Journal of Economic Behavior and Organization 191 (2021) 916–945

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Bolstering community ties as a mean of reducing crime[☆]

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ARTICLE INFO

Article history:
Received 3 February 2021
Revised 13 September 2021
Accepted 17 September 2021
Available online 19 October 2021

JEL classification:
C23
I18
J18
K42

Keywords:
Crime
Community ties
Difference-in-differences
Public policy

ABSTRACT

Recent evidence indicates that alternative policies based on building community can reduce crime, especially in disadvantaged neighborhoods. In this paper we study the effects on local crime rates of bolstering community ties. We take advantage of the quasi-random deployment of a community health policy (*Barcelona Salut als Barris*, BSaB) that aims to improve health outcomes and reduce inequalities in the most disadvantaged neighborhoods through community-based initiatives. To test whether BSaB reduces crime, we follow a difference-in-differences approach and make use of detailed data from local police and Barcelona City Council administrative records. We find that BSaB significantly reduces a category we term "intimate crimes" in the short term and drug crimes in the long term. The young offender crime rate is also lowered. Evidence suggests that this is due to tighter-knit communities. These results provide evidence in favor of non-traditional crime prevention policies.

1. Introduction

The existing 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). Although 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), such networks also increase the opportunity cost of committing a crime. The latter 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". Coleman (1988) already related strength of social sanction to social network closure. Additionally, systemic models of com-

☆ We are grateful to the Editor and two anonymous referees for helpful comments and insights that helped improve this paper. We thank Elia Díez and Maribel Pasarín at the Barcelona Public Health Agency (ASPB) and IGOP researchers Raquel Gallego and Ernesto Morales at the Autonomous University of Barcelona (UAB) for their insightful comments on the program we report on. We thank Daniel Albalate, Antonio di Paolo, Emma Duchini, Kilian Heilmann, Juan S. Morales, Matteo Sandi, Ana Tur-Prats, Judit Vall and participants at SMYE Brussels, ALCAPONE 2019, EM-UEA Amsterdam, UEA Philadelphia, LACEA 2019 and EEA Manchester for feedback on previous versions of this paper. All remaining errors are our own. This work was supported by the Spanish Ministry of Economy and Competitiveness (grant no. ECO2016-75912-R), RECERCAIXA (grant 2015ACUP00037) and Spanish Ministry of Science, Innovation and Universities (grant no. PID2019-109813RB-I00).

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https://doi.org/10.1016/j.jebo.2021.09.022
0167-2681/

munity organization are built on the notion that well-developed local network structures reduce crime (Flaherty and Brown 2010). This reduction is related to the fact that networks may increase returns on non-criminal activities and raise detection probabilities. Community-based interventions and initiatives can play a crucial role in this regard, 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 highly relevant to the economics of crime. The ultimate goal is to better understand the empirical determinants of criminal activity, how social networks deter or encourage them, and how they interact with socioeconomic factors. Specifically, in this paper we argue that initiatives that bolster community ties in disadvantaged neighborhoods can succeed in reducing local crime rates, especially for crimes that are not driven by a monetary incentive. This postulate relies on the aforementioned systemic models of community organization (Flaherty and Brown 2010) and on social disorganization theories (Sampson 1988). We test our hypothesis by analyzing a community health policy implemented in a quasi-random fashion in the city of Barcelona. *Barcelona Salut als Barris* (BSaB), meaning "Barcelona Health in the Neighborhoods", was deployed in some of the city's most disadvantaged neighborhoods to reduce local disparities.[1] It was run in each neighborhood by the local health center together with local social agents and the community itself. To analyze its effects, we apply a difference-in-differences methodology combined with a rich set of controls and time and space fixed effects. For our data, we use a unique geocoded reported crime dataset provided by the local police force, which we enrich with Barcelona City Council sociodemographic controls.

Estimates suggest that the observed reduction in criminal actions can be attributed to the implementation of BSaB. Specifically, we find that offense rates for young offenders drop in neighborhoods that benefit from BSaB. The policy also reduces crimes against persons as well as crimes involving a very close personal link between offender and victim, which we label "intimate crimes". The reduction is close to 28% and only occurs in the short term. We also find a reduction in drug crimes one year after the policy is implemented. Finally, our evidence suggests that results are not due to improved health or unemployment in the participating neighborhoods, but rather are linked to a more robust social fabric. This result is supported by an increase in the association density in participating neighborhoods.

The novelty of this research resides in the following factors: (1) The policy deployment sequence provides a conditionally exogenous variation in the drivers of community ties at a low geographical level, which allows us to determine causal links. In this way, we answer a crucial question by adding to the existing causal evidence on the effects of social ties on crime. (2) Our research uses a detailed geocoded database that includes recorded victims, offenders, and crime typologies. By adding socioeconomic variables, we assemble detailed data on local crime and other characteristics within the city of Barcelona. This data enhances the accuracy and richness of our analysis, as it allows us to carry out several heterogeneity exercises. (3) This work contributes to research conducted outside the United States and considers a city whose residents are heterogeneous in terms of economic and demographic characteristics. Our findings contribute to the academic research and offer specific guidance for policy-making to deter criminal activity by moving beyond traditional approaches. (4) This case study may benefit other cities, given that the policy recommendations that emerge apply to similar urban settings. Together, these features constitute the external validity of our exercise.

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. After presenting our main results in Section 6, in Section 7 we provide 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) detect a large number of social interactions in criminal behavior. The authors present a model in which social interactions explain the high cross-city variation in crime rates in the United States. 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 for crime is highest in petty crimes and lowest in murder and rape.

There are different approaches to crime prevention, and measures to fight crime can broadly be categorized as either "hard" or "soft" policies. Hard policies involve heavy policing and sturdy prosecuting measures, whereas soft policies focus on reducing crime-triggering disparities. Contributions to the literature have shown that, in many circumstances, "tough-on-crime" measures can exacerbate the situation and impose a high cost to society, in both monetary and welfare terms. As an alternative, innovative strategies to prevent crime assign a crucial role to new societal agents. Lewis and Salem (1981) indicate that programs with a social control perspective strengthen the local community's capacity to exert social control. Cozens (2008) argues that crime prevention through environmental design has potential benefits for public health and may deliver safer environments. The "soft" policies set of strategies is of particular importance in deprived areas, where social

[1] Even if BSaB can be considered a community health program, interventions within it are broad and include a wide range of activities, such as organized exercise, healthy recreational activities for youngsters, activities to help individuals with drug related problems, or a parenting skills program (see Section 3.1 for further details).

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 prevention programs" should "engage innovative mechanisms for investing in crime prevention efforts." Lawless (2006) analyzes the New Deal for Communities program, an English area-based initiative that aims to transform deprived neighborhoods. 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. Machin et al. (2011) analyze a law that changed the compulsory school leaving age in England and Wales and show significant decreases in property crime. The authors find that improving education can enhance social benefits and reduce crime.

Meanwhile, the literature has extensively debated social capital: what it is and how to measure it. Putnam et al. (1994) set the stage for such considerations when analyzing the effects of social engagement. Since then, social capital has been defined and measured in several different ways. Jackson (2020) considers seven forms of social capital. The author 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 definition is the one we refer to throughout this paper.

It is most certainly the case that community capital can play an important role in many economic spheres, and the economics of crime is a significant case in point. A number of papers focus on social capital as a driver of crime at the local geographical level, including Hirschfield and Bowers (1997), Lederman et al. (2002), Buonanno et al. (2009) and Akçomak and Ter Weel (2012). However, the results do not lead to clear conclusions as they focus on different types of crimes and different definitions of social capital. While Buonanno et al. (2009) find that associational networks have a negative and significant effect on property crime and Lederman et al. (2002) state that trust has a significant and negative effect on violent crime rates, 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 negative relationship between social cohesion and crime levels in disadvantaged areas.

More recently, and regarding the causal impact of social capital on crime, Damm and Dustmann (2014) state that social interactions are an important channel through which neighborhood crime affects individual criminal behavior, particularly as regards to violent crimes by young males. Additionally, Sharkey et al. (2017) incorporate what they term the "systemic" model of community life[2] and estimate the causal effect on violent crime reduction of non-profits focused on reducing violence and building stronger communities. The authors estimate that a higher presence of organizations focusing on crime and community life achieves significant reductions in violent and property crime. Similarly, García-Hombrados (2020) investigates the 2010 earthquake in Chile and finds that it strengthened community life and ultimately led to a decrease in crime in the affected neighborhoods. The author presents robust estimates consistent with an informal guardianship mechanism reported after natural disasters. The improvement in social capital at the community level facilitated cooperation among neighbors and boosted the adoption of community-based measures to prevent crime.

Regarding community-based public initiatives to fight crime, Gonzalez and Komisarow (2020) study the effect of community-based monitoring on crime in the context of a school safety initiative, finding that overall crime in treated blocks drops by 17% relative to non-treated blocks. Moreover, and for the same program, Sanfelice (2019) provides a cost-benefit analysis showing the program to be more cost-effective than police presence. Previous research by Wilson and Chermak (2011) indicates that a critical area to study is the impact of community-based programs on cohesiveness to reduce community violence and to identify which program components are linked to reductions in violence. The authors enumerate actions to be included in a community improvement strategy, including reducing the isolation of neighborhood residents and linking them to social service organizations as well as linking organizations to each other.

Our conceptual framework assumes that BSaB, a local social program, can increase trust and cooperation among participating individuals and their contact networks. Such a feature of the program can relate to the contact hypothesis (Allport et al. 1954). This theory suggests that properly managed contact should reduce issues such as stereotyping, prejudice, and discrimination, which commonly arise between individuals with different backgrounds. This kind of contact leads to better intergroup and personal interactions. Relatedly, we can also frame our theoretical predictions in the influential work by Putnam (1993). The author established that social interactions and cooperation increase among members of community associations. Having been exposed to higher degrees of civic engagement, these individuals develop wider social networks, heightened norms of reciprocity, and higher levels of trust among individuals.

[2] Sociologists also have devoted efforts to understanding the link between social capital and crime rates. They rely on social disorganization theory and systemic models of community attachment. Social disorganization is defined as the inability of a community structure to realize the shared values of its residents and maintain adequate social controls (Sampson 1988; Sampson and Groves 1989). This theory has recently been linked to the concept of social capital, defined as those features of social organization (networks, norms of reciprocity, and trust) that facilitate cooperation among citizens for mutual benefit. The systemic model of community attachment (Flaherty and Brown 2010) emphasizes the effect of community structural characteristics on neighborhood friendship and associational ties and their effect on informal social control and crime levels. The systemic model hypothesis is that more extensive social ties decrease crime rates since communities with more comprehensive friendship and associational ties have more significant potential for informal social control due to social cohesion. Regarding empirical contributions, 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 in part on the racial makeup of the neighborhood. 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 citizens' social relations. 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.

