The Spillover Effects of Releasing Offenders: Evidence from Ecuador

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Abstract

This paper examines the extent to which recently released offenders influence the criminal behavior of individuals in the neighborhoods they rejoin. Using a novel dataset containing comprehensive data on arrests, prison releases, and place of residence for the universe of men in Ecuador and employing a difference-in-difference design around a mass pardon, I find evidence of criminogenic effects from releasees on their neighbors. I estimate a monthly elasticity of new arrests with respect to releases of 0.23, much of which appears to be caused by contagion between peers and family members. These peer effects extend to both individuals with and without prior criminal experience. The findings suggest a detrimental impact of offender reentry, highlighting the importance of peer interactions in the spread of criminal behavior.

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1 Introduction

Incarceration remains one of the most widely used methods to punish criminals and deter potential offenders. In 2024, over 10 million people were imprisoned globally, a number that has steadily risen over the past decade (Fair & Walmsley, 2024). A concern with the reintegration of these offenders is their potential contribution to rising crime rates, mainly because of their risk of recidivism. In the US, about two-thirds of released individuals are rearrested within three years, while in Latin America, over 30% of former inmates are convicted within one year of release (Doleac, 2023; Fazel & Wolf, 2015).

However, recidivism may not be the only way released offenders affect crime, as they can influence the behavior of other community members. A releasee may transmit criminal knowledge and experiences gained in prison to their social connections. Previous studies have documented the spread of criminal traits among peers.[1] Moreover, reintegrating offenders into society can affect the behavior of individuals beyond their direct network by changing the role of gangs, shifting perceptions of the risks and rewards of criminal activity, and introducing new criminal role models (Helfgott, 2015; Morenoff & Harding, 2014; Petersilia, 2000; Sutherland, Cressey, & Luckenbill, 1992). The degree and direction of these effects depend on factors such as the offender's characteristics, prison experience, and the community's institutional structure. For instance, a high-risk violent offender with weak social ties may have minimal influence on others. In contrast, a releasee with a well-connected network could spread criminal traits throughout the community.

This paper investigates whether recently released offenders influence the criminal behavior of individuals in the neighborhoods they rejoin. I provide evidence of criminal peer effects between released offenders and individuals with and without prior criminal records. The existing literature on criminal peer effects has primarily focused on how interactions among criminals influence each other's behavior, a phenomenon known as reinforcing peer effects. For example, inmates who share prison facilities often replicate the crimes of their peers upon release, and exposure to hardened offenders increases the likelihood of recidivism (Philippe, 2017; Stevenson, 2017; Bayer et al., 2009). Additionally, offenders are less likely to reoffend when a larger share of their neighborhood's criminal peers remain incarcerated (Billings & Schnepel, 2022). In line with these findings, I show that released offenders increased the probability of arrest of their criminal peers in the neighborhood. Furthermore, I document a

[1] For references on criminal peer effects between family members see: Norris, Pecenco, and Weaver (2021); Bhuller, Dahl, Løken, and Mogstad (2018), within schools: Billings and Hoekstra (2024); Billings, Deming, and Rockoff (2014); Billings, Deming, and Ross (2019), and within prisons see: Stevenson (2017); Bayer, Hjalmarsson, and Pozen (2009); Philippe (2017); Drago and Galbiati (2012).

novel result: the presence of introductory peer effects, where criminals influence individuals with no prior criminal records, suggesting that released offenders can spread criminal behavior beyond those already involved in illegal activities.

Estimating these spillover effects poses two challenges: data availability and the non-random selection of offenders' residences across neighborhoods. The task requires information on the residence and criminal activity of all individuals living in a neighborhood, not just those with criminal backgrounds. Additionally, offenders' location upon release is not random. Former convicts decide where to reside, often returning to poorer areas with high crime rates (Harding, Morenoff, & Herbert, 2013). As a result, a simple OLS comparison between neighborhoods with and without recently released offenders would lead to biased estimates.

This paper addresses these challenges in Ecuador. Using over two million public documents detailing all penal cases between 2016 and 2022, I constructed a novel and unique dataset that tracks the arrests and prison releases of all individuals aged 18 and older. I combined this data with confidential records on the neighborhood of residence for all Ecuadorian nationals from 2002 to 2021.

I exploit a mass pardon in an event-study design to estimate the causal effects of releasing offenders on arrests. In February 2022, the Ecuadorian president pardoned individuals who had served at least 40% of their sentence and were convicted of robbery, theft, or fraud. Within a month, the pardon increased the number of released offenders returning to a neighborhood by 31% and the number of neighborhoods that received a former convict by 26%. I leverage the extensive margin variation to compare the probability of arrest for individuals in communities that received released offenders with those in non-treated areas.

I find that the release of offenders generates criminal spillovers in the neighborhoods to which they return. On average, within six months of the pardon, the probability of arrest for individuals living in neighborhoods that received a released offender increased by 0.005 percentage points (6.8% relative to the mean) compared to those in control areas. This effect represents a monthly elasticity of releases-to-arrest of 0.2, meaning that for every additional release per 1,000 residents, the neighborhood's arrest rate (excluding recidivism) increases by 0.94 per 1,000 inhabitants.

Importantly, these effects are not unique to neighborhoods receiving pardoned offenders. No difference exists between the impacts in communities with pardoned offenders and those with non-pardoned offenders. It suggests that the spillover effects stem from the broader presence of former offenders rather than the specific conditions of their release.

The effects of releasing offenders on arrests extend to individuals regardless of their prior criminal history. The probability of arrest for people with previous criminal experience increased by 0.046 percentage points (10.9% relative to the mean). In contrast, the likelihood of arrest for individuals with no criminal records rose by 0.002 percentage points (4.6% compared to the mean). These coefficients imply that 42% of the main effect comes from people without criminal experience, suggesting that releasing offenders generates new crimes and contributes to the formation of new criminals.

To explain the origins of these effects, I present evidence of a direct contagion mechanism between released offenders and their social connections. First, I document an increase in criminal partnerships between non-released individuals and released offenders. On average, the probability of being arrested alongside a released offender for individuals in treated neighborhoods rose by 0.001 percentage points (49% relative to the mean) compared to those in control areas. Arrests involving first-time offenders represent 47% of this effect.

Further, I show how the effects spread through family networks. I identify potential family connections by leveraging the Spanish naming structure, which allows the linkage of individuals through shared last names. Individuals sharing a last name with a released offender saw a 0.02 percentage point increase in their probability of arrest (a 22% increase compared to the mean) and a 0.005 percentage point rise in the likelihood of being arrested alongside a released offender (a 157% increase relative to the mean). These results suggest that family connections account for 27% of the overall effects and 41% of the effects observed among individuals without a criminal history.

Finally, I provide suggestive evidence on the role of prisons and incarceration in shaping these effects. Evidence from Latin America indicates that imprisonment can exacerbate former inmates' criminal involvement due to factors such as limited rehabilitation programs, overcrowded conditions, and violent prison environments (Escobar, Tobón, & Vanegas-Arias, 2023; Munyo & Rossi, 2015; Di Tella & Schargrodsky, 2013). These challenges are likely contributors to the observed spillover effects. A longer duration of imprisonment (i.e., more time served) tends to amplify spillover effects. In contrast, offenders from prisons with higher participation in rehabilitation programs did not create negative spillovers.

This paper contributes to several strands of the literature. First, it adds to the body of work on criminal peer effects by documenting the transmission of criminal behavior from offenders to individuals without criminal records. Existing research on this area explores how criminal skills are transmitted among individuals with a criminal past, whether through interactions in prison or outside of it (Billings & Schnepel, 2022; Damm & Gorinas, 2020;

Stevenson, 2017; Bayer et al., 2009). Research on the transmission of criminal behavior to non-criminals focuses on minors and their exposure to crime-prone peers, finding that children who share classrooms with peers from disadvantaged backgrounds or whose peers' parents have criminal records are at greater risk of engaging in criminal activity as adults (Billings & Hoekstra, 2024; Billings et al., 2019, 2014). This paper expands the literature by examining how released offenders influence the criminal behavior of adults without prior criminal records, offering insights into criminal contagion beyond juvenile and offender-to-offender contexts.

Second, I contribute to the literature investigating the effects of prison releases on crime rate. Most papers studying this question are within the criminology literature and present correlational evidence of a positive relationship between aggregate crime (at national or regional levels) and the number of releases (Roodman, 2020; Hipp & Yates, 2009; Raphael & Stoll, 2004; Clear, Rose, Waring, & Scully, 2003). Buonanno and Raphael (2013) stands out within this area. They use a regression-in-discontinuity design to find that the national crime rate increased immediately after the 2006 Italian collective pardon. However, this literature provides limited insight into how releases affect non-incarcerated individuals, leaving open the question of whether crime increases due to recidivism or broader contagion effects. This paper addresses that gap by focusing on the behavior of non-released offenders and presenting evidence of their involvement in the increased crime rate.

Finally, my paper adds to the literature about incarceration in developing countries, particularly Latin America. Previous research has shown that imprisonment can increase a former inmate's likelihood of reoffending due to factors such as limited access to rehabilitation programs, poor prison conditions, overcrowding, and the prevalence of gangs and violence (Escobar et al., 2023; Munyo & Rossi, 2015; Di Tella & Schargrodsky, 2013). These challenges hinder successful reintegration into society (Blattman, Duncan, Lessing, & Tobón, 2024; Tobón, 2022). This paper extends the literature by presenting suggestive evidence that prison experiences affects both former inmates and non-incarcerated individuals. Longer sentences tend to exacerbate negative spillover effects, while access to rehabilitation programs can significantly mitigate these outcomes.

The rest of the paper is organized as follows. Section 2 presents the institutional background about the mass pardon. Section 3 shows the data sources and variable construction. Section 4 discusses the event-study design and its validity. Section 5 contains the results on the effects of releasing offenders on arrests, while Section 6 discusses possible mechanisms that may explain these results. Section 7 shows suggestive evidence on the role of prisons, and Section 8 concludes.

2 Ecuadorian Prison System and the Mass Pardon

Ecuador's crime policies have historically favored punitive measures, with incarceration often serving as the primary response to crime (Verdugo, 2023). In 2020, the country's prison overcrowding rate reached 22%, exceeding the regional average by 25 percentage points (Fair & Walmsley, 2024). The system is marked by gang infiltration, high levels of violence, and limited access to rehabilitation programs. For instance, between 2021 and 2022, 11 gang-related prison riots led to over 413 deaths (Primicias, 2022). Additionally, a 2022 census reported that only 43% of inmates participated in any form of rehabilitation program (Instituto Nacional de Estadisticas y Censos, 2022).

Under this scenario, between late 2021 and early 2022, the president of Ecuador issued a series of mass pardon decrees. On November 22, 2021, he signed two decrees (Nos. 264 and 265) pardoning offenders convicted of transit-related misdemeanors and inmates suffering from severe illnesses, such as terminal cancer or tuberculosis. On February 21, 2022, he signed a third decree (No. 355), which pardoned individuals convicted of robbery, theft, or fraud who had served at least 40% of their sentences.[2] The pardon excluded those being prosecuted for other crimes or convicted of murder, sexual violence, crimes against the nation, or violence against women.

The main objective of these decrees was to reduce prison overcrowding by releasing less dangerous individuals. Between October 2021 (the month before the first pardon) and March 2022 (the month after the second pardon), more than three thousand prisoners were released, reducing the prison population by 10% and lowering the overcrowding rate by 9.5 percentage points (from 22.32% to 12.82%). Panel A of Figure 1 displays the monthly evolution of the number of released offenders between January 2021 and December 2022.

To access the pardon, convicts had to demonstrate to a judge that they met the requirements. It involved a process where the defense attorney petitioned the prison director for documents confirming the inmate's time served and requested information from the courts regarding other ongoing judicial processes. Once the attorney gathered the required information, they could petition the local judge for the pardon. If all conditions were satisfied, the judge granted the pardon and released the individual under conditional terms. These conditions often included living at a designated residence and reporting to the court on specific dates. Due to this process, the release of offenders did not occur immediately after the pardon was signed. As depicted in Figure 1, most releases occurred in March 2022, the month following the pardon.

[2] Link to Decree 264, Decree 265, and Decree 355. Accessed on July 30, 2024.

