0.194 (0.191)
Drugs	-0.018 (0.018)	-0.036 (0.033)
Observations	3,264	3,264
Neighborhood FE	Y	Y
Year-Month FE	Y	Y
Neighborhood-Time trends	Y	Y
F-stat First Stage		8.645

Notes: This table reports the results of the Instrumental Variables estimation for the 2008-2014 period. Each column presents a different type of crime and the specification mimics that of column (4) in Table 4. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units. The coefficient showed is that of interest in an IV setting, being \widehat{assoc}_{pc} . Confidence intervals are based on standard errors clustered at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Finally, to disentangle if there are heterogeneous effects across neighborhoods, we apply the Bacon decomposition to our estimates (Bailey and Goodman-Bacon (2015); Goodman-Bacon (2018)). Goodman-Bacon (2018) shows that a Difference-in-Differences estimator is a weighted average of all possible two-group/two-period Difference-in-Differences estimators, and also shows which terms or groups matter most.¹⁹ For this case, results from the Bacon Decomposition are shown in Table 11. They evidence that the estimates previously found for intimate and anger crimes are driven by comparing treated versus never treated observations, rather than from comparison of early versus late treated units. This is shown by the weight such variation source has in comparison to the others. This last results indicates that differences between treated units is not the main driver behind

¹⁹The ‘bacondecomp’ package in Stata implements a Bacon decomposition of a Difference-in-Difference estimator with variation in treatment timing, based on Goodman-Bacon (2018)

our estimates. Hence, heterogeneity in outcomes due to different priorities by neighborhoods does not seem to be a determinant features of the analysis.²⁰ This builds to the fact that the content of the policy is less relevant than the fact of connecting people. Such analysis reinforces the evidence in favor of the community ties hypothesis.

Table 11: Bacon decomposition of Difference-in-Differences estimates for Intimate crimes

	Comparison type weight	
Difference-in-Difference estimate	-0.075	
Earlier T vs. later C	-0.080	0.060
Later T vs. earlier C	-0.036	0.050
T vs. never treated	-0.077	0.891
T vs. already treated	-	-

Notes: This table presents the Bacon decomposition of the baseline Difference-in-Differences estimates of the BSaB policy. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units. The package shows three types of comparisons, which differ by control group: (1) Always treated, a group treated prior to the start of the analysis serves as the control group; (2) Never treated, a group which never receives the treatment serves as the control group. (3) Timing groups, or groups whose treatment started at different times can serve as each other’s controls: (3.1) those treated later serves as the control group for an earlier treatment group and (3.2) those treated earlier serve as the control group for the later group. Also shown are the component due to variation in controls across always treated and never treated groups, and the “within” residual component.

7 Conclusions

In this paper we estimate the effect of bolstering community ties on local crime rates. To do so, we take advantage of the quasi-random nature of a community health policy rolled out in the city of Barcelona from 2008 to 2014 (BSaB). The policy was implemented in 12 of the 49 potential neighborhoods and covered around a quarter of the targeted population. Even though the aim of the policy itself was to improve health outcomes in these underprivileged neighborhoods, we assess whether the community feature of BSaB may have led to an increase in social tissue, and consequently to a reduction in crime.

Using a staggered Difference-in-Differences approach and administrative records from the Local Police, we find that this is the case. Concretely, there is a reduction crimes against the person, which is related to the reduction in intimate crimes. These fall by 25% and only in the short term. Drug crimes also see a reduction but in the longer term. For outcomes on offense rates, there is a reduction in that of younger individuals. Results also indicate that BSaB increases per capita associations in participating neighborhoods. They do not evidence an effect on self-rated health, mental health and unemployment

²⁰Figure A1 in the Appendix provide further results of the baseline specification when removing neighbourhoods one at a time to show that our results are not dependent on the inclusion or exclusion of a particular neighbourhood.

rates across treatment and control neighborhoods. For this, we support that the strengthening of community ties is likely to be a key mechanism. This is also backed up by the Bacon decomposition that indicates no heterogeneity on outcomes between treated units, making the meetings themselves more important than its contents.

