The Spillover Effects of Prisoner Releases: Evidence from Ecuador

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Abstract

Do released offenders influence the criminal behavior of individuals in the neighborhoods they rejoin? Using a unique dataset on arrests, prison releases, and places of residence for the universe of men in Ecuador and exploiting a mass pardon in a difference-in-difference design, I find evidence that released offenders contribute to increased criminal activity among their neighbors. On average, one additional release leads to an increase of 0.85 arrests, excluding the released offenders themselves. First-time offenders account for 42% of this effect, with the primary mechanism being the spread of criminal behavior through peer and family networks. These peer effects are larger for defendants who served longer portions of their sentences, suggesting that time spent in prison may intensify criminal behavior. Finally, I show that access to job training programs during incarceration can help mitigate these effects.

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1 Introduction

Every day, thousands of individuals are incarcerated worldwide, with most eventually returning to their communities. A primary policy concern regarding prisoner reentry is its potential impact on crime rates due to recidivism (Yukhnenko et al., 2020). In the US, estimates suggest that 44% of released inmates are rearrested within one year (Doleac, 2023; Durose et al., 2014), while the average one-year recidivism rate across Latin America is 39% (Bergman et al., 2020; Fazel and Wolf, 2015). However, the impact of released offenders on crime extends beyond recidivism. Former prisoners may affect local crime rates by influencing the behavior of members in their social networks and the broader communities to which they return. These spillover effects involve a wider range of individuals, potentially making them more significant than the direct effect of reoffending.

Theoretically, the direction of these spillover effects is ambiguous. On one hand, effectively rehabilitated inmates may play a positive role in crime prevention within their communities. Former offenders can share knowledge of the negative consequences of criminal behavior, discouraging others from following a similar path. There are anecdotal reports of ex-offenders joining NGOs or community groups, where they work to steer at-risk individuals away from crime.[1] On the other hand, incarceration may increase inmates' criminal skills. While in prison, individuals are often exposed to hardened peers, gang recruitment, and an environment that deteriorates their human capital (Aizer and Doyle, 2015; Mueller-Smith, 2015). Once released, an offender may draw members of their social network into new criminal activities, either by passing on newly acquired criminal skills or by actively recruiting them into gangs (Sviatschi, 2022).

In this paper, I study whether recently released offenders influence the criminal behavior of individuals in the neighborhoods they rejoin. Estimating these effects is challenging due to strict data requirements, as it necessitates information on the residence and criminal activity of all neighborhood residents, including those with and without arrest records. This constraint has limited prior research on prisoner reentry and neighborhood peers, which has primarily focused on how released offenders' recidivism is influenced by their criminal peers in their community (Billings and Schnepel, 2022; Kirk, 2015). This approach provides little insight into whether released offenders can influence the criminal behavior of other residents. Further, it overlooks the impact on the largest population segment: those without prior criminal experience.

To address this gap, I analyze prison releases and arrests among Ecuador's entire adult

[1] See here for examples in Canada, US, and Ecuador.

male population, providing evidence of the influence of criminal peers within neighborhoods. This represents the first empirical analysis demonstrating that former offenders influence the criminal behavior of their neighborhood peers, even those without prior criminal experience. Notably, I find that 40% of the observed increase in criminal activity comes from first-time offenders.

For this project, I build a unique dataset tracking prison releases, arrests, and places of residence for all men aged 18 and older in Ecuador. I collect information on prison releases and arrests by scrapping and extracting data from over two million public documents containing all penal cases between 2016 and 2022. Then, I match this information with individual records of places of residence at the neighborhood level from 2002 to 2021, obtained from voting registration locations.

I use this data in an event-study design focused on a mass pardon. 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, this pardon led to a 31% increase in the number of released offenders and a 26% increase in the number of neighborhoods that received a former convict. I exploit the extensive margin variation generated by the pardon to compare the probability of arrest for individuals living in neighborhoods that received a released offender with those in neighborhoods that did not receive an offender during the pardon period but had previously received inmates.

My findings reveal that released offenders generate significant criminal spillovers in the neighborhoods where they return. On average, the monthly 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 result indicates that for every additional release, there are 0.85 new arrests (excluding recidivism), leading to an elasticity of arrests with respect to releases of 0.18. When released offenders are included in the analysis, the number of arrests increases by 0.98. The difference between these estimates reflects the mechanical rise in crime due to recidivism.

The influence of former offenders extends 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 findings suggest that 42% of the overall impact comes from people without criminal experience, suggesting that contact with former offenders not only leads to new crimes but also contributes to the creation of new criminals.

Two mechanisms may explain these results. First, there could be a direct contagion effect from released offenders to their social connections. Previous studies have documented the spread of criminal traits among individuals sharing the same environment, such as prisons or schools (Billings and Hoekstra, 2024; Stevenson, 2017).[2] Second, reintegrating offenders into society can affect the behavior of individuals beyond their direct network by changing the salience of gangs, shifting perceptions of the risks and rewards of criminal activity, and introducing new criminal role models (Helfgott, 2015; Petersilia, 2000).

I find evidence consistent with the first mechanism: a contagion from released offenders to individuals within their direct network. Measuring social connections in illegal activities is challenging since complete records of families, friends, and criminal associates rarely exist (Corno, 2017). To address this, I focus on criminal partnerships and family networks. First, the data I collected details all the individuals arrested for the same crime. I exploit this information to create an indicator of joint arrests between released offenders and non-released individuals within a neighborhood to define criminal partnerships. On average, the likelihood of being arrested alongside a released offender for individuals in treated neighborhoods rose by 49% relative to the mean compared to those in control areas. First-time offenders represent 47% of the magnitude of this effect.

Additionally, I show that the influence of released offenders spreads through family networks. I link individuals with the same last name to identify potential family connections. Individuals sharing a last name with a released offender experienced 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) compared to individuals in treated neighborhoods with different last names. These findings suggest that family connections account potentially for 27% of the overall effects and 41% of the effects observed among individuals without a criminal history.

Lastly, I examine the role of prisons and incarceration in explaining these effects. Studies from other Latin American countries suggest that imprisonment can exacerbate the likelihood of former inmates returning to criminal activities due to factors such as a lack of rehabilitation programs, overcrowding, and violent conditions in prisons (Escobar et al., 2023; Munyo and Rossi, 2015; Di Tella and Schargrodsky, 2013). These issues likely contribute to the observed spillover effects. Focusing on individuals convicted of the same crime who served different

[2] For references on criminal peer effects between family members see: Norris et al. (2021); Bhuller et al. (2018a), within schools: Billings and Hoekstra (2024); Billings et al. (2014, 2019), and within prisons see: Stevenson (2017); Bayer et al. (2009); Philippe (2017); Drago and Galbiati (2012)

sentence lengths because of the pardon, I find that longer imprisonment amplifies the spillover effects. Conversely, I find evidence of the potential benefits of rehabilitation programs in mitigating these adverse effects. Offenders released from prisons with higher participation rates in rehabilitation programs did not lead to any significant spillover effects.

This paper contributes to several areas of research. First, it adds to our understanding of criminal peer effects by documenting the transmission of criminal behavior from released offenders to their neighbors. 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 and Schnepel, 2022; Damm and 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 and Hoekstra, 2024; Billings et al., 2019, 2014). I further expand this knowledge by showing that criminal peers can influence individuals without criminal experience.

Second, it contributes to the knowledge about incarceration in developing countries, particularly for Latin America. Previous research in the region 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 and Rossi, 2015; Di Tella and Schargrodsky, 2013). These challenges make it difficult for former inmates to successfully reintegrate into society (Blattman et al., 2024; Sviatschi, 2022; Tobón, 2022; Carvalho and Soares, 2016). This paper expands on the existing literature by presenting evidence that experiences in prison can affect not only former inmates but also non-incarcerated individuals. Further, it provides evidence of the potential benefits of social rehabilitation programs in mitigating these spillover effects.

Finally, it contributes to our understanding of prisoner reentry on subsequent crime. 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 and Yates, 2009; Raphael and Stoll, 2004; Clear et al., 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 advances the field by employing individual-level data to distinguish the contributions to crime between recidivism and new offenses. Moreover, it decomposes these new offenses into those committed by individuals with prior criminal experience and those committed by first-time offenders.

The rest of the paper is organized as follows. Section 2 presents the institutional background of the mass pardon. Section 3 shows the data sources and summary statistics. Section 4 contains the emprirical strategy and the main results of the paper, while Section 5 discusses possible mechanisms that may explain these results. Section 6 shows evidence of the role of prisons conditions, and Section 7 concludes.

2 Ecuadorian Prison System and the Mass Pardon

Ecuador's crime policies have historically emphasized punitive measures, with incarceration often being the main response to crime (The Economist, 2024; Verdugo, 2023). However, this emphasis on imprisonment has not been matched by efforts to rehabilitate inmates or improve prison facilities. In 2021, the average prison overcrowding rate was 29%, which was higher than the regional average (Fair and Walmsley, 2024). The system is also marked by gang infiltration, high levels of violence, and limited access to rehabilitation programs. Between 2021 and 2022, eleven gang-related prison riots led to over 413 deaths (Primicias, 2022). Further, a census conducted by the end of 2022 reported that only 43% of inmates participated in any rehabilitation program (Instituto Nacional de Estadisticas y Censos, 2022).

With the objective of reducing overcrowding, 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 traffic offenses 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.[3] The pardons 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 the prison population by releasing the least 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

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

2022. Similarly, the pardon also increased the number of neighborhoods from which released offenders originally came. Panel B of Figure 1 shows this increase.

To access the pardon, inmates 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 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, one month after the pardon.

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 of alcohol 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 a third of the incarcerated population in 2023.

3 Data and Summary Statistics

This paper uses a comprehensive dataset of the entire population of prison releases, arrests, and neighborhood-level residences for all male adults in Ecuador. This section outlines the main variables used in the study and their respective sources.

Place of Residence: The primary source of information is the voting registry compiled by the Ecuadorian electoral agency, the *Consejo Nacional Electoral* (CNE). Since voting is mandatory in Ecuador, the registry provides information on all nationals aged 16 or older, regardless of whether they vote. The dataset includes details such as names, national identification numbers, sex at birth, date of birth, and the polling station where individuals are registered. I have access to these records for all elections held between 2002 and 2021.[4]

In addition to obtaining demographic data, I use this registry to define neighborhoods and identify their residents. The CNE assigns individuals to the polling station nearest their

[4]The specific years for which I have information are: 2002, 2004, 2006, 2009, 2013, 2014, 2017, 2019, and 2021.

registered address, thereby grouping neighborhoods into a common voting location. Based on this setup, I consider all individuals registered to vote in the same location as neighbors. Each neighborhood typically contains around four thousand people in urban areas, roughly equivalent to a census tract in the U.S. When individuals first appear in the registry at age 16, they are usually registered at the same address as their parents. To change a polling station, individuals must submit proof of residence, such as a government-issued utility bill (e.g., electricity or water bill). These updates can only occur six to ten months before each election.

Arrests: The data on arrests comes from records published by the *Consejo de la Judicatura*, the institution overseeing Ecuador's judicial system. This organization operates a public webpage called SATJE, where all judicial courts must upload documents related to cases they manage. SATJE hosts information on civil and criminal cases. The only confidential cases are those involving minors, violence against women, and acts against national security.

I retrieved the criminal cases involving all the individuals in the voting registry. This information includes the suspected crime, arrest date, and the identities of all individuals involved in each arrest. However, the data does not indicate whether these arrests resulted in a conviction.[5]

Prison Releases: The data on prison releases also comes from the website SATJE. Unlike the arrest data, information about prisoner releases does not come in a structured format. For each release, SATJE provides access to the Release Warrant (*"Boleta de Excarcelación"*), a document issued by the judge to authorize a release based on either sentence competition or a pardon. Since each court secretary drafts the release warrant individually, the document structure varies by case. I web-scrap all release warrants issued between 2016 and 2022 and employ OpenAI's LLM with Retrieval-Augmented Generation (RAG) to extract relevant information from each document (Lewis, 2021). For each case, I collect information including the offender's full name, national ID number, nationality, arrest date, crime committed, type of release, and release date.[6]

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

[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%.

3.1 Sample Description

I compile all the data at the individual-by-month level. For non-released individuals, I aggregate daily arrest data into monthly observations and assign everyone to a neighborhood based on the polling station for the 2021 election. I match this information to released offenders using their residence at the time of arrest. While I lack data on the neighborhoods where former offenders reside after the pardon, records from 2016 to 2021 indicate that 95% of inmates returned to the neighborhood where they lived at the time of their arrest.

For the analysis, I focus on men aged 18 to 40 residing in urban areas. The crime literature highlights young men as the demographic most likely to engage in criminal activity (Aizer and Doyle, 2015; Billings et al., 2014; Bayer et al., 2009). In the entire dataset, 89% of released individuals and 93% of those arrested are men. Further, I limit the analysis to urban centers because polling stations do not accurately reflect spatial proximity between individuals in rural areas. In rural settings, polling stations are centralized in the main town, requiring residents from surrounding villages, who may not be close, to travel to vote. This situation reduces the potential contact between released offenders and their neighbors. Additionally, data from a 2022 carceral census indicate that over 87% of inmates live in urban areas.

Table 1 presents summary statistics. Panel A shows descriptive information for all residents in the sample. On average, individuals are 28 years old, and 6% have an arrest record (not necessarily a conviction). The probability of an individual being arrested each month is 0.074%, with most arrests occurring only once per month. The likelihood of being detained alongside a released offender is 0.003%, and 56% of all arrests involve multiple individuals detained for the same offense.

Panel B describes the characteristics of the released offenders. The majority are male, serving an average sentence of 26 months and an entry age of 30. Among releases, 36% are conditional, including pardons. For offenders released from 2016 to 2021, 95% return to their pre-arrest neighborhoods. Finally, Figure 2 maps neighborhood-level releases and arrests. Panel A shows neighborhoods in Quito divided by whether they received a released offender following the pardon. Panel B displays the percentage change in, comparing pre and post pardon periods.

4 Effects of the Mass Pardon

This section discusses the empirical strategy, presents the results for the first-stage effects on the number of released offenders, and shows the main results on the probability of arrests.

4.1 Empirical Strategy

To estimate the impact of the pardon on the probability of arrest, I use an event study design, with the treatment assigned at the neighborhood level. I define 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. Additionally, I exclude 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. Panel A of Figure 2 shows the spatial distribution of treated and control neighborhoods in Ecuador's capital, Quito.

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.

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

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. $\textit{With Offender}_n$ is an indicator equal to one if neighborhood n was treated (i.e., received an 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, and u_{int} is the error term. I omit the dummy for the month before 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 $k = -1$. I cluster the standard errors at the neighborhood level.

The event-study coefficients from Equation 1 represent the intent-to-treat (ITT) effect of the pardon, given the potential measurement error in individuals' places of residence. To begin with, people may reside in locations different from their registered addresses. Although changing one's voting location requires proof of residence, some individuals move without updating their addresses or may use those of relatives or friends to vote in other areas. Moreover, the neighborhoods to which released offenders return may not be the ones they lived in at the time of arrest. Incarceration can weaken connections with previous social networks and foster ties with incarcerated peers, both of which can increase the likelihood of offenders relocating to different neighborhoods upon release. These limitations may bias the results toward zero, meaning that the effects reported here should be viewed as a lower bound of the actual impact of released offenders on the probability of arrest.

4.2 Changes in Released Offenders

I begin the analysis by calculating the "first stage" effect of the pardon. Specifically, I estimate the difference-in-difference version of Equation 1 using the number of released offenders and the release rate per 1,000 residents at the neighborhood level as outcomes. Table 2 indicates that the pardon increased the presence of released offenders in treated neighborhoods across all measures. On average, treated neighborhoods received 0.14 more monthly releases (77% of the mean), 0.17 more releases per 1,000 inhabitants (113% of the mean), and are 13 percentage points (90% of the mean) more likely to receive a released offender than counterfactual neighborhoods.

Figure 3 displays the event-study coefficients from Equation 1. It shows that treated neighborhoods received more released offenders than counterfactual neighborhoods only between February and April 2022 ($t \in [0, 2]$). During these three months, the average number of offenders released returning to the treated neighborhoods increased by 0.41 compared to control neighborhoods. This pattern is mechanical, driven by the assignment of treatment and control neighborhoods, which directly maps to releases within the first three months following the pardon. For all months after April 2022 ($t > 2$), the number of releases originating from treated and control neighborhoods is the same.

Moreover, Figure 3 supports the argument that the selection between treatment and control neighborhoods is due to quasi-random variation in the timing of releases, rather than a systematic selection bias. It is possible that treated neighborhoods might be more likely to receive released offenders at any point in time, not solely during the pardon period. If this were true, an observed increase in arrests in treatment neighborhoods relative to controls could reflect selection differences rather than the effects of exposure to released offenders. However, Figure 3 provides evidence against this concern, showing that the only difference in releases occurs mechanically within the three months when individuals were pardoned. There is no observed difference in release rates before the pardon or after the last pardoned offender's release. This result suggests that, in the absence of the pardon, the number of released offenders from treated and control neighborhoods would have been the same.

4.3 Effects of the Pardon on Arrests

Figure 4 reports the event-study coefficients $\hat{\beta}_k$ from Equation 1 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 4) 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 4 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.

4.4 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 and 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 and Hoekstra, 2024; Damm and 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 5 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 6 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.

4.5 Robustness

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 A.2 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.

Differential Path of Releases: Another concern is that treated and control neighborhoods may have a differential path regarding releases. Specifically, treated neighborhoods can potentially have more releases than control neighborhoods after the treatment. If this is true, the effects computed in this section would eb upward-biased since treated neighborhoods would have received more releases than the control group. In Appendix A.3 I show how the number of releases and the rate of release only differ during the pardon. For months where there were no pardoned individuals ($t > 3$) the rate of release between treated and control neighborhoods is the same.

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 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.

5.1 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 7 presents the event study coefficients for this outcome.

The estimates in Figure 7 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.

5.2 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 et al., 2018a,b; Dobbie et al., 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 8 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 8 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 A.4 presents robustness checks for the measures of family connection. Figure A8 displays the estimates after excluding individuals with the most common last names. Additionally, Figure A7 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.

5.3 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

9 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 9 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.

6 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 and Schargrodsky, 2013; Munyo and Rossi, 2015; Tobón, 2022; Bhuller et al., 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 10 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 10 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.

7 Conclusions

Incarceration is one of the most popular strategies governments use to combat crime. Over the last decade, many countries in Latin America have adopted *Mano Dura* policies, where incarceration serves as a central component (The Economist, 2024). In this paper, I demonstrate that incarceration and prisoner reentry can counteract efforts to fight crime. I present the first causal estimates of the spillover effects of released offenders on their peers' involvement in criminal activity within their neighborhoods.

By focusing on Ecuador, I found that one additional released offender per 1,000 residents in a neighborhood increased the monthly number of arrests by 0.73. This effect was not solely driven by individuals with criminal records, those without any prior arrest history accounted for 40% of the increase in arrests. Further, I demonstrated how this effect spreads through peer networks, particularly among family members. These findings highlight the broader impact that offender releases have on crime, affecting not only reoffending rates but also contributing to the emergence of new criminals.

Finally, from a policy perspective, I present evidence of how these spillover effects are a function of the experiences of offenders while in prison. Specifically, individuals released from prisons where a higher share of inmates who participated in rehabilitation programs while incarcerated showed no spillover effects. This result emphasizes the importance of access to rehabilitation programs and suggests that focusing on rehabilitation during incarceration can lead to better outcomes for communities.

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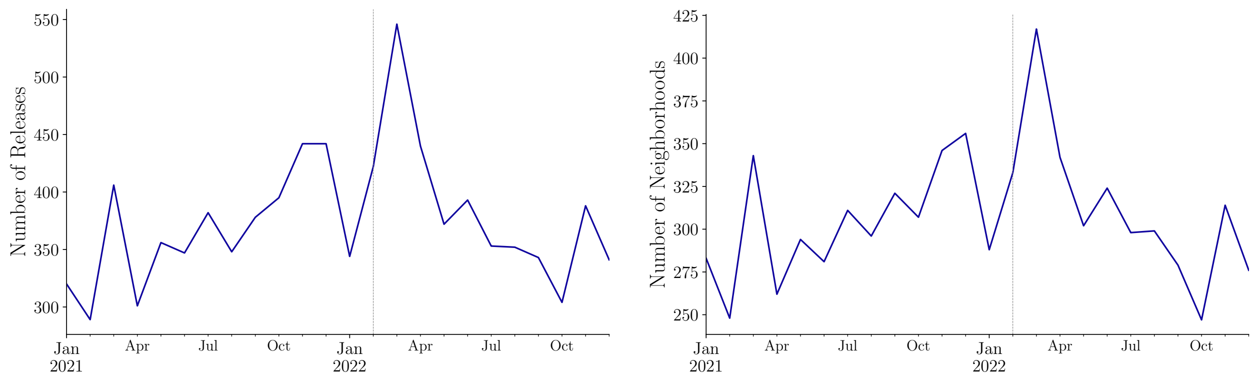
Figures

Figure 1: Monthly Prison Releases

Panel A. Number of Released Offenders

Panel B. Neighborhoods with Released Offenders

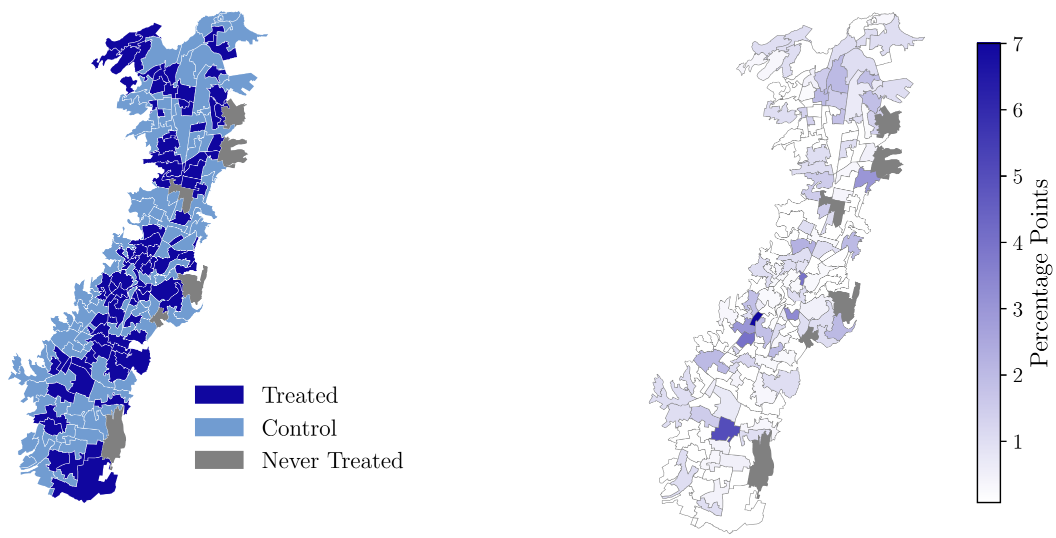


Notes: Panel A shows the monthly evolution of the total number of released offenders from January 2021 to December 2022. Panel B displays the number of neighborhoods that received a released offender over the same period. The vertical dashed lines indicate the date of the mass pardon in February 2022. The sample is restricted to releases into urban neighborhoods and excludes traffic-related offenders.

Figure 2: Releases and Arrests in Quito

Panel A. Neighborhoods with Released Offenders

Panel B. Change in Arrests

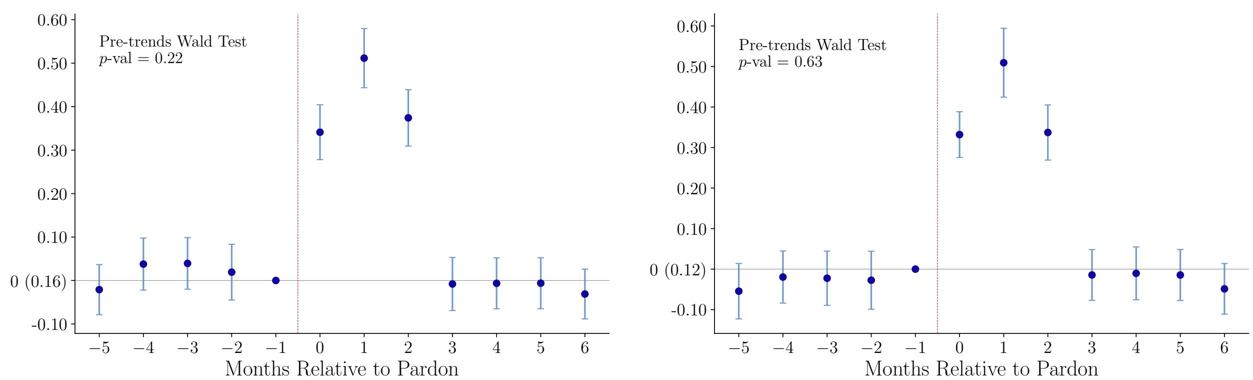


Notes: The figures present maps of neighborhoods in Ecuador's capital, Quito. Panel A distinguishes treated, control, and never treated neighborhoods based on whether they received a released offender within three months of the mass pardon. Panel B shows the percentage change in arrests for individuals residing in each neighborhood, with darker colors indicating a larger increase in arrests among residents of those neighborhoods.

Figure 3: Variation in Released Offenders (First Stage)

Panel A. Number of Releases

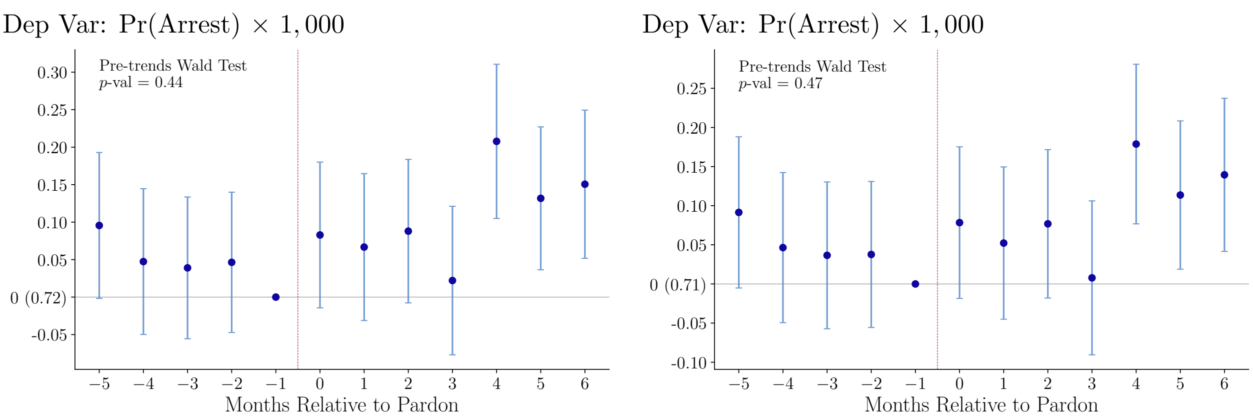
Panel B. Release Rate



Notes: The figure displays the regression coefficients for the difference in the number of releases (Panel A) and release rate by 1,000 residents (Panel B) between treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). The coefficients at $t = -1$ are normalized to zero. The mean of the dependent variable at $t = -1$ is shown in parentheses on the y-axis. The unit of observation is at the neighborhood-by-month level. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

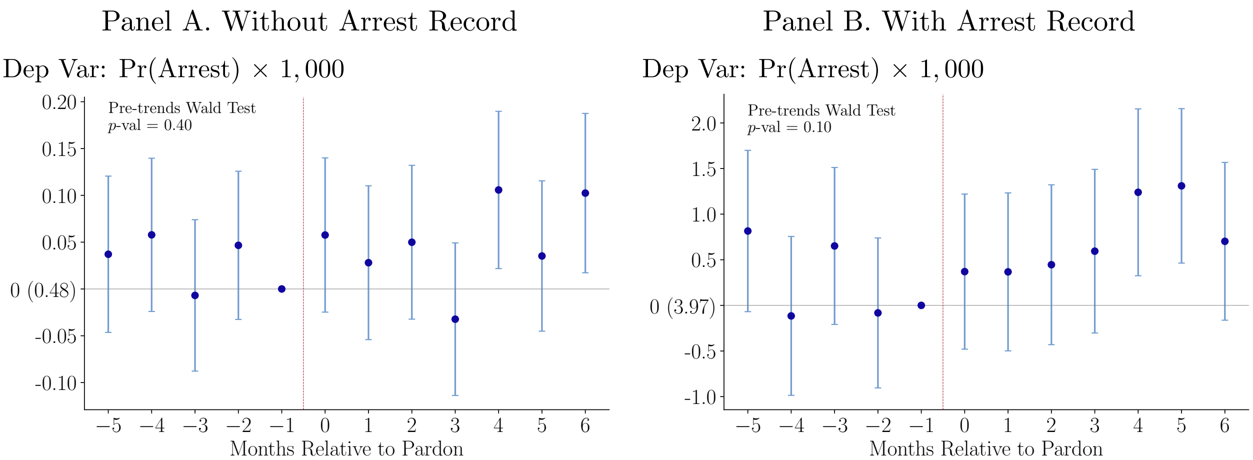
Figure 4: Effects of the Mass Pardon on Arrests

Panel A. All Arrests | Panel B. Without Recidivism



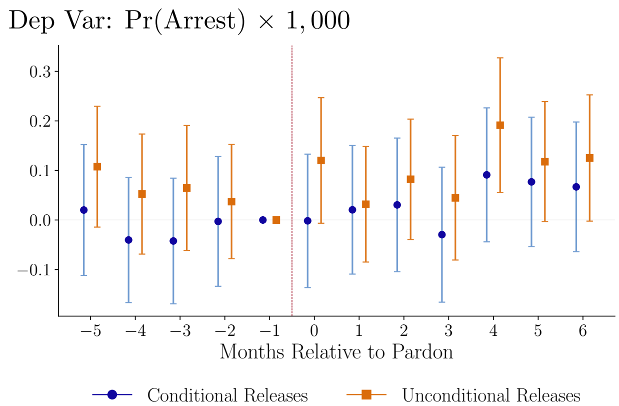
Notes: The figure displays the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) between people living in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). The coefficients at $t = -1$ are normalized to zero. The mean of the dependent variable at $t = -1$ is shown in parentheses on the y-axis. The unit of observation is at the individual-by-month level. The bars represent the 95 percent confidence intervals, 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 excludes released offenders from the estimation sample.

Figure 5: Effects by Residents' Criminal Experience



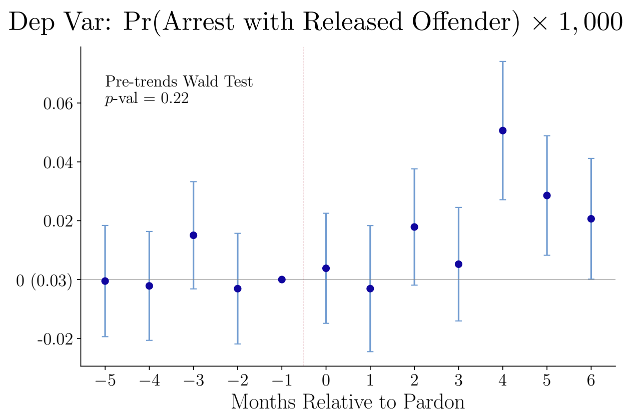
Notes: The figure displays the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) between individuals living in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). Each panel shows estimates from a separate regression based on individuals' arrest history before the pardon. Panel A shows estimates for individuals with no arrest records ($N = 28,587,505$), while Panel B focuses on people with at least one arrest record ($N = 1,987,011$). In both panels, the sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding those released from prison in the last year. The coefficients at $t = -1$ are normalized to zero. The mean of the dependent variable at $t = -1$ is shown in parentheses on the y-axis. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

Figure 6: 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 (i.e., the β_k from Equation 1). The circled dots represent the effects for individuals in neighborhoods that received only a pardoned offender (conditional release), while the squares display the estimates for people in areas treated with offenders who completed their sentences. The sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding those released from prison in the last year. The coefficients at $t = -1$ are normalized to zero. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

Figure 7: Formation of Criminal Partnerships

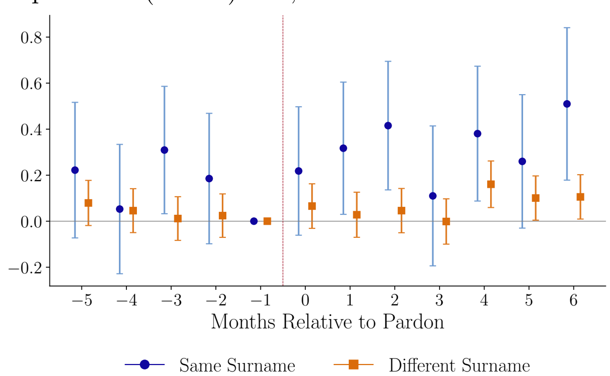


Notes: The figure displays the regression coefficients for the difference in the probability of being arrest alongside a released offender (multiplied by 1,000) between people living in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). An arrest is considered to involve a released offender if the offender was released within one year before the arrest. The unit of observation is at the individual-by-month level. The sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding released offenders. The coefficients at $t = -1$ are normalized to zero. The mean of the dependent variable at $t = -1$ is shown in parentheses on the y-axis. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

Figure 8: Criminal Spillovers in Family Networks

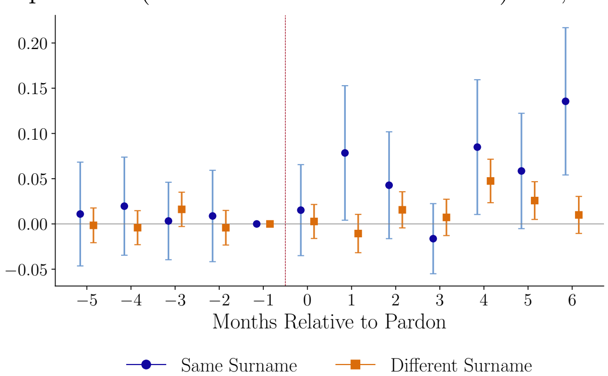
Panel A. Individual Arrests

Dep Var: Pr(Arrest) $\times$ 1,000



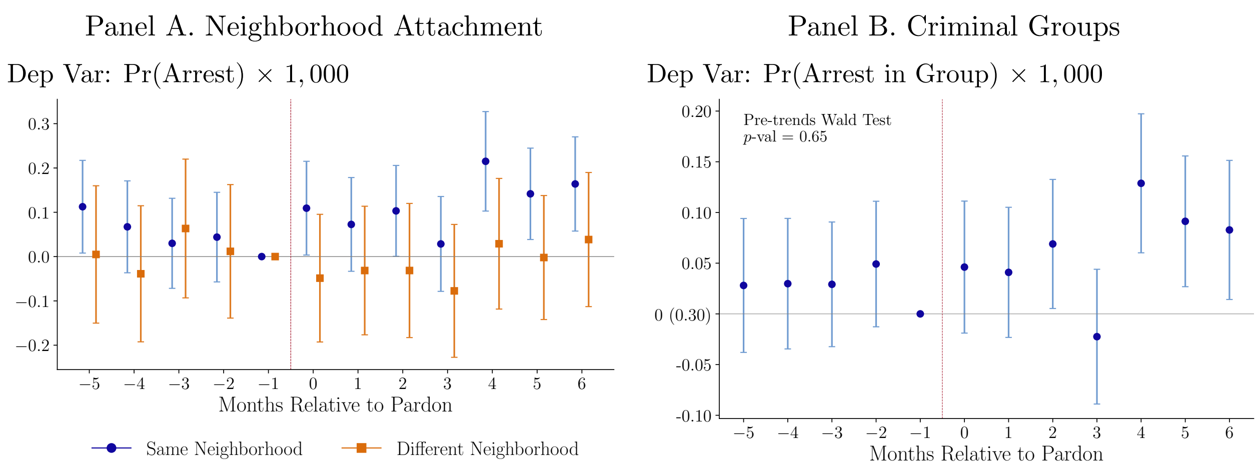
Panel B. Partnerships with Releasees

Dep Var: Pr(Arrest with Released Offender) $\times$ 1,000



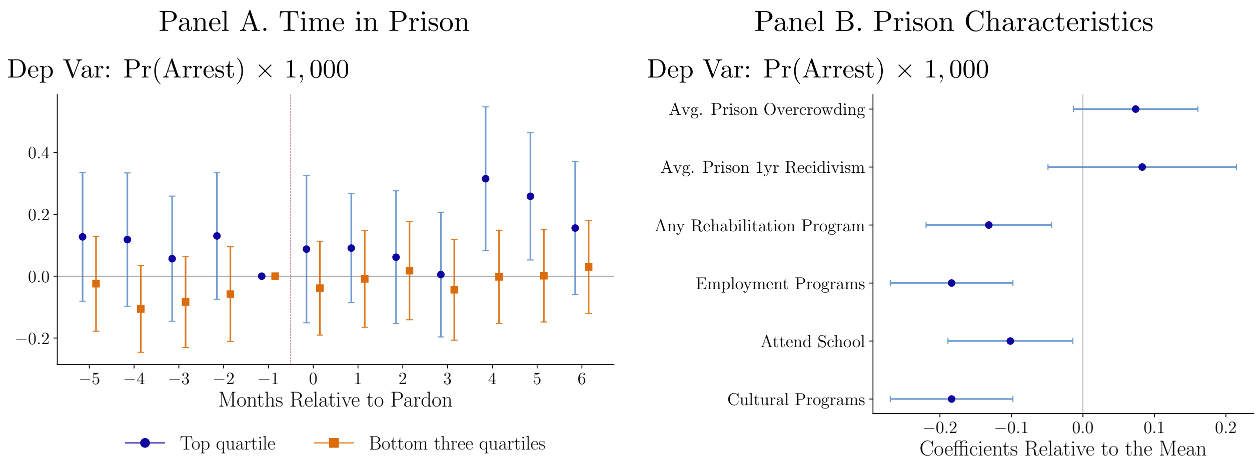
Notes: Each panel displays the regression coefficients for the difference in outcomes between people living in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). The circled dots represent the effects on individuals sharing a surname with a released offender, while the squares display the estimates for those with a different surname. The outcome in Panel A is the probability of arrest (multiplied by 1,000), while in Panel B is the probability of being arrested alongside a released offender (multiplied by 1,000). An arrest is considered to involve a released offender if the release occurred within one year before the arrest. All regressions control for the share of last name within a neighborhood. The sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding released offenders. The coefficients at $t = -1$ are normalized to zero. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

Figure 9: Neighborhood Exposure and Formation of Criminal Groups



Notes: Each panel shows 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 1). The coefficients at $t = -1$ are normalized to zero. Panel A uses the probability of being arrested as the outcome, while Panel B focuses on the probability of being arrested in a group (both multiplied by 1,000). In Panel A, the circled dots represent estimates for neighborhoods where the released offender returns to the same neighborhood he resided at age 18, whereas the squares represent estimates for releasees returning to any other neighborhood. The sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding released offenders. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

Figure 10: Incarceration Conditions



Notes: Panel A 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 1). The circled dots represent the coefficients for neighborhoods treated with released offenders who served more time in prison (top quartile), while the squares show estimates for neighborhoods treated with released offenders who served less time (bottom three quartiles). The treated sample only includes pardoned individuals. Panel B presents estimates corresponding to the interaction of each prison characteristic (displayed on the y-axis) with the difference-in-difference estimator. Each point estimate is rescaled to represent a standard deviation increase in the prison characteristic, with effects relative to the mean. All confidence intervals are at the 95% level, with standard 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)
Treated × Post Pardon = 1	0.173***	0.144***	0.133***
	(0.013)	(0.014)	(0.010)
N. Neighborhoods	2,195	2,195	2,195
Mean Dep. Var.	0.152	0.185	0.148
Observations	24,145	24,145	24,145

Notes: The table shows the difference-in-difference coefficients of the effect of the pardon between treated and control neighborhoods. The unit of observation is a neighborhood-by-month pair. Column 1 uses as outcome 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 releasee. The sample includes all urban neighborhoods that received at least one released offender between 2016 and 2021. The time frame of reported is between September 2021 ($t = -5$) and August 2022 ($t = 6$). Standard errors clustered by neighborhood in parentheses. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

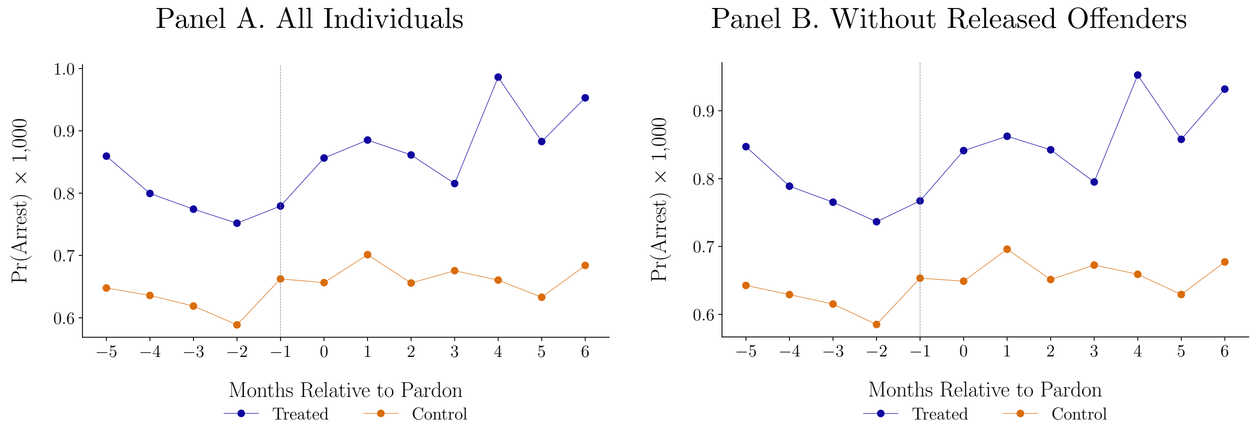
Table 3: Effects of Mass Pardon on Arrests

	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 4. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

A Robustness

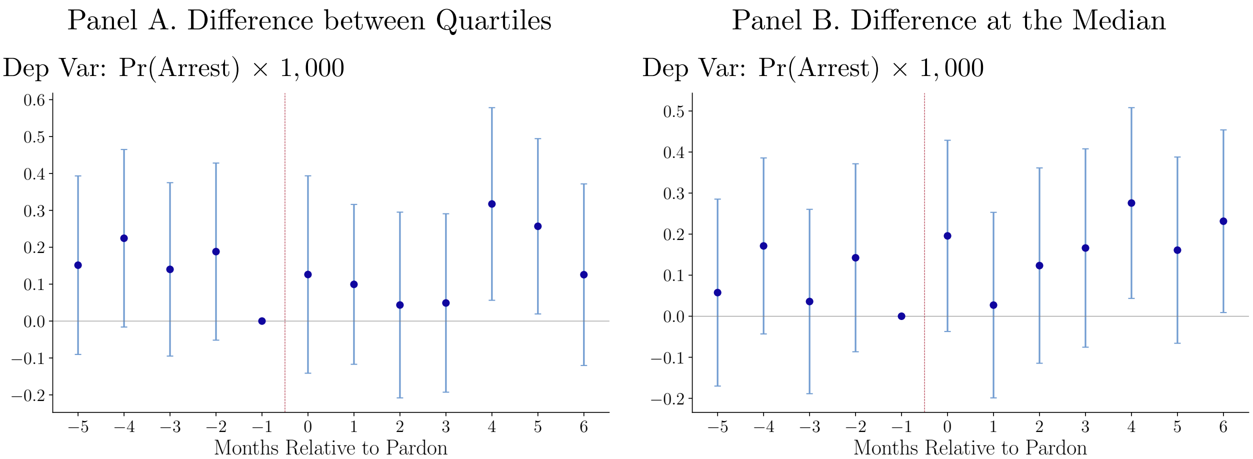
Figure A1: Probability of Arrest - Raw Means



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.

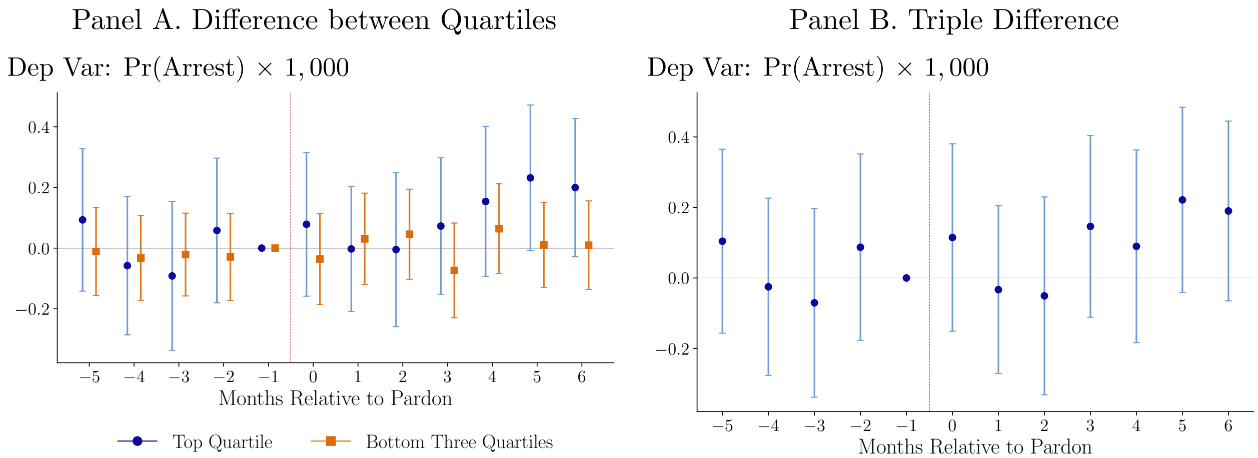
A.1 Released Offender Criminal Experience

Figure A2: Differences in Time Served



Notes: The figure displays the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) by the time served by released offenders, between people in and control neighborhoods, relative to the month before the pardon. Panel A shows the interaction coefficients for inmates on the top quartile with respect to the bottom three quartiles. Panel B shows the same coefficients but divided by the median. The sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding released offenders. The coefficients at $t = -1$ are normalized to zero. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

Figure A3: Releasees' Criminal Experience



Notes: The figure shows heterogeneity effects of the impact of released offenders on the probability of arrest by the number of previous arrests of the released offender. Panel A shows the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) in different regressions depending on the inmates number of previous arrests. The circled dots shows estimates for released offenders on the top quartile on the distribution, and the squares are the estimates for neighborhoods treated with offenders in the bottom quartile. Panel B shows the estimates corresponding to the tripple difference estimator, with the heterogeneity based on whether the number of previous arrests is in the top quartile or not. The sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding those released from prison in the last year. The coefficients at $t = -1$ are normalized to zero. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

A.2 Staggered Timing of Releases

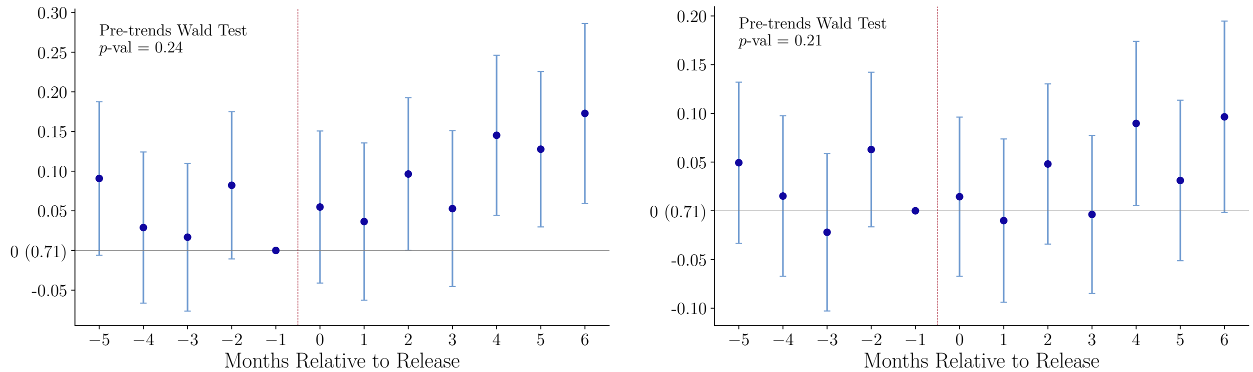
Figure A4: Staggered Offenders Release

Panel A. All Individuals

Dep Var: Pr(Arrest) $\times$ 1,000

Panel B. No Criminal Records

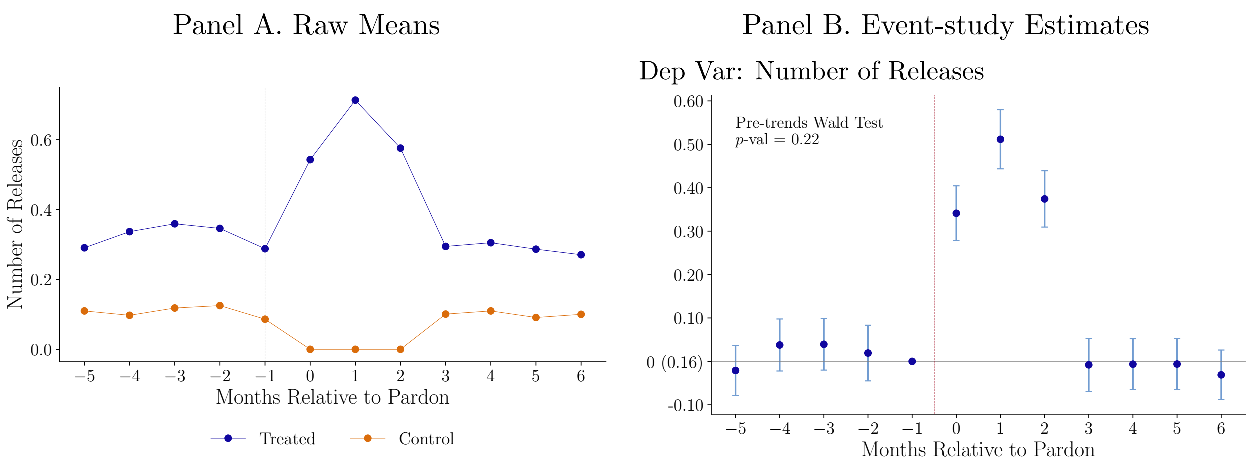
Dep Var: Pr(Arrest) $\times$ 1,000



Notes: The figure displays the regression coefficients for 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 first release after February 2022. Each panel shows estimates from a separate regression based on individuals' arrest history before the pardon. Panel A shows estimates for individuals with no arrest records, while Panel B focuses on people with at least one arrest record. In both panels, the sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding those released from prison in the last year. The coefficients at $t = -1$ are normalized to zero. The mean of the dependent variable at $t = -1$ is shown in parentheses on the y-axis. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

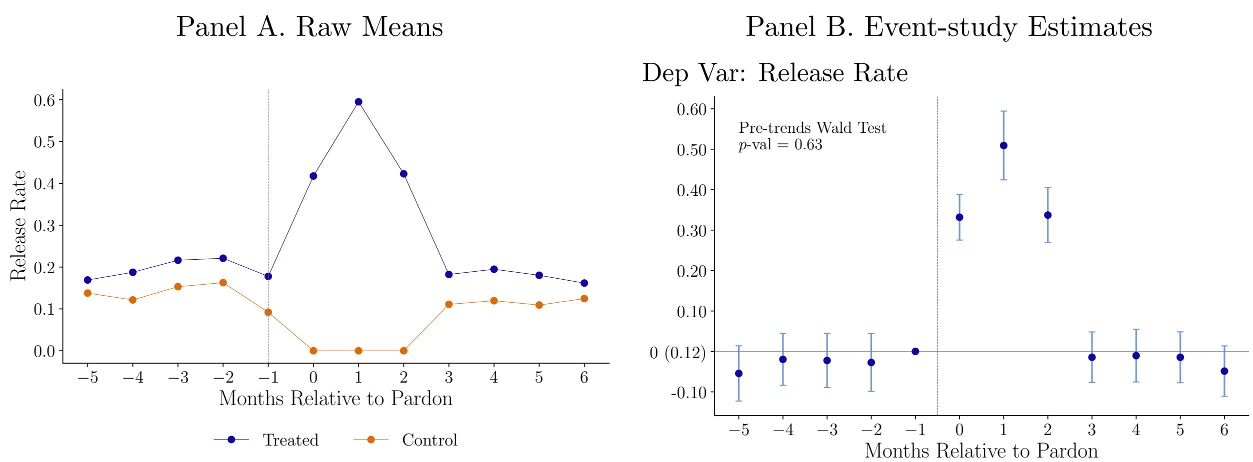
A.3 First-stage Estimates

Figure A5: Mass Pardon and Number of Releases



Notes: The figure shows the effect of the mass pardon in the number of releases. Panel A shows the raw means of the number of releases. Panel B shows the event-study coefficients for the difference in the number of releases between treated and control neighborhoods relative to the month before the pardon. The sample includes all urban neighborhoods that had at least one release between 2016 and 2021.

Figure A6: Mass Pardon and Release Rate



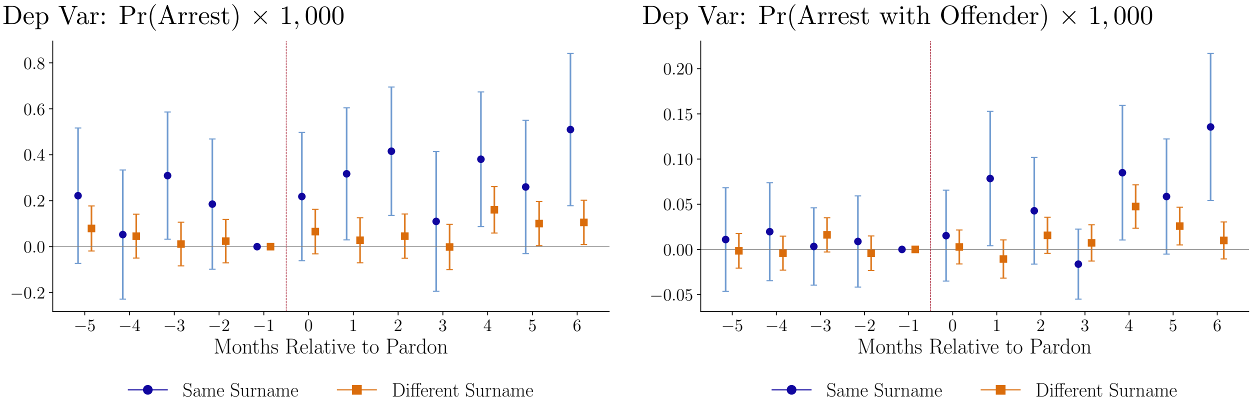
Notes: The figure shows the effect of the mass pardon in the release rate by 1,000 individuals. Panel A shows the raw means of the release rate. Panel B shows the event-study coefficients for the difference in the release rate between treated and control neighborhoods relative to the month before the pardon. The sample includes all urban neighborhoods that had at least one release between 2016 and 2021.

A.4 Family Connections

Figure A7: Controlling by National Frequency of Last Names

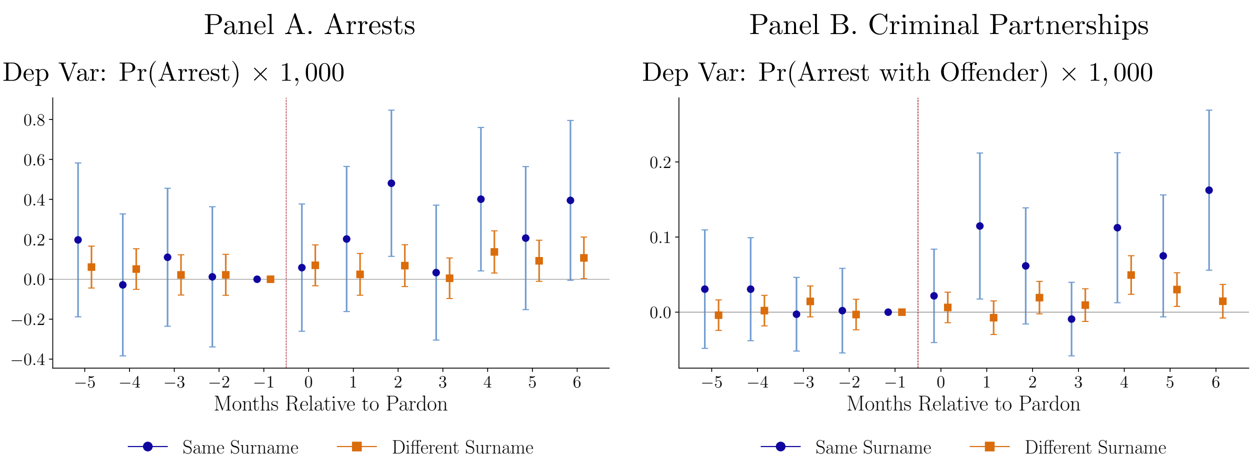
Panel A. Arrests

Panel B. Criminal Partnerships



Notes: Each panel 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 1). The circled dots represent the effects on individuals sharing a surname with a released offender, while the squares display the estimates for those with a different surname. The outcome in Panel A is the probability of arrest (multiplied by 1,000), while in Panel B is the probability of being arrested alongside a released offender (multiplied by 1,000). An arrest is considered to involve a released offender if the release occurred within one year before the arrest. All regressions control for the share of last names in the country. The sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding released offenders. The coefficients at $t = -1$ are normalized to zero. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

Figure A8: Family Connections, without Top 10 Last Names



Notes: Each panel 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 1). The circled dots represent the effects on individuals sharing a surname with a released offender, while the squares display the estimates for those with a different surname. The outcome in Panel A is the probability of arrest (multiplied by 1,000), while in Panel B is the probability of being arrested alongside a released offender (multiplied by 1,000). An arrest is considered to involve a released offender if the release occurred within one year before the arrest. All regressions control for the share of last names in the country. The sample includes all men between 18 and 40 years old living in urban neighborhoods, excluding released offenders and individuals with the 10 most common last names. The coefficients at $t = -1$ are normalized to zero. The bars represent the 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

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 * $p < 0.1$.

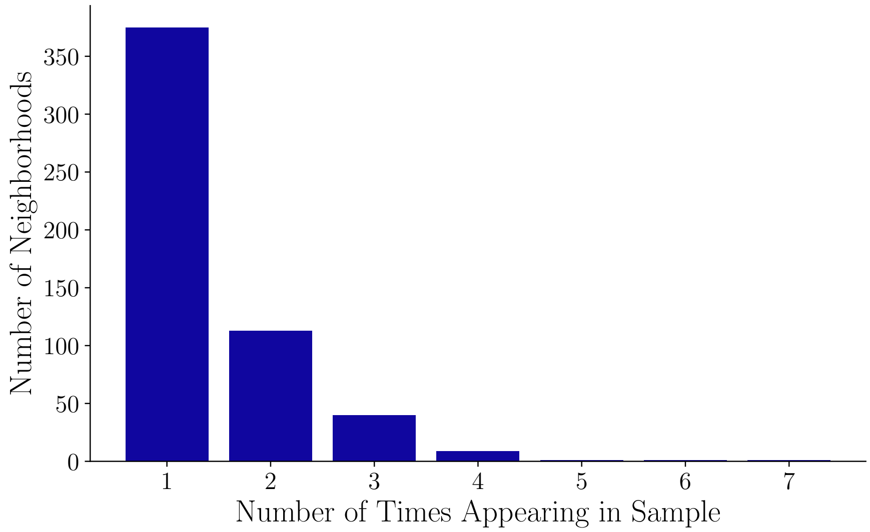
Table B4: Effects of Pardon on Probability of Arrest

	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 * $p < 0.1$.

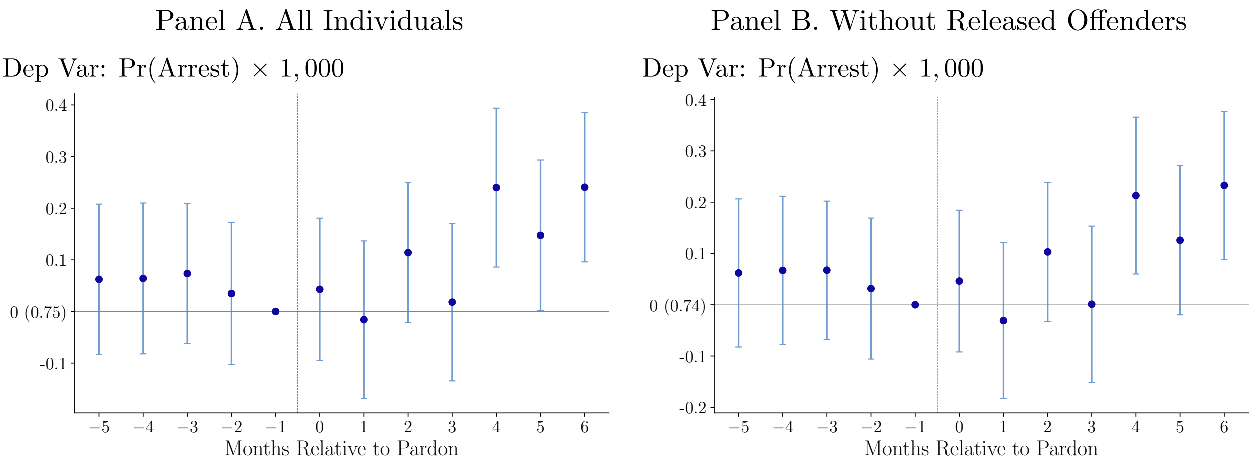
B.3 Figures

Figure B1: Number of Repeated Controls



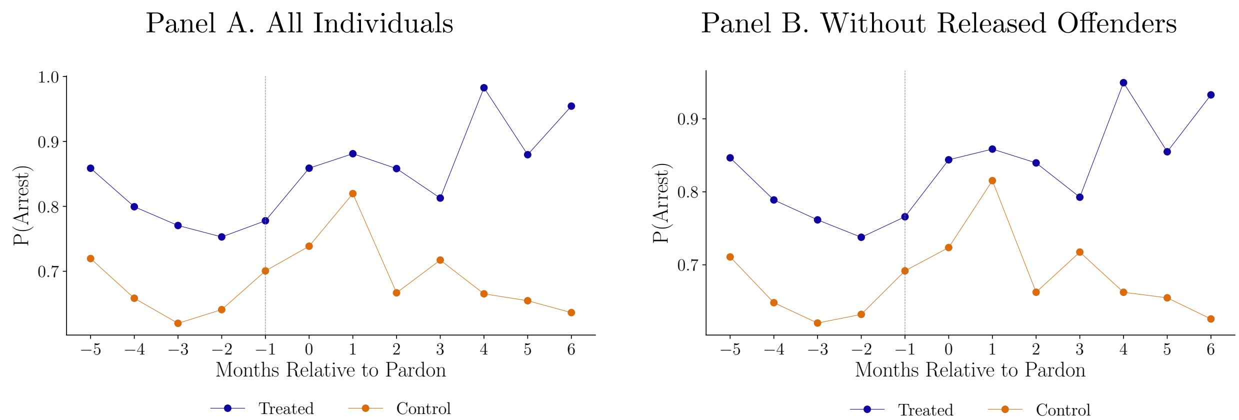
Notes: The figure shows a histogram for the number of times a matched control neighborhood appears in the final sample. There are 540 unique control neighborhoods plotted, with the average neighborhood appearing 1.4 times and the median appearing 1 time.

Figure B2: Effects of Mass Pardon on Arrests



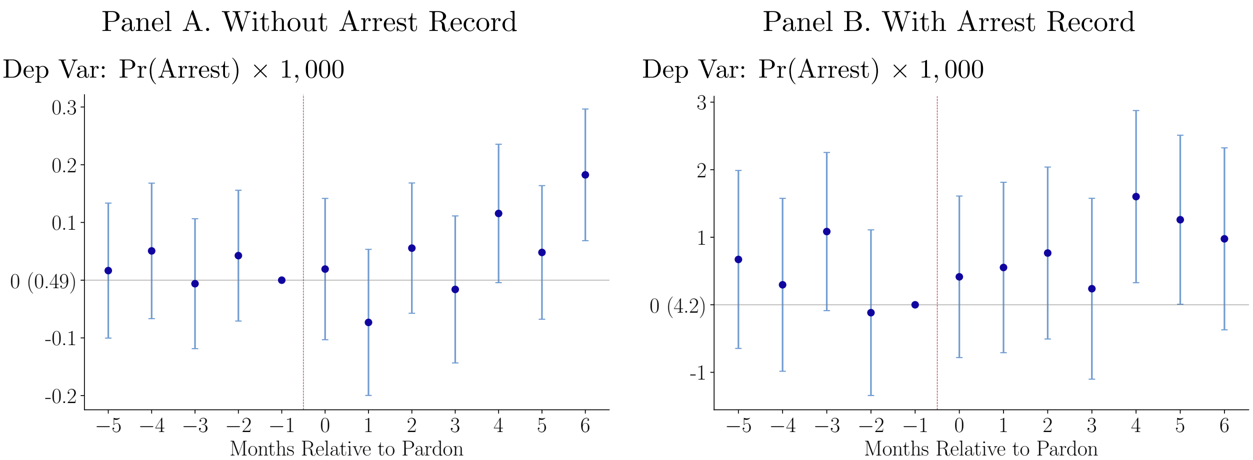
Notes: The figure displays the regression coefficients for the difference in the probability of arrest between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_j from equation 1. The coefficients at $t = -1$ are normalized to zero. On the y-axis, 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.

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.

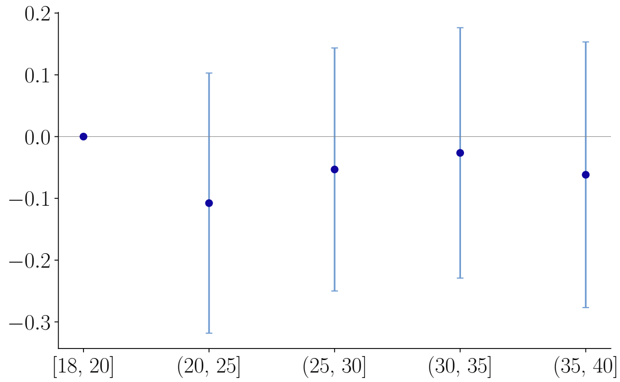
Figure B4: Effects by Residents' Criminal Records



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 1. 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.

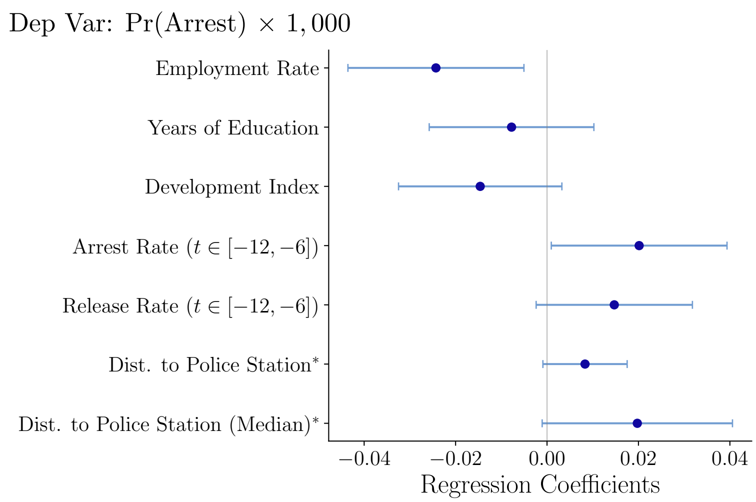
C Heterogeneity Results

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 1. 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

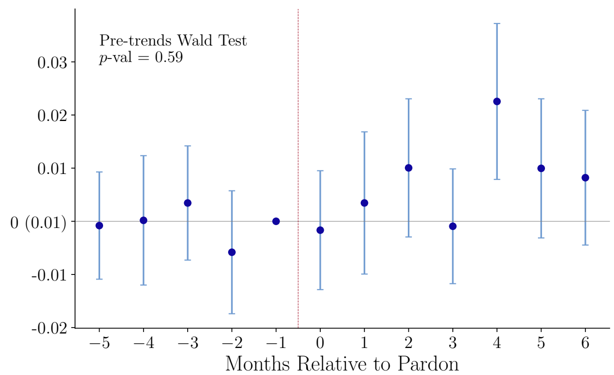


Notes: The figure displays the heterogeneity coefficients for the difference-in-difference estimation of the effect of the pardon. Each coefficient is re-weighted so it is expressed in standard deviations. The bars correspond to the 95 percent confidence interval with standard errors clustered at the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

Figure C3: Criminal Partnerships by Resident's Arrest Records

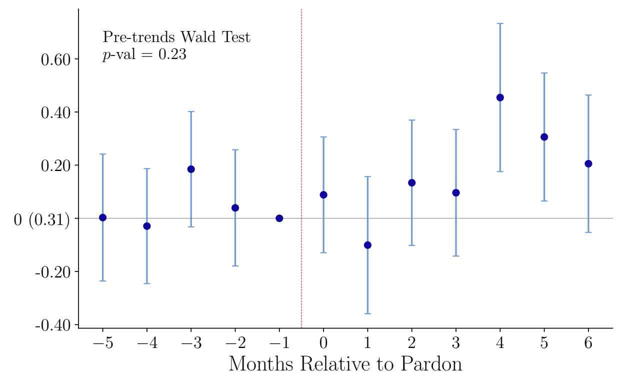
Panel A. Without Arrest Record

Dep Var: Arrested with Released Offender



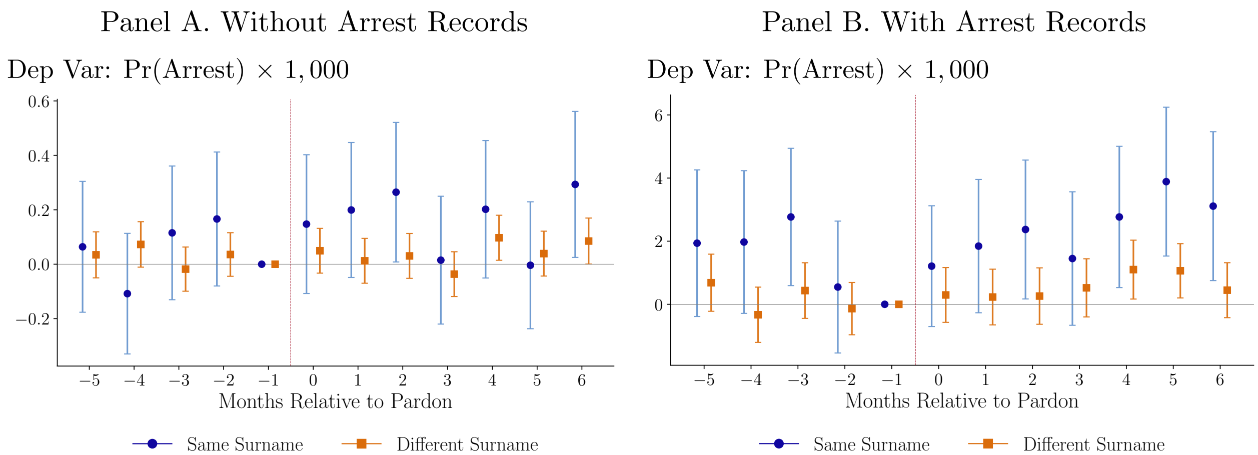
Panel B. With Arrest Record

Dep Var: Arrested with Released Offender



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 1. 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.

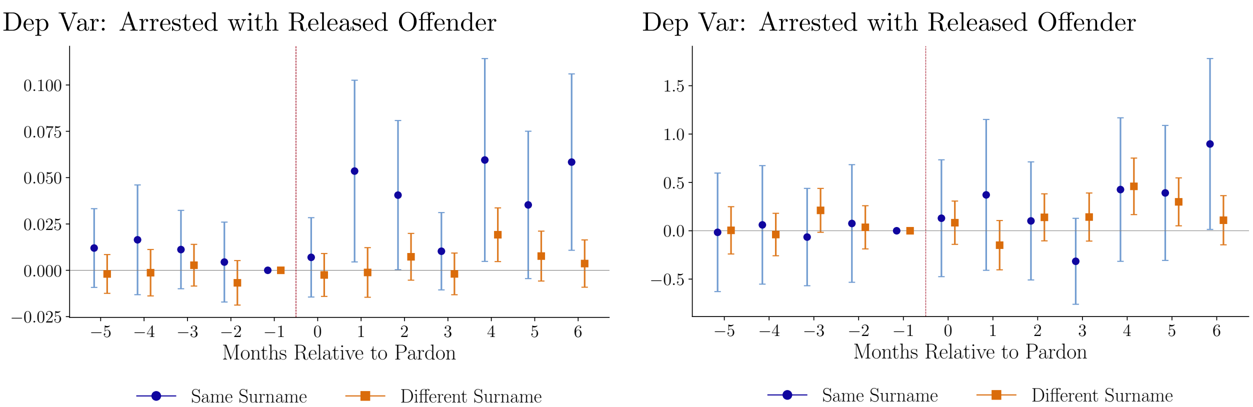
Figure C4: Effects on Arrests by Criminal Records and Family Networks



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

Panel A. Without Arrest Records | Panel B. With Arrest 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 1. 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.