In this paper, I focus on the effects of the last decree (No. 355). I do not include the first set of pardons, as the individuals affected were not typical criminals in Ecuador. Most were incarcerated for traffic misdemeanors—often related to driving under the influence or fatal accidents—and their prison terms typically lasted less than a month. Only around 100 inmates with terminal illnesses were released. In contrast, the third decree affected individuals convicted of the most common crimes in Ecuador: robbery and theft, which accounted for over 18% of the incarcerated population in 2023. **Appendix C** shows that releasing non-violent criminals (transit convicts or with a terminal illness) produces null effects.

3 Data

This project generates a comprehensive dataset that captures the entire population of prison releases, arrests, and neighborhood-level residences for all individuals in Ecuador. This section outlines the main variables used in the study and their respective sources.

Place of Residence: The primary data source is the confidential voting registry compiled by the Ecuadorian electoral agency (Consejo Nacional Electoral, CNE). This dataset covers the universe of individuals of legal voting age in all national elections between 2002 and 2021.[3] It provides individual-level records, including general demographic information such as names, national identification numbers, sex at birth, and date of birth. Importantly, it also contains the polling station where individuals are registered to vote.

I define a neighborhood using the people registered at the same polling station. The National Electoral Council (CNE) assigns voting locations based on individuals' registered home addresses, where the area served by a polling station approximates a neighborhood. In urban areas, a polling station typically serves a population of about four thousand people, roughly equivalent to a census tract in the U.S. or Canada.

Two characteristics of the data are worth mentioning. First, voting is mandatory for individuals aged 18 to 65. Therefore, I observe the neighborhood of residence even if people did not vote. Second, to change their registered address, individuals must provide proof of residence, such as a federal or local government-issued utility bill (e.g., electricity or water). Address updates can only occur six to ten months prior to each election. People typically first appear in the dataset at age 16, often registering at the same polling station as their parents.

[3]From 2009 the legal age to vote is 16, before that it was 18. Each election happens on average every two and a half years. The specific years for which I have information are: 2002, 2004, 2006, 2009, 2013, 2014, 2017, 2019, and 2021.

Since the pardon occurred in February 2022, I define the neighborhood of residence for non-released individuals using the polling station registered during the 2021 election. For released offenders, I use the neighborhood on the voting location recorded at the time of their arrest.

Arrests and Prison Releases: The data on prison releases and arrests comes from the *Consejo de la Judicatura*, the institution overseeing Ecuador's judicial sector. This information is publicly available through SATJE, a website where courts must upload documents related to their cases. SATJE contains records for both civil and criminal cases, tracking each case from judge assignment to final verdict. The site offers open access to most records, except for cases involving minors, violence against women, and national security, which are confidential.[4]

There are several ways to look for cases, including by names of the involved parties, their national identification numbers, or a unique case identifier. To collect the data on prison releases, I first downloaded general information for all possible cases using the unique case identifier. This identifier consists of a 14-digit sequence: the first five digits represent the court, the following four represent the year the case began, and the final digits are a court-specific sequential case number. After obtaining a list of all courts' identification numbers through a public information request, I downloaded all case numbers and descriptions between 2016 and 2022. From the list of cases, I selected the ones related to prison releases, which are recorded independently from the original offender's conviction case.[5]

For each release case, I downloaded a document titled *"Boleta de Excarcelación"* (Release Warrant), where a judge orders the inmate's release based on either the completion of their sentence or a pardon. Since each court's secretary drafts the release warrant, the documents do not have a consistent structure across cases. I used OpenAI's LLM paired with Retrieval-Augmented Generation (RAG) to extract information from each document (Lewis et al., 2021). I gathered information such as the offender's full name, national identification number, nationality, arrest date, crime, type of release, and release date.[6]

The data on arrests also comes from SATJE. I used the name and national identification number to search for all cases involving people on the voting registry, excluding transit-

[4] Access to the webpage: `https://procesosjudiciales.funcionjudicial.gob.ec/busqueda`

[5] I selected as the start of my sample 2016 because a new penal code was introduced in Ecuador between late 2014 and the beginning of 2015. This new code modified several penal regulations. Thus, choosing only cases that started after the new law allows us to compare cases treated under the same regime.

[6] In an earlier version of this project, I fine-tuned a Named Entity Recognition model on top of XML-RoBERTa to extract the data, achieving over 85% accuracy on a thousand document samples. OpenAI with RAG's accuracy is close to 100%.

related felonies. Contrary to the data about released offenders, general information about arrests comes in a structured format. Given a person's name and identification number, SATJE provides the date and cause of arrest. However, it does not indicate whether the arrest resulted in a conviction or future incarceration. The data contains all criminal arrests, excluding sexual crimes, violence against women, and crimes against national security.

Neighborhood characteristics: I rely on the 2022 Ecuadorian population census to gather household and individual data for each neighborhood. The variables I collected include access to public services (such as public water, electricity, garbage collection, and sewage), dwelling conditions (floor and ceiling quality), and individual characteristics (age, gender, education level, employment status, and race).

I compiled the data at the individual-by-month level. For non-released individuals, I aggregated daily arrest data into monthly observations. I then matched this information to a release using the residence of the convict at the time of their arrest. Although I lack data on the neighborhood where former offenders reside after release, records from 2016 to 2021 show that 95% of inmates returned to the neighborhood where they lived at the time of their arrest.

Sample selection: Throughout the paper, I focus on studying men aged 18 to 40 residing in urban areas. I chose the sample because the crime literature highlighted young men as the demographic most likely to engage in criminal activity (Aizer & Doyle, 2015; Billings et al., 2014; Bayer et al., 2009). In the entire dataset, 89% of released individuals and 93% of those arrested were men. I restricted the analysis to urban areas due to the more reliable correspondence between polling stations and neighborhoods. In rural areas, polling stations are centralized in the main town, with residents from surrounding villages traveling to vote, which diminishes the validity of using polling stations to measure neighborhoods.

4 Research Design

In this section, I outline the event-study design and the sample used to examine the effects of the pardon, provide summary statistics, and discuss the validity of the empirical strategy.

4.1 Econometric Specification

To estimate the impact of the pardon on the probability of arrest, I used an event study design, with the treatment assigned at the neighborhood level. I defined treated neighborhoods as those that received a released offender between February and April 2022, while the controls are those that did not receive a released offender during this period. I chose a three-month window to define the treatment as April 2022 is the last month with a registered pardon release. Further, I excluded 271 neighborhoods that have never received a released offender from the sample to increase the ex-ante comparability between treated and control neighborhoods. The final sample consists of 775 treated and 1,420 control neighborhoods.

Throughout the specifications, I used a twelve-month window around the pardon. Thus, the analysis goes from September 2021 ($t = -5$) to August 2022 ($t = 6$). Equation 1 shows the event study regression.

$$y_{int} = \sum_{k=-5}^{6} \beta_k \mathbf{1}\{t = t^* + k\} \times Pardon_n + \alpha_n + \delta_t + \mu_{int} \tag{1}$$

where y_{int} is the outcome variable (e.g., probability of arrest times 1,000) for individual i living in neighborhood n at month t. $Pardon_n$ is an indicator equal to one if neighborhood n was treated (i.e., received a release offender because of the pardon), and $\mathbf{1}\{t = t^* + k\}$ are event time dummies relative to the date of the pardon (t^*), February 2022. α_n and δ_t are neighborhood and month fixed effects. I omitted the dummy for the month previous to the pardon (January 2022) in the specification, so that β_k identifies the changes in the probability of arrest y_{int} between treated and counterfactual neighborhoods relative to the same difference at $k = -1$. u_{int} is the error term. I clustered the standard errors at the neighborhood level.

The estimated coefficients represent Intent-to-Treat (ITT) effects. Two characteristics of the neighborhood data support this interpretation: individuals may not live in the neighborhood where they are registered to vote, and released offenders may not necessarily return to their pre-incarceration residences. Although changing one's voting address requires proof of residence, people may move without updating their address or use a relative's or friend's proof of address to vote elsewhere. Moreover, I do not observe the neighborhood where a former offender resides after the pardon. Using data on releases from 2016 to 2021, for which I can observe offenders' post-release residences, I found that 95% of them returned to the same neighborhood. While the rate of return to the original neighborhood is high, and there is no systematic reason to believe individuals modify their voting address in a way that would affect the results, the data limitations suggest that the estimated coefficients are likely biased toward zero. Therefore, the effects reported here should be considered a lower bound of the actual impact.

4.2 Summary Statistics

Table 1 presents summary statistics for the main variables. Panel A reports statistics at the individual-by-month level for all individuals in the sample. Panel B provides statistics for the released offenders during this period.

On average, individuals in the sample are 28 years old, with 6% having an arrest record (not necessarily a conviction or incarceration). The probability of an individual being arrested in a given month is 0.074%, with the vast majority being arrested only once per month. The likelihood of being detained with an offender released within the past year is 0.003%. Additionally, 56% of all arrests involve the detention of more than one person for the same crime.

Regarding the released offenders, the majority are male. On average, they serve a sentence of approximately 26 months and are 30 years old upon entry to prison. Of all releases, 36% are conditional, including pardoned individuals. Furthermore, for offenders released between 2016 and 2021, where neighborhood data is available post-release, 95% return to the neighborhood where they resided at the time of their arrest.

4.3 Validity of the Design

The empirical specification compares outcomes between people in neighborhoods that received a released offender and those in communities with no releases after the pardon. The key identification assumption holds that outcomes in treated and control neighborhoods would have followed parallel trends in the absence of the pardon. Although it is impossible to test this assumption directly, throughout the paper, I present evidence that no violations of parallel pre-trends occurred in the months leading up to the pardon, based on the event-study coefficients for $k < 0$, evaluated pointwise and jointly.

However, even in the presence of parallel pre-trends, there is a possibility that control municipalities do not represent an adequate counterfactual. I discuss some of these concerns below.

Staggered arrival of inmates: A potential concern with the design is the staggered release of offenders following the pardon. As explained in the institutional background, inmates had to follow a procedure to access the pardon's benefits, which could delay their return to the neighborhood beyond February 2022. Since the president signed the pardon at the end of February, most releases occurred in March, as shown in Figure 1. **Appendix C** demonstrates that the estimates remain consistent even when accounting for the staggered rollout of releases.

Non-Comparativeness of Controls: One concern is the quality of the controls in replicating a valid counterfactual for treated neighborhoods. Even after excluding neighborhoods that never received a released offender from the sample, there may still be differences between neighborhoods in the control and treatment groups. To address this concern, Appendix B implements a matched difference-in-difference design to replicate the estimates. Table B4 and Figure B2 in Appendix B show that the estimates remain consistent when using more comparable neighborhoods.

Influence of former releases: Another potential concern is the influence of releases before the pardon. Prison releases happen daily, meaning some neighborhoods would have received released offenders before February 2022, either in the control or treatment groups. In **Appendix C**, I restrict the sample to neighborhoods with no releases five months before the pardon. The results are robust in this matched sample and are larger than the main effects.

Mechanical effect due to changes in policing: One concern is that policing efforts may have shifted in response to the pardon, potentially explaining the observed effects if treated neighborhoods saw increased policing relative to their counterfactuals. However, several factors argue against this interpretation. First, the neighborhood of arrest often differs from the neighborhood of residence. Criminals typically live in poorer areas but travel to wealthier ones to commit economically motivated crimes, such as robbery, the main crime targeted by the pardon. Therefore, any post-pardon police response would likely increase in crime-prone areas, which may not align with the neighborhoods where released offenders reside.

Ideally, testing for changes in policing would require monthly neighborhood-level data on police presence, but such data is unavailable. To approximate this, I conducted a heterogeneity analysis using police station locations in the three largest cities in Ecuador. The results show no statistically significant differences in outcomes between neighborhoods with nearby police stations, as detailed in Figure C2 on Appendix C. Finally, anecdotal evidence suggests that police officers had little awareness of the pardon. In interviews with a sample of officers and prosecutors, none reported knowing that the pardon had even occurred.

5 Effects of the Mass Pardon

This section examines the effect of the mass pardon. The first part quantifies the increase in the number of released offenders. Then, it presents the main effects on the probability of arrest and a heterogeneity analysis.