Despite crime not being one of the specific targets of the policy itself, it is clearly indirectly linked to them, as they reflect local disparities. For this we understand that the policy was successful in achieving one of its goals. However, we further understand that improvements in policy design are needed, as some key crime categories were not affected by the program. In light of the results on the underlying mechanisms, if new initiatives are to be carried on, cooperation with existing local institutions is crucial.

This paper indicates that not only traditional policies against crime work. New and less disruptive means of reducing criminal activity in disadvantaged neighborhoods can be effective. Additionally, these policies speak from an efficiency angle. Concretely, BSaB had an annual cost of 500.000 euros in 2015. This implies a cost of 5.000 euros per annual activity, 70 euros per active participant and 2 euros per potential participant. Hence, from a cost-effectiveness perspective, the policy also evidences positive points.

Even if constructing community ties is more challenging than deploying traditional policing, this type of innovative (alternative) policies may work better in several contexts. In Buonanno et al. (2009) authors state that a policy of promotion of associational life may usefully complement traditional anti-crime policies. Moreover, Takagi et al. (2012) argue that policy-makers should not neglect policies aimed at reducing inequalities to promote social cohesion, social stability, and safer neighborhoods. A better understanding of the interactions between social cohesion and public policy is essential if we are to reduce criminal activity induced by the lack of integration of some citizens facing substandard social and economic conditions.

