The Spillover Effects of Prisoner Releases: Evidence from Ecuador

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UBC

Job Market Paper

This version: November 8, 2024

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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. Thus, focusing only on prior criminals misses nearly 50% of the effect.

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. (2018), 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 enhances our understanding of prisoner reentry and its relationship with neighborhood crime. While prior studies have largely focused on recidivism rates among released offenders (Billings and Schnepel, 2022; Kirk, 2015) or on correlations between release numbers and crime rates (Buonanno and Raphael, 2013; Roodman, 2020; Hipp and Yates, 2009; Clear et al., 2003; Raphael and Stoll, 2004), this paper takes a more granular approach. Using individual-level data, it distinguishes between recidivism and new offenses as separate factors influencing crime. Furthermore, it provides the first causal estimates of the proportion of new crimes arising from interactions with recently released individuals.

Second, this paper adds to our understanding of criminal peer effects by examining how criminal behavior may spread from released offenders to their neighbors. Prior research has focused on the transmission of criminal skills among those with a history of crime, whether through prison interactions or post-release networks (Billings and Schnepel, 2022; Damm and Gorinas, 2020; Stevenson, 2017; Bayer et al., 2009). Studies examining the transmission of criminal behavior to non-offenders have primarily focused on youth, showing that minors exposed to disadvantaged or crime-prone peers—such as classmates whose parents have criminal backgrounds—are at greater risk of engaging in criminal activity as adults (Billings and Hoekstra, 2024; Billings et al., 2019, 2014). This paper extends the literature by demonstrating that criminal peers can influence individuals without prior criminal involvement.

Third, this paper contributes to the body of work on the spillover effects of incarceration. Existing studies have largely examined the impact of removing harmful peers through incarceration, with some studies showing that the incarceration of marginal offenders can benefit families (Norris et al., 2021; Arteaga, 2021). This research complements that literature by exploring the opposite effect: what happens when offenders return to their communities.

Finally, this paper contributes to the literature on prisons and crime in Latin America. Previous studies have highlighted that imprisonment in the region often increases the likelihood of recidivism due to challenges such as limited access to rehabilitation programs, poor prison conditions, overcrowding, and the presence of gangs and violence (Escobar et al., 2023; Munyo and Rossi, 2015; Di Tella and Schargrodsky, 2013). These factors hinder former inmates' successful reintegration into society (Blattman et al., 2024; Sviatschi, 2022;

Tobón, 2022; Carvalho and Soares, 2016). This paper expands on this literature by providing evidence that prison experiences can impact not only former inmates but also individuals without direct criminal histories. Additionally, it highlights the potential benefits of social rehabilitation programs in mitigating these spillover effects.

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 prison overcrowding rate was 29%, higher than the Latin American average. The system is also marked by gang infiltration and high levels of violence. 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).

To reduce 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

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

monthly evolution of the number of released offenders between January 2021 and December 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 use the extensive margin variation generated by the final pardon. I exclude the first set of pardons from the analysis, as these cases involve individuals who were not representative of Ecuador's typical criminal population. Most individuals pardoned in the initial wave were imprisoned for traffic-related misdemeanors, often due to driving under the influence of alcohol or involvement in fatal accidents, with prison terms generally lasting less than a month. In contrast, the third decree primarily affected individuals convicted of Ecuador's most prevalent crimes –robbery and theft– which together represented over a third of the incarcerated population in 2023. Since this paper focuses on the spread of criminal behavior rather than regulatory violations like traffic misdemeanors, I examine the effects of the final decree, which released individuals convicted of more serious offenses, unlike those pardoned under the first two decrees.

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 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]

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

[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

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.

RAG's accuracy is close to 100%.

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 choose 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 had not received a released offender within five years prior to the pardon. This approach allows me to focus on neighborhoods that had at least one returning offender in recent years, thus enhancing the ex-ante comparability between the treatment and control groups. 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:

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

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. $\mathit{Released\ Offender}_n$ is an indicator equal to one if neighborhood n was treated (i.e., received an offender after 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

In this subsection, I present the baseline estimates of the effect of released offenders on the probability of arrest of their neighbors. First, I estimate Equation 1 on the sample of all individuals residing in neighborhood n at month t, which includes both released offenders and other neighborhood residents. Panel A of Figure 4 displays the event-study coefficients from this regression, and Table 3 summarizes these effects using the difference-in-difference version of the event study.

Panel A of Figure 4 indicates that the presence of released offenders increases the probability of arrest for residents in the neighborhoods they rejoin. On average, the probability of arrest among individuals in treated neighborhoods rose by 0.006 percentage points (equivalent to 8.2% of the mean) compared to residents in control neighborhoods. This effect becomes statistically significant starting in the fourth month following the pardon. Between the fourth and sixth months after the pardon, the average increase in arrest probability reaches 0.016 percentage points (22% of the mean).

This initial result contains two distinct effects. First, it captures the recidivism rate of former offenders. Since recently released individuals have a higher likelihood of reoffending, their inclusion in the sample mechanically raises the probability of arrest in treated neighborhoods relative to control neighborhoods. Second, it may contain the spillover effects generated by the offenders on the broader neighborhood. To isolate these spillover effects, I exclude recently released offenders from the sample. Panel B of Figure 4 displays the estimates from this restricted sample.

Panel B of Figure 4 indicates that released offenders create criminal spillovers in their neighborhoods. On average, the pardon increased the probability of arrest among individuals in treated neighborhoods by 0.005 percentage points (equivalent to 6.8% of the mean) compared to residents in control neighborhoods. Like the full-sample analysis, this effect is statistically significant only between the fourth and sixth months after the pardon. During this period, the average probability of arrest rose by 0.014 percentage points (19% of the mean).

Notably, there is no evidence of violations of the assumption of parallel trends. On both 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. 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.

To obtain a more generalizable estimate, I calculate the elasticity of arrests with respect to the number of released offenders. Considering that, on average, each neighborhood has 1,060 individuals, the monthly elasticity is 0.18. This estimate indicates that one additional released offender leads to approximately 0.85 new monthly arrests, not accounting for recidivism. When released offenders are included in the analysis, the elasticity increases to 0.19 and the implied number of arrests to 0.98.

To contextualize these estimates, I first compare them to Ecuador's recidivism rate. The spillover effects align closely with the rearrest rate in Ecuador. The probability of being rearrested within six months of release is 10%, while the estimated increase in the probability of arrest due to released offenders is 6.8%. This suggests that the estimated spillover effects are within a similar range to typical rearrest rates.

Next, I compare these findings to related empirical studies examining the link between offender releases and crime rates. Existing research in this area primarily offers correlational evidence on the relationship between the number of released offenders and aggregate crime rates. For instance, Buonanno and Raphael (2013) examines the impact of released offenders on provincial crime rates following the 2006 Italian pardon, finding an estimate roughly three times larger than the one documented in my paper. My findings are more closely aligned with the studies by Hipp and Yates (2009) and Vieraitis et al. (2007), who report state-level correlations in the U.S., finding an elasticity of releases on robbery rates of 0.18 and 0.16, respectively.

4.4 Who are the affected individuals?

The results indicate that released offenders increase the criminal participation of their neighbors. In this subsection, I examine neighborhood characteristics that may make certain areas more susceptible to these effects. I demonstrate that released offenders influence individuals with prior criminal experience and those without. Additionally, I find that neighborhoods with higher socio-economic status experience a minor increase in arrest rates compared to lower socio-economic areas.

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 people. Previous research has shown that offenders can strengthen their criminal skills through exposure to other offenders—a concept known as reinforcing peer effects (Damm and Gorinas, 2020; Stevenson, 2017). Evidence of introductory peer effects (offenders influencing 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).

Understanding whether released offenders influence individuals without prior criminal experience is essential, as this group makes up most of the population—individuals with criminal records only represent 6.5% of the sample. If released offenders do impact those without prior criminal history, it could result in larger spillover effects due to the greater share of the population affected. Moreover, creating new offenders increases the number of individuals engaged in criminal activity, which could contribute to higher crime rates in the long term.

To decompose the effects, I estimate Equation 1 using a stratified regression based on individuals' arrest history before the pardon. Panel A of Figure 5 presents the event-study coefficients for individuals without prior criminal records, while Panel B shows the estimates for those with an arrest history. The results indicate that released offenders increase the probability of arrests in both groups. For individuals without a criminal record, the likelihood of arrest in treated neighborhoods increased by 0.002 percentage points (equivalent to a 4.6% increase relative to the mean) after the pardon compared to similar individuals in control neighborhoods. The corresponding increase for individuals with a prior arrest record is 0.046 percentage points, representing a 10.9% rise relative to the mean.

