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Sweeping up Gangs: The Effects of Tough-on-Crime Policies from a Network Approach†

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Globally, gang proliferation is fought primarily through sweeps. This paper uses a difference-in-differences strategy to examine their causal impact on crime for arrested individuals and their peers. Analysis focuses on the Metropolitan Area of Barcelona, which underwent a notable policy shift. Findings reveal significant crime reductions among those arrested and their peers of 85 percent and 31 percent, respectively. Evidence suggests the first stems from incapacitation and the second from less criminal environment. A counterfactual exercise suggests that broader targeting of key players could have reduced crime by 43 percentage points more. This implies network analysis could enhance policy formulation. (*JEL* C31, H56, K14, K42, Z18)

Norms and social networks significantly influence individuals' decisions to engage in criminal activities, serving as sources of role models, learning opportunities, and information diffusion (Glaeser and Sacerdote 1999). This influence is particularly pronounced within gangs, defined as "any durable, street-oriented youth group whose involvement in illegal activity is part of its group identity" (Weerman et al. 2009, 20). Given the great economic and social costs of gang violence, actions to tackle them are of paramount importance (Barao et al. 2021). Traditional measures to fight gangs, such as harsh punishment and strict prosecution, have been widely employed with coordinated police actions, known as sweeps, being a prevalent intervention (Scott 2003). However, as noted by Scott (2003) and Weisburd and Majmundar (2018), the efficacy of gang sweeps has not been sufficiently evaluated. Since then, Chalfin, LaForest, and Kaplan (2021) have provided the best analysis of gang-targeted interventions to date.

This paper investigates the impact of gang sweeps on crime among arrested individuals and their peers. Exploiting the staggered deployment of the sweeps and a

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difference-in-differences strategy, I compare arrests before and after the sweeps, using not-yet-treated (NY) individuals as controls. For swept areas, I assess broader crime effects and other welfare determinants, comparing outcomes before and after the sweeps, using similar areas as controls. Lastly, to assess the optimality of the policy, I compare the actual crime reductions caused by the sweeps with counterfactual scenarios based on the removal of different sets of key players in the gangs (Ballester, Calvó-Armengol, and Zenou 2006; Lee et al. 2020).

For this analysis, I use police records spanning the 2008–2014 period for the Metropolitan Area of Barcelona (MAB). This setting is particularly compelling: In response to the escalating gang activity, there was a policy change that established a specialized police unit for gang sweeps and intensified judicial prosecution. The dataset includes information on individual arrests, enabling tracking of individuals over time and identification of criminal networks. Notably, significant reductions in crime are observed among both arrested individuals and their peers following the sweeps, with arrested individuals experiencing an average reduction in crime of 85 percent of their pre-sweep mean. The reduction in crime among peers is significant for first-degree peers (31 percent), while no significant effect is observed for second-degree peers. The findings are suggestive that the decrease among peers is primarily driven by deterrence rather than increased caution, as impulsive and noticeable crimes show the most significant effects. Moreover, a counterfactual exercise reveals that sweeps could have led to an additional 43 percentage point decrease in crime had they targeted a broader set of key players, underscoring the potential of network analysis to enhance police interventions.

This study mainly contributes to the existing literature on crime and networks in two ways. First, it estimates the direct effects of sweeps on crime, coming from the effect on arrested gang members, as well as their indirect effect, coming from the spillovers on peers. This contribution follows those of Kling, Ludwig, and Katz (2005); Bayer, Hjalmarsson, and Pozen (2009); Damm and Dustmann (2014); Lindquist and Zenou (2014); Philippe (2017); Corno (2017); Bhuller et al. (2018); Billings, Deming, and Ross (2019); and Billings and Schnepel (2022), extending existing research for gangs and delving into the mechanisms that give rise to them. Second, the analysis sheds light on crime-fighting strategies, offering insights into how policy effectiveness can be enhanced through well-informed interventions. While Chalfin, LaForest, and Kaplan (2021) focus on a policy shift toward precision policing, and Lindquist and Zenou (2019) provide an overview of policy lessons, few studies have involved counterfactual policy exercises to extract recommendations. Concretely, Chalfin, LaForest, and Kaplan (2021) examine the impact of precision policing on gun violence in New York City, evaluating the effects on localized areas, particularly in public housing. This paper extends the analysis by not only studying area-level outcomes in crime, but also focusing on direct and indirect individual outcomes, the underlying mechanisms, and an assessment of the effectiveness of the sweeps through the provision of long-standing yet little-contrasted theoretical counterfactual scenarios.

The remainder of the paper is structured as follows: Section I discusses the tough-on-crime approach to crime prevention and its application in the MAB, Section II presents the data used in the analysis, Section III outlines the methodology employed, Section IV presents the results of the research, and Section V concludes with final remarks.

I. Policy Approaches to Gang Control

Different strategies have been employed in fighting gang crime. In the United States, policies have often leaned toward a tough-on-crime approach, characterized by stringent law enforcement measures such as police search and seizure, strict criminal codes, and severe sentencing (Lynch and Sabol 1997). Economic literature has long emphasized the potential deterrent effect of the justice system (Becker 1968; Ehrlich 1973), and empirical studies support this notion. For instance, Levitt (1997) found that imposing tough sanctions deters criminal activity, while Di Tella and Schargrodsky (2004) observed a significant deterrent effect of visible police presence on crime. Additionally, interventions involving large-scale police efforts have been shown to reduce street crime (Machin and Marie 2011), and the introduction of professional police forces has been associated with a decrease in violent crimes (Bindler and Hjalmarsson 2021).

However, the tough-on-crime approach can have drawbacks. It can be costly (Lynch and Sabol 1997) and ineffective (Kovandzic, Sloan III, and Vieraitis 2004), and may lead to discriminatory outcomes (Arora 2018). As an alternative, other crime prevention strategies have been implemented, focusing on addressing underlying social disparities. These interventions often involve new societal actors and are particularly relevant in deprived areas where social interventions are most needed and where a strong police presence may sometimes be disruptive (Crowley 2013). While these interventions are generally less expensive (Gonzalez and Komisarow 2020; Domínguez and Montolio 2021), they may yield results over longer time frames (Lawless et al. 2010), needing coordination across multiple agencies (Machin, Marie, and Vujić 2011).

Case Study: MAB.—The MAB first identified the presence of Latin gangs in 2002. Over the subsequent decade, the prevalence of such criminal groups steadily increased. The initial wave of gangs included the *Latin Kings* and *Ñetas*, associated with migratory flows from Ecuador. The emergence of *Black Panthers* and *Trinitarios* gangs, linked to migration from the Dominican Republic, followed. The final wave involved *Mara Salvatrucha* and *Barrio 18* from El Salvador and Honduras. Estimates placed gang membership around 2,500 individuals by 2012.[1] These gangs consisted predominantly of men aged 12 to 25 of Latin American origin, although Spaniards and other nationalities were also involved (Herrero-Blanco 2012). Figure 1 illustrates the pattern of gang arrests in the MAB region during this period, with police describing the situation as a cause for "concern but not alarm" (Herrero-Blanco 2012, 121).

The expansion of gangs in this setting relates to the specific context in which it occurred. First, Catalonia, the MAB, and Spain experienced a significant demographic

[1] https://www.eldiario.es/politica/bandas-juveniles.html.

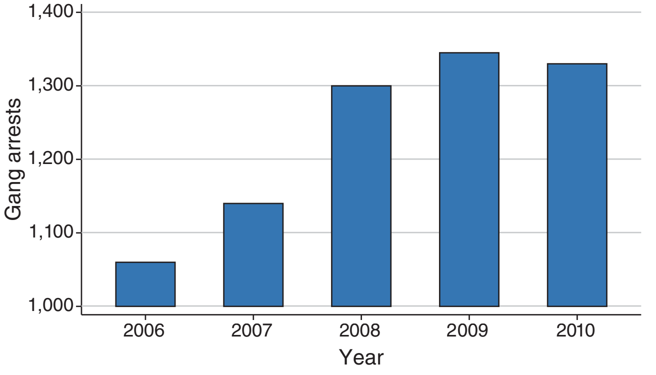

Figure 1. Arrests of Gang Members in the MAB Region

Source: Herrero-Blanco (2012)

shift in the 2000s, with a notable increase in migrant population.[2] Second, a change in security enforcement occurred between 1994 and 2008, with regional police replacing national police, granting greater autonomy to MAB security forces. Third, victimization data indicated a rise in criminal incidents among the population.[3] Moreover, public perception of insecurity in the MAB was on the rise from 2007.[4]

Gang Taxonomy.—The existing literature identifies various factors influencing gang involvement, including social disorganization, neighborhood gang presence, limited social and economic opportunities, monetary incentives, lack of social capital, family disorganization, school-related issues, and street socialization (Weerman et al. 2009; Feixa 2012; Blattman et al. 2021). Traditional gang definitions also often emphasize the role of violence and criminal activities in shaping group identity. However, in this context, these elements do not fully define gang identity.

While many gangs in the MAB share similarities with their counterparts in the Americas, they also exhibit distinct characteristics. Unlike gangs in other contexts, those in the MAB typically lack strong hierarchical structures, have weaker ties to drug distribution networks, and engage in less frequent violence (Córdoba Moreno 2015). The evolution of these groups in the MAB has been marked by lower levels of violence, partially due to limited access to firearms (Weerman et al. 2009), though still higher than typical in European contexts. Gang typologies

[2] Source: Catalan Institute of Statistics (*Idescat*). In Catalonia, the share of non-Spaniards went from 2.9 percent in 2000 to 15.7 percent in 2012 (https://www.idescat.cat/indicadors/id=basics). South American migration, in particular, contributed significantly (https://www.idescat.cat/indicadors/id=basicstema=estra).