5.1 Changes in Released Offenders

In this subsection, I calculate the magnitude of the mass pardon on the variation in releases. To achieve this, I estimated Equation 2 using data from the treated and control neighborhoods between September 2021 ($t = -5$) and August 2022 ($t = 6$).

$$y_{nt} = \beta After\ Feb22_t + \alpha_n + t + \epsilon_{nt} \tag{2}$$

where y_{nt} represents the outcome variable (e.g., the number of released offenders) in neighborhood n at month t. The variable $After\ Feb22_t$ is an indicator that takes the value one for all months after the pardon, α_n are neighborhood fixed effects, t represents a linear time trend, and ϵ_{nt} is the error term.

Table 2 presents the estimates from Equation 2, using three distinct dependent variables: release rate per 1,000 residents (Column 1), number of releases (Column 2), and the probability that a neighborhood receives a former offender (Column 3). The pardon increased the presence of released offenders in neighborhoods across all variables. On average, after the pardon, neighborhoods received 0.05 more offenders (29% of the mean), representing a rise in the release rate of 0.03 (17% of the mean). Also, the probability that a neighborhood received at least one offender rose by three percentage points (19% of the mean). All the effects are statistically different from zero.

5.2 Effects of the Pardon on Arrests

Figure 2 reports the event-study coefficients $\hat{\beta}_k$ from Equation 2 on the probability of arrests (times 1,000). Table 3 summarizes the effects by averaging them using the difference-in-difference version of the event study.

Column 1 and Column 3 of Table 3 present the effects on the complete sample of men aged 18 to 40, including individuals recently released from prison. The difference-in-difference coefficient shows that, on average, individuals residing in neighborhoods receiving a released offender after the pardon are 0.006 percentage points more likely to be arrested than those in areas without a release. The effect corresponds to an 8.2% increase in the probability of arrest relative to the mean. The accompanying event study (Panel A of Figure 2) illustrates the dynamic effects of the pardon. Although all coefficients are positive, they become statistically significant starting four months after the pardon. Between months four and six, the probability of arrest increased by 0.016 percentage points (23% of the mean).

This result masks two distinct mechanisms. On one hand, it contains the recidivism rate of former offenders. As recently released individuals have a higher risk of reoffending, including them in the sample mechanically increases the probability of arrest in treated neighborhoods compared to control neighborhoods. On the other hand, they capture the spillover effects generated by the released individuals on the general population. If former inmates influence the criminal behavior of their neighbors, then the increased likelihood of arrest could extend beyond the released offenders themselves, affecting other residents in the neighborhood. This spillover effect would suggest a broader impact of the pardon on community-level criminal behavior.

To capture only the spillover effects, Panel B of Figure 2 and Column 2 of Table 3 present the estimates from Equation 1 in a sample that excludes individuals released from prison within the last year. The difference-in-difference estimates and the event study plot show that the presence of released offenders increases the probability of arrest of people in the neighborhood to where the offenders return. On average, the probability of arrest of people living in neighborhoods where an offender returns after the pardon increased by 0.005 percentage points (6.8% relative to the mean) compared to people in non-treated neighborhoods. The event study plot shows a similar pattern to the one in the complete sample. The effects become statistically significant four months after the release. Between four and six months after the pardon, the probability of arrest of people in treated neighborhoods increased by 0.015 percentage points (20% of the mean).

Notably, there is no evidence of violations of the assumption of parallel trends in the event-study plots. All the coefficients on the lags of the treatment ($k < 0$) are pointwise indistinguishable from zero. Moreover, the Wald test for joint statistical significance on all lags yields a p-value of 0.47, indicating no evidence that the coefficients are jointly different from zero. The only lag pointwise statistically significant at the 1% level is for $t = -5$. Although this coefficient is statistically insignificant at the 5% level, Appendix B shows that in the matched difference-in-difference design, all lags are indistinguishable from zero at the 1% level, and the magnitudes of the estimates are almost identical to the main results of the paper.

To further support the plausibility of the parallel trends assumption, Figure A1 shows the evolution of the raw means for the probability of arrest. Before the pardon, both groups exhibit similar trends. Four months after the pardon, the likelihood of arrest increases in the treated group, while the control group remains unchanged.

5.3 Characterizing the effects

The results indicate that the pardon increased the probability of arrest for individuals living in neighborhoods that received a former offender. In this subsection, I conduct various heterogeneity analyses to assess the external validity of these effects and to explore the characteristics of the affected individuals.

New Crimes or New Criminals: Do released offenders primarily influence individuals with a criminal past, or do they also contribute to creating new criminals? Upon their release, former offenders interact with both types of individuals: those with prior criminal behavior and those who do not have a delinquent record. Previous research has shown that offenders can strengthen their criminal capital through exposure to other offenders—a concept known as reinforcing peer effects (Damm & Gorinas, 2020; Stevenson, 2017). Evidence of introductory peer effects (offenders influence individuals without a criminal history) remains scarce. The closest studies show that childhood exposure to disadvantaged environments (not direct contact with criminals) increases adolescent and adult criminal involvement (Billings et al., 2019; Billings & Hoekstra, 2024; Damm & Dustmann, 2014).

Distinguishing between these two types of influences is crucial for understanding the composition of crime rates. If former inmates primarily affect individuals with prior criminal records, the increased arrests may reflect increased crimes committed by the same individuals. Conversely, if released offenders also influence individuals without a criminal history, this suggests a rise in the number of new criminals in the neighborhood.

To assess whether the effect comes from people with criminal records or first-time offenders, I estimate Equation 1 using a stratified regression based on the arrest history of individuals before the pardon. Figure 3 presents the results in two separate panels: Panel A shows the event study coefficients for individuals without criminal records, and Panel B shows the estimates for those with an arrest record. In both cases, receiving a released offender increases the likelihood of an arrest. On average, the probability of arrest of individuals living in treated neighborhoods without criminal records increased by 0.002 percentage points (4.6% relative to the mean) compared to individuals without criminal records in the control group. The corresponding coefficient for individuals with arrest records increased by 0.046 percentage points (10.9% relative to the mean).

These coefficients suggest that 40% of the overall increase in arrests stems from new criminals, while 60% is attributable to individuals with prior criminal records. I calculated each group's contribution by weighting the point estimate in each subsample based on their population share. Although former criminals account for most of the effect, there is still a significant impact on first-time offenders.

Pardoned vs Non-Pardoned Releases: Another consideration is whether the observed effects come from unique aspects of the pardon or if they reflect a more general pattern associated with releasing offenders. This distinction is crucial for assessing the relevance of the findings and determining if similar outcomes would occur in other offender release scenarios beyond the specific context of the pardon.

Pardoned individuals might behave differently from non-pardoned inmates after their release. The pardon could change perceptions of the severity of punishment, leading pardoned individuals to revise their beliefs about crime penalties, potentially thinking that future pardons are possible. As a result, they may be more likely to engage in criminal activity or influence others compared to non-pardoned releasees, which could explain the increase in the arrest rate observed in the main results.

I find evidence contrary to this hypothesis, favoring that the estimated effects reflect the general impact of releasing offenders. First, mass pardons are rare in Ecuador. The president is the only authority that can issue them. The president often uses pardons as a political tool rather than a mechanism to alleviate prison overcrowding. In a given electoral cycle, fewer than five pardons are typically granted, making it unlikely that the pardon significantly altered releasees' beliefs.

Second, although pardons are rare, there exists the possibility that a single one may be enough to change the criminal behavior of the releasees. To demonstrate that pardoned individuals do not exclusively drive the effects, I estimated a stratified regression similar to Equation 1, splitting the sample based on whether the released inmate received a conditional release (pardoned) or an unconditional one (non-pardoned). Figure 4 displays the event study coefficients for these regressions. The results show no statistically significant difference between the effects of pardoned and non-pardoned releasees. The p-value of the Wald statistic testing for joint equality of all coefficients where $k \geq 0$ is 0.85, indicating that we cannot reject the null hypothesis that the coefficients are equal. Thus, no evidence exists that the effects are specific to the pardon.

Neighborhood Characteristics: Finally, I examine whether neighborhood characteristics influence the arrest rate. Figure C2 presents the heterogeneity estimates from the difference-in-difference analysis in Equation 1, interacted by various neighborhood characteristics. The results suggest that releases have a lower impact in more developed neighborhoods. This trend appears across three variables: residents' employment rate, average education level, and an index measuring access to public services and housing conditions. However, only the employment rate effect is statistically significant. Conversely, the likelihood of arrest increases in more disadvantaged neighborhoods. In areas with higher pre-existing crime rates, an offender's release has a more substantial effect than in more secure communities.

6 Mechanisms

Two alternative explanations can justify the increased probability of arrest following an offender's release. First, released offenders may influence the criminal behavior of their peers. Upon reentering society, they reconnect with their social networks, and if crime spreads through social ties, ex-offenders can affect the criminal activities of those within their circles. Second, releasees may influence individuals beyond their immediate network. The presence of former criminals can shift perceptions of the risks and rewards associated with criminal activity, introduce new role models, and alter gang dynamics within the community—factors that can drive behavioral changes across the neighborhood.

In this section, I present evidence supporting the direct contagion of criminal behavior from released offenders to their social connections within the neighborhood. I begin by demonstrating the formation of criminal partnerships, following a similar approach to (Billings et al., 2019). Next, I explore how family connections can facilitate the spread of criminal behavior. Finally, I provide suggestive evidence on the formation of delinquent groups.

6.1 Criminal Partnerships

The most direct way to test the influence of released offenders on individuals in the neighborhoods they rejoin is to examine whether they form criminal partnerships. If a former offender impacts their neighbors' criminal behavior, the likelihood of these individuals committing crimes together should rise. To test this hypothesis, I estimated Equation 1, using the probability of being arrested alongside a recently released offender as the outcome variable. I defined a released offender as someone who exited jail within the past year. Thus, the set of released offenders updates monthly as new individuals reenter the community. As in almost all specifications, I excluded releasees from the estimation sample. Figure 5 presents the event study coefficients for this outcome.

The estimates in Figure 5 show that, on average, within six months after the pardon, the probability of being arrested with a released offender increased by 0.001 percentage points for individuals living in neighborhoods that received a released offender compared to those in neighborhoods that did not. This effect represents a 49% increase relative to the outcome mean. Similar to the main results, the coefficients become statistically significant four months after the pardon. Between four and six months after the pardon, the probability of being arrested with a released offender increases by an average of 0.003 percentage points for people in treated neighborhoods.

These results suggest that, after reentering society, former offenders associate with their neighbors to commit crimes. Understanding whether their criminal partners have prior records is crucial for interpreting the direction of the contagion effect. If partnerships form between a releasee and a first-time offender, the criminal influence likely originates from the releasee. However, the influence may go both ways when partnerships involve individuals with prior criminal experience. It may be that releasees reconnect with inactive former criminals and encourage them to reengage in criminal activity. Alternatively, the neighborhood the releasee returns to may already be characterized by criminality, making it difficult for the former offender to reintegrate as they encounter other active criminals.

Figure C3 further disaggregates the analysis based on individuals' prior criminal history. The results show that individuals with and without criminal records are more likely to be arrested alongside a recently released offender after the pardon. For individuals with a criminal history, the effect corresponds to an increase of 64% relative to the mean. For those without prior records, the effect represents an increase of 40% of the mean. The associated coefficients imply that 47% of the effect comes from the direct transmission of criminal behavior to new criminals.

To analyze the composition of these partnerships, I estimated Equation 1 separately for individuals with and without a prior criminal record. Figure C3 presents the event study coefficients for each regression. The results show effects for both groups: the probability of being arrested alongside a released offender increased for individuals with and without arrest records. For those with a criminal history, the average effect is 0.0008 percentage points (64% relative to the mean), while for those without records, the effect is 0.01 percentage points (40% of the mean). These coefficients suggest that 47% of the effect arises from the direct transmission of criminal behavior from releasees to new offenders. Therefore, the influence originates approximately in the same magnitude from both types of contacts.

6.2 Family Connections

The previous subsection provided evidence of the direct influence of released offenders on their neighbors. However, the absence of a joint arrest with a releasee does not necessarily imply a lack of direct influence. For example, a released offender may commit a crime with a friend or family member, and due to idiosyncratic factors, only the friend gets arrested, while the releasee avoids apprehension. Alternatively, the releasee might orchestrate the crime without directly participating. In both cases, the releasee directly influences their associates, which the joint arrest measure fails to capture.