These coefficients suggest that 40% of the overall increase in arrests arises from individuals with no prior criminal record. This finding contrasts with previous studies focusing exclusively on interactions among criminally active neighborhood peers (Billings and Schnepel, 2022; Kirk, 2015). My results indicate that focusing solely on individuals with prior criminal histories overlooks more than half of the effect.

Neighborhood Characteristics: The characteristics of neighborhoods where released offenders return may influence the magnitude of the spillover effects. Neighborhoods with a higher share of educated individuals, a greater proportion of the population employed, and better access to public services may offer their residents more economic opportunities in the legal sector, including former offenders. As a result, individuals in these neighborhoods may be less susceptible to the influence of released offenders than those in poorer communities.

To estimate these effects, I use data from the 2022 Ecuadorian population census, which provides household and individual-level information for all neighborhoods. I gather information about access to public services (e.g., public water, electricity, garbage collection, and sewage), dwelling conditions (e.g., quality of floors and ceilings), and individual characteristics (e.g., age, gender, education level, employment status, and race). I then interact each neighborhood characteristic with the difference-in-difference estimator to assess the effects of the pardon.

Figure C2 shows the estimates of the heterogeneity analysis based on the neighborhood characteristics. Overall, the figure indicates that the spillover effects created by released offenders are lower in more developed neighborhoods. Specifically, these effects are statistically significant in areas where a higher proportion of the population is employed. In contrast, crime-prone neighborhoods, which are characterized by higher pre-existing crime rates, experience larger spillover effects. These results suggest that the structural composition of neighborhoods may play a role in preventing the spread of crime.

4.5 Do spillovers differ between pardoned and non-pardoned offenders?

One characteristic of the empirical strategy is that treated neighborhoods may receive pardoned offenders, inmates released after serving their sentences, or a combination of both. However, the spillover effects from each type of releasee may differ, as pardoned individuals could behave differently from non-pardoned inmates after their release. The pardon could alter an inmate's perception of the severity of punishment, leading them to revise their beliefs about crime penalties and possibly think that future pardons could occur. Additionally, since their time in prison was shorter than initially intended, this may disrupt their rehabilitation process. Consequently, pardoned individuals may be more likely to reoffend or influence others compared to non-pardoned releasees, which could explain the increased arrest rate in the main results.

I split the treated sample into pardoned and non-pardoned releasees to test this hypothesis. I exclude neighborhoods that received both types of offenders. Figure 6 displays the event study coefficients for these regressions. The plot reveals no statistically significant difference between the effects of pardoned and non-pardoned releasees. The p-value of the Wald statistic, testing for the joint equality of post-treatment coefficients (where $k \geq 0$), is

0.85, indicating that we cannot reject the null hypothesis that the coefficients are equal.

These findings indicate that the spillover effects are not limited to the characteristics of pardoned individuals. Instead, they demonstrate the broader impact that released offenders have on neighborhood arrests, regardless of the type of prison release. This conclusion is further supported by the infrequent use of pardons in the country. Pardons are often a political tool rather than a mechanism to alleviate prison overcrowding. In any given electoral cycle, fewer than five inmates are typically pardoned, making it unlikely that pardons significantly alter releasees' beliefs.

4.6 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. 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.

Matched 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 show that the estimates are similar when using more comparable neighborhoods.

Staggered arrival of inmates: A potential concern with the design is the staggered release of offenders following the pardon. As outlined in the institutional background, inmates had to undergo a specific procedure to qualify for the pardon, meaning their actual release may have occurred after February 2022. As shown in Figure 1, most releases took place in March 2022, the month following the pardon. To address this, I replicate the estimation from Equation 1 using the number of months since an offender's release as the time variable. Figure A.2 demonstrates that the estimates remain similar in magnitude to those in the main specification.

Differential Path of Releases: Another concern is that treated and control neighborhoods may follow different release trajectories even after the pardon ends. Specifically, treated neighborhoods could have more releases than control neighborhoods after the treatment. If this is the case, the effects could be upward-biased, as treated neighborhoods would have received more releases than the control group for reasons beyond the pardon. Figure 3 and Appendix A.3 demonstrate that the number of releases and the release rate only differ during the pardon period. The release rate between treated and control neighborhoods was identical for months when no pardoned individuals were released ($t < 0$ and $t > 3$).

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 in 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 Criminal Peer Effects within Neighborhoods

Two alternative explanations can explain the increased probability of arrest following a release. First, released offenders may influence the criminal behavior of their direct 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 (Hipp and Yates, 2009).

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. Next, I explore how family connections can facilitate the spread of criminal behavior. Finally, I provide suggestive evidence of the spread of criminal groups outside prison.

5.1 Formation of Criminal Partnerships

The most direct way to test whether released offenders directly influence individuals in their neighborhoods is to observe whether they commit crimes together. If a former offender impacts their neighbors' criminal behavior, the likelihood of these individuals forming a criminal partnership should rise. To test this hypothesis, I estimate Equation 1, using the probability of being arrested alongside a recently released offender as the outcome variable. I define 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 exclude releasees from the estimation sample.

Figure 7 illustrates that the pardon led to an increase in criminal partnerships involving former inmates. On average, the probability of being arrested with a released offender increases 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. Like 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.

As with the main estimates, I further explore the composition of the effects between individuals with criminal records and first-time offenders. Understanding whether criminal partners have prior records reveals information about the direction of the peer effects. 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.

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 0.008 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 Networks

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 family members. Research in the economics of crime has shown that criminal behavior spreads through peer interactions (Stevenson, 2017; Corno, 2017). Building on this work, I provide evidence that the reentry of offenders into society increases the criminal involvement of their relatives inside the neighborhood.

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. This implies that the typical person has approximately 30 family members living in the same neighborhood.

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. 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 releasee 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 A7 displays the estimates after excluding individuals with the most common last names. Additionally, Figure A8 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 the Spread of Gangs

In this subsection, I present evidence of the transmission of criminal capital from released offenders to individuals in the neighborhoods they rejoin beyond their observable network. First, I show that the main effects come from offenders returning to the neighborhoods where they grew up. Then, I provide evidence of the spread of gangs into the 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. A similar phenomenon has been documented in El Salvador (Sviatschi, 2022) and Brazil (Phillips, 2023; The Economist, 2024).

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

In this section, I present evidence on the criminogenic role of incarceration on neighborhood crime through contact with released offenders. Further, I present suggestive evidence that access to rehabilitation programs within prisons can help to mitigate the negative spillover effects.

One of the main characteristics differentiating the offenders used as a treatment in this paper is that they had been recently released from prison. 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.

To evaluate whether incarceration influences the spillover effects, I conduct a heterogeneity analysis focusing solely on the time served by pardoned individuals. I concentrate the analysis on neighborhoods that received only pardoned offenders because it allows me to isolate the impact of incarceration from the individual characteristics of the offenders. An analysis that includes all released offenders could mix the effects of incarceration with the offenders' inherent criminal abilities, as individuals with more criminal skills often receive longer sentences. By focusing exclusively on pardoned offenders, I can compare neighborhoods with felons who were convicted of similar crimes, making their pre-prison criminal skill levels comparable. The time served by these offenders depends solely on the timing of their arrests.

Panel A of Figure 10 reveals that neighborhoods with offenders who served longer prison sentences experienced a more pronounced increase in the probability of arrest after the pardon. In these neighborhoods, the likelihood of arrest of individuals in the treated group rose by an average of 0.005 percentage points (equivalent to a 7.9% increase relative to the mean) in comparison to control areas. In contrast, no statistically significant effect is observed for individuals in neighborhoods with offenders who served shorter sentences.

To further support these findings, Figure A9 presents a similar analysis, using the number of prior arrests as an alternative measure of criminality. If the previous analysis confounds offenders' inherent criminal skills, then we would expect that releasees with a higher number of previous arrests would show similarly amplified effects. However, Figure A9 indicates that the effects do not vary based on the number of arrests before their incarceration. This supports the interpretation that time served in prison is criminogenic.

Prison Characteristics: Next, I examine specific characteristics of the prisons from which the released offenders came. I focus on three prison-level attributes: overcrowding, one-year recidivism, and participation in rehabilitation programs. I calculate 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 implied by 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 results in this section suggest that incarceration experience may play a role in explaining the spillover effects from released offenders. The results present 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, with imprisonment being a pilar of such strategies (The Economist, 2024). My findings reveal that incarceration without a focus on rehabilitation can backfire in the fight against crime, by providing the first causal estimates of criminal spillover from released offenders to their neighborhood peers.

The analysis reveals that each additional released offender increases the number of monthly arrests by 0.85, with 40% of this effect stemming from individuals newly drawn into criminal activity. However, this estimate likely represents a lower bound, as minors are excluded from the analysis. This group likely is more vulnerable and prone to be recruited easily by criminal groups (Sviatschi, 2022).

Additionally, these findings are relevant to other countries besides Ecuador. Many Latin American countries share similar crime rates, incarceration policies, and socioeconomic conditions, suggesting that these results could generalize across the region. Additionally, the study provides insights into why prison overcrowding resurges following mass releases, a phenomenon observed in other countries that have implemented similar policies, such as Italy (Buonanno and Raphael, 2013).

Finally, the findings highlight a key policy implication: access to prison rehabilitation programs may improve reentry outcomes. The analysis shows that spillover effects are lower for offenders released from prisons that provide educational and job-training programs, suggesting that expanding such programs within prisons could help mitigate the adverse effects of incarceration.

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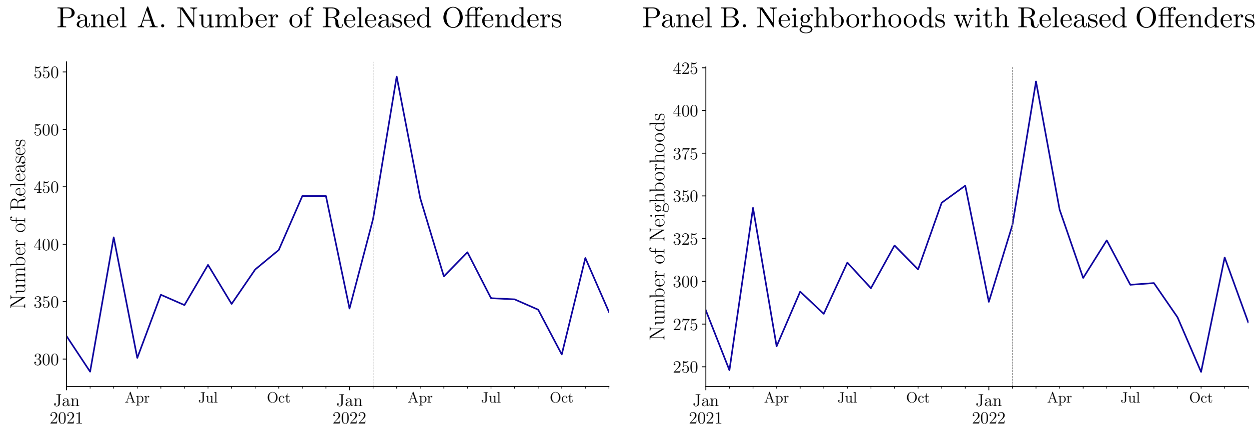
Figures

Figure 1: Monthly Prison Releases

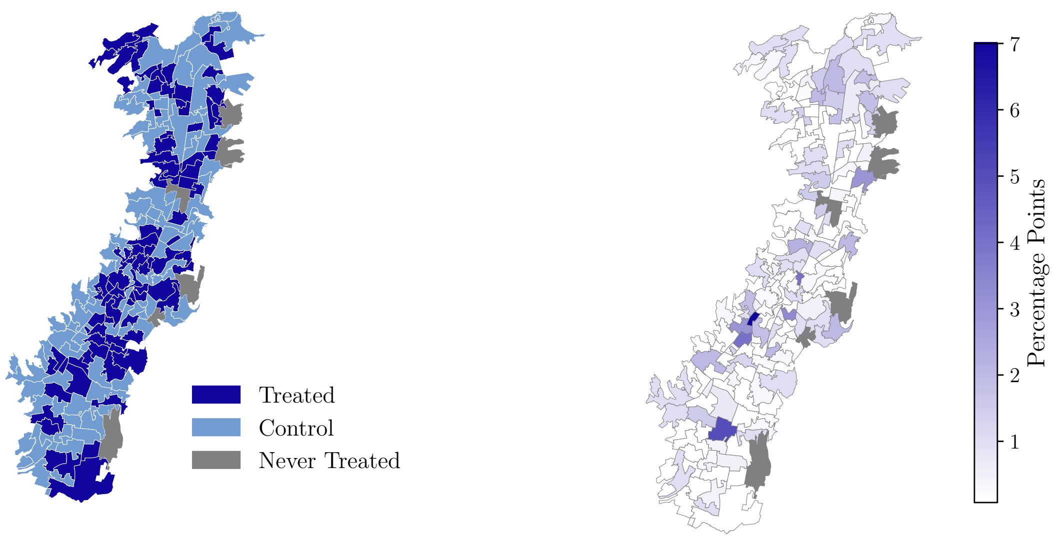


Notes: Panel A shows the monthly evolution of the total number of released offenders from January 2021 to December 2022. Panel B displays the number of neighborhoods that received a released offender over the same period. The vertical dashed lines indicate the date of the mass pardon in February 2022. 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

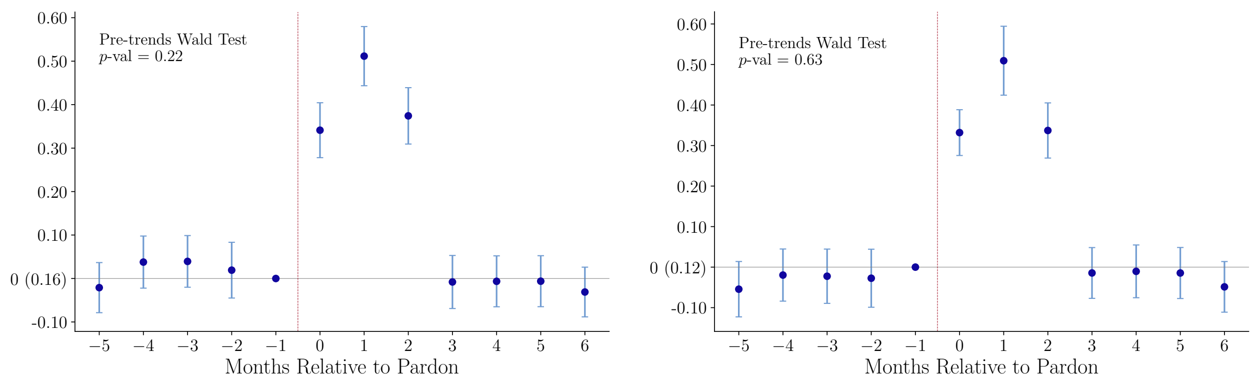
Panel B. Change in Arrests



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.

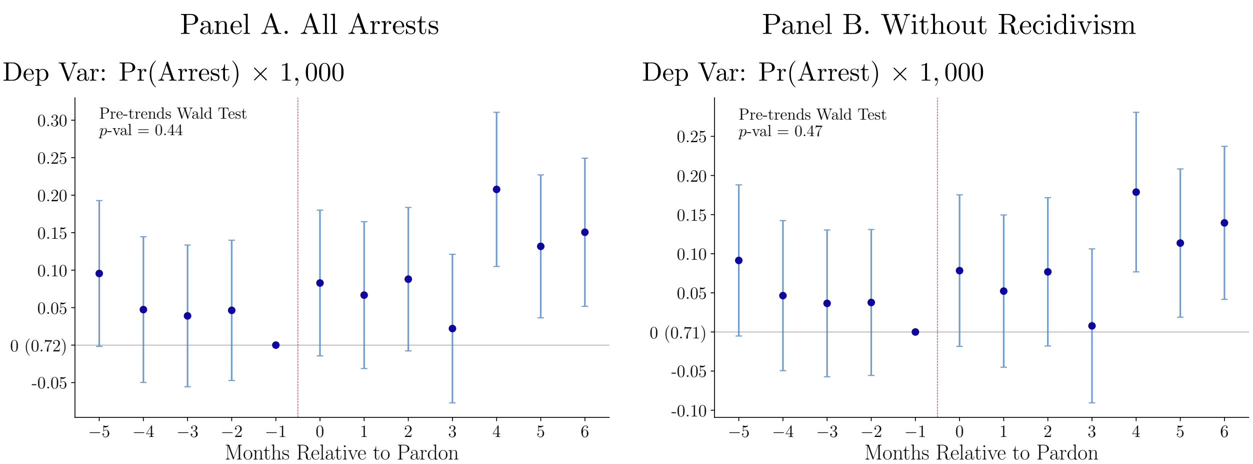
Figure 3: Variation in Released Offenders (First Stage)

Panel A. Number of Releases   Panel B. Release Rate



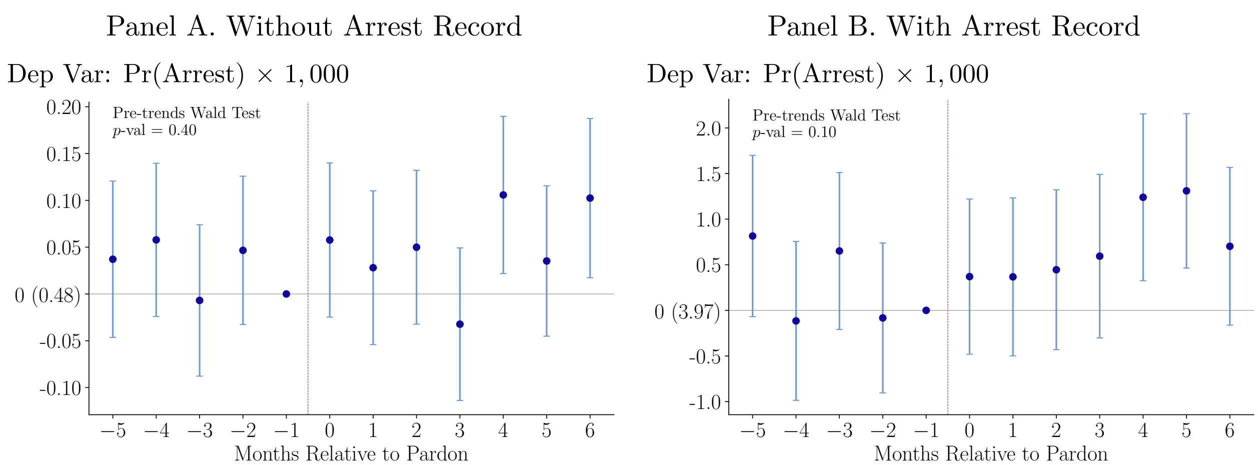
Notes: The figure displays the regression coefficients for the difference in the number of releases (Panel A) and release rate by 1,000 residents (Panel B) between treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). The coefficients at $t = -1$ 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



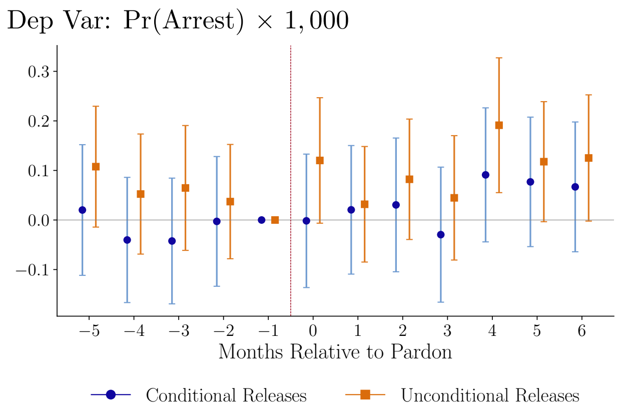
Notes: The figure displays the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) between people living in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). The coefficients at $t = -1$ are normalized 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.

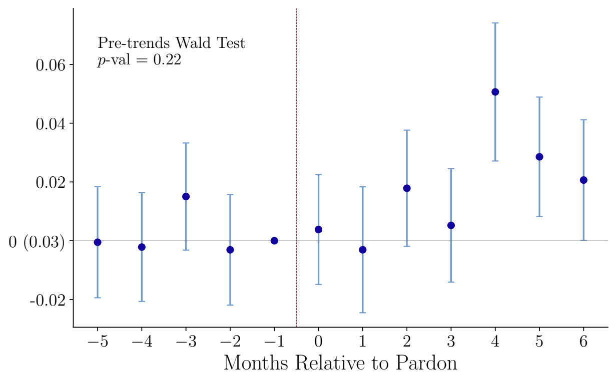
Figure 6: Heterogeneity by Type of Release



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

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

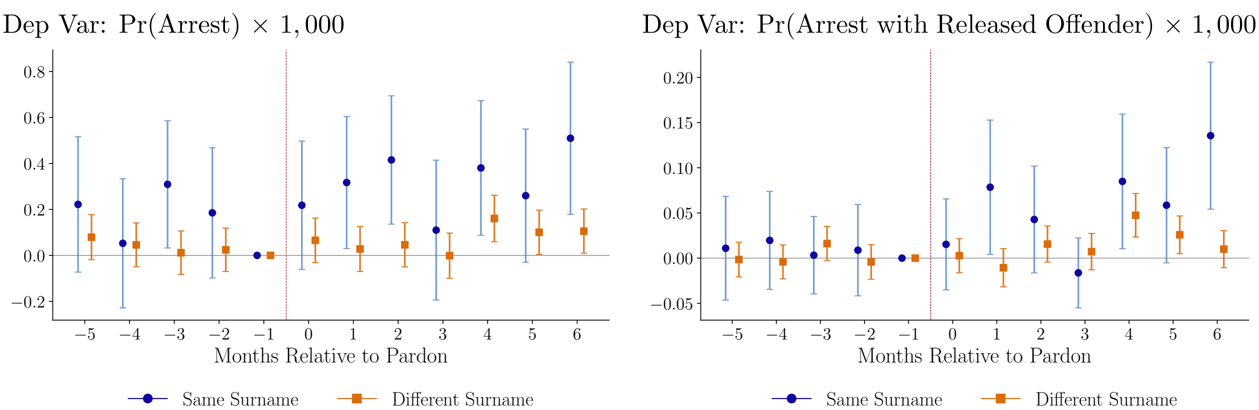


Notes: The figure displays the regression coefficients for the difference in the probability of being arrest alongside a released offender (multiplied by 1,000) between people living in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). An arrest 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

Panel B. Partnerships with Releasees



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.

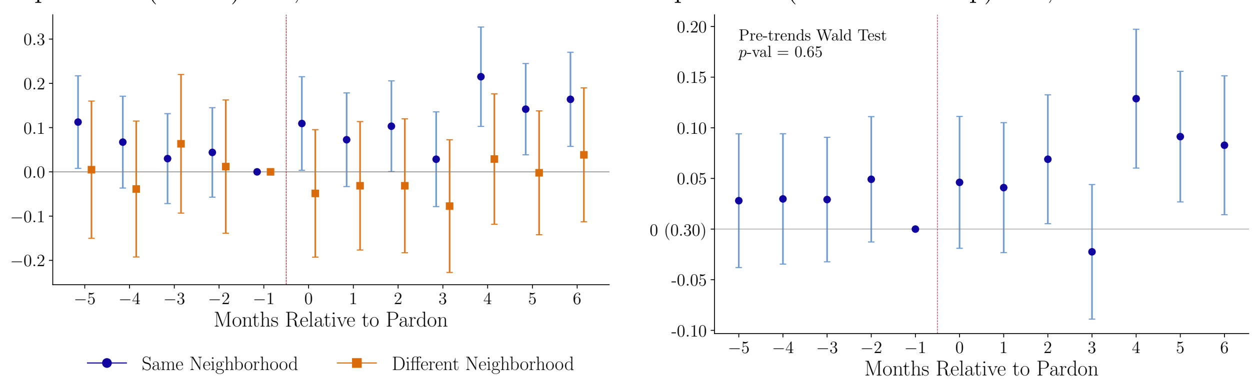
Figure 9: Neighborhood Exposure and Formation of Criminal Groups

Panel A. Neighborhood Attachment

Panel B. Criminal Groups

Dep Var: Pr(Arrest) × 1,000

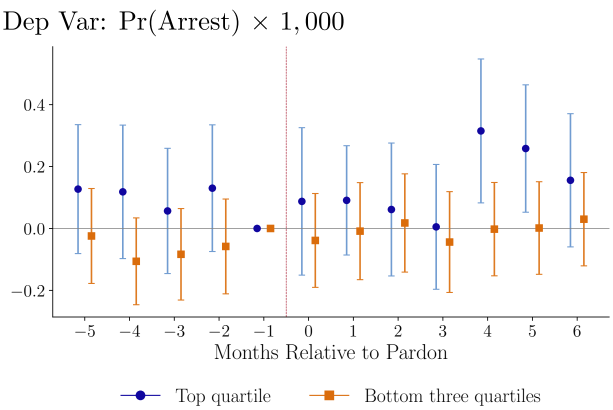
Dep Var: Pr(Arrest in Group) × 1,000



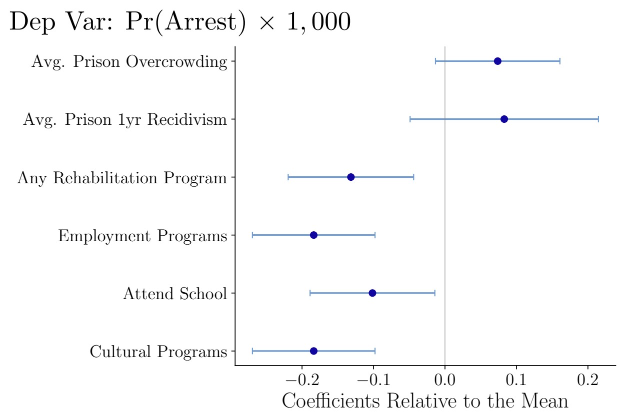
Notes: Each panel shows the regression coefficients for the difference in outcomes between people living in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). The coefficients at $t = -1$ are normalized to zero. Panel A uses the probability of 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

Panel A. Time in Prison



Panel B. Prison Characteristics



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 * $p < 0.1$.

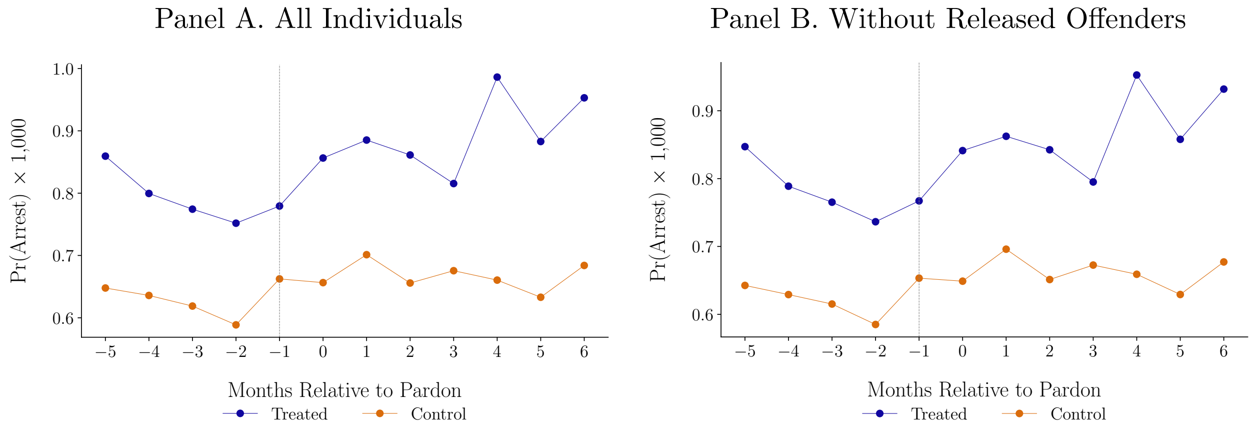
Table 3: Effects of Mass Pardon on Arrests

	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



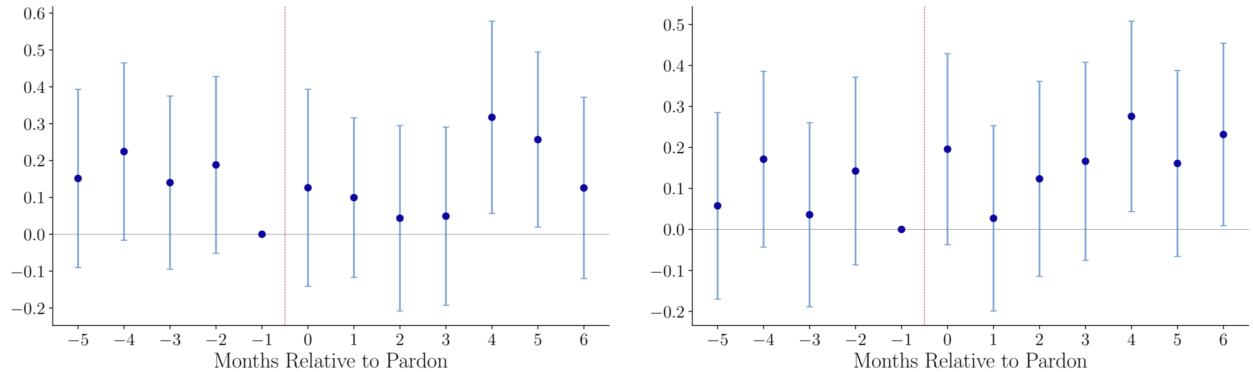
Notes: The figure displays the raw means of the probability of arrest (multiplied by 1,000) for individuals aged 18 to 40 living in treated and control neighborhoods. The data covers the period from September 2021 ($t = -5$) to August 2022 ($t = 6$). The sample includes all neighborhoods that received 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

Panel A. Difference between Quartiles | Panel B. Difference at the Median

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

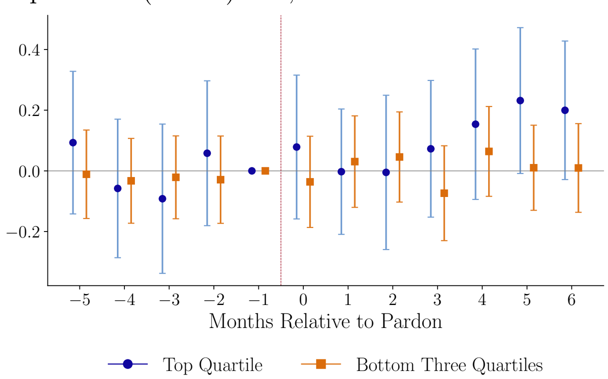


Notes: The figure displays the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) by the time served by released offenders, between people in and control neighborhoods, relative to the month before the pardon. Panel A shows the interaction coefficients for inmates 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

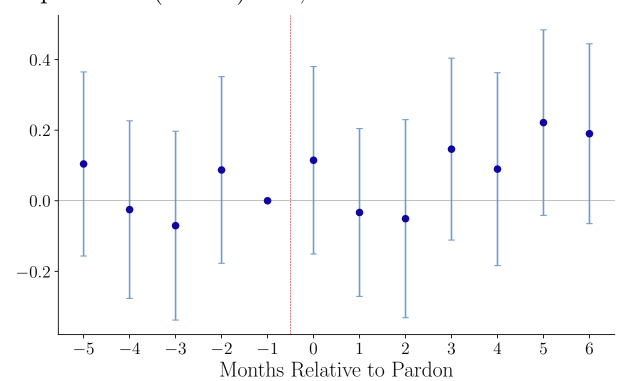
Panel A. Difference between Quartiles

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



Panel B. Triple Difference

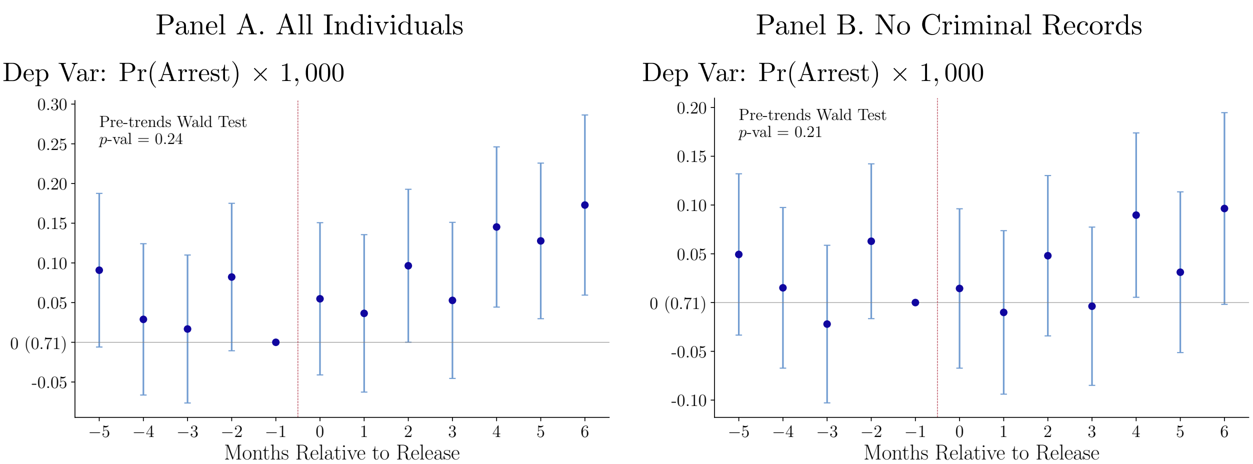
Dep Var: Pr(Arrest) $\times$ 1,000



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

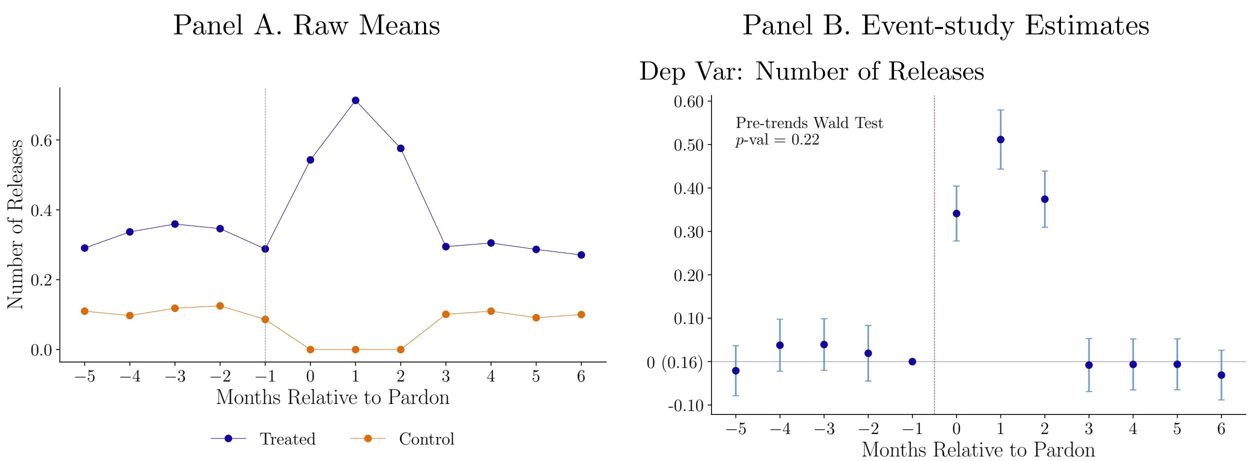
Figure A4: Staggered Offenders Release



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

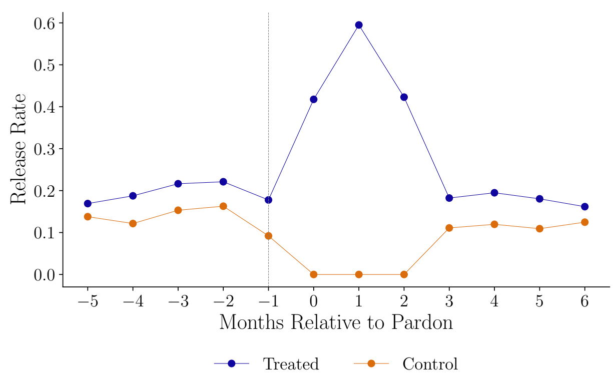
Figure A5: Mass Pardon and Number of Releases



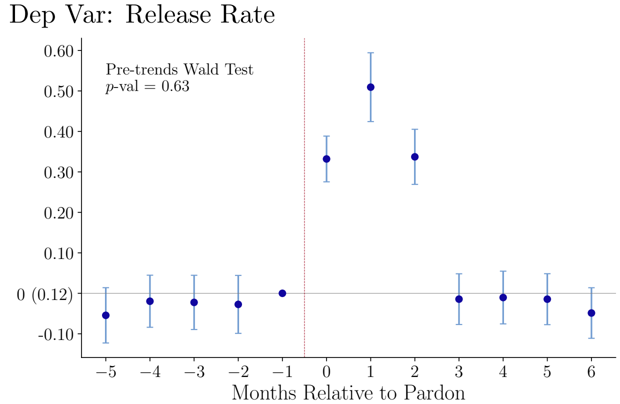
Notes: The figure shows the effect of the mass pardon in the number of releases. Panel A shows the raw means of the number of releases. Panel B shows the event-study coefficients for the difference in the number of releases between treated and control neighborhoods relative to the month before the pardon.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

Figure A6: Mass Pardon and Release Rate

Panel A. Raw Means



Panel B. Event-study Estimates



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

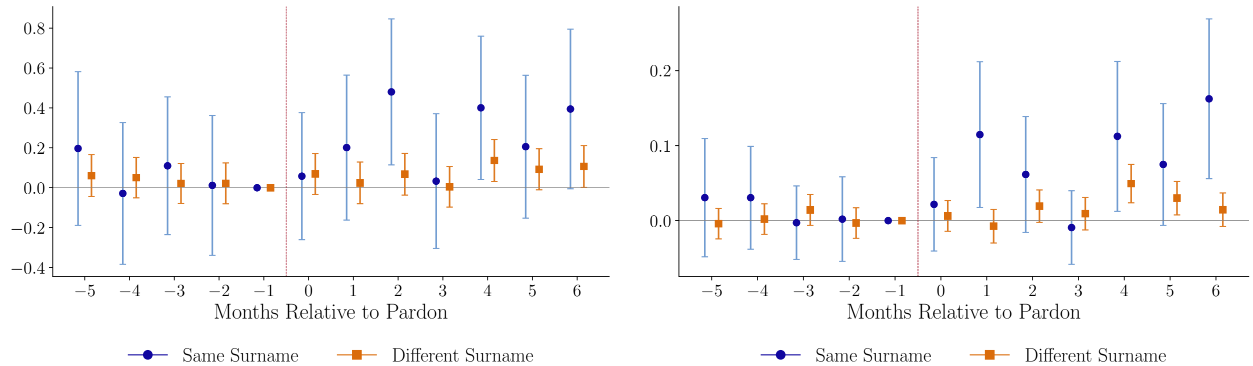
Figure A7: Family Connections, without Top 10 Last Names

Panel A. Arrests

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

Panel B. Criminal Partnerships

Dep Var: Pr(Arrest with 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 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.

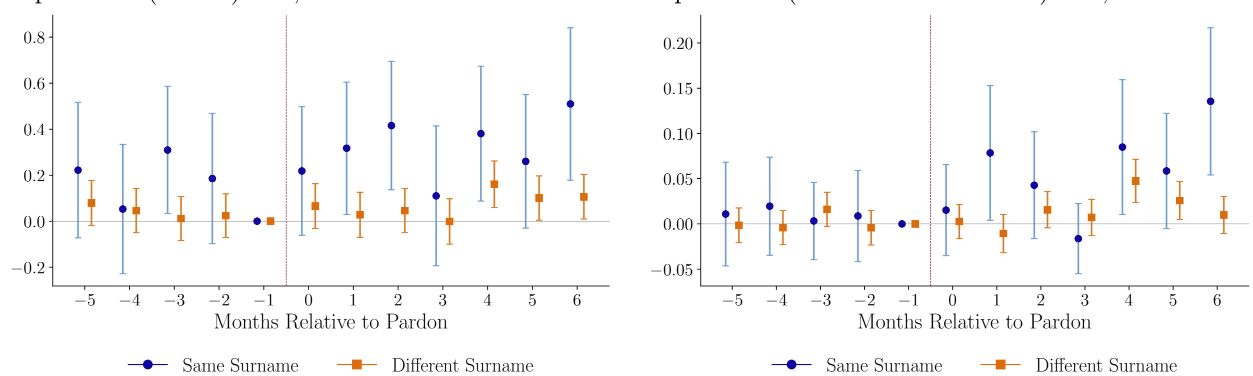
Figure A8: Controlling by National Frequency of Last Names

Panel A. Arrests

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

Panel B. Criminal Partnerships

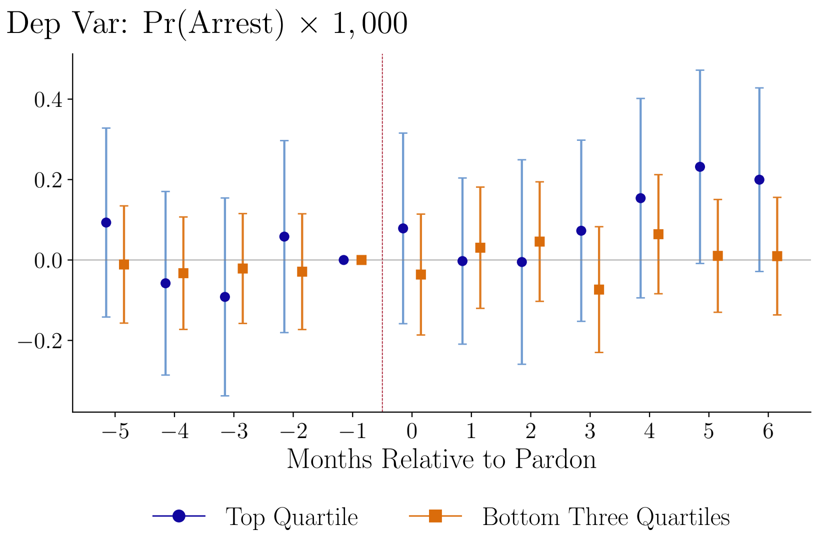
Dep Var: Pr(Arrest with Offender) $\times$ 1,000



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 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.

A.5 Prison Experience

Figure A9: Effects by Number of Previous Arrests



Walt test: $\beta_{k\in[0,6]}$, p-value $= 0.24$. $\beta_{k\in[4,6]}$, p-value $= 0.25$

Notes: The figure displays 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). The circled dots represent the effects on individuals living in neighborhoods that received an offender with a higher number of previous arrests, while the squares display the estimates for people in neighborhoods that received an offender in the bottom three quartiles of the distribution of number of arrests. 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.

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 * $p < 0.1$.

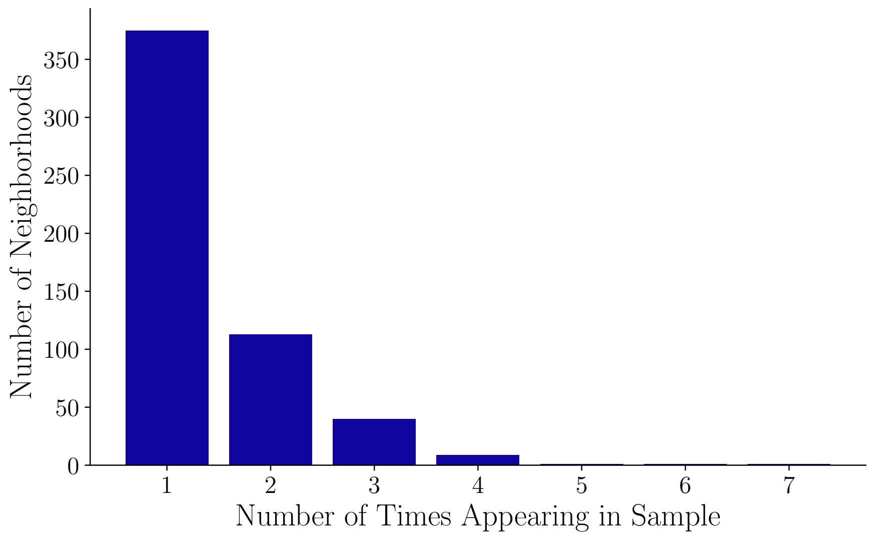
Table B4: Effects of Pardon on Probability of Arrest

	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 * $p < 0.1$.

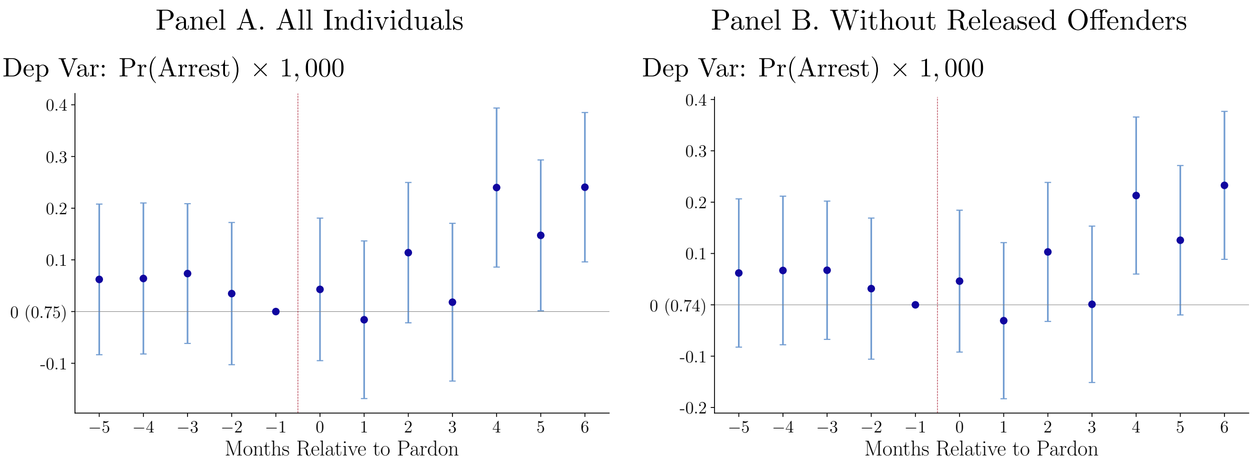
B.3 Figures

Figure B1: Number of Repeated Controls



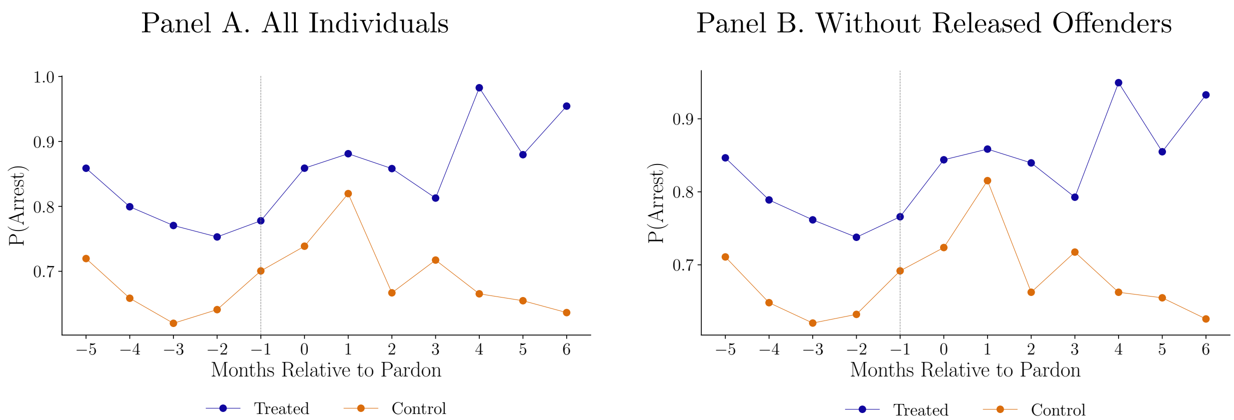
Notes: The figure shows a histogram for the number of times a matched control neighborhood appears in the final sample. There are 540 unique control neighborhoods plotted, with the average neighborhood appearing 1.4 times and the median appearing 1 time.

Figure B2: Effects of Mass Pardon on Arrests



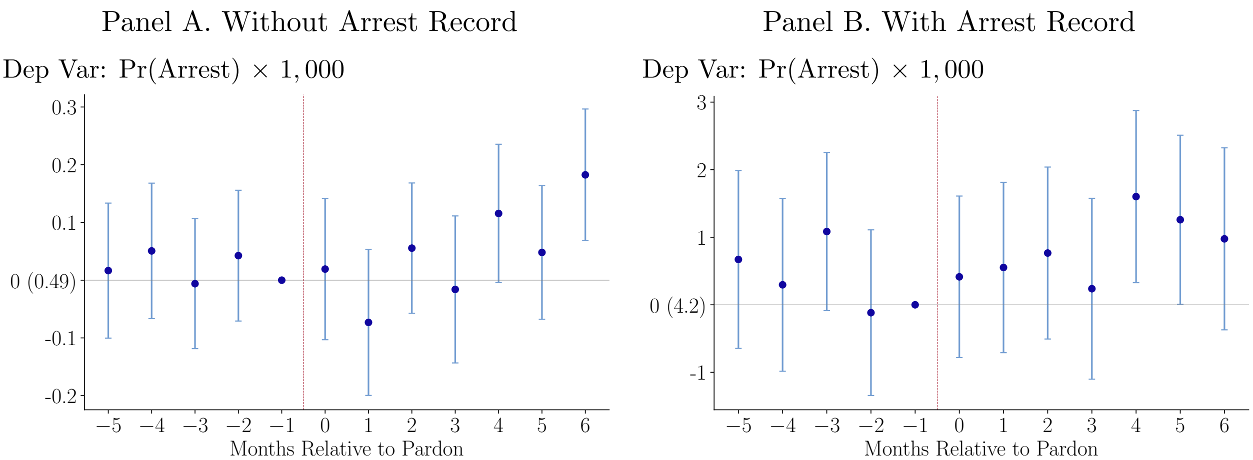
Notes: The figure displays the regression coefficients for the difference in the probability of arrest between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_j from equation 1. The coefficients at $t = -1$ are normalized to zero. On the y-axis,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