Considering these strands of the literature, the impact of increased social capital on crime is a priori uncertain. While social networks could certainly act as an information mechanism for illegal activities, norms of reciprocity and trust could conversely increase the social cost of crime. In our setup, we understand that the design of BSaB sets the appropriate conditions for interpersonal contact to lead to higher cooperation and improved personal connections. Criminology literature has extensively used contact theory to explain reductions in offense rates among individuals in specific circumstances. BSaB could be another such instance. By design, BSaB reached different target populations such as elderly persons, young people, or migrants. Because of this, its effects relate to the type of local social capital framed by Putnam (1995) as "bridging" social capital, a type that connects people from different networks and backgrounds and increases trust and reciprocity among individuals. These features may lead to fewer offenses due to, for instance, a higher neighborhood guardianship mechanism.[3]

3. Institutional setup: *Barcelona Salut als Barris*

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 vital in the planning, design, and delivery of local services. According to the LGA, such action can help build a community and develop social capacity by creating social networks. Improving community cohesion and safety are mentioned among its many benefits.

Community action is defined by the Barcelona City Council as "a process of stimulating cooperative social relationships between members of a community, a human collective that shares space and a sense of belonging that results in reciprocal links and support, and that motivates members to become central agents in the improvement of their own reality" (Ajuntament de Barcelona 2005). Therefore, the purpose 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 the individual sphere.

In 2005, two local health authorities in the city of Barcelona, namely the Barcelona Public Health Agency (ASPB)[4] and the Barcelona Healthcare Consortium (CSB), began to work with a number of different stakeholders from all ten districts in the city to develop the BSaB community health program, which aims to improve health outcomes and reduce inequality between the disadvantaged neighborhoods and the rest of the city. The program has continued to operate without interruption since 2008.[5] BSaB is implemented through community-based interventions and targets neighborhoods where per capita income is below 90% of the city median.[6]

Prior to our research, local authorities had already performed some analyses of BSaB. While Díez et al. (2012) describe the experience, achievements, lessons, and challenges of implementation, Sánchez-Ledesma et al. (2017) characterize the priority-setting procedure. They state that the community approach to health stimulates and empowers the community, encourages mutual support, and raises its members' profile by making them responsible for improving their reality. Additionally, Barbieri et al. (2018) state the need to identify key indicators for measuring and characterizing community action for health and devise an index to that end. However, this literature on BSaB primarily provides descriptive analyses, and causal analysis is yet to be undertaken.

3.1. Description of the program

BSaB was deployed between 2008 and 2014 in 12 of the 49 potentially participating neighborhoods, out of the 73 in Barcelona city. The 49 candidate neighborhoods were those considered deprived, in which average per capita income was below 90% of the city median.[7] The 12 neighborhoods finally included in BSaB are home to 15% of the city population and 25% of the potentially participating population. A key feature is that the progressive roll-out of BSaB to these neighborhoods did not follow any specific pattern in terms of socioeconomic or demographic characteristics. This feature allows it to be regarded as a quasi-random experiment.[8] The deployment and timing of BSaB are shown in Fig. 1 and Table A3 of the Appendix.

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3 Putnam (1995) also refers to a contrasting type of social capital: the bonding type. Bonding social capital develops in homogeneous groups, and may reduce global social capital by inducing out-group antagonism.

4 All acronyms are derived from the original name in Catalan.

5 The program was kept running despite changes in governing party, both at regional and at city level. In 2005, the center-left Socialist Party was in power both in Catalonia (the regional government) and in Barcelona (Barcelona City Council). It was then ousted from both by the center-right *Convergència i Unió* coalition in 2010 and 2011 respectively. Since 2015, Barcelona City Council is led by *Barcelona en Comú*, a left-leaning party.

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

7 Neighborhoods receiving BSaB were also "deprived" in terms of social ties. See Table A2 in the Appendix for a correlation matrix between social ties (proxied by local associations) and socioeconomic indicators (income, unemployment, house prices, and other social conditions).

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

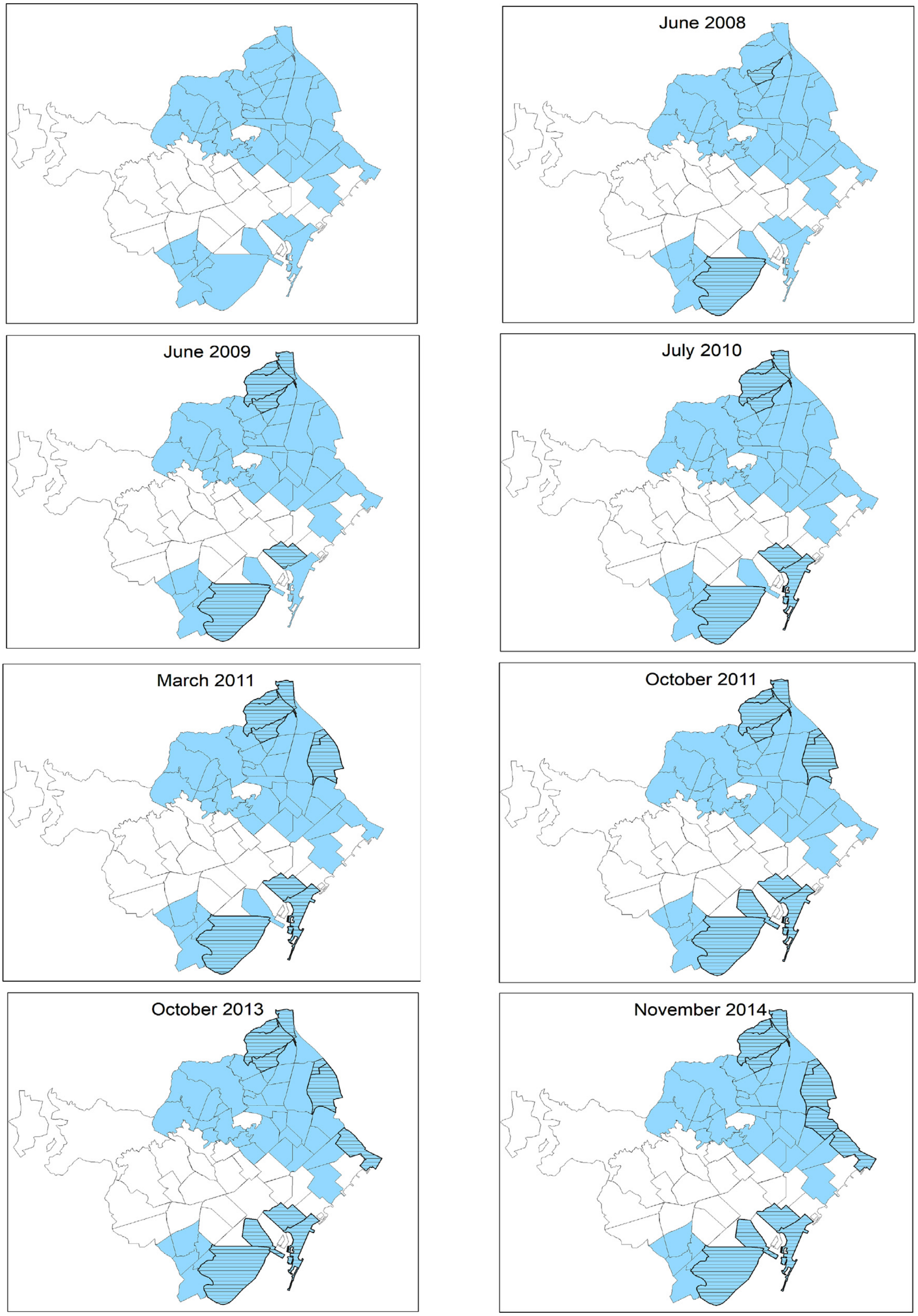


Fig. 1. Deployment of BSaB interventions in the city of Barcelona. *Notes:* Dark-shaded areas are all neighborhoods potentially targeted by BSaB on the basis of per capita income (49 neighborhoods). Hatched areas are the participating neighborhoods (12 neighborhoods by 2014).

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

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

Interventions were intended to facilitate non-competitive physical activity, social relationships, healthy recreation, health literacy, and sexual health. They included substance addiction care and prevention, training and job placement, sexual and reproductive health advice, parenting skills training, mental health care, and healthy leisure workshops (Díez et al. 2012; Generalitat de Catalunya 2014; Comissionat de Salut 2016).[9] However, each neighborhood received a unique combination of interventions, making a heterogeneous analysis by intervention type unfeasible.

By way of example, in the neighborhood of Ciutat Meridiana, one of the activities was named *Divendres Alternatius* ("Alternative Fridays") and aimed to provide healthy leisure activities for adolescents aged 14–18. Its first edition included over 200 individuals, of whom 73% were male and around 60% were foreign. Respondents to user satisfaction surveys were very satisfied, and a quarter of participants stated that the activities should be run more often. Another example is the "SIRIAN" program at Bon Pastor neighborhood, which aimed to increase awareness of contraception, reproductive health, and 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 key factor in the policy roll-out is that these interventions were mainly managed and run in each neighborhood by the local health center (CAP) alongside a community group that included civic entities, community associations, and social workers. There are 70 local health centers citywide, and most of them have catchment areas that exactly match a single neighborhood.[10] Each CAP has a specific area and population under its responsibility, as set by government rules. Hence, spillovers from one neighborhood to another are highly unlikely.[11] Importantly, all interventions were run from the beginning with a community perspective, by involving the steering group, the local community, and the authorities. This communal component led us to hypothesize that BSaB boosted community ties and in this way reduced local crime rates.

3.2. Potential mechanisms: the community component

Theoretically, the BSaB policy might affect criminal activity via different pathways. Initially, the most obvious is health: improved health status of the target population might reduce criminal activity. For instance, Bondurant et al. (2018) estimate the effects of expanding access to substance-abuse treatment on local crime rates in United States counties, and indeed find decreases in local rates of violent and financially motivated crimes, but not immediately.

However, due to the characteristics of BSaB, we argue and later show that improvements in health are not the primary outcome driver. Instead, we claim that a mechanism of community ties is operating here.[12] As previously mentioned, a body of research documents the association between community capital and not 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 substantial social capital can 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 neighborhood residents, closer neighborhood links are expected. As a result, informal social control may also arise, increasing the probability of offenders' being arrested, potentially leading to a fall in the area's crime rate. Following Putnam et al. (1994), Buonanno et al. (2009) and Guiso et al. (2011) among others, we use the number of associations per capita, or association density, as a measure of community ties at the neighborhood level.

Several findings can help disentangle the underlying mechanisms in this setting. Firstly, we estimate the timing of the effects in criminal activity through an event-study exercise. We posit that if the crime rate responds relatively fast to the policy, it is harder to attribute the reaction to improved health among the population. Health-related improvements should take some time to materialize, as in Bondurant et al. (2018). Secondly, we assess whether BSaB impacts local association density, our proxy for community ties. Thirdly, we examine whether there have been any changes in the health status of residents in participant and non-participant neighborhoods. Additionally, we analyze whether there have been changes in

[9] See Table A4 in the Appendix for a complete list of activities run within BSaB.

[10] Every resident in Barcelona is assigned to a CAP according to their home address. In a sense, their catchment areas (called a "basic health area") can be seen as akin to school districts in the United States. Basic health areas largely match neighborhoods.