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Appendix

Table A1: Neighborhood characteristics: population and rent

District		Neighborhood	Pop 07	Pop 14	Rent 07	Rent 14	Low Inc.	Treatment
0	0	Barcelona City	1.603.178	1.613.393	100	100	NA	NA
1	1	el Raval	46.595	48.471	64,7	65,9	Y	Y
1	2	el Barri Gotic	27.946	15.911	86,5	98,5	N	N
1	3	la Barceloneta	15.921	15.181	66,7	84,5	Y	Y
1	4	Sant Pere, Santa Caterina i la Ribera	22.572	22.674	80,2	92,5	Y	Y
2	5	el Fort Pienc	31.521	31.785	107,9	104,5	N	N
2	6	la Sagrada Família	52.185	51.562	101,8	92,4	N	N
2	7	la Dreta de l'Eixample	42.504	43.749	137,6	165,3	N	N
2	8	l'Antiga Esquerra de l'Eixample	41.413	41.975	126,5	127,8	N	N
2	9	la Nova Esquerra de l'Eixample	58.146	57.863	116,9	109,1	N	N
2	10	Sant Antoni	37.988	38.369	103,8	97,8	N	N
3	11	el Poble Sec - Parc Montjuic	39.579	40.674	73,3	66,3	Y	Y
3	12	la Marina del Prat Vermell - Zona Franca	1.005	1.151	80,4	39,4	Y	N
3	13	la Marina de Port	29.327	30.286	80,2	72,0	Y	N
3	14	la Font de la Guatlla	10.064	10.406	90,4	77,6	Y	N
3	15	Hostafrancs	15.771	15.919	82,7	76,8	Y	N
3	16	la Bordeta	18.592	18.451	81,9	76,0	Y	N
3	17	Sants - Badal	24.085	24.245	85,9	79,6	Y	N
3	18	Sants	40.272	41.102	89,5	85,8	Y	N
4	19	les Corts	46.400	46.205	130,4	125,4	N	N
4	20	la Maternitat i Sant Ramon	23.938	23.735	127,9	112,6	N	N
4	21	Pedralbes	11.413	11.670	193,6	251,7	N	N
5	22	Vallvidrera, el Tibidabo i les Planes	4.038	4.615	146,4	162,8	N	N
5	23	Sarria	23.316	24.691	174,9	195,2	N	N
5	24	les Tres Torres	15.325	16.381	215,3	217,8	N	N
5	25	Sant Gervasi - la Bonanova	23.634	25.378	182,2	191,8	N	N
5	26	Sant Gervasi - Galvany	46.454	46.648	187,0	192,1	N	N
5	27	el Putxet i el Farro	28.990	29.041	150,2	140,2	N	N
6	28	Vallcarca i els Penitents	15.381	15.454	113,2	101,6	N	N
6	29	el Coll	7.190	7.307	91,7	81,6	Y	N
6	30	la Salut	13.072	13.256	113,0	107,3	N	N
6	31	la Vila de Gracia	50.409	50.680	101,9	118,1	N	N
6	32	el Camp d'en Grassot i Gracia Nova	34.535	34.146	104,3	103,7	N	N
7	33	el Baix Guinardo	25.816	25.587	96,6	86,6	Y	N
7	34	Can Baro	8.998	8.887	81,2	77,4	Y	N
7	35	el Guinardo	35.038	35.698	93,0	82,0	Y	N
7	36	la Font d'en Fargues	9.621	9.467	103,5	102,0	N	N
7	37	el Carmel	32.745	31.728	72,0	56,6	Y	N
7	38	la Teixonera	11.332	11.379	72,2	69,6	Y	N
7	39	Sant Genis dels Agudells	7.069	6.865	85,7	80,0	Y	N
7	40	Montbau	5.105	5.082	85,5	70,0	Y	N
7	41	la Vall d'Hebron	5.476	5.422	96,5	86,9	Y	N
7	42	la Clota	445	529	89,9	90,1	Y	N
7	43	Horta	26.638	26.591	85,9	82,2	Y	N
8	44	Vilapicina i la Torre Llobeta	25.672	25.500	83,0	64,0	Y	N
8	45	Porta	23.470	24.424	75,3	58,3	Y	N
8	46	el Turo de la Peira	15.102	15.471	65,4	50,6	Y	N
8	47	Can Peguera	2.143	2.288	49,8	51,0	Y	N
8	48	la Guineueta	15.394	15.090	82,0	56,0	Y	N
8	49	Canyelles	7.539	7.014	76,7	61,0	Y	N
8	50	les Roquetes	15.756	15.668	60,9	50,8	Y	Y
8	51	Verdun	12.301	12.239	63,8	50,8	Y	N
8	52	la Prosperitat	26.696	26.171	72,6	53,7	Y	N
8	53	la Trinitat Nova	8.011	7.462	53,0	34,7	Y	N
8	54	Torre Baro	2.105	2.682	58,0	45,6	Y	Y
8	55	Ciutat Meridiana	10.929	10.356	59,4	39,2	Y	Y
8	56	Vallbona	1.267	1.353	51,6	39,9	Y	Y
9	57	la Trinitat Vella	9.992	10.268	74,8	45,9	Y	N
9	58	Baro de Viver	2.397	2.508	44,5	60,5	Y	Y
9	59	el Bon Pastor	12.332	12.758	66,2	59,6	Y	Y
9	60	Sant Andreu	55.171	56.496	85,9	76,6	Y	N
9	61	la Sagrera	28.469	28.914	88,1	74,9	Y	N
9	62	el Congres i els Indians	13.896	14.076	86,5	72,7	Y	N
9	63	Navas	21.454	21.949	92,9	83,3	Y	N
10	64	el Camp de l'Arpa del Clot	38.604	38.130	93,4	80,9	Y	N
10	65	el Clot	26.796	27.082	88,5	81,0	Y	N
10	66	el Parc i la Llacuna del Poblenou	13.104	14.814	103,2	88,6	N	N
10	67	la Vila Olímpica del Poblenou	8.783	9.391	132,8	150,8	N	N
10	68	el Poblenou	30.181	33.425	94,5	95,4	Y	N
10	69	Diagonal Mar i el Front Marítim del Poblenou	9.775	13.351	101,1	168,8	N	N
10	70	el Besos i el Maresme	22.652	23.191	61,7	58,9	Y	Y
10	71	Provençals del Poblenou	18.731	20.184	85,7	91,7	Y	N
10	72	Sant Martí de Provençals	26.261	26.018	81,5	67,6	Y	N
10	73	la Verneda i la Pau	29.452	28.903	74,8	57,2	Y	Y

Notes: This table presents population, rent and treatment characteristics of all 73 neighborhoods in the city of Barcelona in 2007 and 2014. Source: Own construction.