To further support the existence of a direct criminal contagion mechanism from released offenders, I analyze the arrest rates of their social connections, focusing specifically on family members. Research in the economics of crime has shown that criminal behavior spreads through peer interactions. Within families, previous studies have documented how parental incarceration influences the long-term criminal behavior of children (Norris et al., 2021; Bhuller, Dahl, Løken, & Mogstad, 2018; Bhuller, Dahl, Loken, & Mogstad, 2018; Dobbie, Grönqvist, Niknami, Palme, & Priks, 2018). Building on these findings, I provide evidence that the reentry of offenders into neighborhoods increases the criminal involvement of their relatives.

The voting registry data does not provide family members links but gives the full names of all people registered in a neighborhood. I approximated family connections using the Spanish naming structure. In Spanish-origin names, individuals typically have two first names and two last names, where the first last name is inherited from the father and the second from the mother. I constructed a family connection indicator based on shared last names, considering a person related to the released offender if any of their last names match those of the released offender. This approach captures relationships between parents, siblings, uncles, and cousins. In my sample, on average, 3% of individuals within a neighborhood are related to each other, and 4% have a relationship with the released offender.

Using the family connections, I run stratified regressions based on whether people are related to any released offender. Figure 6 presents the event-study plots of the estimations. In all regressions, I control for the frequency of the combined last names within a neighborhood. Panel A shows that the probability of arrest of individuals in treated neighborhoods who share a last name with the released offender increased by 0.02 percentage points (22% relative to the mean) compared to people in the control group after the pardon. An effect statistically significant at the 1% level. In contrast, the likelihood of arrest for individuals without a shared last name rose by 0.004 percentage points. These coefficients imply that 27% of the total effect on arrests comes from individuals related to the released offender.

Panel B of Figure 6 shows a similar pattern, using the probability of being arrested alongside a released offender as an outcome variable. On average, after the pardon, the probability of being arrested jointly with a released offender increased by 0.005 percentage points (157% of the mean) for family members of the realesee in comparison to people in the control group. The corresponding increase for non-family members was 0.001 percentage points (42% of the mean). These coefficients indicate that 26% of the effect comes from individuals from the same family as the released offender.

One final consideration involves determining whether the family members arrested after the pardon had prior criminal records. Figures C4 and C5 extend the earlier analysis by breaking it down according to the criminal histories of the residents. Both figures show that the pardon increased the probability of arrest and the likelihood of being arrested alongside a released offender for individuals with and without criminal records. When accounting for the sample size of each group, family members explain 41% of the effect observed among individuals with no prior criminal history. Further, family connections account for 19% of the effect for those with a criminal past. These results suggest that family connections play a more significant role in generating new criminals than influencing relatives already involved in crime.

Appendix C presents robustness checks for the measures of family connection. **Figure C** displays the estimates after excluding individuals with the most common last names. Additionally, **Figure C** replaces the control for the frequency of last names within the neighborhood with a control for the frequency of last names at the national level.

In summary, the analysis demonstrates that released offenders directly influence the criminal behavior of their family members. Family connections account for approximately 25% of the overall effects observed. Furthermore, family membership with the released offender explains 40% of the increase in arrests among individuals without prior arrest records.

6.3 Neighborhood Attachment and Band Formation

This subsection presents suggestive evidence of the transmission of criminal capital from released offenders to individuals in the neighborhoods they rejoin beyond their observable network. First, it shows that the main effects come from offenders returning to the neighborhoods where they grew up. Then, it provides evidence of the formation of criminal bands in the affected communities.

The influence of released offenders on their neighbors depends on the strength of their social network upon reentry. Offenders returning to the neighborhoods where they grew up will likely have broader and more cohesive connections than those entering a new community for the first time, thus exerting a more significant influence than newcomers. I test this by examining whether the released offender returned to the neighborhood where he was first registered to vote. In Ecuador, individuals are first registered to vote at the age of 16. I use the location of this initial registration as a proxy for the neighborhood where they grew up. In the sample, 75% of releasees returned to their youth neighborhoods.

Using this variation, I conducted a heterogeneity analysis, distinguishing between offenders who returned to their original neighborhoods and those who did not. Panel A of Figure 7 presents the results of the stratified regression based on whether the offender returned to his youth neighborhood. The results show that the effect is statistically significant only for offenders who returned to their original communities. On average, individuals in these neighborhoods experienced a 0.007 percentage point increase in their probability of arrest (9% relative to the mean) after the pardon compared to those in control neighborhoods. These findings suggest that ties to the neighborhood play a role in spreading criminal behavior.

Another indicator that released offenders influence the criminal behavior of the communities is the formation of criminal organizations. Crime is a social phenomenon, with individuals forming bands or gangs to commit offenses. Gangs are particularly prevalent in Latin American and Ecuadorian prisons. Thus, upon release, former convicts might spread their criminal affiliations to the communities they rejoin.

To test this, I require information about gang membership at the neighborhood level, which does not exist. As a proxy for gang affiliation, I used joint arrests. Joint arrests refer to instances where two or more individuals got arrested for committing a crime together. If members of gangs commit crimes together, this measure will imprecisely capture the spread of criminal bands.

Panel B of Figure 7 displays the event study coefficients using the probability of being arrested in a group as an outcome. On average, the probability of being arrested jointly with another individual increased by 0.004 percentage points (11% of the mean) after the pardon compared to individuals in control neighborhoods. As with the main effects, the coefficients become statistically significant four months after the pardon. Notably, 65% of this effect comes from individuals with no prior criminal record before the analyzed period, suggesting the influence of released offenders in fostering the inclusion of new offenders into criminal organizations.

7 The Role of Prisons

Incarceration can alter the criminal behavior of convicts. Research in Latin America suggests that prison tends to enhance inmates' criminal skills, while evidence from developed countries indicates that rehabilitation-focused imprisonment can reduce future criminal activity (Di Tella & Schargrodsky, 2013; Munyo & Rossi, 2015; Tobón, 2022; Bhuller, Dahl, Løken, & Mogstad, 2020). Regardless of the direction of the effect, these traits may spread through the community once offenders are released.

In this section, I present suggestive evidence on the role of incarceration in explaining the effects observed in this study. I show that individuals who served longer sentences tend to have a more significant impact on neighborhood crime and access to rehabilitation programs may help mitigate these effects. It is important to note that the coefficients reported in this section are not causal estimates. I do not test for random allocation of individuals to prison. For instance, the length of imprisonment may reflect the prison's influence and the offender's idiosyncratic factors.

First, I demonstrate that the effect primarily stems from individuals who spent more time in prison. I conducted a stratified regression, distinguishing between offenders who served above or below the median sentence length. In cases where more than one offender returned to the same neighborhood, I used the maximum sentence served among the realesees. The median time served in the sample is 24 months, with offenders below the median serving an average of 4 months and those above the median serving an average of 55 months.

Panel A of Figure 8 shows the event study coefficients using the probability of arrest as the outcome. The estimates show no significant effects for individuals in neighborhoods where offenders with shorter prison stays returned. However, the effect concentrates on those living in areas that received an offender who served a longer sentence. On average, individuals in these neighborhoods experienced a 0.009 percentage point increase in the probability of arrest (12% relative to the mean). These results suggest that more extended incarceration periods may intensify the criminal influence of released offenders.

Next, I examined specific characteristics of the prisons from which the released offenders were discharged. I focused on three prison-level attributes: overcrowding, one-year recidivism, and participation in rehabilitation programs. I calculated the overcrowding rate between June 2021 and November 2021. For recidivism, I computed the average probability of rearrest within one year for all individuals released between 2016 and 2021. Lastly, data on rehabilitation programs comes from self-reported information collected in the 2022 prison census.

Panel B of Figure 8 shows a heterogeneity analysis based on the characteristics of the prison from where the offender came on the effect of releases on the probability of arrest. Each point displays the estimate of the interaction of a characteristic (e.g., overcrowding) with the difference-in-difference estimator corresponding to Equation 1. The coefficients show that offenders from worse prisons (those with higher overcrowding and recidivism rates) increase the probability of arrest by 10% of the mean more than releasees from better prisons.

Additionally, prisons with higher inmate participation in rehabilitation programs mitigate the spread of criminal behavior. A one standard deviation increase in rehabilitation program participation reduces the likelihood of arrest for individuals in treated neighborhoods by 12% of the mean. I analyzed three types of programs: formal education (primarily high school), employment-oriented training, and cultural activities, with all three showing a negative effect on arrest rates.

In summary, the incarceration experience may play a role in explaining the increased probability of arrest. The results present suggestive evidence that worse prisons increase the magnitude of the effect, but access to rehabilitation programs while incarcerated can help mitigate the impact.

8 Conclusions

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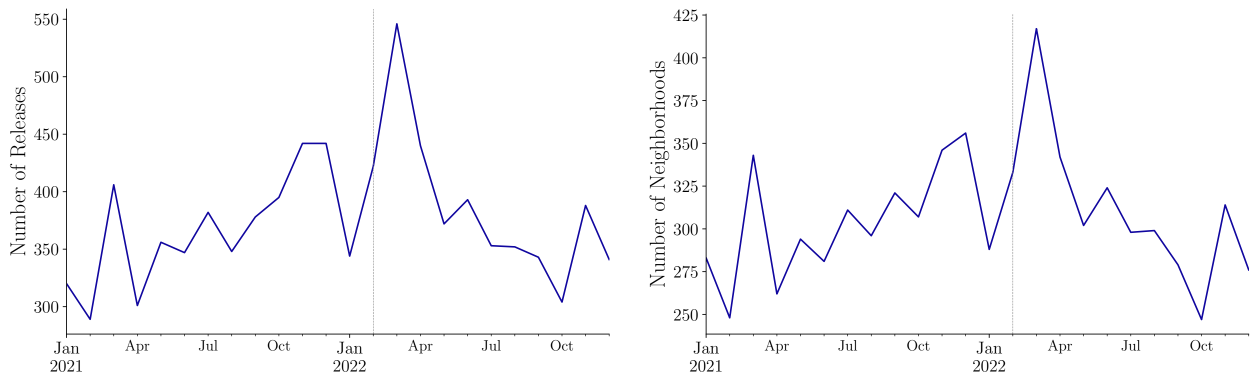
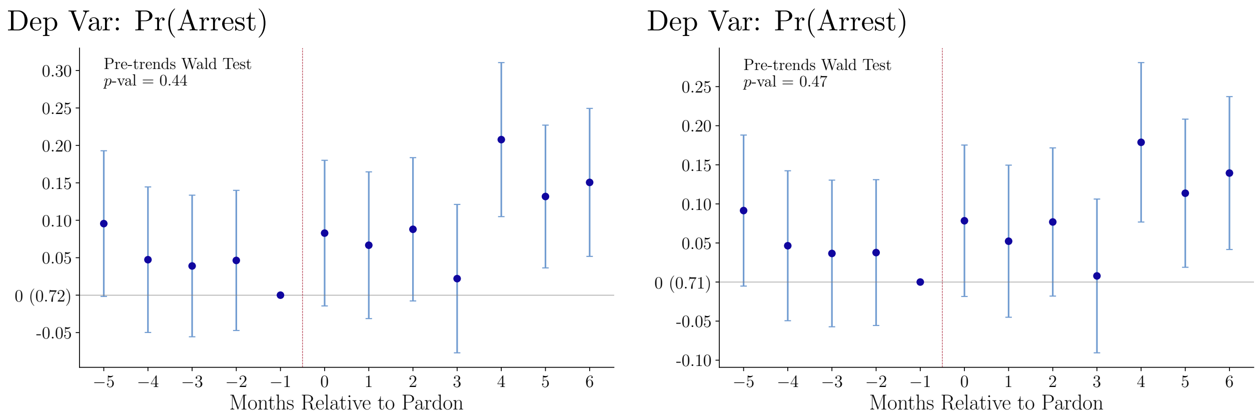
Figures



Panel A. Number of Released Offenders   Panel B. Neighborhoods with Released Offenders

Figure 1: Evolution of Released Offenders

Notes: Panel A shows the evolution of the total number of released offenders between January 2021 and December 2022. Panel B displays the number of neighborhoods that received a released offender over the same period. The vertical dashed lines mark the date of the pardon, February 2022. The sample includes all releases to urban neighborhoods.