[3] Source: Public Safety Survey of Catalonia (*Enquesta de seguretat pública de Catalunya*). The survey shows that in Catalonia, in 2000, 4.5 percent of respondents remembered being victims of any crime in the past 12 months; this percentage had increased to 7.7 percent in 2005 and to 8.5 percent in 2012 (https://interior.gencat.cat/enquestadeseguretatpublicadecatalunya/). For reference, the International Crime Victim Survey shows that in 2005, this value was 13.7 percent in Madrid, 17.8 percent in Paris, 21.7 percent in São Paulo, and 23.3 percent in New York (https://wp.unil.ch/icvs/Table-A8.pdf).

[4] Source: Barcelona City Hall (https://dades.ajuntament.barcelona.cat/enquesta-barometre-municipal/, https://www.barcelona.cat/infobarcelona/en/tema/city-council).

vary, with most exhibiting hierarchical but relatively unsophisticated organizational structures. In terms of criminal activities, gang members in the MAB have been implicated in crimes related to belonging to criminal organizations, offenses against public health (typically involving limited-scale drug markets), and violent robberies (Herrero-Blanco 2012). Police interventions have uncovered weapons such as knives, imitation firearms, and small quantities of drugs. While interpersonal crimes, including assaults and threats, are prevalent, homicides are rare.

Given these contextual differences, the concept of gangs in the MAB was adapted to refer to "new organized and violent youth groups" (*Nuevos Grupos Juveniles Organizados y Violentos*, Herrero-Blanco 2012, 114). These groups are considered "new" as they have recently emerged, adopting behavioral models from American gangs without necessarily having an ideological background. They are deemed "organized" due to their varying but defined internal structures and disciplinary measures. They are labeled "violent" due to their propensity for violence against individuals perceived as threats to themselves, the group, or their peers. The term "youth" encompasses both adults and minors involved in these groups, while "group" reflects the varying sizes and compositions of these entities. Among these characteristics, violence stands out as a primary focus for law enforcement agencies.

Policy Approach.—From a policy perspective, the approach of the local authorities toward addressing gang activity can be divided into two distinct periods.

Between 2004 and 2012, authorities took a lenient stance. In 2004, Barcelona City Hall commissioned a report on the gang situation (Feixa 2012). During this time, gangs were offered the opportunity to integrate into society and formalize their existence as legally recognized associations.[5] The initiative aimed to provide visibility to these groups and encourage members to forsake violence explicitly. However, the majority of gang members did not embrace this approach (Herrero-Blanco 2012; Córdoba Moreno 2015).

In 2012, authorities adopted a more stringent approach. In December 2011, with the enactment of Decree 415/2011, the local police established a specialized unit to address gang-related issues. Known as the "central unit of organized and violent youth groups,"[6] and acknowledging the specific characteristics of the groups present in the MAB, this unit was tasked with investigating crimes affecting public safety and health committed by gangs. The establishment of this unit involved reallocating existing police resources rather than hiring additional personnel. Specifically, 30 community police officers with experience in dealing with gang-related matters were reassigned exclusively to tackle this issue. Consequently, a zero-tolerance policy toward gangs was implemented, including measures such as gang sweeps.

Gang sweeps involve coordinated police actions aimed at arresting a large number of offenders (Scott 2003; Chalfin, LaForest, and Kaplan 2021). These operations are meticulously planned, well coordinated, and highly focused (targeting individuals identified by the central police unit as the leaders), with officers being clear

[5] For instance, the cultural association *Latin Kings y Queens de Cataluña* registered in July 2006 and the socio-cultural association *Ñetas* followed suit in March 2007.

[6] *Unidad central de grupos juveniles organizados y violentos* in Spanish.

about their operational objectives and duties. Theoretically, gang sweeps can reduce crime and disorder in two ways. First, by increasing the certainty of arrest and subsequent punishment, they deter criminal activity. Second, they heighten offenders' perceptions of the likelihood of being apprehended and punished, thereby reducing the number of active offenders. This intensified enforcement concentrates police resources on apprehending remaining offenders, potentially leading to broader crime reduction effects.

Furthermore, alongside this police initiative, there was an enhancement of judicial enforcement. The enactment of a new law (Act 6/2009) established criteria for identifying criminal groups, resulting in stricter judicial outcomes for offenders.[7] This law outlines the criteria that police units must consider when attributing a criminal act to gang activity. Two evaluations are required: first, determining whether the individual exhibits indicators of gang affiliation; and second, verifying whether the crime is connected to gang activity by assessing the involvement of other gang members and the presence of gang-related indicators or previous police investigations.

Thus, in late 2011, authorities substantially shifted their approach to gangs. The new approach involved a specialized police unit, large-scale sweeps, and tougher judicial enforcement. As no other concurrent policy changes impacting crime occurred, this context sets the stage for evaluating the effectiveness of a tough-on-crime approach toward gangs.

II. Data

For this study, I focus on the MAB, which stands out as one of the most significant settings for Latin gangs outside the Americas. This distinction is attributed to Spain's migration phenomenon in the early 2000s and the allure of large cities to the incoming population. The MAB encompasses 36 municipalities and is currently home to 5.8 million inhabitants, making it the third-largest metropolitan region in Europe.[8]

First, this research uses the administrative dataset of all registered crimes and offenders in the MAB, provided by the local police. It spans the years 2008 to 2014 and contains information on all individuals arrested, including their characteristics such as gender, date of birth, and country of birth, and details about the arrest, such as date and time, geographical coordinates, and type of crime (out of 150 categories). An arrest indicates that an individual is booked and taken to a police station, though the outcome after booking (e.g., release or custody awaiting trial) is not included in the available data. Each arrested individual in the dataset is assigned a unique yet anonymous identifier, facilitating the tracking of their criminal history. Furthermore, the dataset enables the identification of co-offenders by matching records based on date, time, geographical coordinates, and type of crime.

Second, data from the specialized gang unit provides information on all sweeps conducted during the initial three years of its operation (2012–2014). This dataset

[7] Although passed in June 2009, Act 6/2009 allowed local governments 18 months to identify relevant criminal groups, characteristics, and actions warranting group convictions. Consequently, it only became applicable in late 2011.

[8] Source: Eurostat (https://ec.europa.eu/eurostat/databrowser/metdemo)

includes details on the date of each sweep, the geographical area targeted, and the number of individuals arrested. Several sweeps occurred during this period, resulting in the arrest of 151 individuals. Sweeps are initiated based on thorough police investigations into gang activities and are executed opportunistically when sufficient evidence for prosecution arises. They are characterized by surprise elements, making it challenging even for the police to predict the full extent of arrests in a given sweep.

The focus of the present analysis refers to individuals arrested in sweeps and their first- and second-degree peers. Arrested individuals are those arrested in the gang sweeps. First-degree peers are individuals not arrested in a sweep that were arrested before a sweep with at least one individual arrested in a sweep. Second-degree peers are individuals not arrested in a sweep, that were not previously arrested with any individual arrested in a sweep, but that were previously arrested with at least one individual identified as a first-degree peer (i.e., they are directly connected to a first-degree peer and not directly connected to an arrested individual). These three categories are mutually exclusive. Out of the 151 individuals arrested in the sweeps between 2012 and 2014, 127 are in the sample data.[9] For these individuals, I identify 413 first-degree and 745 second-degree peers. Supplemental Appendix Figure A1 plots the number of arrested individuals in sweeps per quarter.

Figure 2 illustrates the network structure of these 1,285 individuals. Each dot represents an individual, and its color indicates their relationship to the sweeps: Dark-blue dots are individuals arrested in the sweeps, mid-blue are first-degree peers, and light-blue are second-degree peers. Each line between two dots indicates that those two individuals were arrested together at least once prior to the sweeps. There are a total of 5,480 registered criminal connections among these individuals. In this way, Figure 2 shows all registered connections through arrests across the focus group. These illustrate how most links take place between arrested individuals (dark-blue dots tend to cluster and exhibit many links between them) and how the network gets sparser as first and second link connections are considered (fewer links are observed between dark-blue dots and mid- and light-blue dots). Another important feature is that clusters of arrested individuals do not seem to be connected directly, indicating there is little connection between individuals arrested in different sweeps.

Combining data on all arrests, gang sweeps, and networks, I construct a panel dataset at the individual–quarter level for the years 2008 to 2014. This dataset includes information on arrests, demographic details, the types of crimes for which individuals were arrested, and their relation to the sweeps.[10] Table 1 describes the 1,285 individuals in the sample in terms of their demographic characteristics and criminal profile.

Panel A provides a picture of the average demographic characteristics of the individuals arrested in the gang sweeps and their peers. As anticipated, and in line with the narrative of these groups in the region, the majority of those arrested and their peers are young men of Latin American nationality. Concretely, 87.2 percent of them are men and 57.7 percent are of Latin American nationality, although there

[9] The remaining ones were not arrested in the MAB but in other areas of Catalonia.

[10] The entire dataset comprises 7,349,804 observations derived from 262,493 arrested individuals over 28 quarters.