Table A2: BSaB activities by scope

Intervention	Target population	Neighborhoods
Early Childhood and Family		
Healthy leisure in sports	Primary-school students	Poble Sec
Healthy leisure in sports	Middle-school students	Roquetes, Bon Pastor, Baro de Viver
parenting skills programs	Parents of children aged 3-5	El Born, Torre Baro, Ciutat Meridiana, Vallbona, Barceloneta
Healthy cooking workshops	Parents of children aged 3-17	Poble Sec
Support for extracurricular activities	Primary-school families	Roquetes, Barceloneta
Adolescents and young adults		
Healthy leisure in sports	High-school Students	Roquetes, Poble Sec, El Born, Torre Baro, Ciutat Meridiana, Vallbona
Healthy leisure at night	Those aged 14-18	Torre Baro, Ciutat Meridiana, Vallbona
Sexual health and counseling	Those aged 14-25	Torre Baro, Ciutat Meridiana, Vallbona, Raval
Sex education on contraception	Under 20s	Torre Baro, Ciutat Meridiana, Vallbona, Bon Pastor, Baro de Viver
Drug Counseling	Under 21s	Roquetes, Poble Sec, Raval
Drugs, violence, and endogamic groups	15-29 year-olds at psycho-social risk	Bon Pastor, Baro de Viver, Raval
Improving self-esteem, empowerment and integration	Pakistani women aged 14-21	El Besos i el Maresme
Adults		
Sex education for adults	Women aged 20-50	Torre Baro, Ciutat Meridiana, Vallbona, Bon Pastor, Baro de Viver
Tai chi in the park	40 and above	Roquetes, Poble Sec, El Born, Torre Baro, Ciutat Meridiana, Vallbona, Bon Pastor, Baro de Viver, El Besos i el Maresme
Obesity, sedentary lifestyle, stress, anxiety, depression	Adults and the elderly	Bon Pastor, Baro de Viver
Elderly		
Memory Groups	The elderly	Roquetes
Help to take a walk around the neighborhood	The elderly	Poble Sec, El Born, Torre Baro, Ciutat Meridiana, Vallbona, El Besos i el Maresme
How to be healthy	The elderly	El Born, Bon Pastor, Baro de Viver, El Besos i el Maresme
All interested parties		
Alcohol abuse	Everyone	Barceloneta
Tobacco addiction	All smokers	Roquetes, Poble Sec
Home-made remedies	Everyone	Roquetes

Notes: This table presents all initiatives undertaken under the BSaB scope. They are categorized by the aim of the intervention, they indicate who is their target population and in which neighborhoods they took place. Source: Own construction.

Data description

Table A3: Description of main variables

Variable	Description	Source	Frequency availability
Crime counts	Registered criminal acts	Catalan police	Geocoded; Exact time
Offender counts	Registered offenders	Catalan police	Geocoded; Exact time
Victim counts	Registered victims	Catalan police	Geocoded; Exact time
Population	Registered inhabitants	Barcelona City Hall	Neighborhood; Year
Crime rate	Crime counts per 1,000 inhabitants	Police and City Hall	Neighborhood; Month
Victim rate	Victim counts per 1,000 inhabitants	Police and City Hall	Neighborhood; Month
Associations	Per capita local associations	Catalan regional authorities	Neighborhood; Month
House prices	House market prices per square meter	Barcelona City Hall	Neighborhood; Month
Unemployment	Registered unemployment rate	Barcelona City Hall	Neighborhood; Month
Tourism	Per capita visitors to neighborhood tourist sites	Barcelona City Hall	Neighborhood; Month

Notes: This table presents a description of the main variables under analysis. It contains a brief description of how each is constructed, its sources and the frequency for which they are available. Source: Own construction.