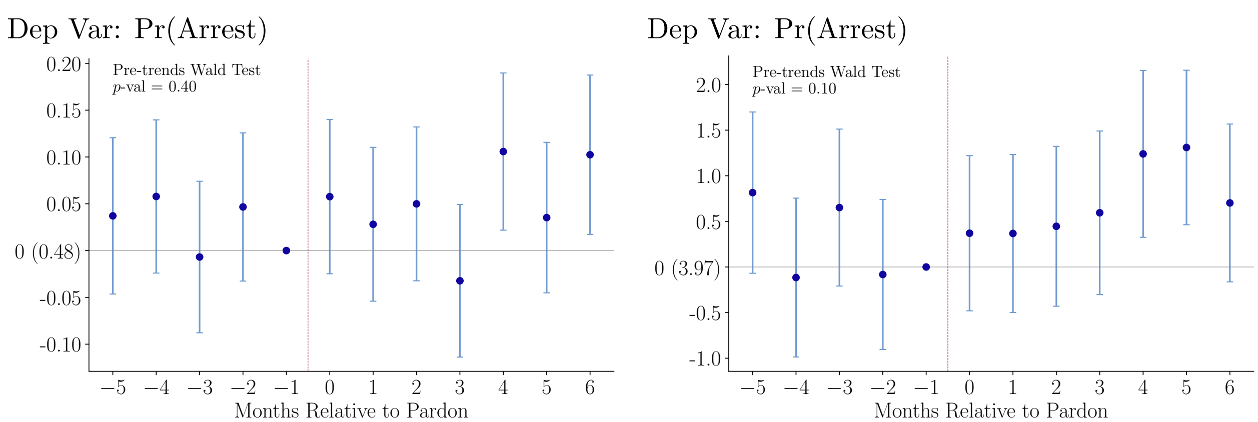


Panel A. All Individuals

Panel B. Without Released Offenders

Figure 2: Effects of Mass Pardon on Arrests

Notes: The figure displays the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 2. The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the probability of arrest at $t - 1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level. Panel A shows the estimates on the sample of all men between 18 and 40 years, including released offenders. Panel B does not include released offenders in the estimation sample.



Panel A. Without Arrest Record

Panel B. With Arrest Record

Figure 3: Effects on Arrests by Residents' Criminal Records

Notes: The figure shows the regression coefficients for the difference in the probability of arrest (times 1,000) between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 2. Each panel shows a stratified regression based on arrest records defined before the pardon. Panel A shows the estimates on people without any arrest record ($N = 28,587,505$), and Panel B shows the estimates only on people with criminal history ($N = 1,987,011$). The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the dependent variable at $t = -1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level.

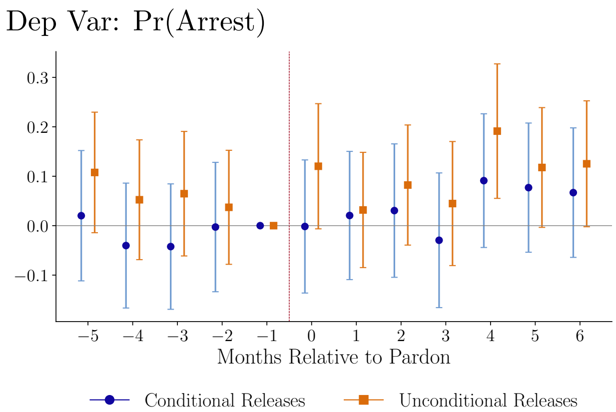


Figure 4: Heterogeneity by Type of Release

Notes: The figure shows the regression coefficients of the difference in the probability of arrest (multiplied by 1,000) between individuals living in treated and control neighborhoods, relative to the month before the pardon, in a stratified analysis. The dots represent the effects of conditionally releasing an offender (including pardoned individuals), while the squares show the estimates for unconditionally released offenders (those who completed their sentence). The coefficients for $t = -1$ are normalized to zero. The bars correspond to the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

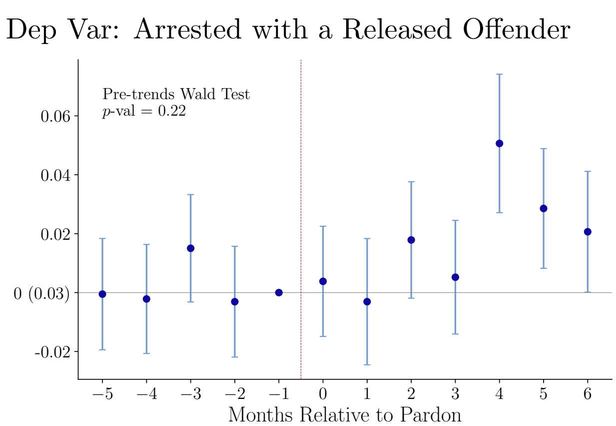


Figure 5: Criminal Partnerships Formation

Notes: The figure shows the regression coefficients for the difference in the probability of being arrest with a released offender (times 1,000) between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 2. The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the dependent variable at $t = -1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level.

Panel A. Pr(Arrest)

Panel B. Arrested with Released Offender

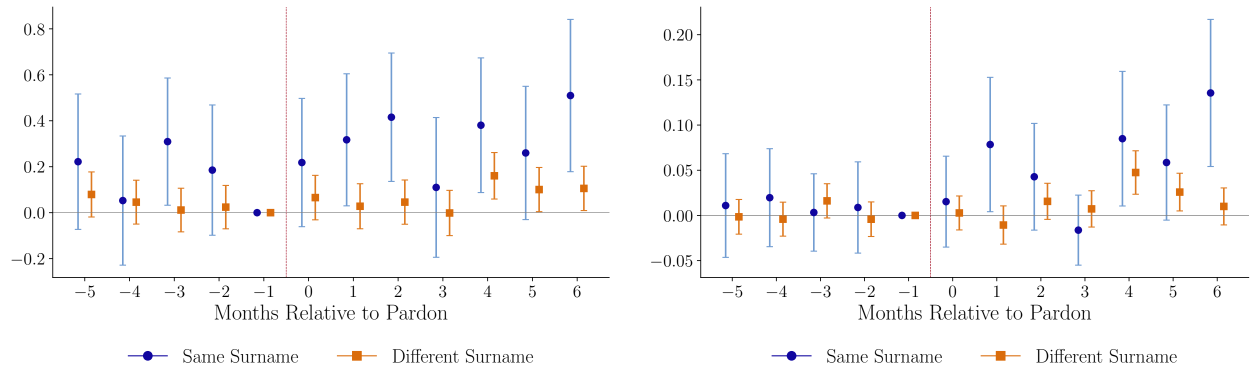
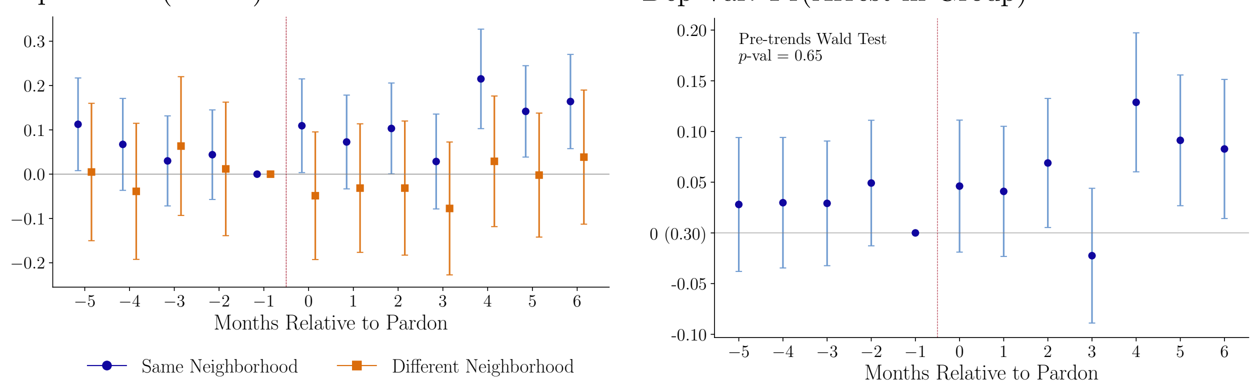


Figure 6: Criminal Spread in Family Networks

Notes: Each panel presents stratified regressions based on whether individuals share a common surname with the released offender. The outcome in Panel A is the probability of arrest (multiplied by 1,000), while in in Panel B, the dependent variable is the probability of being arrested alongside a released offender (multiplied by 1,000). Each subplot displays the regression coefficients for the difference in outcomes between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 2. The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the dependent variable at $t = -1$. All regressions control for the share of last name within a neighborhood. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level.

Dep Var: Pr(Arrest)

Dep Var: Pr(Arrest in Group)

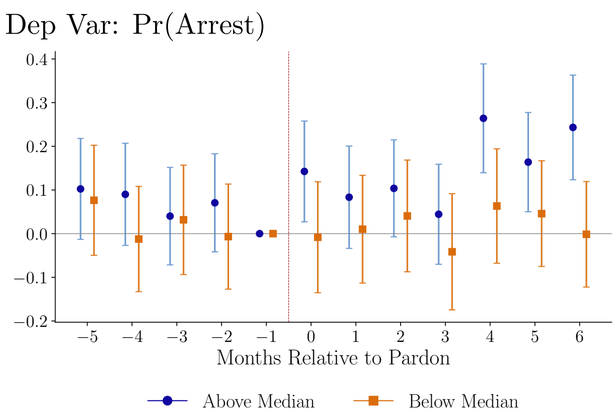


Panel A. Neighborhood Attachment

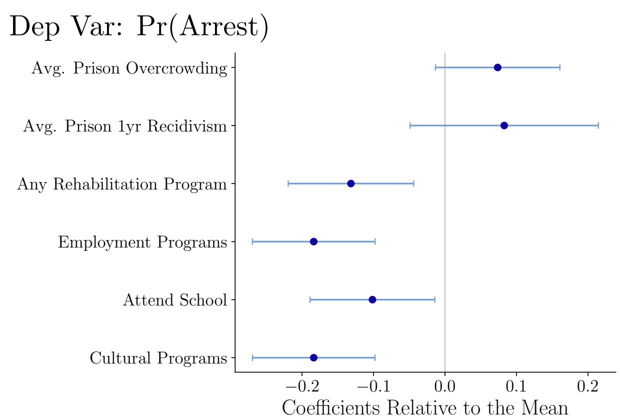
Panel B. Band Formation

Figure 7: Neighborhood Exposure and Band Formation

Notes: Panel A presents stratified regressions based on whether a released offender returns to the same neighborhood as when he was 18 years old. Panel B uses as an outcome the probability of being arrested in group (multiplied by 1,000). Each subplot displays the regression coefficients for the difference in outcomes between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 2. The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the dependent variable at $t = -1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level.



Panel A. Time in Prison



Panel B. Prison Characteristics

Figure 8: Released Offender's Prison Experience

Notes: Panel A shows the event-study coefficients of an stratified regression based on the time served in prison by the released offenders. Each point estimate displays the difference in the probability of arrest (times 1,000) between people living in treated and control neighborhoods relative to the month before the pardon. Panel B displays the estimates corresponding to the interaction of each variable with the difference-in-difference estimator. Each point estimate is rescaled to represent an standard deviation increase in the prison characteristic, and the effect be relative to the mean. All the confidence intervals are at the 95% with errors clustered at the neighborhood level.

Tables

Table 1: Summary Statistics

	Mean	SD	p50	N
Panel A: General Population				
Pr(Arrest) $\times$ 1000	0.74	27.18	0.00	30,574,516
Number of Arrests $\times$ 1000	0.77	28.74	0.00	30,574,516
Pr(Arrest with Released Offender) $\times$ 1000	0.03	5.68	0.00	30,574,516
Pr(Group Arrest) $\times$ 1000	0.32	17.75	0.00	30,574,516
Age	28.24	6.33	27.87	30,574,516
Previous Arrest = 1	0.06	0.25	0.00	30,574,516
Same Last Name as Released Offender	0.04	0.20	0.00	30,574,516
Last Name Frequency	0.03	0.05	0.01	30,574,516
Panel B: Released Offenders				
Male	0.89	0.31	1.00	4,552
Age at Release	33.10	9.90	31.01	4,552
Age at Entry	30.91	9.61	28.79	4,552
Time in Jail (months)	26.69	25.61	20.27	4,552
Conditional Release = 1	0.36	0.48	0.00	4,552
Same Neighborhood as First Registry	0.74	0.44	1.00	4,460
Same Neighborhood as when Arrested (2016-2021)	0.95	0.21	1.00	33,724

Notes: The table shows summary statistics for the main variables used in the paper, between September 2021 ($t = -5$) to August 2022 ($t = 6$). Panel A presents information for the general population in sample at the individual-by-month level. Panel B presents data for all the releases in the period. The only variable computed with a different sample is *Same Neighborhood as when Arrested*, which was calculated using all releases between 2016 and 2021.