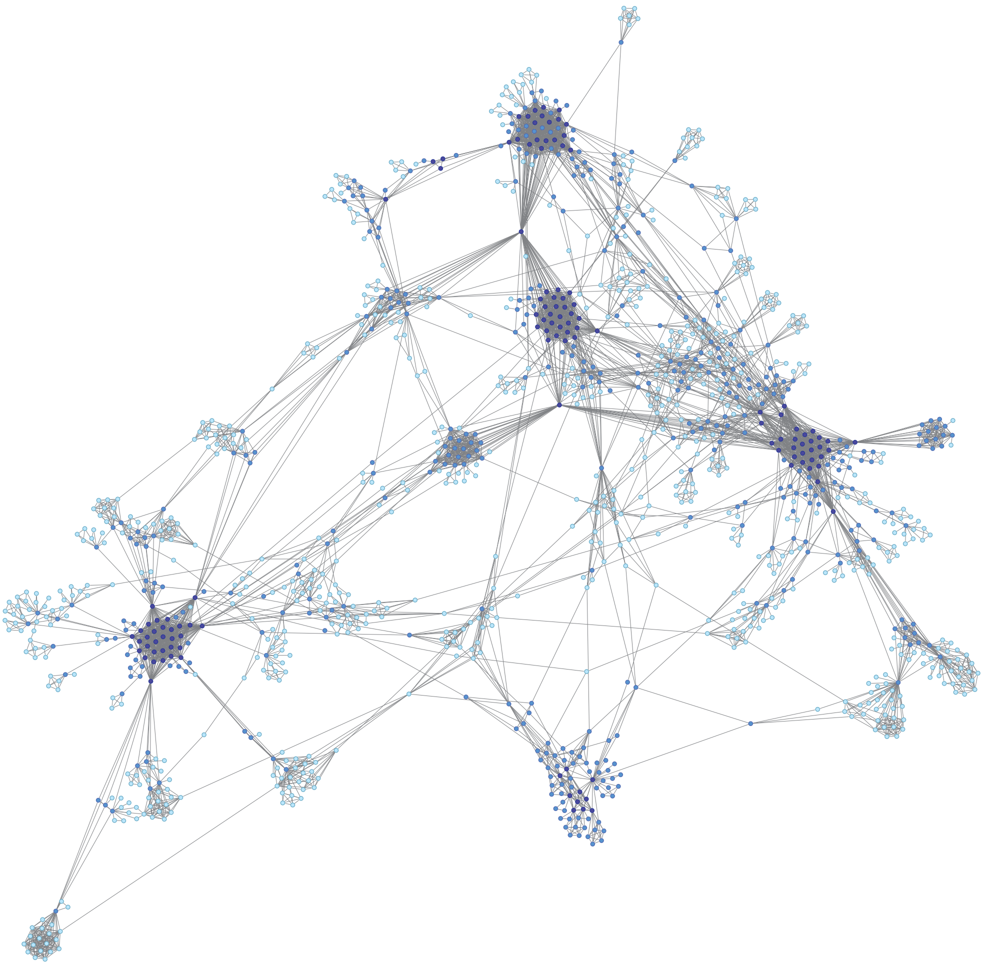

Figure 2. Criminal Gang Structures, before Gang Sweeps

Notes: This graph presents the network structure of those arrested in the sweeps and first- and second-degree peers before the gang sweeps. Each dot is an individual: Dark-blue dots are individuals arrested in the sweeps, mid-blue dots are first-degree peers, and light-blue dots are second-degree peers. Each line indicates a registered criminal link between individuals.

is also a notable proportion of individuals with Spanish nationality (31.8 percent). Their average age over the sample period (2008–2014) is 22.6 years. Regarding their criminal profile, which is shown in panel B, each individual registers on average 4.7 arrests, and 3.1 of these are in groups. Additionally, they have an average of 13 criminal peers and the average age at the first arrest is 21.7 years. Their criminal profile also reveals that, on average, 55.7 percent of their arrests are for crimes against property, 27.0 percent for crimes against people, and 17.3 percent for other types of crimes. While demographic characteristics are relatively similar between those arrested and their peers, there are differences in arrest patterns. The number of arrests remains relatively constant, but those arrested in sweeps tend to have

Table 1—Descriptive Statistics for Individuals Arrested in Sweeps and Their Peers 2008–2014 (Observations = 1,285)

	Mean	Median	Standard deviation	Minimum	Maximum
Panel A. Demographic profile					
Share female	0.128		0.335	0	1
Share Spanish	0.318		0.466	0	1
Share Latin American	0.577		0.494	0	1
Age	22.644	21	8.056	9	69
Panel B. Criminal profile					
Number of arrests	4.691	3	5.008	1	48
Number of arrests in group	3.131	2	3.082	1	34
Number of peers	12.689	7	14.090	1	119
Age at first arrest	21.687	19	7.834	9	65
Number of arrests for crimes against property	2.615	1	3.858	0	46
Number of arrests for crimes against people	1.266	1	1.559	0	12
Number of arrests for other crimes	0.810	0	1.396	0	17

Note: This table reports descriptive statistics for the 1,285 individuals that the analysis focuses on (arrested, first-degree peers, and second-degree peers) regarding their arrests and demographic characteristics. Source: author's construction from local police data.

fewer charges related to property crimes and more related to "other crimes"[11] (see Table A1 in Supplemental Appendix A).

III. Methodology

A. *Sweeps Analysis*

Using the panel structure described in the previous section, I conduct several empirical exercises.

First, I estimate a static two-way fixed effects (TWFE) regression specification as follows:

$$(1) \qquad Crime_{it} \; = \; \alpha_i + \phi_t + \beta \, . \, Treat_{it} + \gamma \, . \, \mathbf{X}_{it} + \varepsilon_{it}.$$

The unit of observation is an individual–quarter pair, and the dependent variable $Crime_{it}$ represents either the number of total arrests or arrests by crime type. Individual fixed effects α_i and period (year–quarter) fixed effects ϕ_t are included. The binary variable $Treat_{it}$ equals 1 if person i receives treatment during period t, where treatment can result (i) from arrest in a police sweep, or through influence for (ii) first- or (iii) second-degree peers. Once treated, an individual remains in the treated state indefinitely, so $Treat_{it}$ remains 1 for all periods following treatment. Control units comprise individuals in the same treatment arm who have not yet been treated, and equation (1) is estimated separately for each treatment arm ((i) arrested, (ii) first

[11] Charges related to belonging to criminal organizations are categorized under "other crimes."

peers, and (iii) second peers). $\mathbf{X}_{it}$ includes additional control variables, which in the main specification will include demographic linear time trends. Other options will include (more stringent) individual-specific time trends and (less stringent) gang-specific time trends. Assuming parallel trends and no anticipation, the coefficient β identifies the average treatment effect (ATT). Standard errors are clustered at the individual level to account for heteroskedasticity and serial correlation, as that is the treatment assignment level.

This choice of control group (i.e., NY individuals in the same treatment arm) is based on finding the most comparable group of individuals in the study. As gang members differ from any other type of offender, this choice seems the most suitable one to provide a counterfactual scenario of arrest trajectories in the absence of sweeps. This leaves a main estimating sample size of 3,556 for arrested individuals, 11,564 for first peers, and 20,860 for second peers. The number of treated observations per quarter relative to the sweep is shown in Supplemental Appendix Figure A2, and a raw plot of the average number of arrests per individual is presented in Supplemental Appendix Figure A3 for arrested individuals and peers, according to whether they are linked to an early or a late sweep. Early sweeps correspond to the first half and late sweeps to the second. This graph shows how crime is developing along similar trends for early and late treated individuals before their corresponding sweep. Alternative control groups (such as all other group offenders, or individuals arrested in arrests similar to sweeps but before the policy change) and alternative control variables (such as different time trends) are also presented in the main analysis.

The identifying assumption is that being treated is unrelated to individual crime patterns at the time of the sweeps compared with all other individuals in the same treatment arm. Importantly, the gang sweeps analyzed in this paper were not based on random arrests resulting from misdemeanors or simple offenses. As explained, sweeps stem from long and complex criminal investigations, targeting individuals identified by the central police unit as the leaders of the gangs. On this matter, there are two points worth highlighting. The first is that the police could not predict all gang members arrested in the specific sweep, which introduces some randomness within each gang in who is arrested and who is a peer. Second, the sweep does not respond to immediate previous crime activity (as it is being prepared long in advance), and the specific timing also depends on gathering sufficient evidence for prosecution and finding a window of opportunity to deploy the sweep, making it unlikely for individuals to anticipate treatment systematically.

Second, I estimate a dynamic TWFE regression model, which regresses the outcome on individual and period fixed effects, along with indicator variables for time relative to treatment:

$$(2) \qquad Crime_{it} \;=\; \alpha_i + \phi_t + \sum_{-D \leq d \leq D; d \neq -1} \beta_d Treat_{it}^{d} + \gamma . \mathbf{X}_{it} + \varepsilon_{it},$$

where $Treat_{it}^{d} = 1$ are indicator variables for individual *i* being *d* periods away from treatment at time *t*, treatment being either (i) arrested in a sweep, (ii) first peer, or (iii) second peer. α_i and ϕ_t are individual and period fixed effects, respectively. The coefficients $\{\beta_{-D}, \ldots, \beta_{-2}\}$ identify anticipation, and coefficients $\{\beta_0, \ldots, \beta_D\}$ identify dynamic treatment effects. As with equation (1), control units in equation (2) comprise individuals in the same treatment arm who have not yet been treated, and equation (2) is estimated separately for each treatment arm.

Finally, I conduct similar analyses to equations (1) and (2) focusing on swept areas rather than individuals. These analyses assess whether socioeconomic outcomes change following sweeps in specific areas. To define treatment, I consider areas where sweeps occurred. An area is defined as a district within the city of Barcelona (ten districts), with the remaining municipalities (35) in the MAB treated as one unit. The analysis examines policing activity, criminal outcomes, and other welfare indicators. This final exercise aims to determine whether the impacts of sweeps extend beyond those associated with individual arrests and criminal outcomes. For this exercise, the control group will comprise nontreated areas that were similar to the treated ones in pre-sweep characteristics and were identified by propensity score matching on nearest neighbors.

B. *Policy Analysis: Assessing the Success of the Sweeps*

The estimation results from the previous subsection, presented in Section IV, provide insights into the actual effects of the sweeps in the MAB. From a policy perspective, several questions arise:

- Was the design of the sweeps optimal? Did the sweeps target the most influential individuals within criminal networks effectively?
- If it was not, how far off were the sweeps? Were there missed opportunities for more impactful interventions?
- What are the policy lessons to extract from this case? What adjustments could be made to future intervention strategies based on the findings?

One way to compare the results of the sweeps with what they could have been is to benchmark them against the effects of removing key players in the gangs. Lindquist and Zenou (2012, 20), following the seminal work of Ballester et al. (2006, 2010) and Ballester and Zenou (2014), define the "key player" as the "individual who, when removed from the network, leads to the largest reduction in aggregate crime." I adopt the same definition in this paper. The main findings in Ballester et al. (2006) suggest that a strategy targeting the removal of the key player results in a greater reduction in criminal activity than removing any other individual. In addition to the author's significant contribution to the literature, their results have important implications for policy design. Specifically, targeting key players may lead to substantial reductions in criminal networks' activity at a fraction of the cost of large-scale interventions.