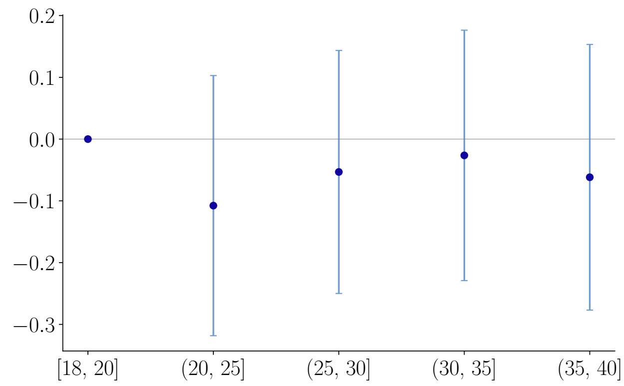
Figure B4: Effects by Residents' Criminal Records



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.

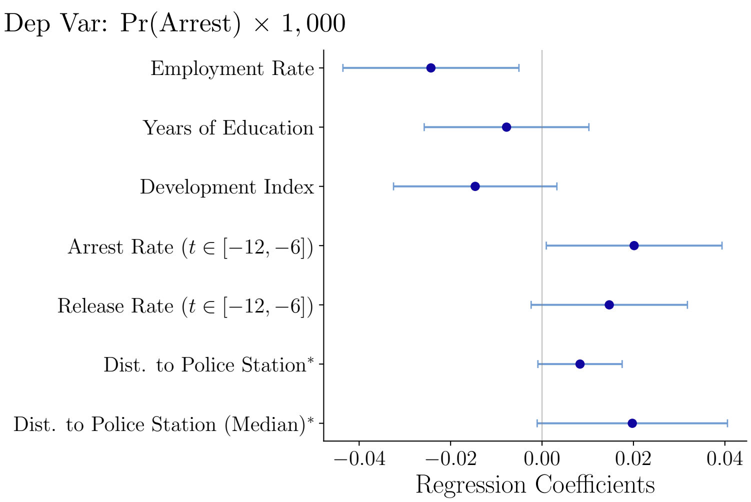
C Heterogeneity Results

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.

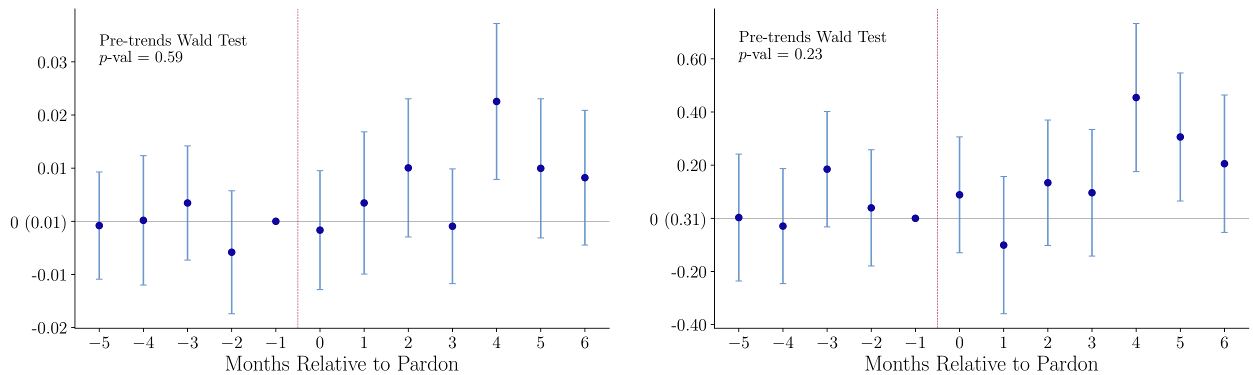
Figure C3: Criminal Partnerships by Resident's Arrest Records

Panel A. Without Arrest Record

Dep Var: Arrested with Released Offender

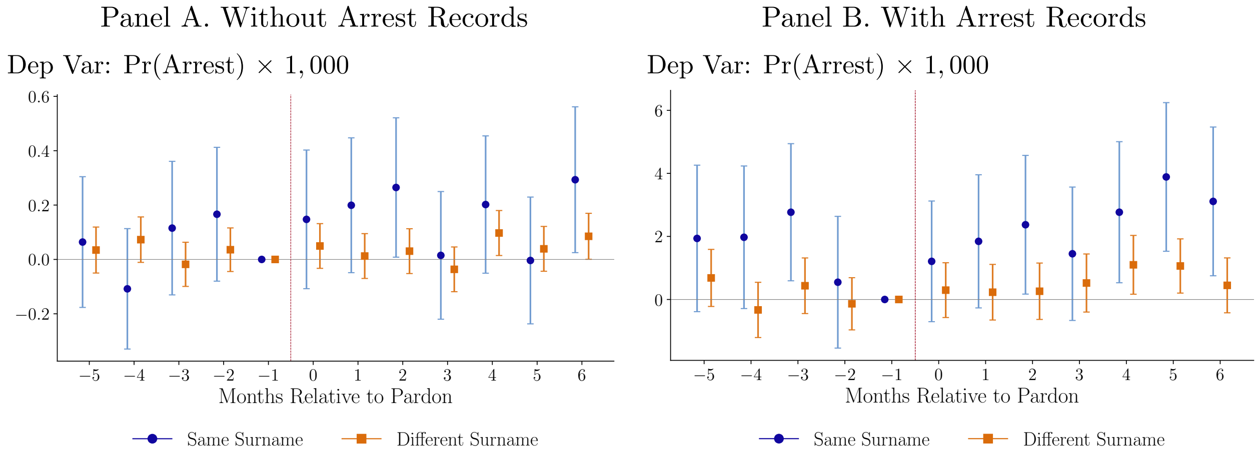
Panel B. With Arrest Record

Dep Var: Arrested with Released Offender



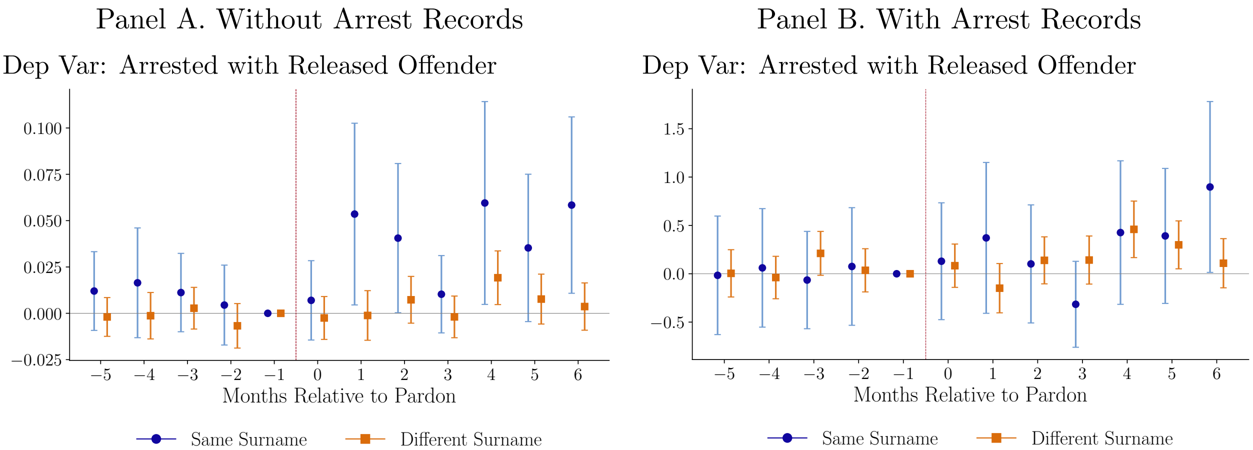
Notes: The figure shows the regression coefficients for the difference in the probability of beign arrested with a released offender (times 1,000) between people living in treated and control neighborhoods relative to the month before the pardon, i.e., the β_k from Equation 1. Each panel shows a stratified 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



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.