Table 2: Changes in Released Offenders

	Release Rate (1)	Number of Releases (2)	Any Release (3)
Post Pardon = 1	0.0259**	0.0532***	0.0283***
	(0.0118)	(0.0110)	(0.0081)
N. Neighborhoods	2,195	2,195	2,195
Mean Dep. Var.	0.1519	0.1848	0.1480
Observations	24,145	24,145	24,145

Notes: The table shows the impact of the pardon on three measures of releases: Column 1 uses the release rate per 1,000 inhabitants, Column 2 uses the number of releases, and Column 3 uses an indicator for receiving at least one release as dependent variable. The unit of observation is a neighborhood-by-month pair. The analysis covers the period from September 2021 ($t = -5$) to August 2022 ($t = 6$), and the sample includes all urban neighborhoods that received at least one released offender between 2016 and 2021. Standard errors clustered by neighborhood in parentheses. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table 3: Effects of Mass Pardon on Arrests

	P(Arrest) x 1000		N. Arrests x 1000	
	(1)	(2)	(3)	(4)
Treated & Post Pardon = 1	0.0616***	0.0502**	0.0621***	0.0506**
	(0.0215)	(0.0212)	(0.0228)	(0.0225)
Neighborhood FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
Includes Offenders	Yes	No	Yes	No
N. Neighborhoods	2,195	2,195	2,195	2,195
Mean Dep. Var.	0.7506	0.7390	0.7780	0.7661
Observations	30,591,926	30,574,516	30,591,926	30,574,516

Notes: The table reports the difference-in-difference estimates of the effect of the mass pardon on the probability of arrest and number of arrest, both multiplied by 1,000. The unit of observation is an individual-month pair. The sample includes all urban neighborhood that received at least one released offender since 2016. The time frame of reported is between September 2021 ($t = -5$) and August 2022 ($t = 6$). Standard errors clustered by neighborhood in parentheses. The results on graph format are in Figure 2. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

A Robustness

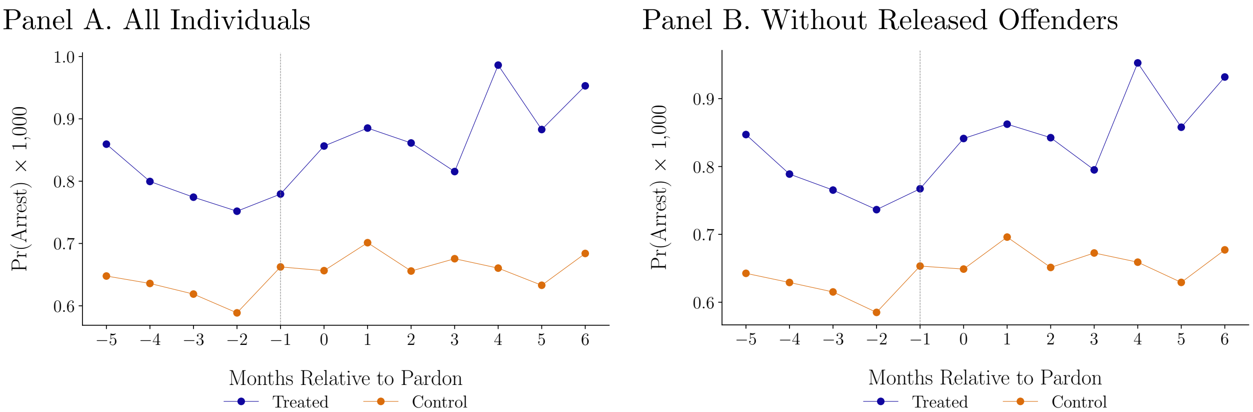


Figure A1: Probability of Arrest - Raw Means

Notes: The figure displays the raw means of the probability of arrest (multiplied by 1,000) for individuals aged 18 to 40 living in treated and control neighborhoods. The data covers the period from September 2021 ($t = -5$) to August 2022 ($t = 6$). The sample includes all neighborhoods that received at least one released offender since 2016. Panel A presents the means for the entire sample, including released offenders. Panel B drops released offenders from the sample.

A.1 Time in Prison

Panel A. Total Time in Prison

Panel B. Maximum Time Served

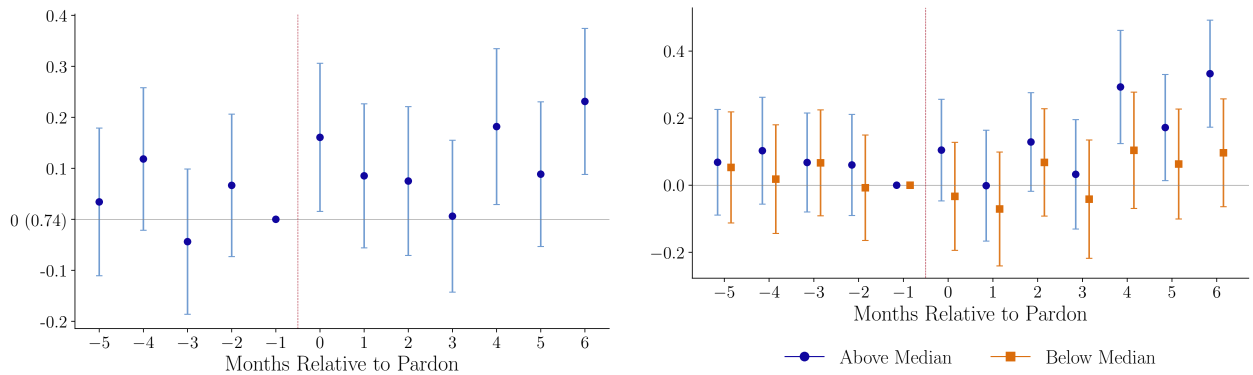


Figure A2: Time in Prison

Notes: The figure displays the coefficients and associated 95 percent confidence intervals for the difference in the probability of arrest between treated and control neighborhoods relative to the month before the pardon. Panel A shows heterogeneity results for the probability of arrest by the sum of time in prison that all released offenders in a neighborhood had. Panel B displays the stratified regression coefficients by the maximum time in jail that released offenders served. In circles are the estimates for offenders with time served above the median, and in squares the ones for inmates below the median.

B Matching Between Neighborhoods

This Appendix presents the results of using a matched event-study design between neighborhoods that received a released offender and those neighborhoods without a released offender.

B.1 Matching Algorithm

I use nearest-neighbor propensity score matching to pair each of the 775 neighborhoods that received a released offender due to the pardon with a control neighborhood. The possible control group comprises all neighborhoods that did not receive a released offender within three months of the pardon (N = 1,691). I chose the three-month window because it marks the final period when pardoned individuals were released.

To perform the matching, I first estimated a logit model using the cross-sectional sample of treated and potential control neighborhoods. The dependent variable is a binary indicator for whether a neighborhood received a released offender following the pardon. The independent variables include, from March to August 2021 (t = [-11, -6]), the average release rate, and the average number of arrests per 1,000 individuals. Also, from the 2022 population census, I included the total population, the share of the male population, the share of people with formal employment, average years of education, and an index measuring access to public services.

Using the predicted values (propensities) from this model, I matched each treated neighborhood with the untreated neighborhood with the closest propensity score without replacement. The final matched sample comprises 1,550 events, representing 775 treated neighborhoods and 540 unique control neighborhoods. On average, each control neighborhood appears 1.4 times in the sample, with the most frequent control neighborhood appearing seven times. Figure B1 displays a histogram showing the distribution of how often each control neighborhood appears in the sample.

Table B1 compares treatment and control neighborhoods across the variables used for matching. Column 5 presents the p-value from a joint regression of each variable on the treatment dummy, with standard errors clustered at the neighborhood level. The results indicate that, before the pardon, none of the variables exhibited statistically significant differences between the two groups.

B.2 Tables

Table B1: Matched Neighborhood's Characteristics

	Treated		Control		T - C	
	Mean	SD	Mean	SD	Diff	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Release Rate ($t = [-11, -6]$)	1.05	1.34	1.01	2.02	0.04	0.74
Arrest Rate ($t = [-11, -6]$)	7.95	4.56	7.61	5.09	0.34	0.29
Number of People	8,025	7,429	8,075	6,591	-51	0.80
Share of Formal Employment	0.20	0.07	0.21	0.07	-0.00	0.47
Share of Men	0.48	0.01	0.48	0.01	0.00	0.97
Years of Education	4.06	0.15	4.04	0.15	0.02	0.11
Access to Public Services	0.11	0.86	0.18	0.73	-0.07	0.21
N. Neighborhoods	775		775			

Notes: The table provides summary statistics for the variables used to match neighborhoods that received a released offender after the pardon with those that did not. The release and arrest rate variables are averaged over the period from 11 to 6 months prior to the pardon. All other variables are derived from the 2022 population census and represent the average characteristics within a neighborhood. Access to public services is measured as the average availability of public water, sewage, electricity, and garbage collection. Column 5 reports the $p-value$ from a joint regression of all variables on a treatment dummy, with standard errors clustered at the neighborhood level.

Table B2: Summary Statistics

	Mean	SD	p50	N
Panel A: General Population				
Pr(Arrest) $\times$ 1000	0.79	28.17	0.00	23,195,907
Number of Arrests $\times$ 1000	0.82	29.91	0.00	23,195,907
Pr(Arrest with Released Offender) $\times$ 1000	0.05	7.07	0.00	23,195,907
Pr(Group Arrest) $\times$ 1000	0.29	16.92	0.00	23,195,907
Age	28.24	6.34	27.86	23,195,907
Previous Arrest = 1	0.07	0.25	0.00	23,195,907
Same Last Name as Released Offender	0.06	0.23	0.00	23,195,907
Last Name Frequency	0.03	0.04	0.01	23,195,907
Panel B: Released Offenders				
Male	0.89	0.31	1.00	4,552
Age at Release	33.10	9.90	31.01	4,552
Age at Entry	30.91	9.61	28.79	4,552
Time in Jail (months)	26.69	25.61	20.27	4,552
Conditional Release = 1	0.36	0.48	0.00	4,552
Same Neighborhood as First Registry	0.74	0.44	1.00	4,460
Same Neighborhood as when Arrested (2016-2021)	0.95	0.21	1.00	33,724

Notes: The table shows summary statistics for the main variables of the paper, between September 2021 ($t = -5$) to August 2022 ($t = 6$). Panel A presents information for the general population in sample at the individual-by-month level. Panel B presents data for all the releases in the period. The only variable computed with a different sample is *Same Neighborhood as when Arrested*, which was calculated using all releases between 2016 and 2021.