Given these points, I conduct a counterfactual exercise comparing the actual changes in crime resulting from sweeps and simulated scenarios based on removing key players. To achieve this, I follow four steps, which are briefly explained in the following textits. Their full explanation can be found in the Supplemental Appendix.

(**a**) **Estimate Peer Effects on Crime for Gang Members:**

This step provides an estimate of peer effects. In this case, peer effects measure how much peers' crimes affect those of an individual.

Identifying peer effects can suffer several problems, as pointed out by Bramoullé, Djebbari, and Fortin (2009, 2020). The first is the reflection problem (Manski 1993). This issue arises from the impossibility of distinguishing the effect of peers on the individual from the effect of the individual on peers if both are determined simultaneously. The second potential issue is that the observed network can be endogenous, and individuals choose who to connect with. If that is the case, it is impossible to identify whether the correlation of behavior among peers comes from the fact that they are connected or from the fact that they share characteristics (homophily). To solve these issues, I follow the instrumental variables approach in three stages proposed by Lee et al. (2020). Concretely, the predicted friends-of-friends' characteristics are used as an instrument for the friends' outcomes. This uses the traditional approach in the network literature related to the incompleteness of the network and the friends-of-friends instrument (addressing the simultaneity problem), augmented by a link formation model, to use a predicted network instead of the observed one (addressing the endogeneity problem). This last point is crucial in this setting, as homophily is a strong factor, making the network likely endogenous. It also alleviates potential concerns about the mismeasurement of links in the arrest data.

(**b**) **Compute Individual Centrality within the Gang:**

This step provides a ranking of how central each individual is to their gang.

To do so, I follow the measure by Ballester and Zenou (2014), which feeds on the peer effects estimates of step (a) as well as on the network structure. For this computation, two assumptions are needed. First, the gang is fixed and does not rewire after the sweeps. Second, individual criminal ability does not depend on the gang they are in. Once all individuals have their centrality computed à la Ballester and Zenou (2014), it is possible to rank all individuals by centrality and identify the key player (Ballester, Calvó-Armengol, and Zenou 2006).

(**c**) **Construct Outcomes of Simulated Sweeps:**

In this step, I calculate the predicted cumulative reduction in crime as a function of the number of top individuals removed. This creates the crime results of simulated sweeps when removing key players. On this point, I first follow Lindquist and Zenou (2014) to compute the predicted crime reduction in a gang after removing the key player. I then extend this approach to compute the predicted crime reduction in a gang after removing a number n of top individuals. The result of this exercise serves as the benchmark of simulated sweeps.

(**d**) **Compare These Simulations with the Actual Results of the Sweeps:**

In the final step, I compare the results of the simulations in step (c) with the actual results of the sweeps, in order to assess their optimality.

Addressing these questions can provide valuable insights for policymakers seeking to optimize strategies for combating criminal activities and improving public safety in contexts like the MAB.

Table 2—Effects of Sweeps on Crime: Baseline Estimates

	Baseline		Alternative specifications					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Arrested								
(DV mean = 0.461)								
Treated × post	−0.417	−0.393	−0.388	−0.388	−0.400	−0.393	−0.469	−0.400
	(0.037)	(0.037)	(0.039)	(0.039)	(0.041)	(0.043)	(0.052)	(0.041)
Observations	3,556	3,556	3,556	3,556	3,556	3,556	3,556	3,556
Panel B. First peers								
(DV mean = 0.213)								
Treated × post	−0.074	−0.066	−0.063	−0.063	−0.046	−0.037	−0.060	−0.046
	(0.024)	(0.023)	(0.024)	(0.024)	(0.024)	(0.025)	(0.030)	(0.024)
Observations	11,564	11,564	11,564	11,564	11,564	11,564	11,564	11,564
Panel C. Second peers								
(DV mean = 0.179)								
Treated × post	−0.035	−0.016	−0.013	−0.013	−0.017	−0.012	−0.057	−0.017
	(0.023)	(0.023)	(0.023)	(0.023)	(0.025)	(0.026)	(0.028)	(0.025)
Observations	20,860	20,860	20,860	20,860	20,860	20,860	20,860	20,860
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year–quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic linear time trends		Yes						
Alternative time trends			Demographic quadratic	Demographic cubic	Gang linear	Gang quadratic	Gang cubic	Individual linear
Control group	NY	NY	NY	NY	NY	NY	NY	NY

(*continued*)

IV. Sweeps Analysis: Results

A. *Baseline Estimates*

Table 2 displays estimates of equation (1) for individuals arrested in the sweeps (panel A), first peers (panel B), and second peers (panel C), considering all arrests as the dependent variable and the TWFE estimator.

Baseline estimates are presented in columns 1 and 2. While the first controls for individual and time fixed effects, the second one further includes demographic-specific (gender, age, and nationality) linear time trends, and it is the more complete and preferred specification. On this matter, demographic-specific linear time trends provide a middle ground between controlling for gang-specific or individual-specific linear time trends, which are also presented in Table 2 and will be discussed in the robustness section. The results in both baseline specifications indicate a significant decrease in crime after the sweeps for arrested individuals and first peers. Considering the estimates in column 2, total crime among the arrested falls by 85 percent of its pre-sweep mean value. For first-degree peers, the reduction in total crime is also significant, of a smaller magnitude, yet still considerable (31 percent). For second-degree peers, sweeps do not significantly affect their criminal activity. Results considering the Sun

Table 2—Effects of Sweeps on Crime: Baseline Estimates (*continued*)

	Alternative control groups			
	(9)	(10)	(11)	(12)
Panel A. Arrested				
(DV mean = 0.461)				
Treated × post	−0.122	−0.123	−0.119	−0.245
	(0.016)	(0.016)	(0.017)	(0.067)
Observations	3,522,680	2,739,772	1,740,088	3,522,680
Panel B. First peers				
(DV mean = 0.213)				
Treated × post	−0.042	−0.044	−0.052	−0.068
	(0.018)	(0.016)	(0.016)	(0.027)
Observations	3,530,688	2,745,400	1,746,928	3,530,688
Panel C. Second peers				
(DV mean = 0.179)				
Treated × post	−0.027	−0.010	−0.010	−0.012
	(0.013)	(0.015)	(0.015)	(0.017)
Observations	3,539,984	2,751,812	1,754,000	3,539,984
Individual FE	Yes	Yes	Yes	Yes
Year–quarter FE	Yes	Yes	Yes	Yes
Demographic linear time trends	Yes	Yes	Yes	Yes
Alternative time trends				
Control group	NT all	NT crime profile	NT demographic profile	NT all, control variables

Notes: This table reports the results of the TWFE estimation of equation (1) for all crimes. Each panel presents treatment effects for a different group: arrested individuals (panel A) and first- (panel B) and second-degree peers (panel C). The observational unit is an individual–quarter pair and estimations are done for each treatment arm separately. Columns 1 and 2 present baseline estimates, columns 3 to 8 present alternative time trends specifications, and columns 9 to 12 have the same specification as column 2 with alternative control groups drawn from the pool of never-treated (NT) individuals. For columns 1 to 8, control units are NY individuals in the same treatment arm. For columns 9 to 12, control units are different subsets of NT individuals. Standard errors clustered at the individual level are shown in parentheses.

and Abraham (2021)—henceforth, SA—estimator provide a similar qualitative interpretation of the findings, although the coefficients are larger in magnitude.

Results on the ATT for all three groups combined (arrested, first peers, and second peers together) and for their pairwise combinations (arrested and first-degree peers; first- and second-degree peers) are presented in Supplemental Appendix Figure A4, following the specification in column 2 of Table 2. These estimates would approximate the aggregate effects of the sweeps. Results show that considering arrested, first peers, and second peers together, sweeps significantly reduced total crime by 29.8 percent of its pre-sweep mean value. However, as Table 2 outlines, the sweeps did not affect second-degree peers. For arrested individuals and first-degree peers together, results show that sweeps resulted in a compound reduction in crime of 51.4 percent of its pre-sweep mean value.

One final way of quantifying the aggregate effects of the sweeps is based on calculating the social multiplier. Following Dahl, Løken, and Mogstad (2014), it is possible to approximate peer effects in treatment following a two-stage approach. Using the estimated effects on peers as a reduced-form estimate and the estimated effects on those arrested as a first-stage estimate, it is possible to obtain the 2SLS estimate of peer effects by computing the ratio between the two. In this case, those results provide an estimate of peer effects of 0.168 $(\frac{-0.066}{-0.393})$. This implies that for every 1 crime reduction among the arrested, there is an additional 0.168 crime reduction among first peers.

B. *Robustness to Alternative Specifications and Control Groups*

This subsection presents several robustness exercises that address potential identification concerns.

The first set of checks keeps the control group constant as the NY individuals in the same treatment arm but runs alternative regressions to the preferred baseline specification. Concretely, these include variations of the time trends included and are presented in columns 3 to 8 of Table 2. As already mentioned, column 2 includes individual and time fixed effects as well as demographic linear time trends. Columns 3 and 4 also include demographic time trends but in quadratic and cubic specifications respectively. These exercises follow the spirit of Bilinski and Hatfield (2018) and their "one step up" approach. Columns 5 to 7 consider gang-specific time trends in their linear, quadratic, and cubic specifications respectively. This set of exercises mimics those in columns 2 to 4, considering a somewhat less stringent specification. Lastly, column 8 presents the most stringent specification by including individual-specific linear time trends. Across this set of alternative specifications, the baseline results for all three treatment groups hold, as the coefficients remain statistically significant for arrested individuals and first peers. Results for second peers are less stable but are still in line with the baseline specification. Additionally, the magnitude of the estimated coefficients remains relatively stable across specifications.