Table A4: Broad and detailed crime categories

Broad	Share	Detailed	Share
Against Person	8.9	Family	0.7
		Gender violence	2.0
		Bodily harm	3.0
		Murder	0.1
		Sexual	0.3
		Threatening behavior	2.5
		Other	0.3
		Against Property	86.6
		Fraud	5.2
		Auto theft	11.4
		Robbery	14.5
		Theft	47.1
		Other	4.5
		Drugs	0.7
		Environment	0.2
		Disobedience	1.8
		Road safety	1.8
Total	1		1

Notes: This table presents a categorization of all crimes available in our administrative database from the Catalan Police. We present both a broad categorization (left panel, 3 categories) and a detailed one (right panel, 17 categories). Source: Own construction from Catalan Police data.

Table A5: Descriptive statistics, crime rates per 1,000 inhabitants. 2007-2014

Variable	All Neighborhoods		Potentially participating	
	Mean	Std. Dev.	Mean	Std. Dev.
All	10.235	15.790	8.758	13.088
Person	0.735	0.882	0.759	0.987
Property	8.957	14.150	7.459	11.116
Other	0.543	1.445	0.540	1.641
Intimate	0.216	0.258	0.239	0.299
Anger	1.465	1.916	1.497	2.195
Drugs	0.065	0.271	0.044	0.181
Family	0.052	0.108	0.057	0.125
Gender violence	0.140	0.208	0.158	0.243
Bodily harm	0.284	0.476	0.271	0.500
Disobedience	0.176	0.424	0.167	0.436
Sexual	0.024	0.073	0.023	0.083
Threatening behavior	0.205	0.339	0.222	0.401
Obs	7,008		4,704	
Income <90% median	0.671		1	
Treatment group			0.245	

Notes: This table presents descriptive statistics for different crime rates under analysis for the 2007-2014 period. Mean and standard deviation are shown for the whole city of Barcelona (73 neighborhoods) and for the potentially treated units (49 neighborhoods). Source: Own construction from Catalan Police data.

Table A6: Descriptive statistic, offense rates per 1,000 inhabitants. 2007-2014

Variable	All Neighborhoods		Potentially participating	
	Mean	Std. Dev.	Mean	Std. Dev.
Men	4.703	8.990	4.492	9.417
Women	1.229	2.278	1.150	1.922
Men under 18	1.387	3.178	1.274	3.331
Men 18-25	14.755	28.065	13.322	28.519
Men 25-35	7.744	19.940	7.717	23.226
Men 35-45	6.038	14.700	6.177	16.887
Men 45-55	4.119	8.677	4.048	9.206
Women under 18	0.540	1.764	0.487	1.575
Women 18-25	4.399	9.488	4.001	9.555
Women 25-35	2.048	4.773	2.045	5.303
Women 35-45	1.584	3.351	1.581	3.611
Women 45-55	1.165	2.934	1.221	3.307

Notes: This table presents descriptive statistics for different offense rates under analysis for the 2007-2014 period. Mean and standard deviation are shown for the whole city of Barcelona (73 neighborhoods) and for the potentially treated units (49 neighborhoods). Source: Own construction from Catalan Police data.