Table B3: Changes in Released Offenders

	Release Rate (1)	Number of Releases (2)	Any Release (3)
Post Pardon = 1	0.1175***	0.1417***	0.0979***
	(0.0202)	(0.0159)	(0.0119)
N. Events	1,550	1,550	1,550
Mean Dep. Var.	0.1810	0.2304	0.1828
Observations	17,050	17,050	17,050

Notes: The unit of observation is neighborhood-by-month from the matched sample, covering the period from September 2021 ($t = -5$) to August 2022 ($t = 6$). The table displays the coefficients from the regression of measures of the presence of released offenders on an indicator variable that takes the value one for all months following the pardon. Standard errors clustered by neighborhood in parentheses. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Table B4: Effects of Pardon on Probability of Arrest

	P(Arrest) x 1000		N. Arrests x 1000	
	(1)	(2)	(3)	(4)
Treated, Post Pardon = 1	0.0657**	0.0532*	0.0697**	0.0576*
	(0.0314)	(0.0314)	(0.0346)	(0.0346)
Neighborhood-Event FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
With Released Offenders	Yes	No	Yes	No
N. Neighborhoods	1,550	1,550	1,550	1,550
Mean Dep. Var.	0.7942	0.7803	0.8247	0.8102
Observations	23,195,907	23,180,405	23,195,907	23,180,405

Notes: The table reports the difference-in-difference estimates of the effect of the mass pardon on the probability of arrest. The unit of observation is an individual-month pair. Standard errors clustered by neighborhood in parentheses. The results on graph format are in Figure B2. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

B.3 Figures

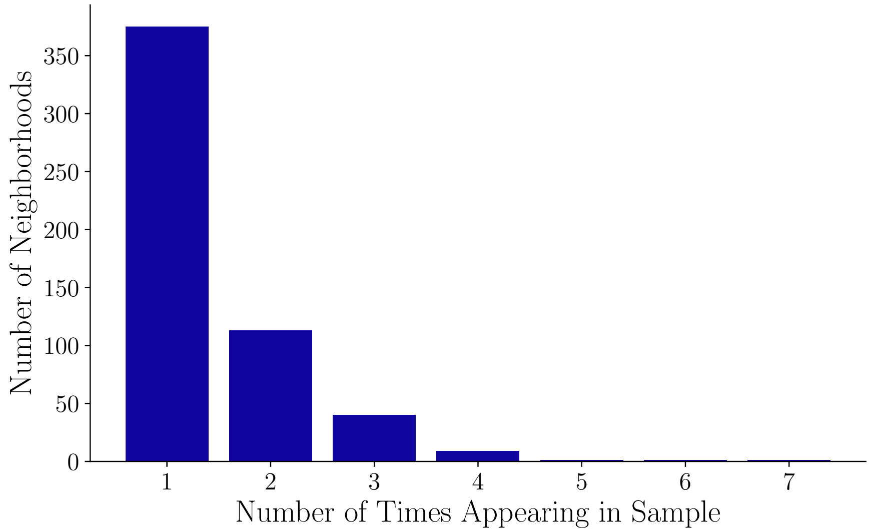


Figure B1: Number of Repeated Controls

Notes: The figure shows a histogram for the number of times a matched control neighborhood appears in the final sample. There are 540 unique control neighborhoods plotted, with the average neighborhood appearing 1.4 times and the median appearing 1 time.

Panel A. All Individuals

Panel B. Without Released Offenders

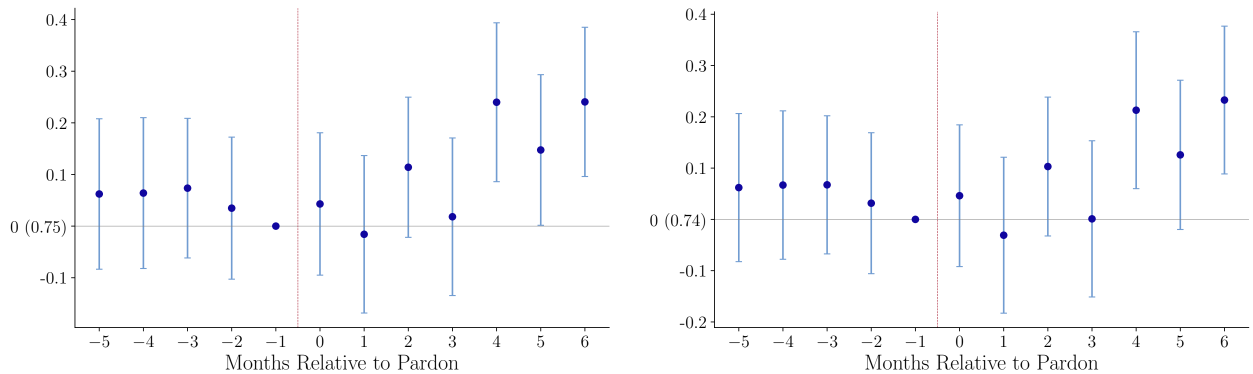


Figure B2: Effects of Mass Pardon on Arrests

Notes: The figure displays the regression coefficients for the difference in the probability of arrest between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_j from equation 2. The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the probability of arrest (multiplied by one thousand), i.e., the dependent variable, at $t - 1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level. Panel A shows the estimates on the sample of all men between 18 and 40 years, including released offenders. Panel B drops the released offenders from the sample.

Panel A. All Individuals

Panel B. Without Released Offenders

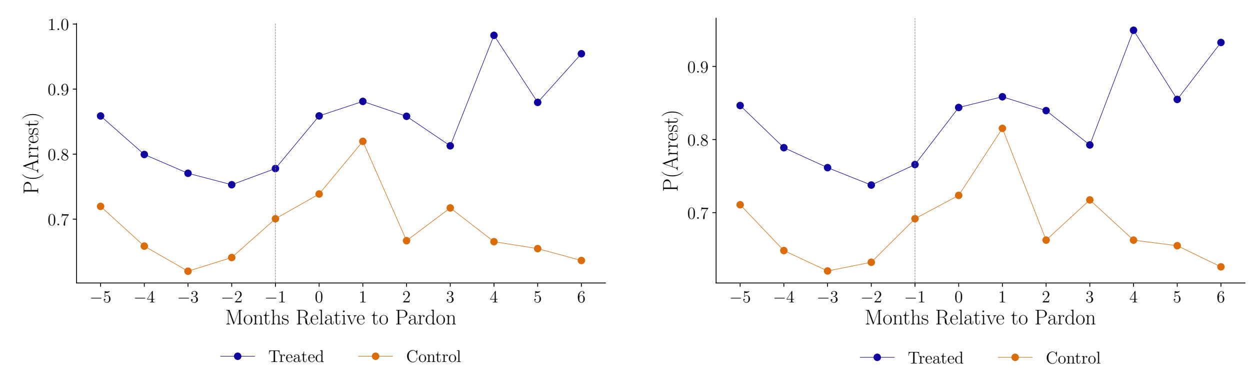


Figure B3: Probability of Arrest - Raw Means

Notes: The figure displays the raw means of the probability of arrest (multiplied by 1,000) for individuals aged 18 to 40 living in matched treated and control neighborhoods. The data covers the period from September 2021 ($t = -5$) to August 2022 ($t = 6$). Panel A presents the means for the entire sample, including released offenders. Panel B shows the means for the same sample but excludes released offenders.

Panel A. Without Arrest Record | Panel B. With Arrest Record

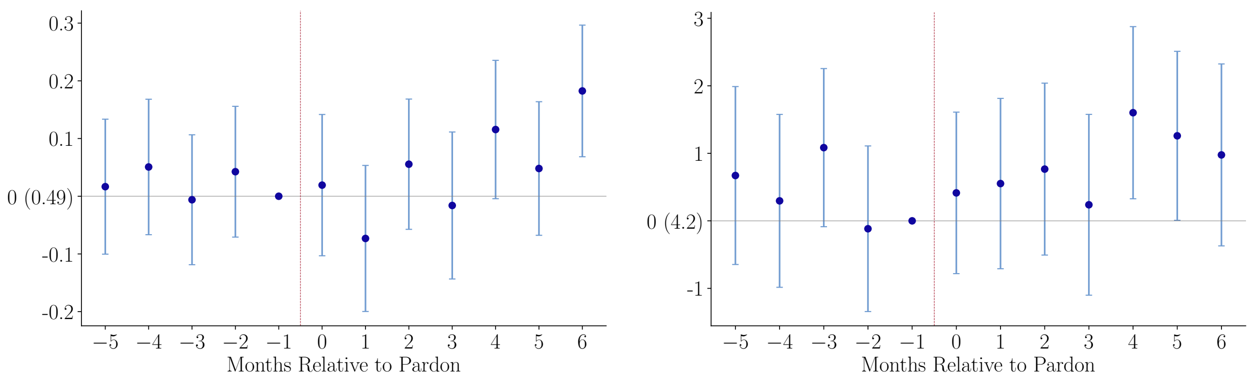


Figure B4: Effects by Residents' Criminal Records

Notes: The figure displays the regression coefficients for the difference in the probability of arrest between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_j from equation 2. Panel A shows the estimates on people without any arrest record ($N = 21,634,761$), and Panel B shows the estimates only on people with criminal history ($N = 1,545,644$). The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the probability of arrest (multiplied by one thousand), i.e., the dependent variable, at $t - 1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level.

C Heterogeneity Results

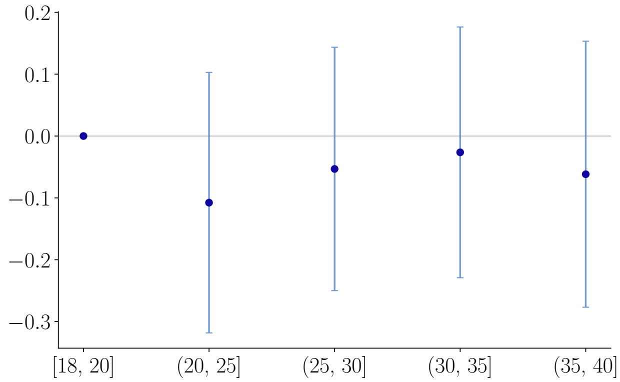


Figure C1: Effects on Arrests by Resident's age

Notes: The figure displays the regression coefficients and the associated 95 percent uniform confidence intervals for the difference in the probability of arrest between treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from equation 2. The coefficients at $t = -1$ are normalized to zero. Panel A shows the estimates on all men between 18 and 40 years, Panel B drops the released offenders from the sample.

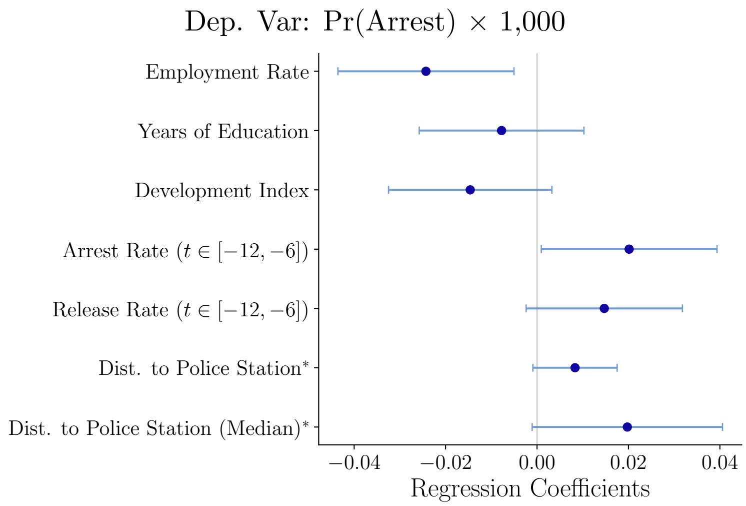
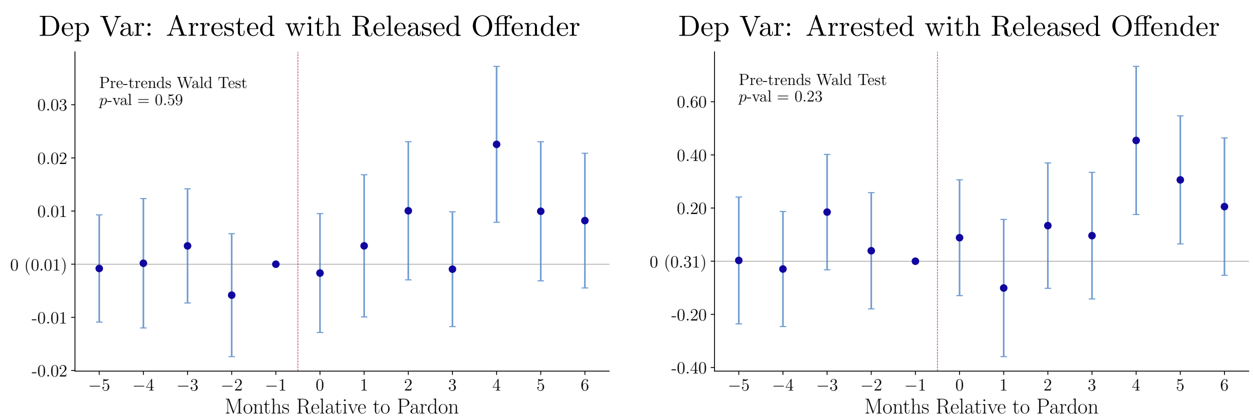


Figure C2: Heterogeneity by Neighborhoods' Characteristics

Notes: The figure displays the heterogeneity coefficients for the difference-in-difference estimation of the effect of the pardon. Each coefficient is re-weighted so it is expressed in standard deviations. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level. * are only computed on a sample of the three major cities in Ecuador.