The second set of checks keeps the preferred baseline specification of column 2 but considers alternative control groups, drawing from the pool of NT individuals in the sample. As the estimating sample changes, so does the number of observations in each regression. These estimations are presented in columns 9 to 12 of Table 2. Column 9 considers all NT group offenders as the control group and, therefore, is the most general approach. Columns 10 and 11 further refine the selection of control units. Column 10 considers NT group offenders that have a similar profile to the treated ones in terms of their arrests patterns prior to the policy change, while column 11 considers NT group offenders that have a similar profile to the treated ones in terms of their demographic characteristics. Lastly, column 12 considers all NT group offenders as the control group and also includes control variables for prior arrests. Across this set of alternative control groups, the baseline results for all three treatment groups also hold, as the coefficients remain statistically significant for arrested individuals and first peers and no effect is found for second peers.

C. *Dynamic Estimates*

The dynamic TWFE specification of equation (2) is shown in Figure 3. The top panel shows results for arrested individuals, the middle one for first peers, and the bottom one for second peers. Endpoints -12 and 6 are binned, which implies assuming constant treatment effects before and after. Due to the potential limitations of TWFE estimates in staggered treatment settings,[12] results using the estimator proposed by SA and considering the last treated cohort as the control group are also shown in Figure 3, labeled "SA." For both estimators, the individual coefficients are presented in Supplemental Appendix Table A2.

Figure 3 and Supplemental Appendix Table A2 provide evidence that the crime drop is sharp and immediate for arrested individuals when considering the TWFE dynamic estimator. Moreover, this reduction persists across the postpolicy period. For first-degree peers, the temporal dynamics of the effect of the sweeps are different from those for offenders. Concretely, the effect seems to increase over time for quarters 1 to 3, and is at a maximum in quarter 3, when considering the TWFE dynamic estimator. This pattern may relate to the average time taken to resolve a process legally. According to statistics from the Spanish judiciary system, the average timescale for "brief procedures" in Catalonia is 9.8 months.[13] For first-degree peers, the crime reduction seems to stabilize just around this time when considering the TWFE dynamic estimator. Results considering the estimator from SA are very similar to those estimated by TWFE, except for the estimated coefficient on six or more quarters since the sweep, which may be linked to the smaller number of observations in these points. For these, the TWFE estimates are smaller for both arrested individuals and first-degree peers. For second-degree peers, no clear pattern arises.

D. *Assessment of Parallel Trends, and the "Honest" Approach*

An important assumption that underpins the identification of the treatment effects using a difference-in-differences design is that of parallel trends. This would support the statement that in the absence of treatment, treatment and control groups would evolve in the same way.

A visual inspection of Figure 3 evidences that there are indeed some pre-policy coefficients that are statistically significant, particularly for arrested individuals. This is further backed up by Supplemental Appendix Table A2. For first peers, no individual pre-policy coefficient is statistically significant at the 95 percent confidence level when considering the TWFE estimator, and one is statistically significant at the 90 percent confidence level when considering the SA estimator. The

[12] Recent advances in econometrics literature (De Chaisemartin and D'Haultfoeuille 2020; Borusyak, Jaravel, and Spiess 2024; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; SA), with summaries by Roth et al. (2023) and De Chaisemartin and d'Haultfoeuille (2023), show that TWFE estimates provide a reasonable estimate of ATTs only when there is no heterogeneity in treatment effects across time or units and propose heterogeneity-robust estimators.

[13] Statistics are available at the regional level. I use the regional average to approximate the MAB (Poder Judicial Espana, n.d.).

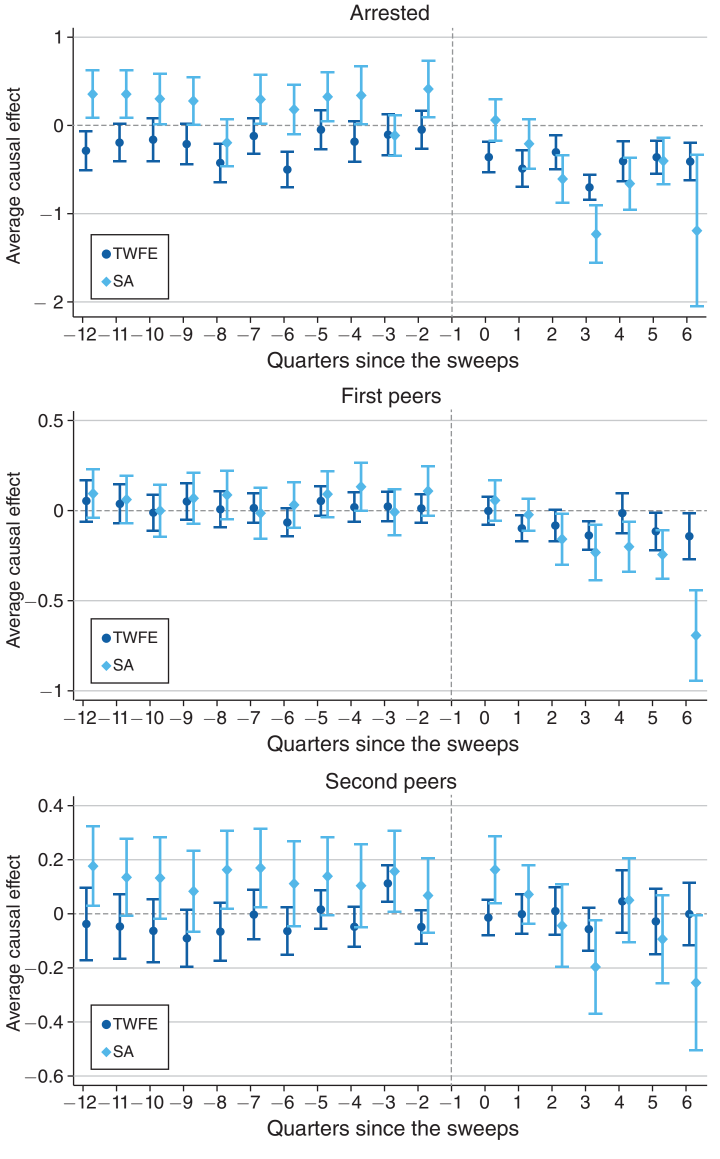

FIGURE 3. EFFECTS OF SWEEPS ON CRIME: DYNAMIC ESTIMATES

Notes: This graph reports the results of the TWFE estimation of equation (2) alongside the SA heterogeneity-robust estimator for all crimes for arrested individuals and first- and second-degree peers. The observational unit is an individual–quarter pair, and estimations are done for each treatment arm separately. Control units are NY individuals in the same group for the TWFE estimator, and the last treated cohort for SA. The 95 percent confidence intervals are based on standard errors clustered at the individual level.

joint significance F-tests for all prepolicy coefficients reject the null hypothesis of all coefficients being equal to zero (no violation of parallel trends) for arrested individuals at traditional confidence levels, while for first peers the assessment is less clear, with p-values of 0.075 and 0.131 for the TWFE and SA estimators respectively.

However, Bilinski and Hatfield (2018, 4) argue that "even when power is adequate, model assumption tests are oriented toward statistical, rather than practical, significance. As sample size grows, they will always eventually achieve statistical significance, indicating a violation of the assumption. However, the magnitude of violation may be too small to have practical importance." They also argue that "researchers should use study context to determine what violation magnitude is meaningful." In this case, it seems to be the case that even if parallel trends tests detect statistically significant violations, they are too small to be of practical importance. This is based on several facts. First, few coefficients are statistically significant individually, and those that are, are not consecutive. Second, these statistically significant coefficients (and largely all prepolicy coefficients) are much smaller than the ones found for the postpolicy period. Third, the estimated slope of the potentially remaining pre-trend is also small when compared with the treatment effect. The estimated slope for arrested individuals is 0.016 (while the treatment effect is -0.393) and the slope for first peers is -0.005 (with a treatment effect of -0.066).

To further back up the argument of nonmeaningful violations of the parallel trends assumption, I present two sets of evidence. The first recalls the "one step up" method proposed by Bilinski and Hatfield (2018). This consists of reporting treatment effect estimates from a model with a more complex trend difference than is believed to be the case and testing that the estimated treatment effect falls within a specified distance of the treatment effect from the simpler model. In this case, Table 2 includes treatment effect estimates from models with quadratic and cubic time trends instead of the baseline linear time trends, and the coefficients remain virtually stable.

The second set of evidence relies on the sensitivity analyses proposed by Rambachan and Roth (2019) in their "honest" approach to parallel trends. The authors propose placing restrictions on the possible values of the posttreatment difference in trends given the (identified) value of the pre-trend. In this setting, as differences in trends seem to evolve smoothly over time, I impose Rambachan and Roth's (2019) smoothness restrictions (or $\Delta^{SD}(M)$ method). This type of restriction imposes that posttreatment violations of parallel trends cannot deviate too much from a linear extrapolation of the pre-trend. In particular, the method imposes that the slope of the pre-trend can change by no more than M across consecutive periods or, in other words, by no more than how quickly the slope of the differential trend can change over time. Imposing a smoothness restriction with $M = 0$ implies that the counterfactual difference in trends is exactly linear, and larger values of M allow for nonlinearities.

Figure 4 reports confidence intervals of a sensitivity analysis for the ATT on crime for arrested individuals and first peers under varying assumptions on the class of possible violations of parallel trends. In the sensitivity analysis for first peers (right panel), the breakdown value for a significant effect is $M \approx 0.003$, which would correspond to allowing the slope of the differential trend to change quarterly by the equivalent of $+/-$ 60 percent of the estimated remaining prepolicy slope. This indicates that even when allowing for large deviation of the estimated remaining pre-trend, results still hold. For arrested individuals (left panel), the treatment effects are statistically significant for the different proposed values of M. This set of findings further probes the robustness of the findings.

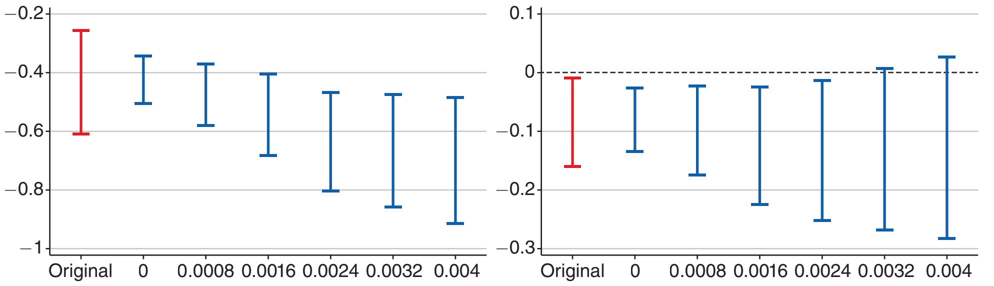

Figure 4. Sensitivity Analysis for Effects of Sweeps on Crime for Arrested Individuals (Left) and First-Degree Peers (Right), Using $\Delta = \Delta^{SD}(M)$

Note: This graph reports confidence intervals of a sensitivity analysis for the ATT on crime for arrested individuals and first peers under varying assumptions on how quickly the slope of the differential trend can change over time, following Rambachan and Roth (2019).

E. *Data Limitations and Implications*

As with any police records, the dataset I use suffers from the issue of "dark figures," meaning it does not capture information about undetected crimes, which is a common limitation in crime data analysis. In that sense, using arrests data could imply that some first-degree peers are not identified, potentially leading to biased estimates of spillover effects. A helpful approach to assess the magnitude of this issue is to analyze second-degree peers as a falsification exercise for nondetected first-degree peers. As the results show no effects for second peers, they alleviate concerns about missing many first links in the first-degree spillover analysis.

Another concern is that of selection and attrition in the sample that could stem from various sources, the most critical being migration. As it is not possible to link the offenders to any other registry, it is not possible to directly test whether first peers have a differential migration pattern from other offenders or the general population.

To provide a partial approximation to the topic, I rely on migration statistics of the period. These indicate that in 2014 there were 7,416,237 inhabitants in the Catalan region, and the net migration flow with the rest of Spain and the world was −35,158, indicating there were more individuals migrating out of than into the region, but the net migration was of small magnitude.[14] This negative net flow also holds for foreign-born residents. Concretely, 10 percent of foreign-born residents between the ages of 15 and 64 migrated out of the region, either to other Spanish regions or outside Spain. However, the top nationalities in terms of return migration for this period were Romanian, Moroccan, and British—none of these being of relevance for the focus group of gang members.

Furthermore, I conduct bounding exercises on the baseline estimates for first-degree peers, removing different sets of 10 percent shares of first peers. These allow approximation of the effects on first peers, taking into account potential sample attrition and applying the general patterns for out-migration of foreign-born

[14] https://www.ine.es/prensa/np917.pdf.

residents. In one exercise, I remove the top 10 percent of first peers by number of arrests before the sweeps. In another, I remove the bottom 10 percent of first peers by number of arrests before the sweeps. These two extreme cases bound the estimates if such potential 10 percent migration was not random. Treatment effects for first peers are −0.050 (standard error 0.025) when removing the bottom 10 percent and −0.079 (standard error 0.019) when removing the top 10 percent. These results, even if they do not directly tackle potential migration issues in this subsample, indicate that if individuals in the sample migrate in the same proportion as all foreign-born residents in the region, the results still hold even when considering nonrandomness of who that 10 percent are.

F. *Mechanism Analysis*

Evidence suggests a mechanical effect driven by incapacitation for individuals arrested in the sweeps. Although the available data only cover outcomes for a relatively short period after the sweeps began, it is highly probable, although unverifiable due to data limitations, that many of these individuals are incarcerated. Several factors contribute to this likelihood. First, Act 6/2009, which accompanied the sweeps, increased the probability of those arrested being held in pretrial detention. Second, the judicial process in the MAB's region takes an average of 9.8 months, indicating that arrested individuals may remain incarcerated for at least this length of time while awaiting trial. Third, individuals arrested in the sweeps typically face charges for serious offenses, which under the Spanish penal code could result in sentences of at least five years in prison. These factors combined suggest a high probability of incarceration for individuals arrested in the sweeps, reinforcing the notion of a mechanical effect driven by incapacitation.

The sweeps could theoretically have a deterrent effect on peers, leading to a reduction in their criminal activity. Conversely, there is also the possibility that the criminal opportunities of those not arrested increase, potentially resulting in heightened violence as they compete for power within the group. This study's results indicate a decrease in registered crimes among first-degree peers and no significant change among second-degree peers. This finding suggests that the deterrent effect outweighs any increase in criminal activity.

For first-degree peers, the diminished incentive to engage in criminal activity following a sweep is influenced by several factors. I now delve into them through heterogeneity analysis across types of crime and individual characteristics. I will also conduct falsification exercises to assess the role of policy enforcement in the matter.

Deterrence? And Why?—A first distinction is whether the reduction in first-degree peers' arrests after the sweeps stems from their own incapacitation, from systematically avoiding detection, or from deterrence. Through a ruling-out exercise, evidence would point toward the third option being the most plausible one.

The available data do not provide direct evidence on this matter, as they lack information on incarceration. However, if these peers were indeed incapacitated due to prior legal rulings, one would expect to observe a different pattern in Figure 3 preceding the sweeps. Concretely, a systematically lower crime level for first-degree peers should be observed earlier. However, this is not evident. Furthermore, only 13 percent of these peers exhibit any criminal activity concurrently with the sweeps (54 individuals). Even if there were an argument for incapacitation among this group, it would concern a relatively small proportion, so could not fully explain the observed results. Moreover, an estimation of the baseline effects for first-degree peers without those 54 individuals, as in column 2 of Table 2, still provides statistically significant treatment effects of -0.101 (standard error 0.020). These observations weaken the argument for the reduction in first-degree peers' arrests stemming from their incapacitation.

The weak case for concurrent incapacitation leaves the explanation of the reduction in arrests for first-degree peers being that they are either avoiding detection or are genuinely deterred. To assess these cases, I examine the effects by crime typologies. Table 3 suggests that the decline in crimes is primarily associated with impulsive behavior (such as damage to property, injuries, or assaults) rather than planned actions (such as thefts). This finding weakens the hypothesis supporting systematic avoidance of detection as impulsive crimes are harder to systematically conceal. Moreover, the nature of the data further undermines this hypothesis: Police records are based on the date the crime occurred rather than the arrest date, encompassing both individuals arrested *in fraganti* and those who managed to evade detection for some time before eventual arrest. So even if first-degree peers avoided detection for some time, if they were detected at some later point then they would still appear in the dataset. Lastly, as highlighted by Lindquist and Zenou (2014), over longer periods of analysis, it becomes increasingly challenging for all active peers to systematically avoid detection.

A second distinction in why first-degree peers may be deterred relates to the driving factors behind engaging in crime. This attributes the decline in crime among first peers to either a loss in "criminal human capital" or changes in the "criminal environment." As noted by Philippe (2017), the former relates to criminal activities that require specialization, knowledge, and planning. These are more likely needed for crimes against property, such as robbery or theft. Changes in the criminal environment could reduce impulsive behaviors or social influences that encourage engagement in crime. Impulsiveness is more likely to be a factor in crimes against the person, such as injuries, threats, or assault, or in crimes against property such as damage. Table 3 summarizes the evidence to disentangle these mechanisms. For first-degree peers, there is a significant reduction in crimes against property and against the person, which are of similar relative magnitudes with respect to their pre-sweep mean. The patterns observed in the table support the hypothesis of a reduction in both "criminal human capital" and "criminal environment."

The last distinction refers to whether the sweeps deter a specific group of first-degree peers. One factor that may deter peers from committing crimes is the increased risk of arrest. Philippe (2017) suggests the hypothesis that heightened perceived sanctions influence individuals with shorter criminal careers to a larger extent as they gain new information, whereas additional information has little impact for more seasoned criminals. Supplemental Appendix Table A3 presents heterogeneity effects of sweeps for first peers based on their percentile in the distribution of

Table 3—Effects of Sweeps: Crime Typologies Estimates

	Property	Person	Other	Property			
	all	all	all	Theft	Robbery	Damages	Fraud
Panel A. Arrested							
Treated × post	−0.183	−0.051	−0.159	−0.021	−0.055	−0.014	−0.092
	(0.026)	(0.019)	(0.023)	(0.010)	(0.017)	(0.007)	(0.015)
Observations	3,556	3,556	3,556	3,556	3,556	3,556	3,556
Mean	0.171	0.114	0.175	0.024	0.057	0.008	0.079
Panel B. First peers							
Treat × post	−0.045	−0.022	0.002	0.007	−0.018	−0.007	−0.021
	(0.018)	(0.010)	(0.008)	(0.012)	(0.009)	(0.004)	(0.007)
Observations	11,564	11,564	11,564	11,564	11,564	11,564	11,564
Mean	0.125	0.062	0.025	0.036	0.043	0.008	0.032
Panel C. Second peers							
Treat × post	−0.016	−0.001	0.001	0.006	−0.003	−0.007	−0.003
	(0.019)	(0.008)	(0.008)	(0.011)	(0.009)	(0.004)	(0.006)
Observations	20,860	20,860	20,860	20,860	20,860	20,860	20,860
Mean	0.106	0.042	0.032	0.041	0.027	0.005	0.024

(*continued*)

number of arrests before the sweeps took place. The analysis reveals no statistically significant difference between peers with high and low frequencies of arrests prior to the sweeps. This outcome aligns with the conclusions drawn by Philippe (2017), indicating that the sweeps do not affect first peers differentially according to their pre-sweeps arrest history.

The Role of Enforcement.—Another issue to address in relation to the reduction in arrests for first peers is changes in police enforcement.

A first consideration is whether a police profiling strategy is at play. Given that the gangs under study are predominantly Latin, it could be the case that individuals with similar characteristics to gang members (young Latin men) would be targeted by the police after implementation of the policy. This profiling would imply that any individual sharing demographic characteristics with those involved in gangs could experience a change in arrest incidence.

To assess this, I compare arrests among all individuals with demographics matching those of individuals arrested in the sweeps (young Latin men, regardless of their gang membership) and arrests among individuals with other demographic profiles that are also perceived as "high crime" in the MAB, before and after the policy change. I do so in a difference-in-differences setting, where the treatment group is the former and the control group is the latter. The analysis, presented in Table 4 reveals no statistically significant differences between the groups. Hence, the results suggest the policy enforcement is linked to a specific targeting of gang members rather than a profiling strategy applied to all individuals with similar demographic characteristics.

A second consideration is whether sweeps operate similarly to any other arrest or whether these sweeps produce different effects. To address this, I identify

Table 3—Effects of Sweeps: Crime Typologies Estimates (*continued*)

	Person			Other	
	Injuries	Threats	Assault	Disobedience	Drugs
Panel A. Arrested					
Treated × post	−0.062	−0.008	0.021	−0.139	−0.013
	(0.017)	(0.006)	(0.005)	(0.019)	(0.006)
Observations	3,556	3,556	3,556	3,556	3,556
Mean	0.097	0.014	0.002	0.150	0.016
Panel B. First peers					
Treat × post	−0.015	−0.006	−0.005	0.004	0.002
	(0.008)	(0.005)	(0.002)	(0.007)	(0.002)
Observations	11,564	11,564	11,564	11,564	11,564
Mean	0.033	0.014	0.008	0.018	0.001
Panel C. Second peers					
Treat × post	−0.002	0.000	0.000	0.000	0.001
	(0.006)	(0.004)	(0.000)	(0.006)	(0.003)
Observations	20,860	20,860	20,860	20,860	20,860
Mean	0.022	0.010	0.000	0.022	0.003

Notes: This table reports the results of the TWFE estimation of equation (1), following the specification in column 2 of Table 2. Each panel presents treatment effects for a different group (arrested individuals, first- and second-degree peers). Each column indicates results for different crime categories. The observational unit is an individual–quarter pair. Control units are NY individuals in the same treatment arm. Standard errors clustered at the individual level are shown in parentheses.

arrests similar to the sweeps (in number arrested, their individual characteristics and the type of crime for which the arrest happens) but made before the policy change. Between 2008 and 2011, five comparable group arrests involved 64 individuals and 56 first-degree peers. For this analysis, I incorporate these individuals into the pool of treated subjects to conduct the same baseline analysis, but with the addition of a triple-difference term that factors in whether the individual was arrested after the policy change. If this term demonstrates statistical significance, it would suggest that the policy change in 2012 had a distinct impact on crime.

The findings in Table 4 indicate notable reductions in crime for arrested individuals and for their first-degree peers, irrespective of whether they were apprehended in a sweep or another similar arrest before the policy change. The triple interaction term reveals a significant negative differential: The decline in crime for individuals arrested in the sweeps is markedly greater than that observed for individuals arrested in previous similar interventions. Thus, there was a more pronounced decrease in criminal activity following the implementation of the gang-fighting policy. Such a result suggests that the toughened strategy is more effective at reducing crime for this type of arrest than previous strategies. However, the opposite trend is observed for first-degree peers: The peers of individuals arrested in sweeps exhibit a lower reduction in crime than those in previous police interventions. This effect might be attributed to the peers of those arrested in sweeps being more prolific criminals than those associated with earlier arrests.

Table 4—Effects of Sweeps: Enforcement Behavior

	Latin gang profile (1)	Arrested (2)	First peers (3)
Treated × post	0.003	−0.246	−0.232
	(0.005)	(0.075)	(0.065)
Treated × post × gang sweep		−0.157	0.166
		(0.090)	(0.068)
Observations	803,880	4,784	12,758

Notes: This table reports the results of different TWFE estimations to assess the role of enforcement in the main findings in Table 2. Column 1 reports the results of a TWFE estimation comparing criminal outcomes of all individuals with the same demographic characteristics as gang members (potentially profiled, and defined as treated in this specification) with criminal outcomes of other individuals perceived as prone to "high crime" (defined as controls in this specification), before and after the policy change. Columns 2 and 3 report the results of a TWFE estimation similar to equation (1) for total crimes, and a triple-difference term (*Treated* × *post* × *gang sweep*). Treatment (arrested or first peer versus not yet), timing (before versus after arrest), and differential effect of gang sweeps with respect to a prior similar police action (yes versus no) are the differences considered. The observational unit is an individual–quarter pair. Control units are NY individuals in the same treatment arm. All regressions include individual fixed effects, time fixed effects, and demographic-specific linear time trends. Standard errors clustered at the individual level are shown in parentheses.

G. *Area-Level Outcomes*

Lastly, I examine the impact of sweeps at a broader level, focusing on the areas where the sweeps took place. The purpose of these exercises is to analyze whether the sweeps had spillover effects beyond the criminal activity of first peers. To do so, I look at the evolution of different outcomes in the areas of the sweeps and follow a difference-in-differences estimation. In this case, the choice of control group is not straightforward. While using NY areas would provide too few observations, using all areas in the MAB would include some areas that are very dissimilar to the ones swept. An area is defined as a district within the city of Barcelona (ten districts), with the remaining municipalities in the MAB (35) considered as one unit.

To address this point, I match treated (swept) areas to other similar areas before the policy change. Concretely, I match areas on pre-sweeps statistics for total crime rates, crime rates against the person, total population, share of foreign-born residents, and share of 16- to 35-year-old residents, with their six nearest neighbors. I consider that matched subset as the control group for a TWFE estimation of equation (1). Supplemental Appendix Table A4 presents summary statistics in terms of crime and other demographic characteristics for treatment and control areas. It indicates that for most variables, treated and control areas are similar before the sweeps took place. The most salient exception is population, with treated areas being, on average, three times larger than control areas, although some control areas are as large as some treated ones.

Table 5 shows the results of the estimation exercise, focusing on three sets of variables: policing, crime rates, and other socioeconomic outcomes. Overall, the results

Table 5—Effects of Sweeps: Area Outcomes

	Police		Crime rates			Socioeconomic		
	Staff	Hours	Property	Person	Other	Rent prices	High school enrollment	In expected school year
Treated × post	2.364	0.225	1.963	0.175	0.254	−0.029	−0.015	0.006
	(2.965)	(1.107)	(1.237)	(0.179)	(0.152)	(0.017)	(0.017)	(0.013)
Observations	156	156	336	336	336	336	336	228
DV Mean	172.103	24.765	12.735	1.781	0.916	630.399	4,774.298	71.482

Notes: This table reports the results of the TWFE estimation following an analogous specification to equation (1). Each column presents results for a different outcome. The observation unit is an area–quarter pair. An area is defined as a district within the city of Barcelona (10 districts), with the remaining municipalities in the MAB (35) considered as one unit, except for police staff and hours where the remaining municipalities are split into police districts. Treated areas are defined as those in which a sweep took place. Control areas are selected via a nearest-neighbor matching procedure on prepolicy characteristics. Standard errors clustered at the area level are shown in parentheses.

indicate little to no change in the swept areas. First, the estimates reveal no statistically significant changes in police staff or daily patrols in police districts where a sweep occurred.[15] This finding is consistent with the fact that the central police unit conducted the sweeps and did not rely on local resources to do so. Consequently, any local variations in routine policing would not be expected. Moreover, it rules out the possibility that the changes in first peers' outcomes are merely a result of changes in local policing after the sweeps took place, strengthening the validity and significance of those findings.

Second, the results show no significant changes in property crime, crimes against the person, or other crimes. The absence of an effect on property crime, which is the most frequent crime type in the sample, carried out by a larger set of individuals, suggests that the sweeps do not have a general deterrent effect on crimes such as pickpocketing or offenses driven by motivations other than gang-related ones. Moreover, the Latin gangs were the primary focus of law enforcement and no other sizable organized crime groups were present in the study's context, limiting the concern of other organized crime groups entering the scene and exploiting criminal opportunities. Last, no effect is found for other socioeconomic variables in the swept areas. On this matter, it must be noted that statistical power is lower in this set of exercises than in the individual ones and that areas are relatively large.

Event-study support is presented in Figure 5. In terms of pre-trends, F-tests of joint significance presented in Supplemental Appendix Table A5 reject the null hypothesis of all coefficients being jointly equal to zero when considering the TWFE estimator while the assessment is mixed when considering the estimator by SA. However, Figure 5 indicates that no systematic trend is observed in the preperiod and Supplemental Appendix Table A5 shows that, except for the number of staff, all estimated coefficients are individually statistically insignificant, similarly to the

[15] The spatial units for these outcomes differ from those for other outcomes as they refer to the police district rather than the municipality district. For districts in Barcelona municipality, the geographical overlap is a perfect match. For the units in which the definition differs, I consider the larger area (police district) as treated if the smaller one (municipality district) is.

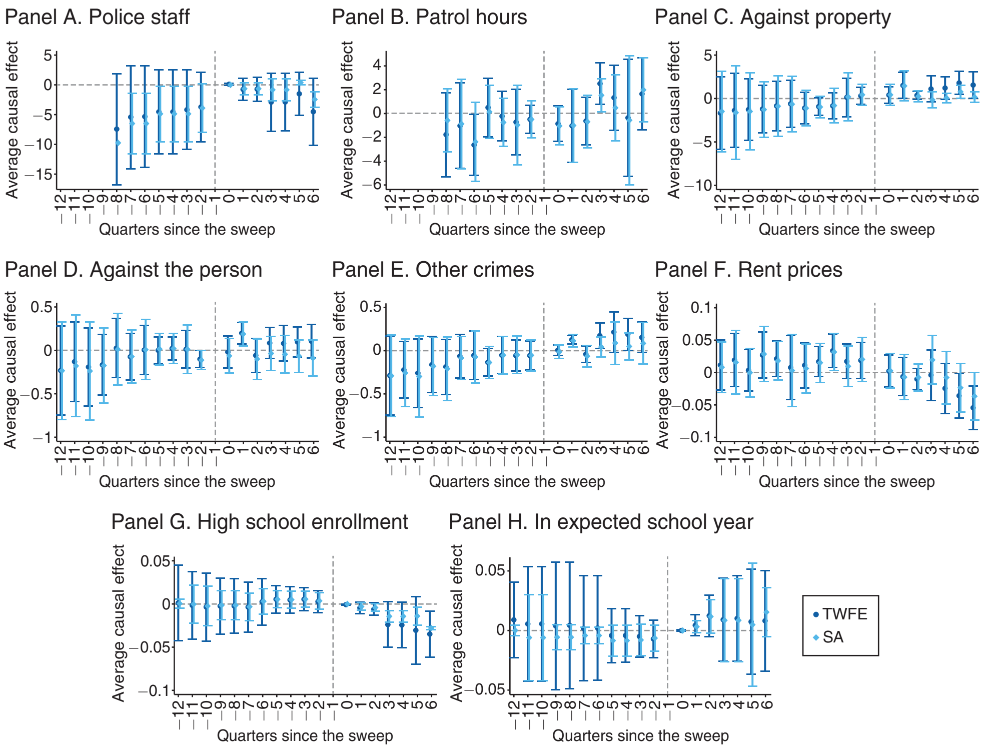

Figure 5. Effects of Sweeps on Crime: Area-Level Outcomes

Notes: This graph reports the results of the TWFE estimation of equation (2) alongside the SA heterogeneity-robust estimator for policing, crime, and other socioeconomic outcomes at the area level. An area is defined as a district within the city of Barcelona (ten districts), with the remaining municipalities in the MAB (35) considered as one unit, except for police staff and patrol hours where the remaining municipalities are split into police districts. Treated areas are defined as those in which a sweep took place. Control areas are selected via a nearest-neighbor matching procedure on prepolicy characteristics. The observational unit is an area–quarter pair. The 95 percent confidence intervals are based on standard errors clustered at the area level.

analysis at the individual level. These sets of evidence would indicate that, if there are differential pre-trends between treatment and control groups, they are not economically meaningful in this context.

V. Policy Analysis: Actual versus Simulated Sweeps

The last step of the analysis is to assess whether the sweeps' design was optimal, whether there were missed opportunities, and what policy lessons to extract from this case.

The proposed way to answer such questions compares the actual effects of the sweeps with the counterfactual effects of removing key players in the gangs. The counterfactual sweeps are constructed following the procedure explained in Section IIIB. This involves estimating peer effects on crime for gang members, computing individual centralities within each gang, and constructing counterfactual crime reductions from sweeps that remove top individuals in the centrality ranking

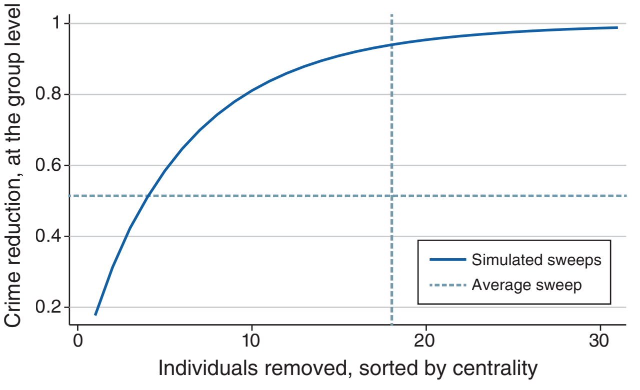

Figure 6. Predicted Reductions in Criminality, by Number of Key Players Removed

Notes: This graph presents the simulated crime reductions as a function of the number of individuals removed, sorted by centrality. Also shown is the average reduction in crime following a sweep and the average number of arrested individuals. The analysis is done using the subset of arrested individuals and first peers.

("key players").[16] This analysis is carried out for the subset of arrested individuals and first peers. Figure 6 shows the results of such a comparison.

As Figure 6 indicates, an average sweep arrests 18 individuals and achieves a crime reduction of 51.4 percent one year after the sweeps for arrested and first-degree peers. It also indicates that by holding the number of individuals arrested constant, the crime reduction would have been 94.3 percent if the sweeps had arrested the top-ranked individuals by centrality instead of the 51.4 percent stemming from the actual arrests. This outcome implies a 43 percentage point larger drop in crime than the one achieved. Evidence also indicates that sweeps would have reduced crime similarly by removing the top four individuals in each gang sorted by centrality, a fifth of the average arrested. This result is in line with results from a "precision policing" strategy, which focuses available resources on a small number of individuals thought to be the primary drivers of violence. On this matter, Chalfin, LaForest, and Kaplan (2021) show that gang sweeps in New York reduced gun violence around public housing by one-third the year after. Figure 6 suggests that by adequately identifying, targeting, and catching the key players in each network, the sweeps could have achieved the same crime reduction with a smaller deployment or they could have achieved a more considerable reduction in crime with the same number of arrests but targeted differently.

The above exercise shows that identifying and tackling a group of key players in each gang could substantially improve crime reduction after police interventions. Nonetheless, key player identification is informationally costly. It only makes sense to consider such a policy if its benefits outweigh the data collection and analysis costs. Moreover, as one of the assumptions made to compute centrality was that the network does not rewire, the key player predictions are only valid in the short term when that assumption is most likely to hold. Lastly, the key player might be

[16] The full set of results from each step is available in the Supplemental Appendix.

an unfeasible target in reality, as it is plausible that they are better protected than others. A key player strategy might not be optimal if its cost is too high in terms of investment or not low enough compared with other approaches. Despite these drawbacks, the previous exercise is a good benchmark to compare current targeting policies. Deciding whether it would be worth moving toward a key player strategy may depend on each scenario.

Network Analysis Considerations.—

Limitations and Implications: One limitation of the present analysis is that the true network is not perfectly observed. Like most applied work, the network under study is derived from data collected from a partial sample, leading to measurement error in the network due to missing data. In this context, the underlying data originate from police records, which may contain measurement errors both on individuals and on links. Link measurement errors can occur if two individuals, who are actually co-offenders, are not arrested together. Individual measurement errors can arise if some gang members are not arrested.

The existing literature offers several methods for addressing measurement errors. One approach is to use a model-based correction, which involves specifying a model that maps the mismeasured network to the true network. Strategies based on network formation models estimate peer effects by predicting the probability of all links, sampled or not, and using these predictions instead of potentially mismeasured links. However, it is essential to have information on individual characteristics that are predictive of link formation. This is precisely the method I use to address the potential issue of link mismeasurement. As already explained, I estimate peer effects considering a predicted network based on a link formation model instead of the observed network, as in Lee et al. (2020). Regarding its effectiveness, simulations in Chandrasekhar and Lewis (2016) demonstrate that model-based corrections effectively eliminate biases from missing data in parameters for peer effects models. Still, it is important to note that while the present analysis addresses the limitation on link measurement, it cannot address the limitation of accounting for only a portion of individuals in the true network.

The limitations of the data and techniques imply that the empirical results may not be generalizable to the entire true network. Model-based corrections, as the one explained and implemented in this analysis, alleviate concerns about the mismeasurement of links or connections across the individuals in the network. However, they do not address issues related to mismeasurement of individuals—that is, individuals that do not appear in the register data. Therefore, the estimates of peer effects can be interpreted accurately only for the observed portion of individuals in the true network. The literature on relaxing the assumption of perfect network knowledge is still evolving, and further research is needed to understand the implications of imperfect knowledge.

External Validity: An essential issue when dealing with gangs is the need for a unified concept or definition. Consequently, it is crucial to understand the concept's meaning in the specific context of this study and how it may or may not translate to other settings. The current focus is on Barcelona, a mid-sized European city where the gang issue in 2012 involved small, less deeply ingrained gangs referred to as "new organized and violent youth groups" by the police and as "gangs-in-process" by the sociological literature (Feixa et al. 2019, 63). In connection with the point above, the Maxson–Klein gang typology (Klein and Maxson 2010) outlines five types of street gangs: traditional, neotraditional, compressed, collective, and specialty. According to Rostami, Leinfelt, and Holgersson (2012), the predominant form of the gang in Europe is the compressed type, consisting of relatively small, short-duration adolescent groups with versatile criminal behavior. Traditional or neotraditional gangs are larger, multigenerational groups with strong territoriality, commonly found in the United States but rare in Europe.

As a result, the study's findings are unlikely to hold in contexts with well-established and far-reaching gangs (traditional or neotraditional), as observed in various instances in the United States or South America. Instead, the external validity applies to situations involving newer and less highly organized gangs (compressed or specialty). As explained by Klein and Maxson (2010), European street gangs generally exhibit less severe violence than in the United States, which is attributed to their recent development, lower firearm availability, and reduced gang territoriality. The study's findings are relevant to scenarios with emerging gangs, as seen in other European cities such as Madrid, Milan, and Genoa (related to the migration of gang members from Latin America), as well as Marseille (Feixa et al. 2019), Manchester and northern cities in the United Kingdom (Smithson, Ralphs, and Williams 2013), or Brussels (Irwin-Rogers et al. 2019). Its findings are also relevant to criminal groups such as the "*Naciones*" in Latin America, as these groups also lie between classic gangs based on illegal activities and youth subcultures centered on leisure and economic pursuits (Feixa et al. 2019).

VI. Conclusions

This paper examines the deployment of gang sweeps and their effects on crime. The analysis considers the impact on individuals arrested in a sweep, their peers, and the swept areas. The study follows a difference-in-differences strategy in the MAB, a context where gangs were a big concern among authorities and citizens and where a drastic policy change took place. The analysis uses criminal network structures from administrative records of the local police from 2008 to 2014.

Results indicate significant reductions in crime for those arrested in the sweeps and first-degree peers. There is an immediate and sharp drop in criminal activity for the former. Alongside average trial and prison times, this is consistent with an incapacitation effect. For first-degree peers, reductions in crime are smaller, shorter term, and focused on more impulsive crimes. These results would point toward the role of deterrence. No spillover effect is found on general deterrence in swept areas. Still, a counterfactual study indicates that sweeps could have decreased crime by 43 percentage points more had they arrested a broader set of key players.

These results show that carefully planned sweeps can be an effective policy strategy and that identifying and targeting key players can lead to even greater crime reductions. This element should be considered when designing crime-fighting policies.

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