Table A7: Descriptive statistics, victim rates per 1,000 inhabitants. 2007-2014

Variable	All Neighborhoods		Potentially participating	
	Mean	Std. Dev.	Mean	Std. Dev.
Men	7.743	10.888	6.848	10.650
Women	6.526	9.311	5.519	7.701
Men under 18	1.366	2.425	1.260	2.563
Men 18-25	16.492	33.832	13.163	32.469
Men 25-35	9.630	16.159	8.584	16.977
Men 35-45	9.167	15.149	8.694	17.224
Men 45-55	14.133	21.686	12.709	22.073
Women under 18	1.545	3.886	1.228	3.528
Women 18-25	19.318	39.312	15.172	37.343
Women 25-35	8.603	12.291	7.620	12.024
Women 35-45	7.015	9.390	6.354	9.275
Women 45-55	11.198	17.678	9.856	16.704

Notes: This table presents descriptive statistics for different victimization rates under analysis for the 2007-2014 period. Mean and standard deviation are shown for the whole city of Barcelona (73 neighborhoods) and for the potentially treated units (49 neighborhoods). Source: Own construction from Catalan Police data.

Table A8: Descriptive statistics, control variables. 2007-2014.

Variable	All Neighborhoods			Potentially participating		
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Associations (per capita)	7,008	1.896	1,763	4,704	1.192	1.21
Tourism (tickets/population)	7,008	1.92	7.98	4,704	2.39	9.54
Reg. unemployment (rate)	5,256	0.07	0.02	3,528	0.08	0.02
House prices (euros/sqm)	4,762	2,362	1,005	3,087	2,023	893

Notes: This table presents descriptive statistics for different explanatory variables of our analysis for the 2007-2014 period. They include local associations per capita, registered unemployment, housing prices and a proxy for touristic pressure. Mean and standard deviation are shown for the whole city of Barcelona (73 neighborhoods) and for the potentially treated units (49 neighborhoods). Source: Own construction from Barcelona City Hall data.

Table A9: Logit regression pre-intervention

P(Treatment)=1	Coef.	Std. Err.	z	P>z
Income	-0.12	0.15	-0.64	0.520
Population	0.00	0.00	0.69	0.490
Mortality	0.06	0.06	1.11	0.68
Teenage birth rate	0.01	0.34	0.03	0.976
Non-Spanish population	0.00	0.00	0.97	0.333
Pensions	-0.02	0.03	-0.61	0.544
House prices	0.52	0.71	0.74	0.461
Overall crime	0.00	0.00	0.05	0.958
Per capita assoc	-0.41	0.62	-0.66	0.509
Tourism	0.07	0.11	0.61	0.540
Prob LR>chi2 =0.0000 ; Pseudo R2=0.7554				

Notes: This table presents the results of a logistic regression of the probability of a neighborhood being treated on several sociodemographic characteristics in a pre-treatment period (average in year 2007). Robust standard errors. Source: Own construction from Barcelona City Hall data.

Table A10: Panel logit regression for intervention timing

P(BSaB)=1	Coef.	Std. Err.	z	P>z
Income	0.03	0.29	0.090	0.925
Population	0.00	0.00	-0.880	0.377
Mortality	0.02	0.02	1.350	0.178
Teenage birth rate	0.40	0.34	1.180	0.239
Non-Spanish population	0.00	0.00	0.880	0.378
Pensions	-0.04	0.04	-1.200	0.230
House prices	-0.51	0.19	-2.730	0.006
Overall crime	0.00	0.00	1.140	0.253
Associations	0.42	0.55	0.770	0.440
Tourism	0.04	0.13	-0.06	0.956
/lnsig2u	5.26	0.53		
sigma_u	13.89	3.66		
rho	0.98	0.009		
Prob W>chi2 =0.01056 ; Prob LR (rho=0)>chi2 =0				

Notes: This table presents the results of a panel logistic regression of the probability of a neighborhood being treated on several sociodemographic characteristics, for the 2007-2014 period. Robust standard errors. Source: Own construction from Barcelona City Hall data.

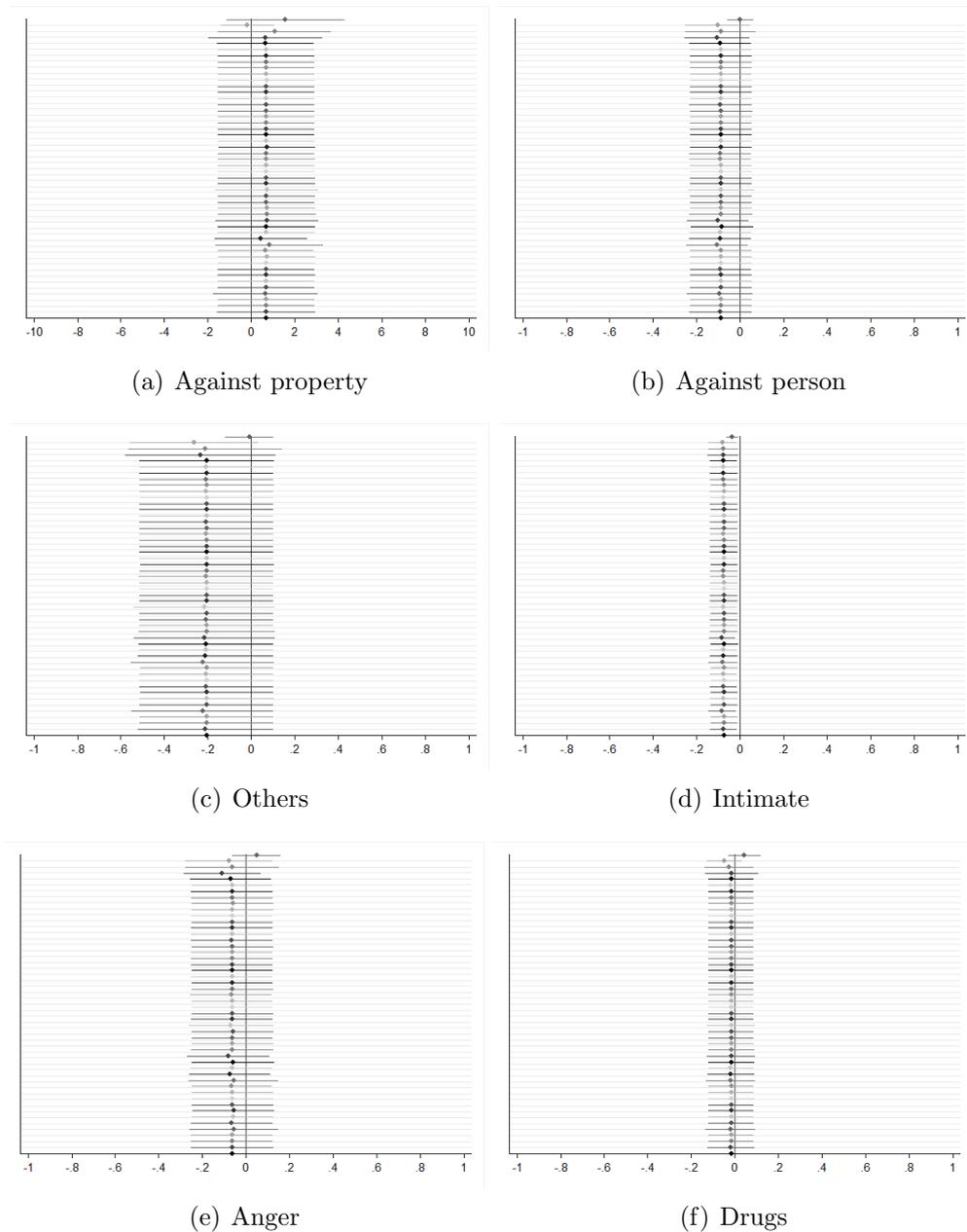
Other outcomes for baseline estimates

Table A11: Baseline results for offenders and victims by age and gender

	Off. U18		Off. 18-25		Off. 25-35		Off. 35-45	
	Male	Female	Male	Female	Male	Female	Male	Female
BSaB	-0.212 (0.143)	-0.656*** (0.235)	-1.634*** (0.562)	-1.169 (0.948)	-0.816 (0.547)	-0.185 (0.165)	-0.063 (0.321)	-0.332 (0.300)
	Vict. U18		Vict. 18-25		Vict. 25-35		Vict. 35-45	
	Male	Female	Male	Female	Male	Female	Male	Female
BSaB	0.158 (0.199)	0.275 (0.213)	5.455 (4.022)	6.108 (5.005)	1.346 (0.910)	0.592 (0.781)	1.031 (0.684)	0.763 (0.825)
Observations	3,264	3,264	3,264	3,264	3,264	3,264	3,264	3,264
Neigh. FE	Y	Y	Y	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Neigh-Time trends	Y	Y	Y	Y	Y	Y	Y	Y

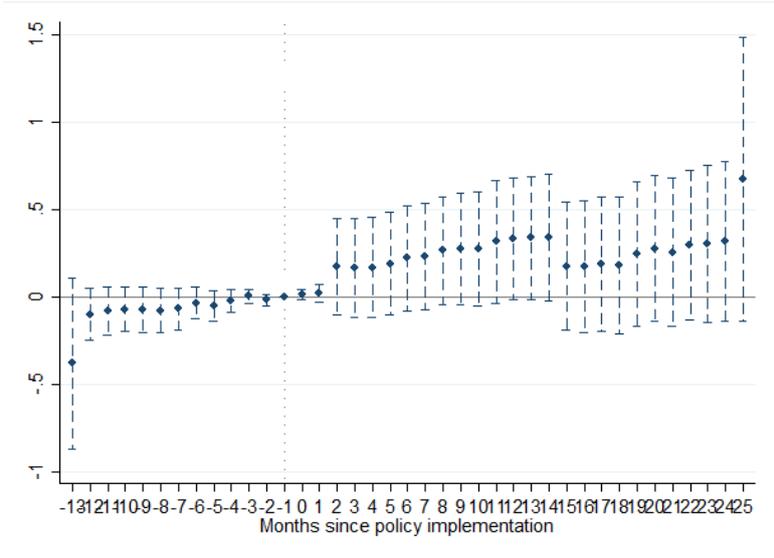
Notes: This table reports the results of the difference-in-differences estimation following equation (2) for the 2008-2014 period, with controls as those of column 4 in Table 4. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing is differs across units. The coefficient showed is that of interest in a DiD setting, being $Treated \cdot Post$. Confidence intervals are based on standard errors clustered at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A1: Difference-in-Differences estimates when removing neighborhoods one at a time



Notes: These graphs report the results of the difference-in-differences estimation removing neighborhoods one at a time. The specification follows equation (2) for the 2008-2014 period, with controls as those of column 4 in Table 4. The coefficient showed is that of interest in a DiD setting, being $Treated \cdot Post$. Confidence intervals are based on standard errors clustered at the neighborhood level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A2: Event study exercise on the effects of BSaB on per capita local associations



Notes: This graph reports the results of an event study exercise derived from the baseline Difference-in-Differences estimation for the 2008-2014 period for per capita local associations. The observational unit is a neighborhood-month pair. Treated units are those in which the BSaB policy took place, while those in which it did not are controls. Treatment timing differs across units. Confidence intervals are based on standard errors clustered at the neighborhood level.

2013

- 2013/1, **Sánchez-Vidal, M.; González-Val, R.; Viladecans-Marsal, E.**: "Sequential city growth in the US: does age matter?"
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2014

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2015

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2016

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- 2016/23, Daví-Arderius, D.; Sanin, M.E.; Trujillo-Baute, E.: "CO2 content of electricity losses"
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2017

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2018

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2019

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- 2019/2, Brutti, Z.; Montolio, D.:** “Preventing criminal minds: early education access and adult offending behavior”
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- 2019/5, Garcia-López, M.A.; Jofre-Monseny, J.; Martínez Mazza, R.; Segú, M.:** “Do short-term rental platforms affect housing markets? Evidence from Airbnb in Barcelona”



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