[11] The unlikeliness of spillovers was also confirmed to us by the authorities running the BSaB program.

[12] We also test whether the program improved local employment prospects by analyzing whether unemployment figures are significantly affected in treated areas as opposed to non-treated areas. Results are shown in the following sections.

registered unemployment, as some of the activities aimed to improve employability and thus might affect engagement in criminal behavior.

4. Data

The primary data source in this paper is a geocoded administrative dataset of all crimes reported in Barcelona from 2007 to 2014. This data is provided by the local police force (*Mossos d'Esquadra*, the regional police). It comprises all reported crimes and includes information on the exact time and place where the crime occurred and the crime type. It contains over one and a half million entries. This degree of detail allows us to estimate the effects of BSaB at a relatively high time frequency and a low geographical level. The dataset also provides unique information on the offenders and victims, where available, which also accounts for some basic sociodemographic characteristics of those involved. Since BSaB targets specific subpopulations through different interventions, it is possible to evaluate whether the targeted groups are more or less likely, after the intervention, to become offenders or victims of a crime.

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

We also account for a set of socioeconomic variables that enrich our main analysis. First of all, we have information from the regional government (*Generalitat de Catalunya*) on registered local associations (registration date and aims), which allows us to understand their presence and prominence. This association density is our proxy for community ties. Additionally, in order to account for business cycles, we have information on registered unemployment and on housing prices per square meter.[13] Finally, we also account for a proxy for tourism pressure.[14] This last variable accounts for potential confounders resulting from the tourism industry, a highly relevant factor in a city highly exposed to sizeable tourism inflows. These last three variables (registered unemployment, housing prices, and tourism pressure) are built from information provided by Barcelona City Council (*Ajuntament de Barcelona*). While local associations, housing prices, and registered unemployment are considered at the neighborhood level, the tourism pressure index is considered at the district level, as a single neighborhood may be too small an area for this type of input. All of these variables are available at the neighborhood-year-month level. A description is shown in Table A5 of the Appendix.

The final dataset of this study comprises 4704 observations at the neighborhood-year-month level, the number resulting from 12 months per year in 8 years for the 49 neighborhoods potentially targeted for BSaB intervention. Crime, offense, and victim rates per 1000 inhabitants are built for each observation, and the socioeconomic variables previously mentioned are available.

4.1. Constructing crime typologies

The database provided by the local police is rich in many ways, one of which is the way crime is codified. There are over 300 codes, covering more than 190 articles of the Spanish Criminal Code. Even though having such a large amount of information can be of great value for research, this codification is not functional for the present analysis. Based on the 300-plus crime types, we construct 17 detailed crime categories, which we then group into 3 broad categories. Both categorizations cover the entire range of recorded crime types. Details of our crime classifications are presented in Table 1.

Moreover, given our setting, we understand that different and more specific crime categories should be designed. To this end, we also construct two less-traditional crime categories that are transverse to the first set, and which are presented in Table 2. First, we create a crime category we name "intimate crimes", which covers the detailed categories of family, sexual, and gender violence. The rationale behind this aggregation is that it summarizes all crimes related to very close personal relationships or interactions. Secondly, following the description by Currie and Almond (2011), we define a crime category we name "anger crimes" that includes the detailed categories of damage to property, bodily harm, disobedience (disobeying authority) and criminal threats. These crimes are not motivated by money or close links, but still have some behavioral or personal component.[15] Except for damage to property, all other categories are crimes against the person. We nevertheless understand that damage to property still needs to be included in the anger category, as property-damaging behavior may result from anger, irritation, or rage. In this regard, the richness of the data allows us to depart from traditionally set crime typologies as in Table 1 and analyze new ones that focus on the crime types we believe the BSaB policy may affect via the community channel, which are outlined in Table 2. This classification helps to better pinpoint the causal effects of community ties on crime.

[13] According to Spain's National Institute of Statistics, 76% of all unemployed individuals are included in the unemployment register. Registered unemployment and housing prices are only available since 2009.

[14] We 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, which is only available on a yearly basis.

[15] Currie and Almond (2011) state that temperamental skills are often proxied by psychological traits, social skills, and behavioral issues.

Table 1
Broad and detailed crime categories.

Broad category	Share Crime %	Detailed category	Share Crime %
Against property	86.6	Damage to property	8.5
		Fraud	5.2
		Car theft	11.4
		Robbery	14.5
		Theft	47.1
Against the person	8.9	Family violence	0.7
		Gender violence	2.0
		Bodily harm	3.0
		Murder	0.1
		Sexual	0.3
		Criminal threats	2.5
		Other	0.3
Other	4.5	Arson	0.0
		Drugs	0.7
		Environmental	0.2
		Disobedience	1.8
		Road safety	1.8
Total	100		100

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

Table 2
Specific crime categories, and distribution by location.

	Share Crime %	Share Residence %	Share Street %	Share Other %
Total crime	100	10	45	46
Intimate	3.0	62	25	13
Family violence	0.7	68	19	13
Gender violence	2.0	64	26	10
Sexual	0.3	36	31	32
Anger	15.9	21	45	35
Damage to property	8.5	21	41	38
Bodily harm	3.0	11	52	38
Disobedience	1.8	8	67	25
Criminal threats	2.5	43	31	26
Drugs	0.7	3	87	10

Notes: This table presents the composition of the categories labeled as “intimate” crimes and crimes of “anger”, as well as their contribution to overall crime. It also indicates what percent of each of these crimes took place in a residence, the street, or other locations. Source: Own construction from local police data.

This last classification indicates that intimate crimes and anger crimes account for almost one out of every five crimes and that anger crimes are much more frequent than intimate crimes. While these categories do not seem to account for a sizeable share of overall crime, they inflict much greater disutility on their victims than other, more frequent types of crime. On this matter, Dolan et al. (2005) indicate that while discounted QALY[16] losses resulting from rapes and sexual assaults are 0.561 and 0.160, the figure is just 0.007 for common assault, demonstrating the importance of dealing with such offenses.

Additionally, Table 2 shows how crime types are distributed by location type. Some typologies have location patterns that are particularly residential. These are indeed the ones we classified as intimate crimes. Some other types, such as criminal threats (included among anger crimes), are also particularly likely to be committed in a place of residence. Because of this location pattern and its relevance in light of the BSaB policy's characteristics, our analysis focuses on both the traditional crime categories and the categories of intimate crimes and anger crimes. We also pay particular attention to drug offenses, as they are closely related to some of the activities within BSaB.

Tables A6–A9 in the Appendix show summary statistics for our dependent variables and controls. Results are shown both for the whole city of Barcelona (all 73 neighborhoods) and for neighborhoods potentially included in BSaB (49 neighborhoods).

[16] Quality-Adjusted Life Years.

5. Methodology

To evaluate the impact of BSaB on local crime, we adopt a difference-in-differences approach where our observational unit is a neighborhood-year-month pair and treatment was implemented over different periods 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, in order to have comparable treatment and control units, the spatial units of analysis are the neighborhoods in Barcelona whose income was below 90% of the city median (those 49 neighborhoods shaded in Fig. 1; the white areas are not part of our analysis). We quantify the BSaB policy's impact as the difference in crime before and after BSaB implementation for neighborhoods where it was implemented (12 shaded and hatched neighborhoods in Fig. 1) and neighborhoods where it was not (37 shaded neighborhoods without hatching in Fig. 1). The identification strategy relies on the fact that the roll-out of BSaB was quasi-random and not correlated with any observable characteristics. Thus, it can be seen as an exogenous change.[17]

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 have either chosen to focus on some types of crime that follow a more geographically concentrated pattern (Warner and Rountree 1997) or construct a unique exposure to the treatment measure (Takagi et al. 2012). In this analysis, we focus on crime types with a precise location pattern, such as those that mostly take place in residences: these are the ones we classify as intimate crimes. We also consider drug-related crimes and anger crimes, due to the nature of the policy. Restricting the study in such a way dispels potential spillover concerns. This focus is also supported by the functioning of the policy itself, as it is run by local health centers that only deliver care and services to the specific neighborhood in which they are located.

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

$$Crime_{it} = \beta_1 + \beta_2(T_{it} \cdot BSaB_i) + \theta X_{it} + \eta_i + \phi_t + \varepsilon_{it} \tag{1}$$

where the observational unit is a "neighborhood-year-month" pair, i is the neighborhood, t is the time period (year-month), the dependent variable is the victim/offense/crime rate per 1000 inhabitants, $BSaB_i = 1$ for participant neighborhoods, $T_{it} = 1$ for the post-treatment periods (different for each treatment unit), X_{it} is a vector of socioeconomic controls, η_i and ϕ_t are neighborhood and year-month fixed effects, and ε_{it} is the error term. In some specifications, we also include interaction terms between baseline neighborhood characteristics and a time trend. This allows for different time trends across neighborhoods with different characteristics.

Given that the data structure accounts for a relatively small number of neighborhoods and a large number of months, we consider Driscoll and Kraay (1998) standard errors for our main analysis, which are robust to very general forms of cross-sectional and temporal dependence. Additionally, observations are weighted by population size. In the case of victims and offenders, we consider as dependent variables specific victim/offense rates per 1000 individuals, considering the characteristics of the victims/offenders in terms of gender and age. In all cases, the estimation of the policy effect is given by β_1.

We also study responses over time following an event-study approach. We perform fixed-effects regressions of the following type:

$$Crime_{id} = \beta_1 + \sum_{d \neq -1} \phi_d (BSaB_i \cdot Time_{id})_{id} + \theta X_{id} + \eta_i + \varepsilon_{id} \tag{2}$$

where the dependent variable is the victim/offense/crime rate per 1000 inhabitants, and the observational unit is a "neighborhood-distance-to-treatment" pair (measured in months), i is the neighborhood, d is the distance-to-treatment period. $BSaB_i = 1$ for participants, $Time_{id} = 1$ are distance-to-treatment indicator variables (different for each treatment unit), η_i is a neighborhood fixed effect, X_{id} is a vector of socioeconomic controls, and ε_{id} is the error term.

We estimate the parameters ϕ_d corresponding to the $(BSaB \cdot Time)$ interactions, leaving $Time_{id} = -1$ 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 $\{\phi_{-M}, \ldots, \phi_{-2}\}$ identify anticipation effects, coefficients $\{\phi_0, \ldots, \phi_M\}$ identify dynamic treatment effects. First of all, this allows us to test the existence 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 will assist in disentangling potential mechanisms behind the results as explained in Section 3.2.

[17] Since this setting could also be analyzed from a staggered difference-in-differences setup comparing late-treated units to early-treated units, later on we apply a Bacon decomposition to assess where the main source of variation is coming from in our setup.

6. Results

6.1. Baseline results

First of all, to tackle possible endogeneity issues of treatment status (having BSaB), in Table 3 we present a set of tests performed on differences between treatment and control neighborhoods, prior to the intervention (specifically, in 2007). These indicate no significant differences between treatment and non-treatment neighborhoods in a set of observable socioeconomic and demographic characteristics. Regarding crime, the crime rates we find are different, but not their rates of growth.

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

	Mean diff.	Std. err.	*p*-value
Panel A: 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
Fertility rate	−4.52	2.26	0.062
Housing prices	−1.62	1.86	0.402
# retired	−45.68	41.95	0.297
Association density	0.04	0.045	0.440
Panel B: Crime rates			
All	−3.48	1.07	0.001
Against property	−3.04	0.95	0.002
Against the 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
Panel C: Crime rate growth			
All	0.01	0.03	0.702
Against property	−0.03	0.04	0.525
Against the 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 neighborhoods in 2007, before the BSaB policy was deployed. Source: Own construction from Barcelona City Council and local police data.

Furthermore, we estimate a logit model where the dependent variable is the treatment indicator $BSaB_i = 1$. We also estimate a panel logit, where the timing of the treatment is also considered. The results of these two exercises (in Tables A10 and A11 of the Appendix) show that socioeconomic variables do not seem to explain either the fact of being treated with BSaB or its timing. Thus, results in Tables 3, A10 and A11 provide evidence that the parallel trends assumption holds in this setting.[18]

Table 4 presents results based on the estimation of Eq. (1) for broad crime rates while Table 5 present the results for offense and victim rates, in both cases weighting each observation by population size and considering standard errors as in Driscoll and Kraay (1998).[19,20] Each column indicates a different specification, each one being more stringent than the previous one. Column 1 present estimates with no fixed effects, column 2 includes neighborhood fixed effects, and column 3 adds year and month fixed effects. Column 4 includes characteristic-specific trends only for those control variables available for the entire sample (association density and tourism), measured in 2007 and column 5 includes characteristic-specific trends for all control variables (association density, tourism, registered unemployment, and housing prices), measured in

[18] This feature is confirmed informally by anecdotal evidence provided by the authorities running BSaB in the Barcelona Public Health Agency (ASPB). At informal meetings, we learned that neighborhood assignment to the intervention did not follow any rule-based procedure, and was a quasi-random decision.

[19] For our main specification we introduce Driscoll and Kraay (1998) standard errors with a maximum lag of three months, as we understand this captures the potential temporal dependence in our setting.

[20] Table A13 and Table A14 in the Appendix present the main results using instead standard errors clustered at the neighborhood level.

Table 4
Effect of BSaB on crime. Broad crime categories.

	(1)	(2)	(3)	(4)	(5)	Control mean
Against property						
	7.897***	0.804	0.687	0.647	0.180	7.461
	(0.602)	(0.705)	(0.661)	(0.662)	(0.616)	
Against the person						
	0.373***	−0.087***	−0.091***	−0.094***	−0.091***	0.760
	(0.033)	(0.032)	(0.027)	(0.027)	(0.027)	
Other						
	0.290***	−0.194***	−0.206***	−0.209***	−0.096**	0.539
	(0.035)	(0.041)	(0.037)	(0.037)	(0.039)	
Observations	4702	4702	4702	4702	3264	
Neighborhood FE		Y	Y	Y	Y	
Year-month FE			Y	Y	Y	
Characteristic-specific trends				Y_{2007}	Y_{2009}	

Notes: This table reports the results of the difference-in-differences estimation following Eq. (1) for the 2008–2014 period. Each column presents a different specification, each more demanding than the previous one. Column 1 present estimates with no fixed effects, column 2 includes neighborhood fixed effects, and column 3 adds year and month fixed effects. Column 4 includes characteristic-specific trends only for those control variables available for the entire sample, measured in 2007 and column 5 includes characteristic-specific trends for all control variables, measured in 2009. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. The coefficient shown is that of interest in a difference-in-differences setting, namely *Treated* · *Post*. Driscoll and Kraay (1998) standard errors are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5
Effect of BSaB on crime, offender and victim categories.

	(1)	(2)	(3)	(4)	(5)	Control mean
Off. U18						
	0.488***	−0.583***	−0.511***	−0.512***	−0.426**	0.897
	(0.066)	(0.152)	(0.155)	(0.154)	(0.164)	
Off. 18–25						
	8.917**	−0.801	−1.196	−1.171	−1.744**	8.751
	(0.782)	(1.003)	(0.903)	(0.905)	(0.872)	
Off. 25–35						
	2.729***	0.125	−0.117	−0.124	−0.595**	5.086
	(0.329)	(0.259)	(0.237)	(0.236)	(0.255)	
Off. 35–45						
	2.464***	0.755***	0.322	0.316	−0.160	3.914
	(0.258)	(0.214)	(0.206)	(0.205)	(0.279)	
Vict. U18						
	1.066***	0.278	0.102	0.214	0.093	1.241
	(0.169)	(0.217)	(0.222)	(0.222)	(0.223)	
Vict. 18–25						
	25.812***	5.346	6.225	6.108	5.784	13.970
	(3.388)	(4.419)	(4.345)	(4.340)	(4.901)	
Vict. 25–35						
	6.680***	0.034	0.752	0.727	1.040	8.143
	(0.661)	(0.916)	(0.801)	(0.804)	(0.730)	
Vict. 35–45						
	5.593***	0.848	0.644	0.629	0.905**	7.518
	(0.431)	(0.564)	(0.459)	(0.466)	(0.424)	
Observations	4702	4702	4702	4702	3264	
Neighborhood FE		Y	Y	Y	Y	
Year-month FE			Y	Y	Y	
Characteristic-specific trends				Y_{2007}	Y_{2009}	

Notes: This table reports the results of the difference-in-differences estimation following Eq. (1) for the 2008–2014 period. Each column presents a different specification, each more demanding than the previous one. Column 1 present estimates with no fixed effects, column 2 includes neighborhood fixed effects, and column 3 adds year and month fixed effects. Column 4 includes characteristic-specific trends only for those control variables available for the entire sample, measured in 2007 and column 5 includes characteristic-specific trends for all control variables, measured in 2009. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. The coefficient shown is that of interest in a difference-in-differences setting, namely *Treated* · *Post*. Driscoll and Kraay (1998) standard errors are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2009. Even if the specification of column 5 loses some observations, it captures important dynamics in this setting that might be missed in the specification of column 4. Because of this, our preferred specification is that in column 5, as it includes characteristic-specific trends at the neighborhood level for our full set of control variables.

Overall, the results for the estimated impact of BSaB on local crime rates indicate that there was indeed a negative and significant impact on crime. Even if we do not see a decrease in criminal activity across all the different aspects studied after the policy implementation, we do see significant reductions in aspects of crucial relevance in light of BSaB. Concretely, regarding the broad crime categories, our estimates shown in Table 4 indicate that BSaB reduces crimes against the person as well as other crime. These effects account for 12% and 18% reductions, respectively. No effect is observed in crimes against property. Turning to offenders and victims, Table 5 evidences a reduction in criminal outcomes for significant subsets of the population. Even if no widespread significant reduction in offense rates is found, there is a significant reduction for younger individuals. As outlined in Table 5, the effect of BSaB on offense rates is negative and significant for individuals below 35 years old and even larger for individuals below 18 years old. Regarding victimization, a significant and positive impact of BSaB is found for individuals between 35 and 45 years of age. For all other age groups, no effect is found. When analyzing these results by age and gender (see Table A12 in the Appendix), we observe that in the case of offenders, the results are led by those for female offenders under 25 and male offenders aged 25–35. For victimization, the aforementioned increase corresponds solely to males.

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

The first feature to note in Fig. 2 is that there do not seem to be any anticipatory effects of BSaB on crime in any of the subfigures. This pattern 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 Fig. 2 reflects, the impact of BSaB is different over time across crime rates. No dynamic treatment effects are found for crimes against property. There is an effect for crimes against the person in the short term (before month 10). For other crimes, an effect is found in the medium-to-long term (months 16 and beyond). Results derived from an estimation á la De Chaisemartin and d'Haultfoeuille (2020) do not differ significantly from the estimations presented in Fig. 2.[21]

We present further 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 and about any experience of being a victim of crime in the past 12 months. Specifically, individuals are asked whether they feel that safety and civility have improved, declined, or remained the same in their neighborhood compared to the previous year. We take advantage of this question to assess the effect of BSaB on neighborhood perception. Specifically, we run a logistic regression on safety and civility having improved, against BSaB in the neighborhood in that year. Estimates are presented in Table 6. The presence of BSaB significantly raises by approximately 3% the probability of perceiving an improvement in safety. 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 are found for perceptions of civility. We believe that the fact that civility is less specific than safety may influence these results.[22]

Table 6
Effect of BSaB on perceptions in the neighborhood.

	Civility	Security
BSaB	−0.007	0.032***
	(0.004)	(0.004)
Observations	21779	21779
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 neighborhood perceptions, each presented in a different column. Data comes from the Safety and Victim Survey for Catalonia, 2007–2014. The unit of observation is an individual in a survey wave. We show average marginal effect from logistic regression with district and year fixed effects and robust standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

[21] See Fig. A1 in the Appendix.

[22] It could be that each respondent has a different concept of civility (as broadly specified in the Survey), and effects may be more difficult to detect.

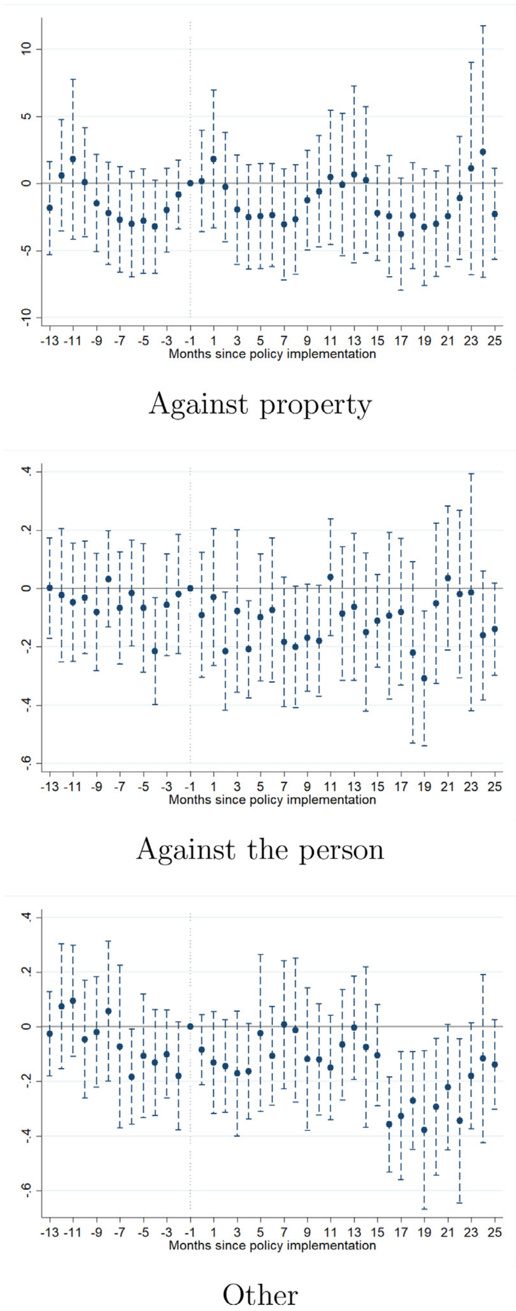


Fig. 2. Effect of BSaB on crime. Event-study exercise, 95% confidence intervals. *Notes:* This graph reports the results of an event-study exercise following Eq. (2) for the 2008–2014 period, for crimes against property, against the person and other crimes. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. Confidence intervals are based on Driscoll and Kraay (1998) standard errors.

Overall, the results above are in line with those of previous studies.[23] Takagi et al. (2012) establish that support networks and social capital are inversely associated with crime. However, crime was measured for any 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 do not find a significant effect for all property crime as a category. 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.

6.2. Alternative crime categories

Next, we move to analyze the effect of BSaB on the alternative crime categories defined in Table 2. These results are shown in Table 7. To an extent, as a result of the reduction in crimes against the person, BSaB had an impact on intimate crime rates: it reduced intimate crime rates by 0.068, a decrease of 28% with respect to the mean. For this category, the results are mainly derived from gender violence crimes. It must be noted that crime rates for intimate crimes are much lower than rates for other categories, so that percentage decreases are of higher magnitude. For anger crimes, results also indicate significant reductions following the implementation of BSaB. In this case, the obtained coefficient entails a reduction of 6.6% with respect to the mean. Regarding drug crimes, another vital result considering the aims of the policy, no direct effect is found for BSaB.

Table 7
Effect of BSaB on crime. Specific crime categories.

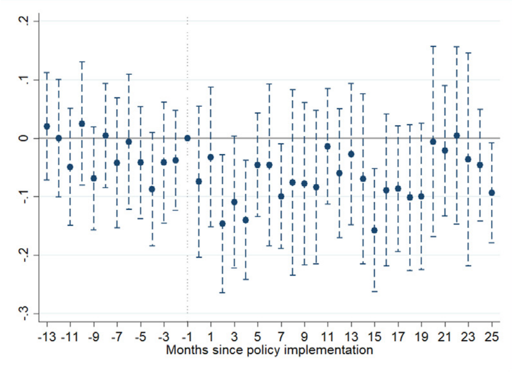
	(1)	(2)	(3)	(4)	(5)	Control mean
Intimate						
	0.057***	−0.101***	−0.076***	−0.077***	−0.068***	0.239
	(0.007)	(0.011)	(0.010)	(0.010)	(0.013)	
Anger						
	0.663***	−0.089	−0.064	−0.065	−0.099*	1.497
	(0.047)	(0.060)	(0.051)	(0.051)	(0.055)	
Drugs						
	0.110***	−0.013	−0.017	−0.018	−0.019	0.044
	(0.021)	(0.020)	(0.021)	(0.021)	(0.020)	
Observations	4702	4702	4702	4702	3264	
Neighborhood FE		Y	Y	Y	Y	
Year-month FE			Y	Y	Y	
Characteristic-specific trends				Y_{2007}	Y_{2009}	

Notes: This table reports the results of the difference-in-differences estimation following Eq. (1) for the 2008–2014 period. Each column presents a different specification, each more demanding than the previous one. Column 1 present estimates with no fixed effects, column 2 includes neighborhood fixed effects, and column 3 adds year and month fixed effects. Column 4 includes characteristic-specific trends only for those control variables available for the entire sample, measured in 2007 and column 5 includes characteristic-specific trends for all control variables, measured in 2009. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. The coefficient shown is that of interest in a difference-in-differences setting, namely *Treated* · *Post*. Driscoll and Kraay (1998) standard errors are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

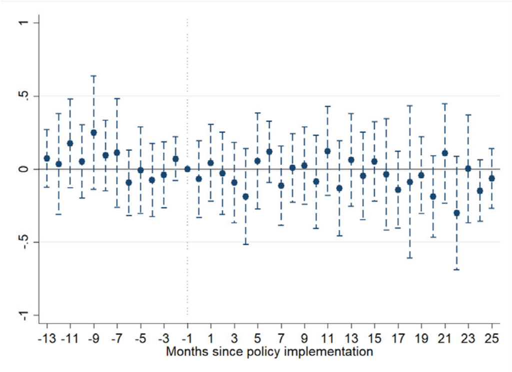
This pattern reflects what happens in the crime categories that are of interest in this paper. In the case of crimes against the person, the effect of BSaB on intimate crime occurs in the short run. The impact is quite immediate, showing a significant decrease two months after policy implementation. However, Fig. 3 also shows that the impact is quite ephemeral, as the effect had already become diluted by month 6. A very different picture is found for anger crime rates. In this case, no dynamic treatment effects are found. Nevertheless, even if our confidence intervals are large and point estimates are not significant, we see negative coefficients from the first year onwards. Finally, in the case of drug crimes, a medium- to long-term effect 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 reducing effects are found 16 months after deployment.

These results are also supported by the evidence shown in Table 8. In it, we present joint significance tests for all lag and lead coefficients. Results indicate that we cannot reject the hypothesis that anticipatory effects are equal to zero, but the null hypothesis can be rejected for dynamic treatment effects.

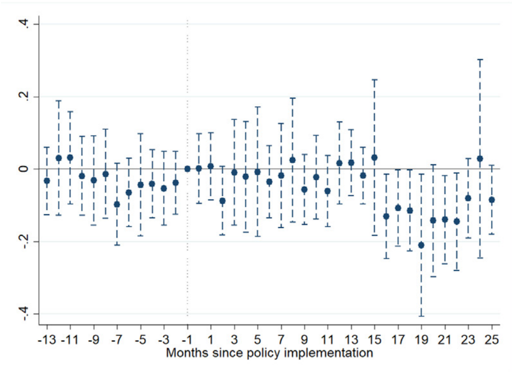
[23] Our conceptual framework is also relevant to assess the potential impact of the program on reporting rates. As in any study using police-reported data, the estimated impact of BSaB on crime could be affected by changes in reporting behavior derived from the program itself. Even if we cannot rule out that changes in reporting rates could be affecting the results found, our conceptual framework explains why this should not be a serious concern. If the program boosts reciprocity and trust among individuals, as we believe (that is, if it impacts the so-called bridging type of social capital), it would be more likely to increase trust in police forces and thus tend to increase reporting rates, if anything. Given that we estimate a reduction in crime caused by BSaB, if reporting changes upwards, our results would reflect a lower bound of the actual effect of the program on crime rates. The results we report in Table 6 on security perception lead us to rule out underreporting as an effect of the policy. In the areas treated by BSaB we find a significant increase in perception of security, which could also be seen also as consistent with the idea that BSaB increases trust in institutions, and would thus be unlikely to decrease reporting rates.



Intimate



Anger



Drugs

Fig. 3. Effect of BSaB on crime. Event-study exercise, 95% confidence intervals. *Notes:* This graph reports the results of an event-study exercise following Eq. (2) for the 2008–2014 period, for intimate crimes, anger crimes and drug crimes. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy was deployed, while those in which it was not are controls. Treatment timing differs across units. Confidence intervals are based on Driscoll and Kraay (1998) standard errors.

Our findings are of value in light of the policy evaluation. The type of crime that BSaB reduced is intimate crimes, a type most likely to affect women. This result is relevant for two reasons. Firstly, many BSaB interventions aimed to empower women and raise awareness of sexual health and education. Moreover, most of the actions targeted younger population groups, which seem to be the ones more positively affected by the program (i.e. lower offense rates). Secondly, our findings indicate that progress was achieved on such an essential issue as violence against women. According to Spain's National Institute of Statistics, over 30 thousand cases of gender violence were registered in Spain in 2018.[24]

[24] https://www.ine.es/prensa/evdvg_2019.pdf

Table 8
Effect of BSaB on crime. Event-study exercise, joint significance tests for anticipatory and dynamic effects.

	F-stat anticipatory F(1,95)	Prob > F	F-stat dynamic F(26,95)	Prob > F
Intimate	0.41	0.525	3.33	0.000
Anger	0.32	0.576	3.03	0.000
Drugs	0.45	0.506	2.68	0.000

Notes: This table reports the results of our joint significance test of the pre and post coefficients of the event-study exercises shown in Fig. 3. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was 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. Robustness exercises

Table 9 presents several robustness checks for the baseline estimates shown in column 5 of Tables 4 and 7. In Table 9, column 1 shows our baseline estimates, columns 2 and 3 show estimates when including other sociodemographic controls, such as tourism pressure and housing prices, at the expense of losing observations. Columns 4 and 5 allow for different windows for potential lag order of autocorrelation (Driscoll and Kraay 1998), being shorter in column 4 (1 lag) and longer in column 5 (12 lags). Finally, column 6 presents the results of a placebo exercise in which we randomly assign a fake BSaB treatment across neighborhoods and time.

Table 9
Effect of BSaB on crime. Robustness exercises.

	Baseline	Controls I: Tourism	Controls II: Tourism + Housing	Cluster I: Short	Cluster II: Long	Placebo
Against property						
	0.180	0.157	0.098	0.180	0.180	0.076
	(0.616)	(0.452)	(0.679)	(0.528)	(0.465)	(0.081)
Against the person						
	-0.091^{***}	-0.091^{***}	-0.108^{***}	-0.091^{***}	-0.091^{***}	0.005
	(0.027)	(0.027)	(0.039)	(0.026)	(0.021)	(0.007)
Other						
	-0.096^{**}	-0.096^{**}	-0.158^{***}	-0.096^{***}	-0.096^{*}	−0.003
	(0.039)	(0.040)	(0.049)	(0.032)	(0.052)	(0.007)
Intimate						
	-0.068^{***}	-0.068^{***}	-0.076^{***}	-0.068^{***}	-0.068^{***}	0.001
	(0.013)	(0.013)	(0.018)	(0.012)	(0.012)	(0.004)
Anger						
	-0.099^{*}	-0.100^{*}	-0.207^{***}	-0.099^{**}	−0.099	−0.011
	(0.055)	(0.053)	(0.048)	(0.047)	(0.063)	(0.009)
Drugs						
	−0.019	−0.019	−0.018	−0.019	−0.019	−0.001
	(0.020)	(0.020)	(0.035)	(0.017)	(0.026)	(0.003)
Observations	3264	3264	2377	3264	3264	3264

Notes: This table reports the results of alternative specifications for the difference-in-differences estimation following Eq. (1) for the 2008–2014 period. Each column presents a different specification. Column 1 shows our baseline estimates (column 5 of Tables 4 and 7), columns 2 and 3 show estimates when including more sociodemographic controls, columns 4 and 5 allow for different windows for potential lag order of autocorrelation, and column 6 presents the results of a placebo exercise. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. The coefficient shown is that of interest in a difference-in-differences setting, namely *Treated* · *Post*. Driscoll and Kraay (1998) standard errors are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

For all these exercises, results reported in Tables 4 and 7 hold. We find that the coefficient estimated for BSaB are stable across these alternative specifications. Moreover, and very importantly, our falsification exercise (column 6 in Table 9), which assigns random treatment in terms of neighborhoods and roll-out, reflects no significant results.

6.4. Mechanism analysis

Our central 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 association density to crime via BSaB. In other words, we assess whether

Table 10
Effect of BSaB on other socioeconomic variables. Potential mechanisms.

	Association density	Registered unemployment	Health status	Mental health
BSaB	0.504***	−0.003	−0.087	−0.064
	(0.171)	(0.003)	(0.081)	(0.157)
Observations	3264	3264	3716	3653
Neighborhood FE	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y
Characteristic-specific trends	Y	Y	Y	Y

Notes: This table presents difference-in-differences estimates of the BSaB policy in outcomes other than crime, each presented in a different column. The observational unit is a neighborhood-year-month pair for association density and registered unemployment; and individuals for health and mental health status. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units and the specification is the same as in our baseline specification for crime. Standard errors clustered at the neighborhood level are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

BSaB increased the number of per capita local associations. As mentioned, information on registered local associations, provided by the regional government, includes registration date, aims, and place of action. The included associations are legal entities registered following the Catalan Associations Act, and are defined as “non-profit organizations, voluntarily formed by three or more individuals to serve a general or specific interest, through the sharing of personal resources, temporarily or indefinitely”. There are different types of associations, based on their main activities (e.g., cultural, civic rights, scientific knowledge, health and social welfare, etc.). Associations that promote civic rights in a particular place, typically neighborhood associations, are very common.

To rule out other potential mechanisms, we carry out similar analyses for health and unemployment outcomes to assess whether these acted as channels for lowering crime rates. For health, we use individual microdata from the Barcelona Health Survey (ESB) for the 2006–2016 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 compare answers from individuals in treatment and control neighborhoods in 2006 (just before BSaB) and 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 (on a range from 1 to 12) indicates a higher risk of poor mental health. For unemployment, we use Barcelona City Council information on registered unemployment rates by neighborhood.

Table 10 presents results on the impact of BSaB on association density, registered unemployment, health status and mental health status. First, results reported in Table 10 show a positive and statistically significant effect of BSaB on association density (per capita local associations).[25] Second, results indicate no statistically significant impact of BSaB on local unemployment. Third, there is no evidence of 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 this last result, Palència et al. (2018) find no evolution of self-rated health for men and women in treatment and control neighborhoods.

Finally, we apply a Bacon decomposition to disentangle whether there are heterogeneous effects across neighborhoods (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. For this case, results from the Bacon decomposition for intimate crimes are shown in Table 11.[26] These results indicate that the estimates previously found are driven by comparing treated versus never treated observations, rather than from comparison of early- versus late-treated units. This evidence is shown by the weight of this variation source compared to the others. These last results indicate that differences between treated units are not the main driver behind the effect of BSaB on crime. Hence, heterogeneity in outcomes due to different priorities in different neighborhoods does not seem to be a determining feature of the analysis. Fig. A3 in the Appendix provide further results of the baseline specification when removing neighborhoods one at a time to show that our results are not dependent on the inclusion or exclusion of a particular neighborhood. This lends weight to the idea that the policy’s specific content is less relevant than the fact of connecting people, reinforcing the evidence in favor of the community ties hypothesis.

[25] Also see Fig. A2 for the event-study exercise on the impact of BSaB on association density.

[26] The coefficient from the Bacon decomposition exercise does not coincide with the our preferred specification as it does not include characteristic-specific trends at the neighborhood level. For this, column 3 of Table 7 is comparable.

Table 11
Effect of BSaB on crime. Bacon decomposition of intimate crimes estimates.

		Comparison type weight
difference-in-differences estimate	−0.075	
Treated vs. never treated	−0.077	0.891
Earlier treatment vs. later control	−0.080	0.060
Later treatment vs. earlier control	−0.036	0.050
Treated 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-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. The results show three types of comparisons, which differ by control group: (1) Already treated, where a group treated prior to the start of the analysis serves as the control group; (2) Never treated, where a group which never receives the treatment serves as the control group. (3) Timing groups, or groups whose treatment stated at different times can serve as each other's controls: (3.1) those treated later serve 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.

In sum, we have observed (1) a change in crime rates within a short interval of time after policy implementation, (2) a positive and significant effect of the BSaB policy onto association density, (3) no effect on health or unemployment, and (4) homogeneous effects across neighborhoods, irrespective of the content or priorities set. Therefore, potential impacts on crime are likely to be due to the community feature of the policy and stronger community ties.

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 (BSaB) rolled out in Barcelona from 2008 to 2014. The policy was implemented in 12 of 49 potential neighborhoods and covered around a quarter of the potentially targeted population. Even though its stated aim was to improve health outcomes in underprivileged neighborhoods, we assess whether the community feature of BSaB led to an increase in community ties and, consequently, to reduced crime rates.

Using a difference-in-differences approach and administrative records from the local police, we find that this is the case. Concretely, there is a reduction in crimes against the person related to reducing intimate crimes, which fall by 28%, but only in the short term. Drug crimes also fall, but in a longer term. For outcomes on offense rates, there is a reduction among younger individuals. Results also indicate that BSaB increases association density in participating neighborhoods but does not affect self-rated health, mental health, or unemployment rates across treatment and control neighborhoods. We thus find support for our hypothesis that the strengthening of community ties is likely to be a key mechanism, a statement also backed up by a Bacon decomposition of the results that indicates no heterogeneity on outcomes between treated units, making the program-related meetings themselves more important than their contents.

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

This paper thus indicates that traditional policies against crime are not the only ones that work and that new means of reducing criminal activity in disadvantaged neighborhoods can be effective. Additionally, our findings on these policies speak to efficiency. BSaB had an annual cost of 500,000 euros in 2015. This number means a cost of 5000 euros per annual activity, 70 euros per active participant, and 2 euros per potential participant. Hence, the policy also evidences positive points from a cost-effectiveness perspective. Even if building community ties is more challenging than deploying traditional policing, alternative policies of this type may work better in several contexts. Buonanno et al. (2009) state that a policy of promoting 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 to reduce criminal activity patterns induced by the lack of integration of some citizen subpopulations facing substandard social and economic conditions.

Appendix

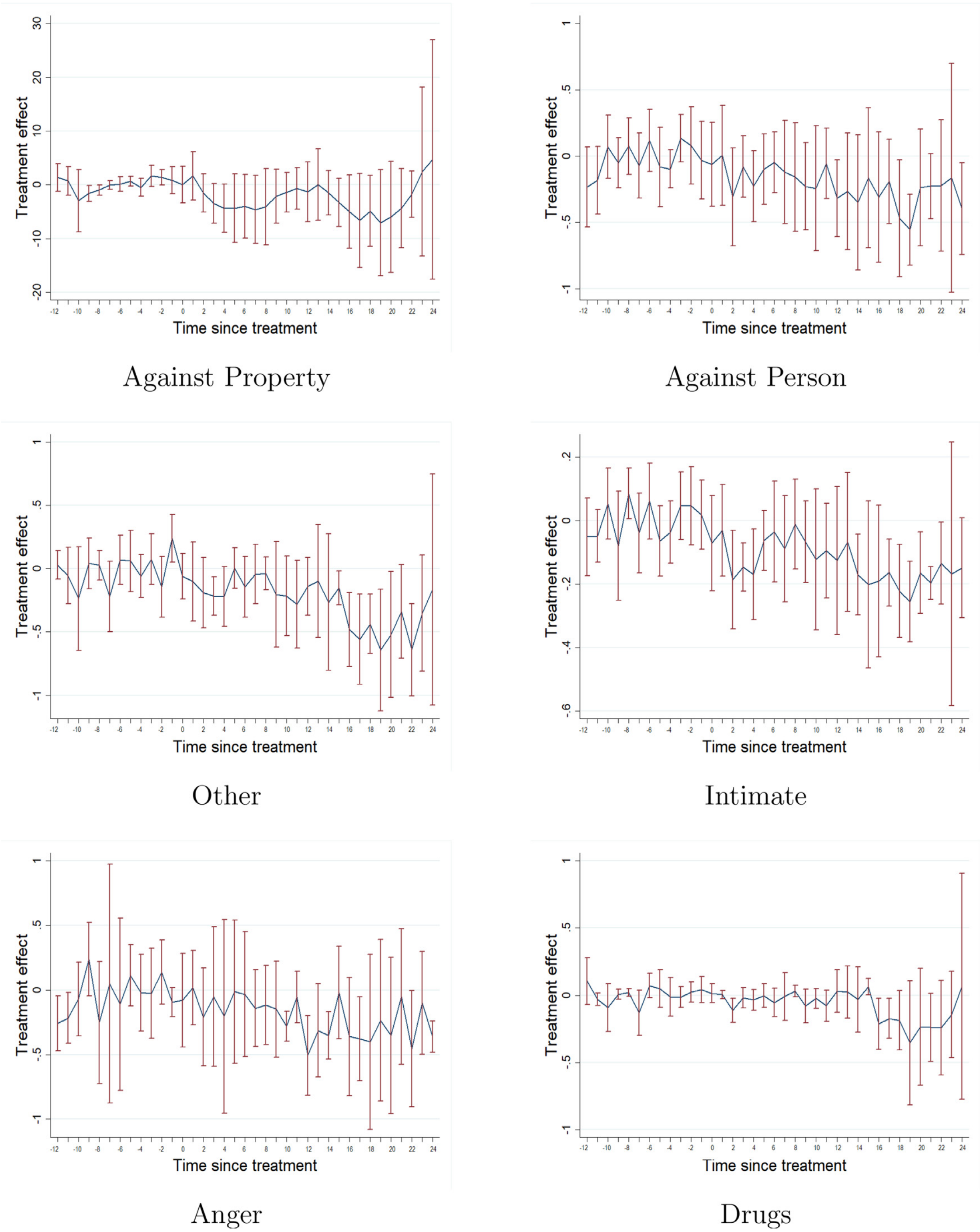


Fig. A1. Effect of BSaB on crime - event study exercise á la De Chaisemartin and d'Haultfoeuille (2020), 95% confidence intervals. *Notes:* This graph reports the results of an event-study exercise derived from the difference-in-differences estimation for the 2008–2014 period for crimes against property, against person, other crimes, intimate crimes, anger crimes and drug crimes considering the estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. Confidence intervals are based on standard errors clustered at the neighborhood level.

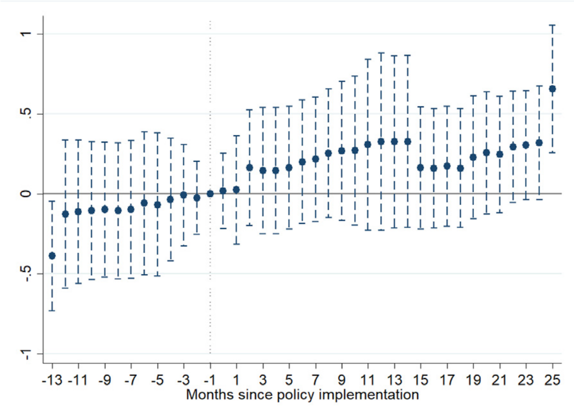


Fig. A2. Effect of BSaB on association density. Event-study exercise, 95% confidence intervals. *Notes:* This graph reports the results of an event-study exercise derived from the baseline difference-in-differences estimation for the 2008–2014 period for per capita local associations. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. Confidence intervals are based on Driscoll and Kraay (1998) standard errors.

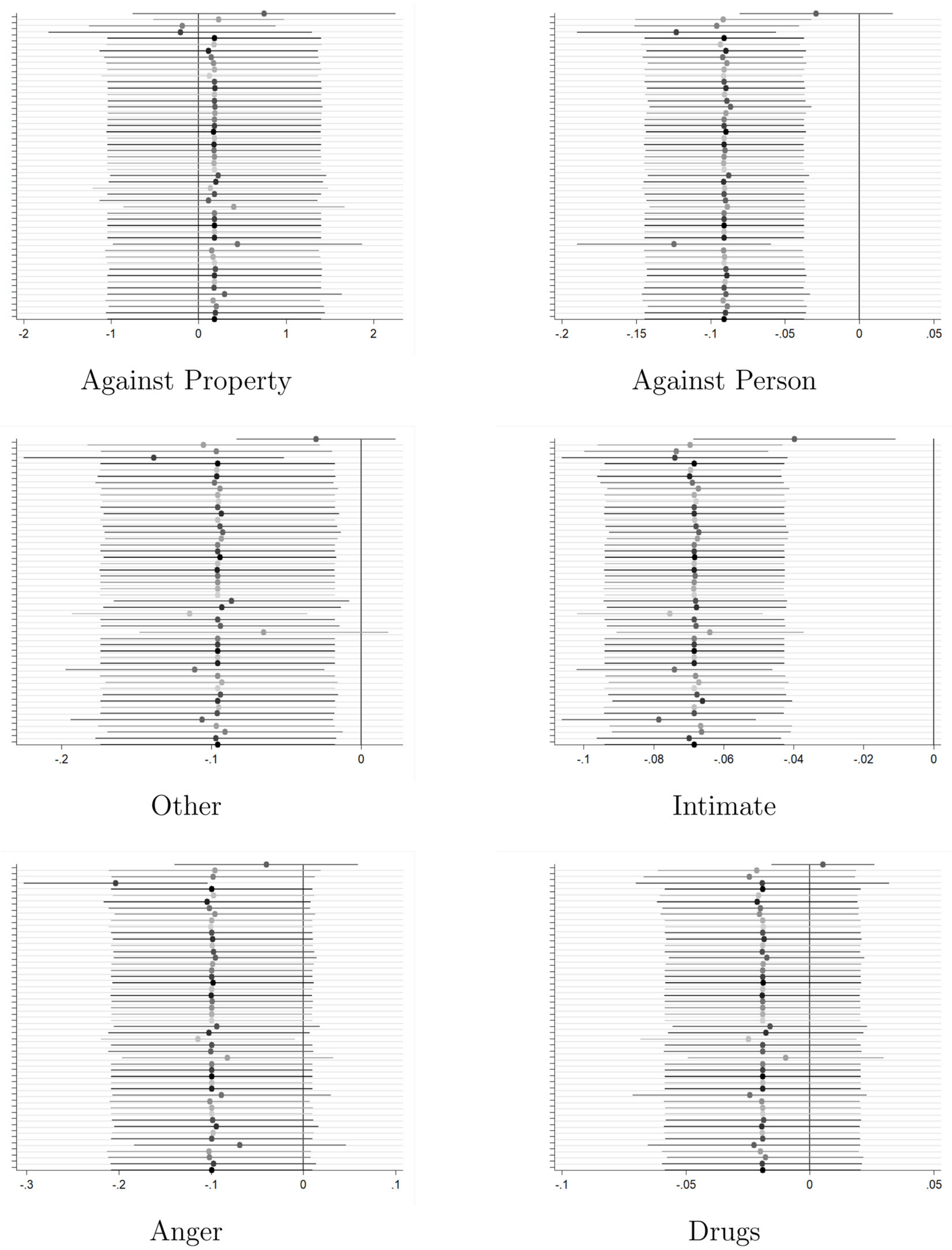


Fig. A3. Effect of BSaB on crime - removing neighborhoods one at a time. *Notes:* These graphs report the results of the difference-in-differences estimation removing neighborhoods one at a time. The specification follows Eq. (1) for the 2008–2014 period, as in column 5 in Tables 4 and 7. The coefficient shown is that of interest in a difference-in-differences setting, namely *Treated* · *Post*. Confidence intervals are based on Driscoll and Kraay (1998) standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A1
Neighborhood characteristics: population and per capita income.

District	Neighborhood	Pop 07	Pop 14	Income 07	Income 14	Low Inc.	Treatment
	Barcelona City	1,603,178	1,613,393	100	100	NA	NA
1	el Raval	46,595	48471	64.7	65.9	Y	Y
1	el Barri Gòtic	27,946	15,911	86.5	98.5	N	N
1	la Barceloneta	15,921	15,181	66.7	84.5	Y	Y
1	Sant Pere, Santa Caterina i la Ribera	22,572	22,674	80.2	92.5	Y	Y
2	el Fort Pienc	31,521	31,785	107.9	104.5	N	N
2	la Sagrada Família	52,185	51,562	101.8	92.4	N	N
2	la Dreta de l'Eixample	42,504	43,749	137.6	165.3	N	N
2	l'Antiga Esquerra de l'Eixample	41,413	41,975	126.5	127.8	N	N
2	la Nova Esquerra de l'Eixample	58,146	57,863	116.9	109.1	N	N
2	Sant Antoni	37,988	38,369	103.8	97.8	N	N
3	el Poble Sec - Parc Montjuïc	39,579	40,674	73.3	66.3	Y	Y
3	la Marina del Prat Vermell - Zona Franca	1.,005	1,151	80.4	39.4	Y	N
3	la Marina de Port	29,327	30,286	80.2	72.0	Y	N
3	la Font de la Guatlla	10,064	10,406	90.4	77.6	Y	N
3	Hostafrancs	15,771	15,919	82.7	76.8	Y	N
3	la Bordeta	18,592	18,451	81.9	76.0	Y	N
3	Sants - Badal	24,085	24,245	85.9	79.6	Y	N
3	Sants	40,272	41,102	89.5	85.8	Y	N
4	les Corts	46,400	46,205	130.4	125.4	N	N
4	la Maternitat i Sant Ramon	23,938	23,735	127.9	112.6	N	N
4	Pedralbes	11,413	11,670	193.6	251.7	N	N
5	Vallvidrera, el Tibidabo i les Planes	4,038	4,615	146.4	162.8	N	N
5	Sarrià	23,316	24,691	174.9	195.2	N	N
5	les Tres Torres	15,325	16,381	215.3	217.8	N	N
5	Sant Gervasi - la Bonanova	23,634	25,378	182.2	191.8	N	N
5	Sant Gervasi - Galvany	46,454	46,648	187.0	192.1	N	N
5	el Putxet i el Farró	28,990	29,041	150.2	140.2	N	N
6	Vallcarca i els Penitents	15,381	15,454	113.2	101.6	N	N
6	el Coll	7,190	7,307	91.7	81.6	Y	N
6	la Salut	13,072	13,256	113.0	107.3	N	N
6	la Vila de Gràcia	50,409	50,680	101.9	118.1	N	N
6	el Camp d'en Grassot i Gràcia Nova	34,535	34,146	104.3	103.7	N	N
7	el Baix Guinardó	25,816	25,587	96.6	8.6	Y	N
7	Can Baró	8,998	8,887	81.2	77.4	Y	N
7	el Guinardó	35,038	35,698	93.0	82.0	Y	N
7	la Font d'en Fargues	9,621	9,467	103.5	102.0	N	N
7	el Carmel	32,745	31,728	72.0	56.6	Y	N
7	la Teixonera	11,332	11,379	72.2	69.6	Y	N
7	Sant Genís dels Agudells	7,069	6,865	85.7	80.0	Y	N
7	Montbau	5,105	5,082	85.5	70.0	Y	N
7	la Vall d'Hebron	5,476	5,422	96.5	86.9	Y	N
7	la Clota	445	529	89.9	90.1	Y	N
7	Horta	26,638	26,591	85.9	82.2	Y	N
8	Vilapicina i la Torre Llobeta	25,672	25,500	83.0	64.0	Y	N
8	Porta	23,470	24,424	75.3	58.3	Y	N
8	el Turó de la Peira	15,102	15,471	65.4	50.6	Y	N
8	Can Peguera	2,143	2,288	49.8	51.0	Y	N
8	la Guineueta	15,394	15,090	82.0	56.0	Y	N
8	Canyelles	7,539	7,014	76.7	61.0	Y	N
8	les Roquetes	15,756	15,668	60.9	50.8	Y	Y
8	Verdun	12,301	12,239	63.8	50.8	Y	N
8	la Prosperitat	26,696	26,171	72.6	53.7	Y	N
8	la Trinitat Nova	8,011	7,462	53.0	34.7	Y	N
8	Torre Baró	2,105	2,682	58.0	45.6	Y	Y
8	Ciutat Meridiana	10,929	10,356	59.4	39.2	Y	Y
8	Vallbona	1,267	1,353	51.6	39.9	Y	Y
9	la Trinitat Vella	9,992	10,268	74.8	45.9	Y	N
9	Baró de Viver	2,397	2,508	44.5	60.5	Y	Y
9	el Bon Pastor	12,332	12,758	66.2	59.6	Y	Y
9	Sant Andreu	55,171	56,496	85.9	76.6	Y	N
9	la Sagrera	28,469	28,914	88.1	74.9	Y	N
9	el Congrés i els Indians	13,896	14,076	86.5	72.7	Y	N
9	Navas	21,454	21,949	92.9	83.3	Y	N
10	el Camp de l'Arpa del Clot	38,604	38,130	93.4	80.9	Y	N
10	el Clot	26,796	27,082	88.5	81.0	Y	N
10	el Parc i la Llacuna del Poblenou	13,104	14,814	103.2	88.6	N	N
10	la Vila Olímpica del Poblenou	8,783	9,391	132.8	150.8	N	N

(*continued on next page*)

Table A1 (*continued*)

District	Neighborhood	Pop 07	Pop 14	Income 07	Income 14	Low Inc.	Treatment
10	el Poblenou	30,181	33,425	94.5	95.4	Y	N
10	Diagonal Mar i el Front Marítim del Poblenou	9,775	13,351	101.1	168.8	N	N
10	el Besòs i el Maresme	22,652	23,191	61.7	58.9	Y	Y
10	Provençals del Poblenou	18,731	20,184	85.7	91.7	Y	N
10	Sant Martí de Provençals	26,261	26,018	81.5	67.6	Y	N
10	la Verneda i la Pau	29,452	28,903	74.8	57.2	Y	Y

Source: Own construction from Barcelona City Council data.

Table A2
Correlation matrix. Social and economic deprivation measures.

	Income	Unemployment	Housing prices	Vehicles	Teen pregnancy	Associations
Income	1					
Unemployment	−0.7854*	1				
Housing prices	0.8117*	−0.6568*	1			
Vehicles	0.4774*	−0.3823*	0.2182	1		
Teen pregnancy	−0.5899*	0.5904*	−0.4489*	−0.4311*	1	
Associations	0.2712*	−0.1809	0.219	0.7006*	−0.1932	1

Notes: This table presents pairwise correlations between socioeconomic measures of deprivation for all neighborhoods in Barcelona prior to BSaB implementation. Source: Own construction from Barcelona City Council data.

Table A3
BSaB deployment by neighborhoods.

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

Notes: The table presents the 12 neighborhoods treated by the BSaB policy in the city of Barcelona from 2008 to 2014, in chronological order with the start date of the program also shown for each one. Source: Barcelona Public Health Agency (ASPB).

Table A4
BSaB activities by scope.

Intervention	Target population	Neighborhoods
Childhood		
Healthy sports leisure	Primary school	Poble Sec
Healthy sports leisure	Middle school	Roquetes, Bon Pastor, Baro de Viver
Parenting skills	Parents of children aged 3–5	El Born, Torre Baro, Ciutat Meridiana, Vallbona, Barceloneta
Healthy cooking	Parents of children aged 3–17	Poble Sec
Extracurricular activities	Primary school	Roquetes, Barceloneta
Adolescents		
Healthy sports leisure	High school	Roquetes, Poble Sec, El Born, Torre Baro, Ciutat Meridiana, Vallbona
Healthy evening leisure	14–18	Torre Baro, Ciutat Meridiana, Vallbona
Sexual health counseling	14–25	Torre Baro, Ciutat Meridiana, Vallbona, Raval
Education on contraception	Under 20	Torre Baro, Ciutat Meridiana, Vallbona, Bon Pastor, Baro de Viver
Drug counseling	Under 21	Roquetes, Poble Sec, Raval
Drugs, violence, groups	15–29 at risk	Bon Pastor, Baro de Viver, Raval
Empowerment, integration	14–21 foreign women	El Besos i el Maresme
Adults		
Sex education for adults	Women 20–50	Torre Baro, Ciutat Meridiana, Vallbona, Bon Pastor, Baro de Viver
Tai chi in the park	Above 40	Roquetes, Poble Sec, El Born, Torre Baro, Ciutat Meridiana, Vallbona, Bon Pastor, Baro de Viver, el Besos i el Maresme
Obesity, stress, anxiety, depression	Adults	Bon Pastor, Baro de Viver
Elderly		
Memory groups	Older adults	Roquetes
Take a walk in the neighborhood	Older adults	Poble Sec, El Born, Torre Baro, Ciutat Meridiana, Vallbona, el Besos i el Maresme
How to be healthy	Older adults	El Born, Bon Pastor, Baro de Viver, el Besos i el Maresme
All interested		
Alcohol abuse	Everyone	Barceloneta
Tobacco addiction	All smokers	Roquetes, Poble Sec
Home-made remedies	Everyone	Roquetes

Notes: This table presents all initiatives undertaken within BSaB. They are categorized by broad aim and information includes their target population and the neighborhoods in which they were held. Source: Own construction from Barcelona Public Health Agency (ASPB) data.

Table A5
Description of main variables.

Variable	Description	Source	Frequency availability
Crime counts	Reported criminal acts	Local police	Geocoded; Exact time
Offender counts	Recorded offenders	Local police	Geocoded; Exact time
Victim counts	Recorded victims	Local police	Geocoded; Exact time
Population	Registered inhabitants	Barcelona City Council	Neighborhood; Year
Crime rate	Crime counts per 1000 inhabitants	Police and Barcelona City Council	Neighborhood; Month
Offense rate	Offender counts per 1000 inhabitants	Police and Barcelona City Council	Neighborhood; Month
Victim rate	Victim counts per 1000 inhabitants	Police and Barcelona City Council	Neighborhood; Month
Association density	Per capita local associations	Regional government	Neighborhood; Month
House prices	House market prices per square meter	Barcelona City Council	Neighborhood; Month
Unemployment	Registered unemployment rate	Barcelona City Council	Neighborhood; Month
Tourism	Per capita visitors to district tourist sites	Barcelona City Council	Neighborhood; Month

Notes: This table lists the main variables under analysis. It contains a brief description of how each is constructed, its sources, and the frequency for which data are available. Source: Own construction from local police, regional government and Barcelona City Council data.

Table A6
Descriptive statistics, crime rates per 1000 inhabitants. 2007–2014.

	All neighborhoods		Potentially participating	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
All	10.235	15.790	8.758	13.088
Against property	8.957	14.150	7.459	11.116
Against the person	0.735	0.882	0.759	0.987
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
Criminal threats	0.205	0.339	0.222	0.401
Obs	7008		4704	
Income <90% of 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 local police data.

Table A7
Descriptive statistics. Offense rates per 1000 inhabitants. 2007–2014.

	All neighborhoods		Potentially participating	
Variable	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 local police data.

Table A8
Descriptive statistics. Victim rates per 1000 inhabitants. 2007–2014.

	All neighborhoods		Potentially participating	
Variable	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 local police data.

Table A9
Descriptive statistics, control variables. 2007–2014.

	All neighborhoods			Potentially participating		
Variable	Obs.	Mean	Std. dev.	Obs	Mean	Std. dev.
Association density	7008	1.896	1.763	4704	1.192	1.21
Tourism (tickets/population)	7008	1.92	7.98	4704	2.39	9.54
Reg. unemployment (rate)	5256	0.07	0.02	3528	0.08	0.02
House prices (euros/sqm)	4762	2362	1005	3087	2023	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 tourism 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 Council data.

Table A10
Logit regression pre-intervention.

P(Treated)=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	0.680
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 Council data.

Table A11
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
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 Council data.

Table A12
Effect of BSaB on crime. Offender and victim categories by age and gender.

	Off. U18		Off. 18–25		Off. 25–35		Off. 35–45	
	M	F	M	F	M	F	M	F
BSaB	−0.213	−0.651***	−2.013	−1.210*	−0.832**	−0.185	−0.055	−0.326
	(0.165)	(0.234)	(1.614)	(0.664)	(0.392)	(0.254)	(0.394)	(0.200)
	Vict. U18		Vict. 18–25		Vict. 25–35		Vict. 35–45	
	M	F	M	F	M	F	M	F
BSaB	0.174	0.269	5.500	6.015	1.383*	0.597	1.050**	0.764
	(0.182)	(0.326)	(4.242)	(5.622)	(0.700)	(0.820)	(0.443)	(0.494)
Observations	3264	3264	3264	3264	3264	3264	3264	3264
Neighborhood FE	Y	Y	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y	Y	Y
Characteristic-specific trends	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table reports the results of the difference-in-differences estimation following Eq. (1) for the 2008–2014 period, and specification of column 5 in Table 5. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing is differs across units. The coefficient shown is that of interest in a difference-in-differences setting, namely *Treated* · *Post*. Driscoll and Kraay (1998) standard errors are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13
Effect of BSaB on crime. Crime categories, clustered standard errors.

	(1)	(2)	(3)	(4)	(5)	Control mean
Against property						
	7.897**	0.804	0.691	0.647	0.180	7.461
	(3.315)	(1.174)	(1.108)	(1.061)	(0.713)	
Against the person						
	0.373***	−0.087	−0.089	−0.094	−0.091	0.760
	(0.115)	(0.074)	(0.071)	(0.068)	(0.054)	
Other						
	0.290**	−0.194	−0.206	−0.209	−0.096*	0.539
	(0.115)	(0.164)	(0.153)	(0.150)	(0.051)	
Intimate						
	0.057**	−0.101***	−0.075**	−0.077**	−0.068***	0.239
	(0.026)	(0.032)	(0.031)	(0.031)	(0.024)	
Anger						
	0.663***	−0.089	−0.063	−0.065	−0.099	1.497
	(0.186)	(0.107)	(0.093)	(0.093)	(0.091)	
Drugs						
	0.110*	−0.013	−0.017	−0.018	−0.019	0.044
	(0.055)	(0.052)	(0.052)	(0.050)	(0.017)	
Observations	4702	4702	4702	4702	3264	
Neighborhood FE		Y	Y	Y	Y	
Year-month FE			Y	Y	Y	
Characteristic-specific trends				Y_{2007}	Y_{2009}	

Notes: This table reports the results of the difference-in-differences estimation following Eq. (1) for the 2008–2014 period. Each column presents a different specification, each more demanding than the previous one. Column 1 present estimates with no fixed effects, column 2 includes neighborhood fixed effects, and column 3 adds year and month fixed effects. Column 4 includes characteristic-specific trends only for those control variables available for the entire sample, measured in 2007 and column 5 includes characteristic-specific trends for all control variables, measured in 2009. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. The coefficient shown is that of interest in a difference-in-differences setting, namely *Treated* · *Post*. Standard errors clustered at the neighborhood level are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A14
Effect of BSaB on crime. Offender and victim categories, clustered standard errors.

	(1)	(2)	(3)	(4)	(5)	Control mean
Off. U18						
	0.488***	−0.583**	−0.513**	−0.512**	−0.426***	0.897
	(0.161)	(0.249)	(0.228)	(0.225)	(0.152)	
Off. 18–25						
	8.917**	−0.801	−1.133	−1.171	−1.744	8.751
	(3.421)	(2.105)	(1.991)	(1.968)	(1.364)	
Off. 25–35						
	2.729**	0.125	−0.105	−0.124	−0.595	5.086
	(1.165)	(0.805)	(0.760)	(0.745)	(0.347)	
Off. 35–45						
	2.464**	0.755**	0.320	0.316	−0.160	3.914
	(0.968)	(0.333)	(0.313)	(0.306)	(0.262)	
Vict. U18						
	1.066*	0.278	0.099	0.093	0.214*	1.241
	(0.582)	(0.199)	(0.192)	(0.182)	(0.120)	
Vict. 18–25						
	25.812*	5.346	6.237	6.108	5.784	13.970
	(13.458)	(5.369)	(5.389)	(5.236)	(4.425)	
Vict. 25–35						
	6.680**	0.034	0.736	0.727	1.040	8.143
	(2.979)	(0.656)	(0.573)	(0.552)	(0.818)	
Vict. 35–45						
	5.593**	0.848*	0.635	0.629*	0.905	7.518
	(2.277)	(0.451)	(0.385)	(0.367)	(0.719)	
Observations	4702	4702	4702	4702	3264	
Neighborhood FE		Y	Y	Y	Y	
Year-month FE			Y	Y	Y	
Characteristic-specific trends				Y_{2007}	Y_{2009}	

Notes: This table reports the results of the difference-in-differences estimation following Eq. (1) for the 2008–2014 period. Each column presents a different specification, each more demanding than the previous one. Column 1 present estimates with no fixed effects, column 2 includes neighborhood fixed effects, and column 3 adds year and month fixed effects. Column 4 includes characteristic-specific trends only for those control variables available for the entire sample, measured in 2007 and column 5 includes characteristic-specific trends for all control variables, measured in 2009. The observational unit is a neighborhood-year-month pair. Treated units are those in which the BSaB policy took place, while those in which it was not are controls. Treatment timing differs across units. The coefficient shown is that of interest in a difference-in-differences setting, namely *Treated* · *Post*. Standard errors clustered at the neighborhood level are shown in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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