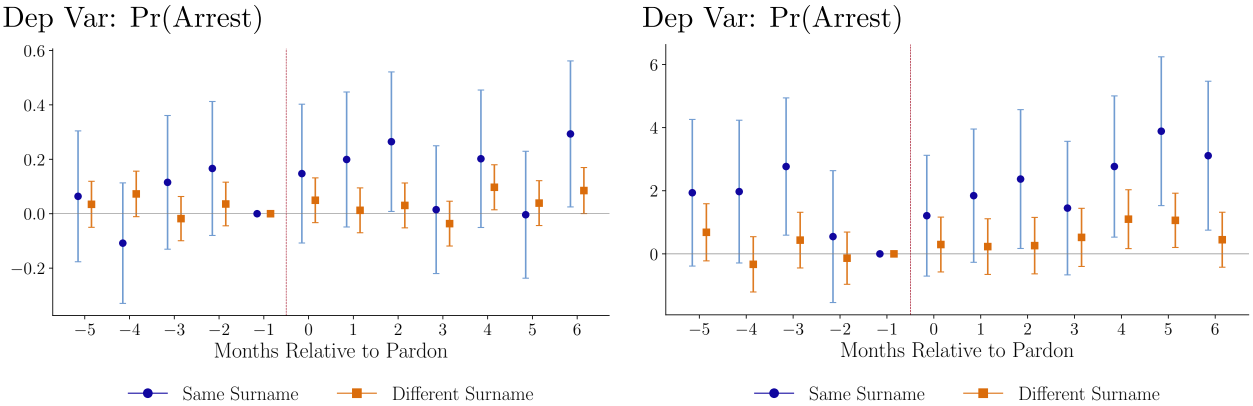


Panel A. Without Arrest Record

Panel B. With Arrest Record

Figure C3: Criminal Partnerships by Resident's Arrest Records

Notes: The figure shows the regression coefficients for the difference in the probability of beign arrested with a released offender (times 1,000) between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 2. Each panel shows a stratified regression based on arrest records before the pardon. Panel A shows the estimates on people without any arrest record ($N = 28,587,505$), and Panel B shows the estimates only on people with criminal history ($N = 1,987,011$). The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the dependent variable at $t = -1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level.

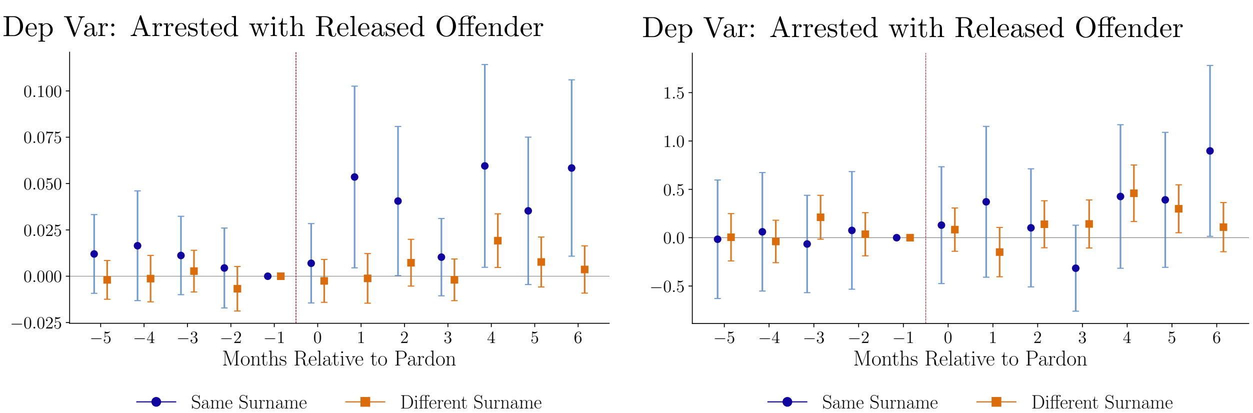


Panel A. Without Arrest Records

Panel B. With Arrest Records

Figure C4: Effects on Arrests by Criminal Records and Family Networks

Notes: The figure shows the regression coefficients for the difference in the probability of arrest (times 1,000) between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 2. Each panel shows a stratified regressions based on whether the the neighborhoods residents have the same last name as the released offender. Panel A shows the estimates on the sample of people without any arrest record, and Panel B shows the estimates only on people with criminal history. The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the dependent variable at $t = -1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level.



Panel A. Without Arrest Records

Panel B. With Arrest Records

Figure C5: Arrests with a Released Offender by Residents' Criminal Records

Notes: The figure shows the regression coefficients for the difference in the probability of being arrested alongside a released offender (times 1,000) between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 2. Each panel shows a stratified regressions based on whether the the neighborhoods residents have the same last name as the released offender. Panel A shows the estimates on the sample of people without any arrest record, and Panel B shows the estimates only on people with criminal history. The coefficients at $t = -1$ are normalized to zero. On the y-axis, in parenthesis is the mean of the dependent variable at $t = -1$. The bars correspond to the 95 percent confidence interval with standard errors clustered at the neighborhood level.

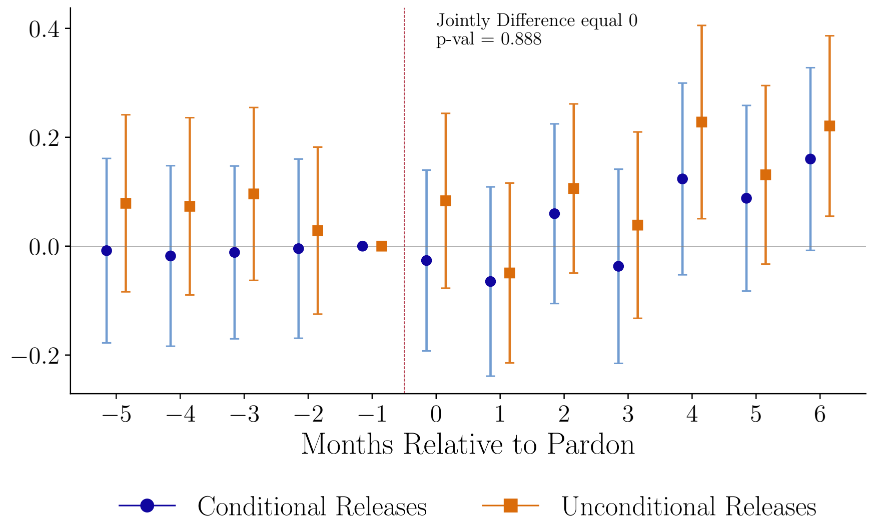


Figure C6: Heterogeneity by Type of Release

Notes: The figure shows the coefficients for the difference in the probability of arrest between people living in treated and matched control neighborhoods relative to the month before the pardon, stratified by the type of release. The circles show the effects for conditional releases (pardoned individuals) and the squares the effects for all other releases. The sample excludes 75 neighborhoods that had both type of releases at the same time. The error bars show 95 percent confidence intervals computed with standard errors clustered at the neighborhood level.

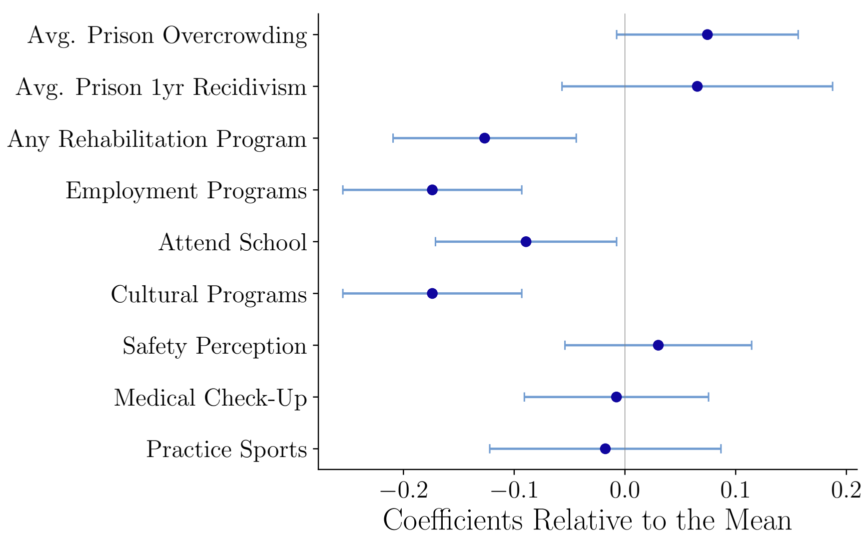


Figure C7: Heterogeneity on the Probability of Arrest by Prison Experience

Notes: The figure shows the difference in the effect of the pardon on the probability of arrest for neighborhoods that received offenders from prisons with characteristics above the median, compared to those below the median. Each row is a separate regression with a different characteristic. *Prison Overcrowding* is measured between $t-7$ to $t-2$. *Recidivism* is the average one year recidivism rate for individuals released between 2016 and 2021. The data on the rehabilitation programs comes from the 2022's Prison Census, and measure average inmate participation on each type of program. The coefficients are rescaled relative to the mean of the dependent variable. The error bars show 95 percent confidence intervals computed with standard errors clustered at the neighborhood level.

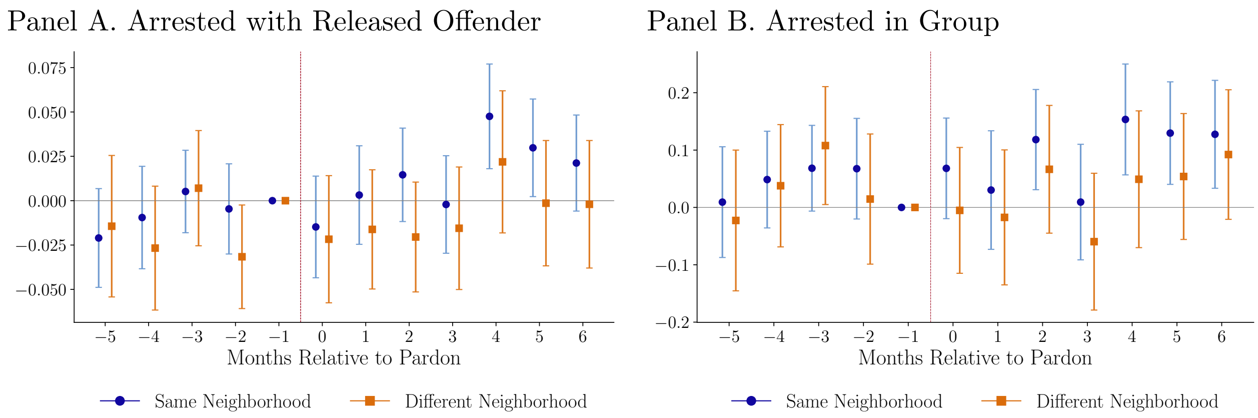


Figure C8: Neighborhood of Origin

Notes:

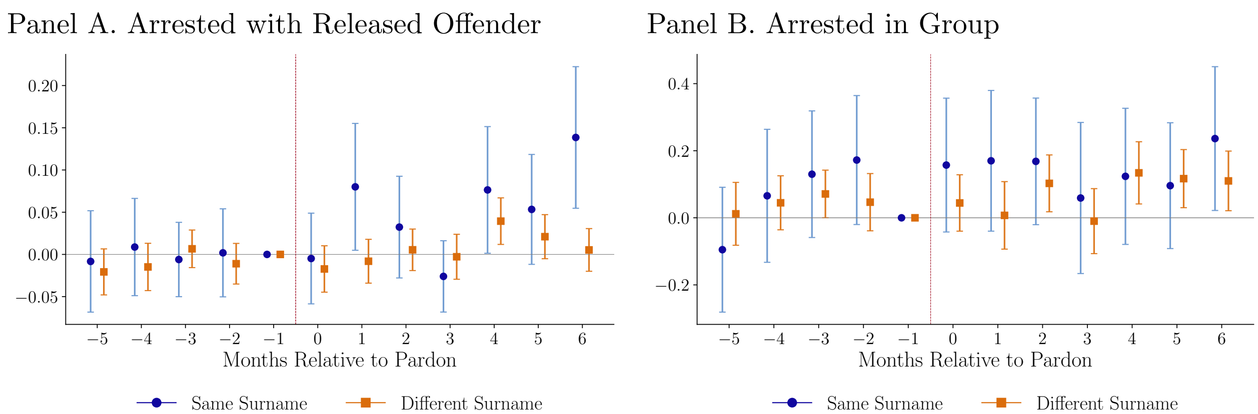


Figure C9: Same Last Name

Notes: