The Spillover Effects of Prisoner Releases: Evidence from an Ecuadorian Mass Pardon

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Abstract

Do released offenders influence the criminal behavior of individuals in the neighborhoods they rejoin? I study this question using a novel dataset linking arrests, prison releases, and residential locations for the universe of adult men in Ecuador and exploiting a mass pardon as a source of quasi-random variation in neighborhood exposure to released offenders. I find that one additional released offender per 1,000 residents generates 0.41 additional arrests per 1,000 residents, corresponding to an elasticity of 0.13. Approximately 60% of this increase is driven by individuals with no prior criminal record. The spillovers are concentrated among individuals sharing pre-existing co-offending networks or early-life social environments with the released offender, while family ties do not generate significant effects. Further, I exploit exogenous variation in time served among pardoned offenders to show that time spent in prison can itself be criminogenic. Finally, I show that access to rehabilitation programs during incarceration mitigates these spillover effects.

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1 Introduction

Every day, thousands of individuals are incarcerated worldwide, most of whom will eventually be released. A primary concern regarding prisoner reentry is their potential impact on crime through recidivism (Yukhnenko et al., 2020). In the United States, 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 approximately 39% (Bergman et al., 2020; Fazel and Wolf, 2015). Yet the impact of released offenders on crime may extend beyond their own reoffending. Former prisoners can affect crime by influencing the behavior of individuals in their social networks and in the broader communities to which they return. These spillover effects involve a larger share of the population, potentially making them more consequential than the direct effect of recidivism alone.

Theoretically, the direction of these spillover effects is ambiguous. On one hand, successfully rehabilitated inmates may contribute to crime prevention within their communities. Former offenders can share knowledge of the negative consequences of criminal behavior, discouraging others from following a similar path. For instance, some ex-offenders join NGOs or community organizations that work to steer at-risk individuals away from crime.[1] On the other hand, incarceration can increase inmates' exposure to hardened peers, facilitate gang recruitment, and deteriorate human capital (Stevenson, 2017; Aizer and Doyle, 2015). Upon release, former prisoners may draw members of their social networks into criminal activity, either by transferring 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 demanding data requirements, as it necessitates information on the residence and criminal activity of all individuals, including those with and without criminal records. This constraint has limited prior research on prisoner reentry and neighborhood peers, which has primarily focused on how the recidivism of released offenders is shaped by the criminal peers in their communities (Billings and Schnepel, 2022; Kirk, 2015). This approach provides little insight into whether released offenders affect the criminal behavior of other residents and, further, 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 adult male population, providing evidence of the influence of criminal peers within neighborhoods. This

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

is the first empirical study to provide causal evidence that former offenders influence the criminal behavior of their neighborhood peers, including those with no prior criminal record. Notably, individuals without prior criminal records account for approximately 60% of the observed increase in criminal activity, implying that exposure to former offenders not only intensifies existing criminal behavior but also draws new individuals into crime.

These findings rely on a novel dataset that links prison releases, arrests, family networks, and residential locations for the universe of adult population in Ecuador. I construct this dataset by extracting information on prison releases and arrests from more than two million public judicial documents covering all penal cases between 2016 and 2022. I then match these records to each individual's neighborhood of residence based on their registered voting address between 2002 and 2021. Because voting is mandatory in Ecuador, this registry provides near-universal residential coverage of the adult population rather than a selected sample. Since it is unlikely that released offenders influence all of their neighbors equally, I restrict the analysis to men aged 18 to 40 without a university degree or professional occupation, as this group is the most likely to be affected by criminal exposure (Gavrilova and Puca, 2022).

To estimate the spillover effects of prisoner releases on this sample, I exploit 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—Ecuador's most prevalent crimes. The pardon generated a 25% increase in the monthly number of released offenders relative to the same period in the previous year. I use an event-study design that compares arrest outcomes for residents of neighborhoods that received a released offender during the pardon with those in neighborhoods that did not, conditioning on neighborhoods that had received at least one released offender in the five years preceding the pardon. Importantly, the analysis focuses on individuals' neighborhood of residence at the time of arrest rather than the location where the arrest occurred. This distinction is important because the mechanism of interest is exposure to released offenders through geographic proximity within neighborhoods. On the estimation sample, the pardon increased the number of released offenders that return to treated neighborhoods by 0.17 additional releases per 1,000 residents relative to control neighborhoods, a 103% increase relative to the pre-pardon mean.

The findings reveal that released offenders generate criminal spillovers in the neighborhoods to which they return. Excluding released offenders themselves, the average probability of arrest for residents of treated neighborhoods increased by 0.007 percentage points (14% of the mean) during the post-pardon period compared to control areas. This estimate implies that each additional release per 1,000 residents leads to 0.41 additional arrests per 1,000 residents per month, corresponding to an elasticity of arrests with respect to releases of 0.13.

When released offenders are included in the analysis, the implied increase in arrests rises to 0.49 per 1,000 residents, with the difference reflecting the direct contribution of recidivism.

The influence of former offenders extends to residents regardless of their prior criminal history. Among individuals with no prior arrest record, the probability of arrest increased by 0.004 percentage points (13% of the mean). Among those with prior criminal experience, it rose by 0.07 percentage points (14% of the mean). Given their much larger population share, individuals without prior records account for approximately 60% of the overall increase in arrests, despite the smaller individual-level effect. Therefore, focusing exclusively on prior offenders would miss the majority of the spillovers generated by prisoner releases.

The evidence points to peer effects as the primary mechanism driving the observed increase in arrests. First, I find a higher incidence of joint offenses involving a released offender: residents of treated neighborhoods were up to 0.023 percentage points (approximately twice the outcome mean) more likely to be arrested alongside a recently released offender than those in control areas. Second, individuals with closer social ties to released offenders experience larger increases in arrest probabilities. I examine three dimensions of social connection: criminal networks, early-life connections, and and family ties, and find that spillover effects are disproportionately concentrated among the first two. For individuals embedded in the same pre-pardon co-offending network as the releasee, the probability of arrest increases by approximately 0.5 percentage points (106% of the mean), accounting for 23% of the overall increase in arrests among prior offenders. Individuals who shared both a childhood neighborhood and birth cohort with the returning offender experience an increase of up to 0.02 percentage points (38% of the mean) relative to other treated-neighborhood residents. In contrast, I find no statistically significant effects among relatives, even when extending the definition of kinship up to the fifth degree.

I rule out alternative explanations based on increased policing or a reduction in the deterrent effect of incarceration generated by the pardon. Proximity to police stations does not predict larger spillover effects, and interviews with public officials suggest no strategic law enforcement response to the pardon. Likewise, I find no difference in spillover effects between neighborhoods exposed to pardoned offenders and those exposed to offenders who completed their sentences during the same period (Wald test p-value $= 0.93$). Together, these findings indicate that the observed spillovers reflect behavioral responses driven by peer influence rather than enforcement or deterrence channels.

Finally, I examine how the prison experience of released offenders shapes the spillovers they generate. Using the pardon as a source of quasi-random variation in time served among

offenders convicted of the same crime, I find that longer incarceration substantially amplifies neighborhood spillovers. Neighborhoods receiving offenders who served above the sample median (approximately 24 months) exhibit an average increase in arrests of 0.008 percentage points (16% of the mean), while those receiving shorter-serving offenders show no statistically significant effect. In contrast, offenders released from prisons with recorded inmate participation in vocational training or educational programs generate no significant neighborhood spillovers, whereas those from prisons with no such programming generate persistent and significant increases in arrests. These results suggest that conditions inside prison shape not only the trajectories of released offenders but also the criminal behavior of those around them, and highlight the potential of prison-based rehabilitation to reduce the external costs of incarceration.

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 among released offenders (Billings and Schnepel, 2022; Kirk, 2015) or on correlations between release numbers and aggregate crime rates (Buonanno and Raphael, 2013; Roodman, 2020; Hipp and Yates, 2009; Clear et al., 2003; Raphael and Stoll, 2004), this paper uses individual-level data to provide the first causal estimates of how released offenders influence crime beyond their own recidivism. Importantly, these spillovers extend to individuals with no prior contact with the criminal justice system, not just to those already engaged in criminal activities.

Second, this paper advances the literature on criminal peer effects by showing that such effects operate outside prison walls and extend to adults with no prior criminal record. Prior research has established that criminal peers shape behavior mainly through within-prison interactions (Bayer et al., 2009; Stevenson, 2017; Damm and Gorinas, 2020), while community-level studies have focused on how the criminal environment affects the recidivism of released offenders themselves (Billings and Schnepel, 2022; Kirk, 2015). Studies of peer effects on non-offenders have concentrated on youth, showing that childhood exposure to crime-prone classmates increases criminal involvement later in life (Billings and Hoekstra, 2024; Billings et al., 2019, 2014). This paper extends both strands by documenting that returned offenders generate criminal peer effects among adults in their neighborhoods, including those with no prior criminal record. Moreover, by examining co-offending networks, early-life connections, and family ties, I show that it is the criminogenic quality of a social tie, not its closeness, that drives the transmission of criminal behavior.

Third, this paper contributes to the literature on the spillover effects of incarceration. Existing work has largely focused on the benefits of removing harmful peers, showing that

incarcerating marginal offenders can benefit families and communities (Norris et al., 2021; Arteaga, 2021; Bhuller et al., 2018). This paper complements that literature by studying the opposite margin: the consequences of releasing offenders back into their communities.

Finally, this paper contributes to the literature on prisons and crime in Latin America. Prior work documents that imprisonment in the region often increases the likelihood of recidivism due to poor prison conditions, overcrowding, limited rehabilitation programs, and gang penetration (Escobar et al., 2023; Munyo and Rossi, 2015; Di Tella and Schargrodsky, 2013). These conditions hinder reintegration into legal society (Blattman et al., 2024; Sviatschi, 2022; Tobón, 2022; Carvalho and Soares, 2016). This paper expands this literature by showing that prison experiences affect not only former inmates but also their neighborhoods, and provides evidence on the external benefits of rehabilitation programs.

The rest of the paper is organized as follows. Section 2 presents the institutional background of the mass pardon. Section 3 describes the data sources and sample construction. Section 4 presents the empirical strategy and main results, while Section 5 discusses mechanisms. Section 6 examines the role of prison conditions, and Section 7 concludes.

2 Ecuadorian Prison System and the Pardon

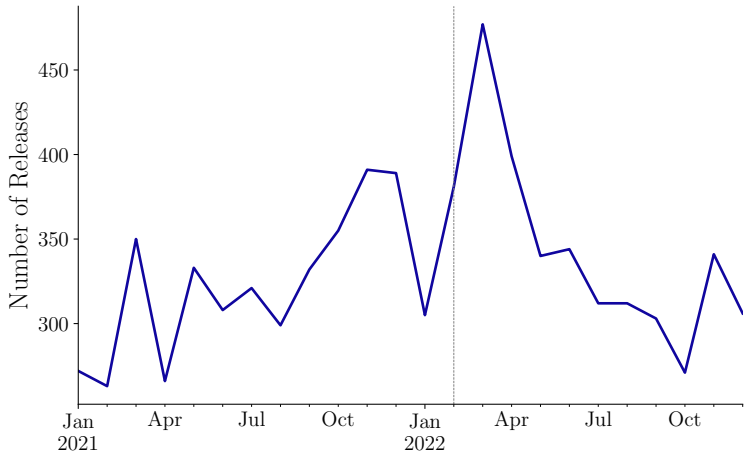
Ecuador has historically treated incarceration as its primary crime control policy (The Economist, 2024; Verdugo, 2023). This approach led the national incarceration rate to nearly triple between 2010 and 2020. However, the expansion of imprisonment has not been accompanied by investments in inmate rehabilitation or improvements to prison conditions. As of 2021, the prison system operated at 27% above capacity, and 38% of detainees were held in pre-trial detention (Fair and Walmsley, 2024). Prisons are also characterized by gang penetration and violence: between 2021 and 2022, eleven gang-related riots resulted in more than 413 deaths (Primicias, 2022). Moreover, a national census conducted at the end of 2022 found that only 43% of inmates participated in rehabilitation programs (Instituto Nacional de Estadisticas y Censos, 2022).

In an attempt to reduce prison overcrowding, the Ecuadorian government issued three presidential pardons between late 2021 and early 2022. The first two decrees (issued in November and December 2021) pardoned individuals convicted of traffic-related offenses and inmates with severe illnesses such as cancer or tuberculosis, excluding those convicted of murder, sexual violence, crimes against the state, or gender-based violence. These decrees primarily affected low-risk individuals whose sentences were generally short, lasting often less than a month (Presidencia de la República del Ecuador, 2021a,b). The last decree, issued

in February 2022, had a broader scope. It pardoned individuals convicted of robbery, theft, or fraud who had served at least 40% of their sentences (Presidencia de la República del Ecuador, 2022). As a result, it applied to individuals convicted of Ecuador's most prevalent property crimes: robbery and theft, which together account for more than one-third of the incarcerated population.

The decrees generated an increase in the number of released offenders. Between October 2021 (the month before the first decree) and March 2022 (the month after the last decree) the average monthly number of released offenders rose by 25% (from 311 to 390) relative to the same period in the previous year. This increase includes both routine releases and pardoned individuals. In total, after the pardon, the prison population declined by approximately 10%, and the overcrowding rate fell by 9.5 percentage points (from 22.3% to 12.8%). Figure 1 displays the monthly evolution of the number of releases between January 2021 and December 2022.

Figure 1: Monthly Evolution of Prison Releases



Notes: The figure shows the monthly evolution of the total number of released offenders from January 2021 to December 2022. The vertical dashed line indicates the date of the last pardon, February 2022. The sample is restricted to releases into urban neighborhoods.

The implementation of the pardons was centralized. Inmates did not need to apply, petition, or hire private lawyers for the process to begin. Instead, the administrative procedure was initiated by the National Prison Authority (*Servicio Nacional de Atención Integral a Personas Privadas de Libertad*, SNAI), which petitioned judges on behalf of all eligible individuals. Once granted, the pardon eliminated the remaining portion of the sentence. This stands in contrast to cases such as the Italian pardon, where reoffending triggers both a new sentence and the reinstatement of the previously forgiven one.

In this paper, I focus on the extensive margin variation generated by the last decree. I

exclude the first two decrees because their beneficiaries are not representative of Ecuador's criminal population. Individuals pardoned in the initial wave were primarily convicted of traffic-related misdemeanors (such as driving under the influence or involvement in fatal accidents), offenses that are not connected to patterns of criminal behavior. In contrast, the third decree applied to individuals convicted of Ecuador's most common crimes, robbery and theft. Because the aim of this paper is to study how criminal behavior spreads within neighborhoods, rather than the consequences of regulatory violations, the analysis focuses on areas affected by the releasees of the February 2022 pardon.

3 Data and Summary Statistics

This paper uses a comprehensive dataset of prison releases, arrests, and neighborhood-level residence information for the universe of adults in Ecuador. This section describes the main variables used in the study, their sources, and the construction of the estimation sample.

3.1 Data Sources

Place of Residence: Information on individuals' neighborhoods of residence comes from the national voting registry maintained by Ecuador's electoral authority, the *Consejo Nacional Electoral* (CNE). Because voting is mandatory in Ecuador, the registry includes all nationals aged 16 or older, regardless of whether they actually vote. The dataset contains basic demographic characteristics (e.g., sex at birth and date of birth), as well as the polling station where each individual is registered to vote. Records are available for all nine national elections held between 2002 and 2021.

I use this dataset to assign individuals to neighborhoods. The CNE groups all residents of a given neighborhood into the same polling station, allowing me to define neighbors as individuals registered to vote at the same location. This assignment is based on each person's registered home address. Individuals typically enter the registry at age 16 with their parents' address, but voters can update their registered address at any point, either upon first registration or in any subsequent electoral cycle, by submitting a government-issued proof of residence (e.g., an electricity or water bill). Importantly, this mapping from polling stations to neighborhoods is valid only in urban settings. In rural areas, each polling station serves several dispersed villages, making it impossible to define meaningful geographic proximity. For this reason, the analysis focuses on urban neighborhoods.

Arrests: The arrest data comes from the *Consejo de la Judicatura*, the institution overseeing Ecuador's judicial system. Through its public platform, SATJE, all judicial courts

must upload documents related to the cases they administer. I retrieve all criminal cases for each individual in the voting registry. These records provide information on the suspected offense, the arrest date, and the identities of all individuals involved in each case. Two remarks are important about this data. First, it excludes arrests involving minors, cases of violence against women, and national security offenses due to confidentiality restrictions. Second, the scraped documents do not report whether an arrest ultimately resulted in a conviction or incarceration.[2]

Prison Releases: Information on prison releases also comes from SATJE, but unlike the arrest data, release records do not follow a standardized format. Each release is documented through a Release Warrant (*"Boleta de Excarcelación"*), drafted individually by the corresponding court secretary to authorize release based on sentence completion or a judicial pardon. I web-scrape all release warrants issued between 2016 and 2022 and use OpenAI's LLM with Retrieval-Augmented Generation to extract relevant information from the documents (Lewis et al., 2021). From each warrant, I collect the offender's full name, national ID number, nationality, arrest date, offense, type of release, and release date.[3]

Demographics: Finally, I supplement these sources with administrative data from the Ecuadorian Civil Registry. These records include demographic information such as date of birth, sex, highest educational level attained, and main occupation (coded using the ISCO ten major groups). Importantly, they also provide parental identifiers, which allows to reconstruct complete family trees for released individuals.

3.2 Sample Description

I compile the data at the individual-by-month level. For non-released individuals, I aggregate arrests into monthly observations and assign neighborhoods based on the polling station in the last election before the pardon (February 2021). For released offenders, I assign neighborhoods using their place of residence at the time of arrest, i.e. before incarceration. Importantly, arrests are assigned to individuals' neighborhoods of residence rather than to the geographic location where the arrest took place. Although I do not observe post-release residential locations, release data from 2016 to 2021 show that 95% of inmates return to the same neighborhood where they lived before incarceration.

For the analysis, I focus on young men at risk of offending. Although the dataset covers

[2]Website: https://procesosjudiciales.funcionjudicial.gob.ec/busqueda

[3]In an earlier version of this project, I fine-tuned a Named Entity Recognition model on XML-RoBERTa, achieving over 85% accuracy on a sample of 1,000 manually labeled documents. The OpenAI RAG pipeline achieves accuracy close to 100%.

the universe of adult residents within each neighborhood, it is unlikely that the presence of a released offender influences all individuals in an area of roughly 4,000 residents on average. Existing evidence on criminal peer effects indicates that such spillovers tend to operate within groups sharing similar demographic and socioeconomic characteristics (Aizer and Doyle, 2015; Billings et al., 2014; Bayer et al., 2009; Gavrilova and Puca, 2022). In my data, released offenders are predominantly male, have an average age of 32, and generally lack post-secondary education or professional occupations (see Table **??** in the Online Appendix). Accordingly, I analyze spillover effects among men between the ages of 18 and 40 without post-secondary education or professional occupations. For completeness, Figure A1 in Appendix A shows that no spillover effects exist among women, individuals with post-secondary education, or those in professional occupations.

The final sample of at-risk men includes approximately 1.9 million individuals residing in 1,786 neighborhoods across 201 urban centers. Panel A of Table 1 reports summary statistics for this population. The probability that an individual is arrested in a given month is 0.05%. Among arrests, 38% involve more than one offender, and 7% of group arrests include a released offender. In terms of prior exposure to the criminal justice system, 4% of individuals in the sample have an arrest record before the pardon.

Table 1: Summary Statistics

	Mean	SD	N
Panel A: At-risk Men			
Age (years)	27.577	5.97	1,931,292
Post-secondary education	0.094	0.29	1,931,291
Professional or managerial occupation	0.056	0.23	1,930,569
Ever Arrested	0.036	0.19	1,931,292
Relative of Released Offender	0.004	0.06	1,931,292
Pr(Arrest) $\times$ 1000	0.513	22.63	30,902,229
Pr(Arrest with Released Offender) $\times$ 1000	0.014	3.80	30,902,229
Pr(Group Arrest) $\times$ 1000	0.197	14.03	30,902,229
Panel B: Released Offenders			
Age at release (years)	32.424	9.77	1,375
Time in jail (months)	27.360	25.00	1,351
Released through pardon	0.414	0.49	1,375
Same Neighborhood as at age 16	0.849	0.36	1,191
Same pre-incarceration neighborhood (2016-2021)	0.963	0.19	29,985

Notes: The table reports summary statistics for the main variables used in the analysis. Panel A describes the study population: men between 18 and 40 years old without post-secondary education or professional occupations, observed between September 2021 ($t = -5$) and December 2022 ($t = 10$). Panel B reports characteristics of individuals released during the months in which the pardon was in effect (February and April 2022). The variable *Same pre-incarceration neighborhood* is computed using all releases between 2016 and 2021.

Panel B describes the characteristics of released offenders during the months in which the pardon was in effect (February and April 2022). The average releasee served approximately 27 months in prison, and 41 percent were released through the pardon. Further, 85% reside in the same neighborhood they lived in at age 16, and 96% return to the same neighborhood in which they lived prior to incarceration. These patterns indicate residential stability among released offenders.

4 Effects of the 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 treatment assigned at the neighborhood level. Treated areas are defined as the pre-incarceration neighborhoods of residence of individuals released during the three-month pardon window, February to April 2022. Control neighborhoods are those that were the pre-incarceration residence of at least one offender in the five years preceding the pardon but not during the pardon period. This restriction excludes 810 neighborhoods. The final sample consists of 706 treated and 1,080 control neighborhoods.

Equation 1 shows the event study specification, covering the period from September 2021 ($t = -5$) to December 2022 ($t = 10$).

$$y_{int} = \sum_{k=-5}^{10} \beta_k \mathbf{1}\{t = t^* + k\} \times \textit{Any Release}_n + \alpha_n + \delta_t + \mu_{int} \tag{1}$$

where y_{int} is the outcome variable (e.g., probability of arrest times 1,000) for individual i living in neighborhood n at month t. $\textit{Any Release}_n$ is an indicator equal to one if neighborhood n is the home of at least one offender released during the pardon. The event-time dummies $\mathbf{1}t = t^* + k$ index months relative to the date of the pardon (t^*), February 2022. The specification includes neighborhood fixed effects (α_i) and month fixed effects (δ_t). I omit the dummy for the month before the pardon (January 2022) in the specification, so the coefficients β_k captures the changes in the probability of arrest (y_{int}) between treated and control neighborhoods relative to the same difference at $k = -1$. Standard errors are clustered at the neighborhood level.

The event-study coefficients (β_k) identify the causal effect of neighborhood-level exposure to released offenders under two standard assumptions: parallel trends and no anticipation. The parallel-trends assumption requires that, absent the pardon, arrest outcomes in treated and control neighborhoods would have evolved similarly over time, while the no-anticipation assumption requires that behavior does not respond to treatment prior to its implementation. Although the parallel-trends assumption is not directly testable, I assess its plausibility by reporting both pointwise and joint statistical tests of the pre-treatment coefficients in all event-study specifications. The no-anticipation assumption is also plausible in this setting,

as the timing of releases was determined administratively by the prison system and was not announced in advance, limiting the scope for behavioral responses prior to release. Under these assumptions, the coefficients β_k can be interpreted as average treatment effects on the treated (ATT) at each event time k, measured relative to the month preceding the pardon.

4.2 Changes in Released Offenders

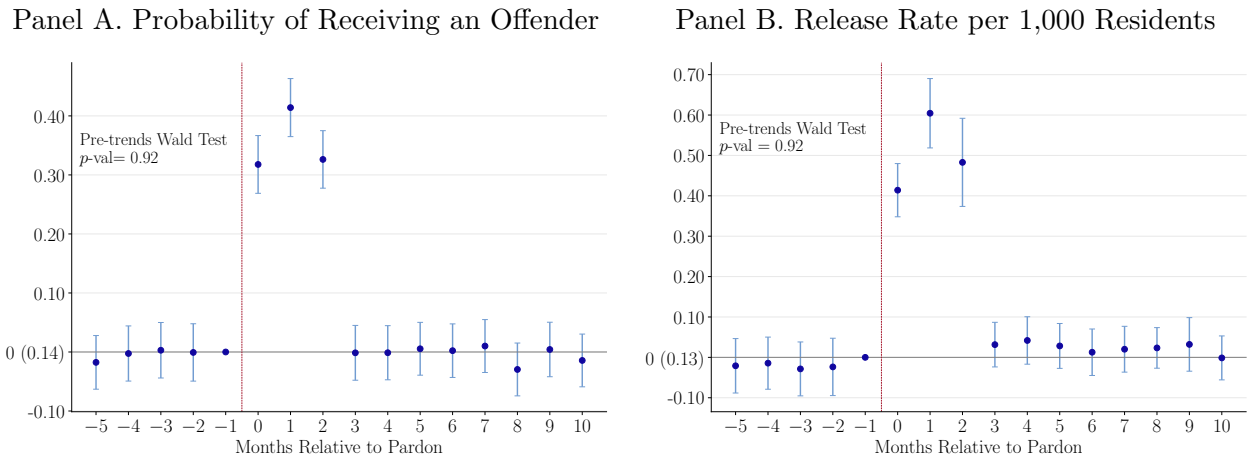
I begin the analysis by quantifying the extent to which the pardon increased neighborhood exposure to released offenders. To do so, I estimate the event-study specification in Equation 1 at the neighborhood-month level, using outcomes that capture both the extensive and intensive margins of exposure. Specifically, I use an indicator variable equal to one if neighborhood n is home to at least one offender released in month t, as well as the rate of offenders released to neighborhood n per 1,000 residents in month t. These estimates correspond to the *first-stage* effect of the pardon. Figure 2 presents the resulting event-study estimates.

Figure 2 shows that differences between treated and control neighborhoods in both the extensive and intensive margins of exposure to released offenders arise exclusively during the months in which the pardon was in effect (February-April 2022). Panel A indicates that, during this period, the probability that a neighborhood is home to at least one released offender was approximately 0.35 percentage points higher in treated neighborhoods relative to controls (twice the pre-pardon mean). Panel B shows that, on average, the rate of released offenders per 1,000 residents was 0.5 higher in treated neighborhoods (2.7 times the pre-pardon mean).

Beyond quantifying the magnitude of the increase in released offenders induced by the pardon, Figure 2 provides evidence supporting the assumption that treatment assignment reflects quasi-random variation in the timing of releases rather than systematic selection across neighborhoods. In particular, the figure alleviates concerns that treated neighborhoods were ex ante more likely to be associated with released offenders. If such selection were present, differences in exposure would be expected both before and after the pardon period. Instead, the estimates show no differential exposure prior to the pardon ($t < 0$) and no persistent differences following the final month of pardon-related releases ($t \geq 3$). This pattern suggests that, absent the pardon, treated and control neighborhoods would have experienced similar release dynamics, reinforcing the interpretation that subsequent differences in arrest outcomes reflect the causal effects of exposure to released offenders rather than pre-existing differences between neighborhoods.

Table A1 in Appendix A reports difference-in-differences estimates that summarize the

Figure 2: Neighborhood Exposure to Released Offenders



Notes: The figure plots the event-study coefficients for the probability of being home to at least one released offender (Panel A) and in the release rate per 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). Treated neighborhoods are those corresponding to the pre-incarceration residence of at least one offender released between February and April 2022. The coefficient at $t = -1$ is normalized to zero. The mean of the dependent variable in the month prior to the pardon ($t = -1$) is reported in parentheses on the y-axis. The unit of observation is the neighborhood-month. Bars denote 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

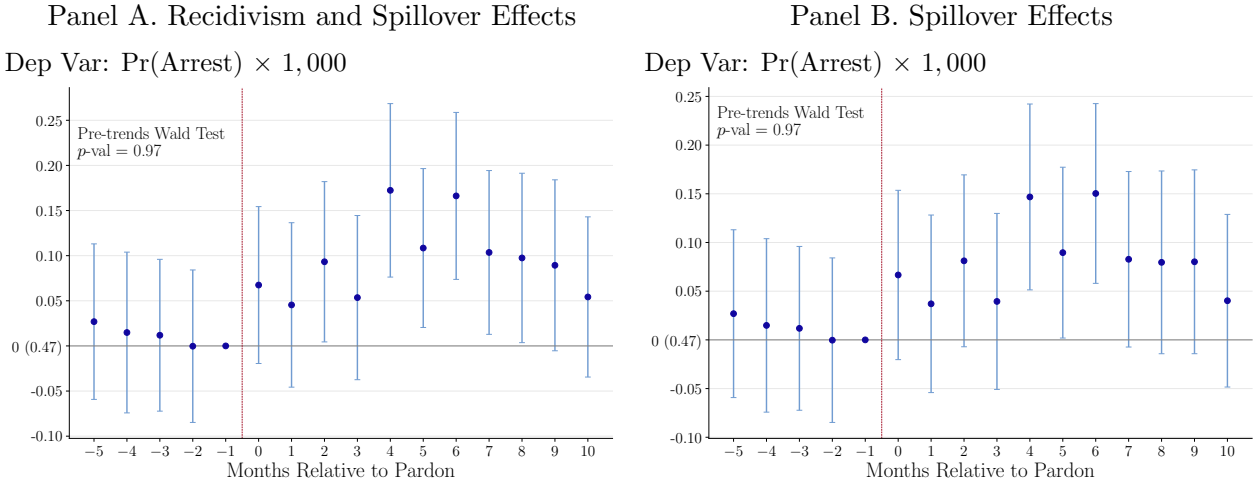
event-study results by comparing average neighborhood exposure before and after the pardon period. The table shows that treated neighborhoods experienced an increase of 0.17 releases per 1,000 residents (103% of the mean) relative to control neighborhoods. I next examine how this increase in neighborhood exposure to released offenders translated into changes in arrest outcomes among at-risk residents.

4.3 Effects of the Pardon on Arrests

I now examine the effect of exposure to released offenders on arrest outcomes among neighborhood residents. I begin by estimating the event-study coefficients using the sample of all individuals residing in a neighborhood, including released offenders. Panel A of Figure 3 presents the resulting event-study estimates. The figure indicates that increased exposure to released offenders raises the probability of arrest among residents of treated neighborhoods. The effects are not immediate but emerge gradually following the pardon, exhibiting an inverted U-shaped pattern over time. Arrest probabilities begin to diverge from those in control neighborhoods around the fourth month after the pardon, peak between four and six months, and then attenuate, becoming statistically indistinguishable from zero by the ninth month. At the peak, the probability of arrest increases by 0.015 percentage points (29% of the mean). Averaged over all the post-pardon period, arrest probabilities in treated

neighborhoods are higher by 0.01 percentage points (18% of the mean) relative to control neighborhoods.

Figure 3: Effects of Released Offenders on Arrests



Notes: The figure plots 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). Treated neighborhoods are those where offenders released under the pardon resided at the time of their arrest. Panel A reports estimates for men aged 18-40 who are at risk of offending (without a degree or professional occupation), including released offenders. Panel B excludes released offenders from the sample. 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. Bars denote 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

This initial result reflects two distinct mechanisms. First, it captures the recidivism of released offenders themselves. Because recently released individuals are more likely to reoffend than the general population, their inclusion in the sample mechanically raises arrest probabilities in treated neighborhoods. Second, the estimates may also reflect spillover effects on other neighborhood residents. To isolate these spillovers, I estimate the event-study specification excluding released offenders from the sample. Panel B of Figure 3 reports the resulting estimates.

The estimates in Panel B indicate that exposure to released offenders increases arrest probabilities among non-released individuals living in treated neighborhoods. As in the full-sample analysis, the effects emerge gradually following the pardon and display an inverted U-shaped pattern. Arrest probabilities become statistically significant between the fourth and sixth months after the pardon. During this period, the probability of arrest among non-released residents increases by 0.013 percentage points (25% of the mean). Averaged over the post-pardon period, arrest probabilities are higher by 0.007 percentage points (14% of the mean) in treated neighborhoods relative to control neighborhoods. These results indicate

that the increase in arrests among residents of treated neighborhoods is driven primarily by spillover effects rather than by recidivism among released offenders.

Notably, the estimates provide no evidence inconsistent with the parallel-trends assumption. In both panels, the coefficients on the pre-treatment leads ($k < 0$) are pointwise indistinguishable from zero. In addition, the Wald test of joint significance for all pre-treatment coefficients yields a p-value of 0.97, indicating that the leads are not jointly different from zero. Further, Figure A2 in Appendix A plots the evolution of raw mean arrest probabilities for treated and control neighborhoods. Prior to the pardon, both groups display similar trends. Beginning approximately four months after the pardon, arrest probabilities increase among residents of treated neighborhoods, while remaining stable in control neighborhoods.

To better interpret the magnitude of these effects, I translate the estimates into elasticities relating arrests to neighborhood exposure to released offenders. The elasticities are computed by scaling the average post-pardon change in arrests by the corresponding change in the rate of released offenders, relative to their means. Excluding released offenders, the estimated monthly elasticity of total arrests per 1,000 residents with respect to the release rate is 0.13. This implies that one additional released offender per 1,000 residents leads to approximately 0.41 additional arrests per 1,000 residents per month among other neighborhood residents. When released offenders are included in the analysis, the elasticity increases to 0.16, corresponding to an implied increase of 0.49 arrests per 1,000 residents.

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 ten months of release is 15%, while the estimated increase in the probability of arrest due to released offenders as share of the mean 16%. This suggests that the estimated spillover effects are within a similar range to typical rearrest rates.

Finally, these findings are similar to correlational estimates between the number of released offenders to aggregate crime rates. For example, Buonanno and Raphael (2013) studies the 2006 Italian pardon and finds an effect on provincial crime rates that is roughly three times larger than the estimate documented here. By contrast, my results are more closely aligned with the findings of Hipp and Yates (2009) and Vieraitis et al. (2007), who document state-level correlations in the United States and report elasticities of releases with respect to robbery rates of 0.18 and 0.16, respectively.

4.4 Who Drives the Spillovers? New Crimes vs. New Criminals

Does the increase in arrests following the pardon reflect intensified criminal activity among individuals with prior offending experience, or the initiation of criminal behavior among previously non-offending residents? Existing research documents *reinforcing peer effects*, whereby interactions among offenders amplify the criminal behavior among those already engaged in crime (Damm and Gorinas, 2020; Stevenson, 2017; Billings and Schnepel, 2022). By contrast, evidence on *introductory peer effects* (the influence of offenders on individuals with no prior criminal record) is limited. The closest related studies focus on long-run exposure to disadvantaged or violent environments during childhood, rather than direct contact with active offenders, and show that such exposure increases criminal involvement later in life (Damm and Dustmann, 2014; Billings et al., 2019; Billings and Hoekstra, 2024). Whether short-run neighborhood exposure to released offenders can induce criminal entry among adults with no criminal records remains unexplored.

Individuals without prior criminal experience constitute the majority of residents exposed to released offenders. In the study sample, residents with a prior arrest record account for only 3.6% of individuals. As a result, arrest responses driven by criminal initiation, even if modest at the individual level, can generate substantial aggregate spillovers.

Table 2 presents the difference-in-difference estimates of the effect of neighborhood exposure to released offenders by subsample. Column 1 presents the main spillover effects, Column 2 focuses only in the sample with no prior arrest records, and Column 3 in the sample that have a criminal record. 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 0.004 percentage points (13% of mean) after the pardon compared to similar individuals in control neighborhoods. The corresponding increase for individuals with a prior arrest record is 0.07 percentage points (14% of the mean).

These estimates imply that individuals with no prior arrest record account for roughly 60% of the overall increase in arrests. Although the individual-level effect is larger among those with prior arrests, the contribution of first-time offenders is bigger due to their larger population share. Thus, focusing exclusively on individuals with criminal histories would therefore miss a sizable portion of the spillover effects generated by the release of offenders.

Table 2: Spillover Effects of the Pardon on Arrests

	Dep. Var.: Probability of Arrest × 1,000		
	All Individuals	No Prior Arrests	Prior Offenders
	(1)	(2)	(3)
$\text{Treated}_n \times \text{Pardon}_t = 1$	0.0706***	0.0432***	0.7219***
	(0.0178)	(0.0147)	(0.2767)
N. Neighborhoods	1,786	1,786	1,780
Mean Dep. Var.	0.513	0.339	5.10
Observations	30,902,229	29,774,140	1,128,089

Notes: The table reports difference-in-difference estimates of the effect of the pardon on the probability of arrest, multiplied by 1,000. The unit of observation is the individual-by-month pair. Column (1) includes all men aged 18-40 at risk of offending (without a degree or professional occupation). Column (2) restricts the sample to individuals without prior arrest records, while Column (3) includes only prior offenders. All three columns focus only on spillovers, excluding released offenders from the analysis. The time frame spans from September 2021 ($t = -5$) to December 2022 ($t = 10$). Standard errors clustered by neighborhood in parentheses. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

4.5 Neighborhood Conditions

An underlying dimension of the results presented so far is their geographic nature: spillover effects operate at the neighborhood level. Previous research shows that neighborhood characteristics shape a range of outcomes, including criminal behavior (Barrios Fernández and García Hombrados, 2025; Billings and Schnepel, 2022; Chetty et al., 2016; Kling et al., 2005). Differences in access to employment, human capital, and exposure to criminogenic peers affect the opportunity cost of crime and the returns to lawful activity. As a result, the same exposure to released offenders may generate different behavioral responses depending on neighborhood conditions.

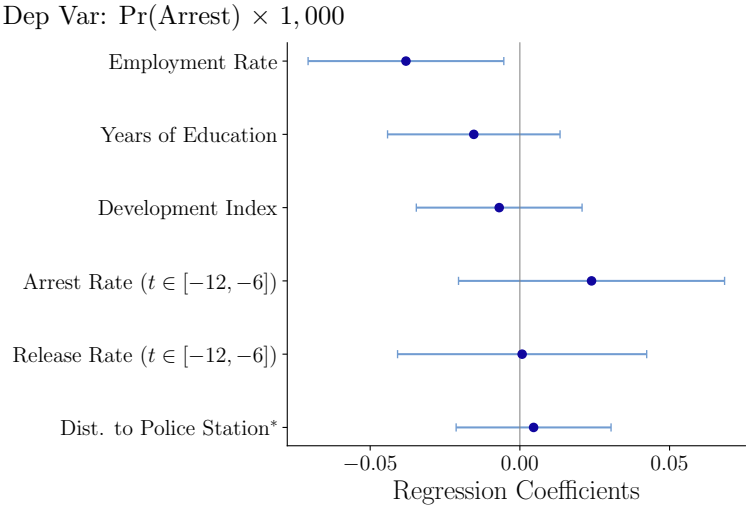
To examine whether neighborhood characteristics shape the transmission of spillover effects, I estimate heterogeneous treatment effects using neighborhood-level measures. Specifically, I estimate the following specification 2.

$$\begin{aligned} y_{int} =& \alpha_n + \delta_t + \theta \mathbf{1}\{t \geq t^*\} \times \textit{Any Release}_n \times \textit{Neighborhood Characteristic}_n \\ &+ \beta \mathbf{1}\{t \geq t^*\} \times \textit{Any Release}_n + \mu_{int}, \end{aligned} \tag{2}$$

where *Neighborhood Characteristic*$_n$ denotes a pre-determined neighborhood-level characteristic measured prior to the pardon. I use the 2022 Ecuadorian population census to gather information on formal employment rate, average years of education, and provision of public services (e.g. electricity, sewage). I complement this with baseline rates of arrests and prisoner releases computed between one year and six months before the pardon ($t \in [-12, -6]$). All other variables are defined as in equation 1. The coefficient of interest is θ, which captures how spillover effects vary with neighborhood conditions.

Figure 4 presents the resulting estimates. For ease of comparison, each coefficient is standardized to reflect a one–standard deviation change in each neighborhood characteristic. The results indicate that the spillover effects are attenuated in neighborhoods with higher formal employment rates. In contrast, I find no significant heterogeneity along the other dimensions, including education, access to public services, or baseline crime rates. This suggests that labor market conditions, rather than broader measures of neighborhood development, may play a role in mediating the transmission of criminal behavior following offender releases.

Figure 4: Heterogeneity by Neighborhoods' Characteristics



Notes: The figure shows estimates of how the spillover effect of exposure to released offenders on the probability of arrest varies with neighborhood characteristics (i.e., the θ coefficients from equation 2). Each coefficient is standardized so that it reflects a one–standard deviation change in the corresponding neighborhood characteristic. 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.

One possible interpretation is that formal employment influences more directly the opportunity cost of criminal activity. Access to formal jobs provides immediate alternatives to criminal behavior and may constrain the reinforcement of criminal activity following exposure to released offenders. By contrast, education levels and access to public services reflect longer-run investments or living conditions that are less likely to alter short-run incentives

faced by residents.

4.6 Robustness

The empirical design relies on the assumption that, absent the pardon, outcomes in treated and control neighborhoods would have followed parallel trends. While the event-study evidence shows no violations of this assumption prior to the policy, additional concerns may arise regarding the adequacy of the control group and the possibility that changes in deterrence or policing, rather than behavioral spillovers, drive the results. Below I discuss some of these concerns.

4.6.1 Deterrence: Pardoned vs. Non-Pardoned Released Offenders

The increase in arrests documented in the main results could arise mechanically through channels unrelated to direct exposure to released offenders. One leading explanation is that the pardon may have altered perceptions of punishment severity, weakening the deterrent effect of incarceration. The pardon mechanically shortened the incarceration spells of eligible inmates, potentially affecting their perceptions of the severity of criminal punishment. By reducing the realized cost of incarceration, the pardon may have led released individuals to revise their beliefs about expected future sanctions, including the likelihood of similar clemency measures. If such belief revisions weaken deterrence, exposure to pardoned offenders should generate larger spillover effects than exposure to offenders who completed their sentences.

To test this hypothesis, I exploit the fact that treated neighborhoods may be exposed either to a pardoned offender or to an offender released after completing their sentence during the same period. If the increase in arrests reflects a reduction in deterrence induced by the pardon, spillover effects should be stronger in neighborhoods exposed to pardoned releasees. I formalize this by estimating equation 3.

$$\begin{aligned} y_{int} = &\sum_{k=-5}^{10} \theta_k \mathbf{1}\{t = t^* + k\} \times \textit{Any Release}_n \times \textit{Pardoned Offender}_n \\ &+ \sum_{k=-5}^{10} \beta_k \mathbf{1}\{t = t^* + k\} \times \textit{Any Release}_n \\ &+ \alpha_n + \delta_t + \mu_{int}, \end{aligned} \tag{3}$$

where $\textit{Pardoned Offender}_n$ is an indicator equal to one if the released individual residing in neighborhood n was pardoned, and all other variables are defined as in equation 1. The

coefficients of interest are the θ_k, which capture the differential spillover effects associated with pardoned releasees relative to non-pardoned releasees. Evidence consistent with a reduction in deterrence would require these coefficients to be positive and statistically significant. By contrast, coefficients that are statistically indistinguishable from zero would indicate that spillover effects do not meaningfully differ by release type.

Figure A3 in Appendix A presents the corresponding event-study estimates. The figure shows no statistically significant differences between the spillover effects generated by pardoned and non-pardoned releasees at any post-release horizon. Consistent with this evidence, the p-value of the Wald test for the joint equality of all post-treatment coefficients ($k \geq 0$) is 0.93, indicating that the null hypothesis of equal effects cannot be rejected.

This result suggests that the increase in neighborhood arrests is not driven by pardon-induced reductions in deterrence. Spillover effects appear similar regardless of whether released offenders were pardoned or completed their sentences, pointing away from explanations based on changes in perceived punishment severity. While pardons are rarely used in this context and therefore unlikely to alter beliefs about future sanctions, the evidence above shows that pardon status does not differentially shape spillover effects.

4.6.2 Increase in Policing

Another potential concern is that the increase in arrests reflects changes in policing rather than behavioral responses to exposure to released offenders. If law enforcement intensified surveillance or enforcement in neighborhoods hosting released offenders, arrest rates could rise mechanically, independent of changes in criminal behavior.

To assess this possibility, I examine whether spillover effects differ by proximity to police stations in the three largest cities in Ecuador, where geocoded information on police infrastructure is available. If increased policing were driving the results, neighborhoods located closer to police stations would be expected to exhibit different post-release arrest patterns. The estimate on Figure 4 shows that there is no evidence of differential spillover effects by proximity to police stations, indicating that the results are not concentrated in areas with greater potential police presence.

Conceptual considerations further weaken the policing-based explanation. In this context, the neighborhood of residence often differs from the neighborhood where crimes are committed and arrests occur, particularly for economically motivated offenses such as robbery, which constitutes the largest share of the crimes associated with released offenders. For individuals not associated with high-severity offenses or high-profile investigations, it is unlikely that

police resources are devoted to sustained residential surveillance, which is instead typically oriented toward crime-prone locations rather than offenders' home neighborhoods.

Finally, annecdotal evidence is consistent with the absence of a policing response. In interviews conducted with a small sample of police officers and prosecutors, none reported being aware of the pardon at the time it was implemented. While anecdotal, this evidence suggests limited scope for coordinated changes in enforcement following the policy.

Taken together, these findings indicate that the observed increase in arrests is unlikely to be driven by changes in policing intensity.

4.6.3 Matched Controls:

Other 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 unobserved 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. Figure B1 show that the estimates are similar both in their dynamics and magnitude to the ones presented in the main results.

4.6.4 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 2 demonstrates 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 $t > 3$).

5 Criminal Peer Effects within Neighborhoods

Increased criminal activity following prisoner releases may arise through different channels. One possibility is direct transmission through social ties, whereby released offenders influence the behavior of individuals in their social networks (Gavrilova and Puca, 2022). A second possibility is indirect transmission, operating through broader changes in perceptions of criminal opportunities, social norms, or gang recruitment, which may affect individuals beyond the releasees' immediate social circles (Hipp and Yates, 2009).

In this section, I present evidence supporting the direct peer-based transmission hypothesis. While indirect mechanisms may contribute to aggregate neighborhood effects, I lack the variation required to separately identify changes in gang dynamics, criminal opportunities, or social norms that may drive these effects. Instead, I focus on whether exposure to released offenders translates into increased criminal behavior among individuals with pre-existing social proximity to the releasees.

I present four sets of results. First, I demonstrate the formation of criminal partnerships involving released offenders. Second, I show that spillover effects are larger among individuals embedded in the same pre-existing criminal networks and among those who grew up in the same neighborhood and belong to similar age cohorts. Finally, I find little evidence that spillover effects operate through family networks.

5.1 Formation of Criminal Partnerships

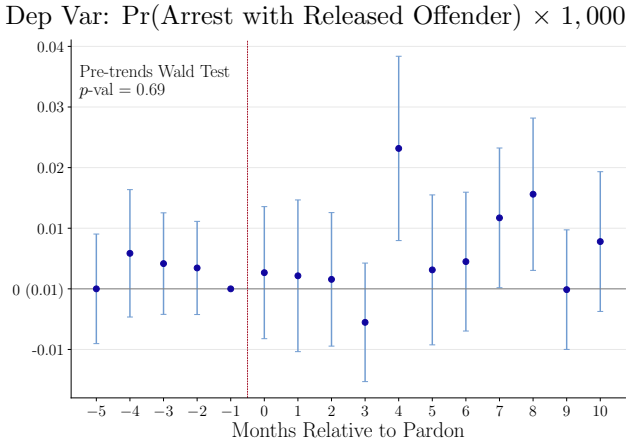
A first empirical test for direct peer-based transmission is to examine whether criminal activity following releases involves collaboration between released offenders and their neighbors. Upon reentry, released offenders may reconnect with prior associates or form new ties in their neighborhood, creating opportunities for co-offending. If spillovers operate through direct peer transmission we should observe an increase in arrests in which non-released residents are apprehended together with a recently released offender.

To test this mechanism, I re-estimate Equation 1 using as the outcome the probability that an individual is arrested in an incident that includes at least one person who exited prison within the previous year. This measure is agnostic about the underlying social network: joint offenders could be friends, relatives, former co-offenders, or new associates. The key point is that the outcome captures direct interaction in offending, regardless of the specific relationship.

I classify an arrest as involving a released offender if at least one arrestee was released from prison within the prior 12 months, so the set of "recently released" individuals updates each month and is not limited to pardon recipients. As in the main analysis, I exclude released offenders from the estimation sample to focus on spillovers onto non-released residents. Figure 5 reports the event-study estimate

The results show that exposure to released offenders is associated with an increase in criminal partnerships involving former inmates. Similar to the main specification, the effects become statistically significant starting four months after the pardon and decline thereafter. At the peak, individuals in treated neighborhoods are 0.023 percentage points (roughly twice

Figure 5: Formation of Criminal Partnerships



Notes: The figure displays the regression coefficients for the difference in the probability of being arrested alongside a released offender 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 the previous 12 months (relative to each month). The sample includes men aged 18-40 at risk of offending (without a degree or professional occupation), 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.

the outcome mean) more likely to be arrested alongside a recently released offender than comparable individuals in control neighborhoods.

Compared to the main spillover results -—which follow a smoother inverted-U pattern—- joint arrests display a sharper post-peak decline. One plausible explanation is short-run incapacitation mechanically embedded in this outcome: once a released offender and a potential partner are arrested in the same incident, both are temporarily removed from the street, reducing the likelihood of observing additional joint arrests involving that released offender (and possibly the same associates) in subsequent months.

As with the main effects, I further characterize the nature of these criminal partnerships by decomposing the estimates based on individuals' prior criminal history. This distinction is informative about the direction of peer-based transmission. Partnerships involving first-time offenders are more likely to reflect influence originating from released offenders, whereas partnerships involving individuals with prior records may capture the reactivation of pre-existing criminal ties or interaction among already criminally active peers. Table A2 in the appendix presents the difference-in-differences estimates corresponding to Equation 1. Combining the estimated coefficients with each group's population share, individuals without prior criminal experience account for 48% of the overall effect on the formation of criminal partnerships. This share is smaller than their contribution to the increase in overall arrests

(60%).

In sum, the results establish that part of the post-release criminal spillovers operates through direct collaboration between released offenders and other residents. At the same time, the analysis remains intentionally agnostic about the precise nature of the ties linking these individuals. In the next section, I impose additional structure on these interactions by examining whether spillovers are concentrated within pre-existing criminal networks.

5.2 Criminal Networks

Among the encounters that released offenders may have upon reentry, interactions with former criminal partners may be meaningful for the emergence of criminal spillovers. Such interactions involve contact with individuals who have previously engaged in criminal activity and who share a documented history of direct collaboration with the releasee. To examine this channel, this section tests whether individuals who belong to the same pre-pardon co-offending network as a releasee experience a higher likelihood of arrest relative to other previously arrested individuals who lack such ties.

Existing empirical work on peer effects among criminals has primarily focused on connections formed within prison facilities. Inmates exposed to higher-risk or more experienced peers while incarcerated are more likely to replicate their behavior after release (Bayer et al., 2009; Stevenson, 2017; Damm and Gorinas, 2020). Related evidence suggests that peer influences extend beyond prison walls, as the availability of criminal peers upon reentry affects recidivism decisions (Billings and Schnepel, 2022). Here, I depart from the focus on connections formed through incarceration to examine peer effects arising from networks constructed from joint arrest records. This approach offers two advantages. First, co-offending ties reflect demonstrated coordination in criminal activity rather than mere shared confinement. Second, because only a small fraction of arrests result in incarceration, arrest-based networks can provide a broader and more comprehensive view of criminal linkages in the population.

To operationalize this mechanism, I use arrest records prior to the pardon to construct co-offending networks. I first link any two individuals who were jointly arrested at least once, capturing direct prior collaboration. Then, I group individuals into the same network whenever they are connected through a chain of past joint arrests. Concretely, if two individuals were arrested together they are considered connected; if a third individual was arrested with either of them, all three are assigned to the same group, and this logic extends to larger chains. In this way, networks correspond to connected components of prior co-offending relationships. These structures reflect what the criminology literature refers to as co-offending

networks: groups of individuals whose relationships are revealed through joint participation in crime (Weerman, 2014; van Mastrigt and Carrington, 2014). While such networks may overlap with gangs or organized groups, they do not require formal membership or stable organizational identities. Instead, they capture revealed patterns of criminal collaboration and represent the underlying structure of prior criminal partnerships.

Across the full set of cases prior to the pardon approximately 20% of criminal cases involve two or more individuals, and 25% of all individuals ever processed by the justice system have been jointly arrested at least once. Applying the connected-component procedure described above yields 82,149 distinct criminal networks, with a median size of two members. Most networks therefore consist of dyadic ties rather than large or highly structured groups. As a result, the estimates in this section should be interpreted primarily as capturing spillovers operating through direct former criminal partners, rather than through complex organizational structures.

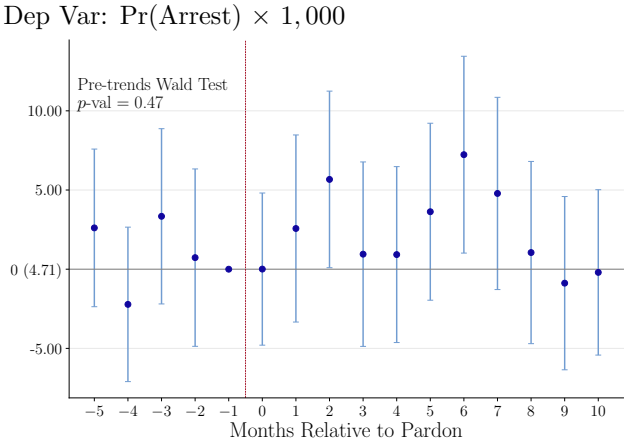
To test whether these ties mediate the spillover effects of prisoner releases, I estimate a heterogeneous event-study specification that interacts the neighborhood-level release indicator with an indicator equal to one if an individual belongs to the same pre-pardon criminal network as at least one releasee returning to that neighborhood ($Same\ Network_i$). I restrict the sample to individuals with prior arrest records, to ensure that the comparison does not confound network exposure with differences between individuals with and without prior criminal history. Specifically, I estimate:

$$\begin{aligned} y_{int} = &\sum_{k=-5}^{10} \theta_k \mathbf{1}\{t = t^* + k\} \times Any\ Release_n \times Same\ Network_i \\ &+ \sum_{k=-5}^{10} \beta_k \mathbf{1}\{t = t^* + k\} \times Any\ Release_n \\ &+ Same\ Network_i + \alpha_n + \delta_t + \mu_{int} \end{aligned} \tag{4}$$

In this specification, the coefficients θ_k capture the additional arrest likelihood among individuals in the same criminal network as returning offenders, relative to other previously arrested individuals in the same neighborhood who are not connected to the releasee. If criminal spillovers operate through prior collaborative ties, the value of θ_k should be positive.

Figure 6 presetns the θ_k estimates from equation 4. It shows that, following the pardon, individuals who belong to the same co-offending network as the released offender become more likely to be arrested relative to other previously arrested individuals in the same neighborhood who are not in the releasee's network. The effect is statistically significant two and six months

Figure 6: Heterogeneous Spillovers by Co-Offending Networks



Notes: The figure displays the event-study coefficients corresponding to the interaction between neighborhood-level exposure to a returning releasee and an indicator for belonging to the same pre-pardon co-offending network on the probability of arrest times 1,000 (i.e., the θ_k from Equation 4). The sample includes men aged 18-40 who had been arrested before the pardon and excludes 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.

after the pardon, with the probability of arrest increasing by approximately 0.5 percentage points (106% of the mean) for individuals embedded in the same criminal network.

Importantly, this specification allows me to quantify the contribution of co-offending networks to the overall spillover effect. In treated neighborhoods, approximately 5% of previously arrested individuals belong to the same pre-existing co-offending network as the released offender (s). To compute the contribution of criminal networks, I divide the weighted marginal effect of belonging to the same network ($s\theta_k$) by the overall effect on the population ($\beta_k + s\theta_k$). Averaging this ratio across post-treatment periods ($k \geq 0$) implies that, on average, 23% of the increase in arrests among prior offenders can be attributed to amplification within pre-existing co-offending networks.

Overall, the findings indicate that past co-offending networks play a central role in the propagation of spillovers from released offenders. Although only a small fraction of previously arrested individuals are directly connected to returning releasees (5%), this subgroup accounts for a disproportionate share of the aggregate increase in arrests among those with criminal experience (23%). I next examine a different form of connection, early-life social connections, to assess whether spillovers also operate through shared social proximity during childhood.

5.3 Early-life Social Connections

In this section, I explore whether the observed neighborhood spillovers operate through pre-existing social ties formed in early life. Individuals who grew up together in the same neighborhood were likely exposed to common schools, local institutions, and peer groups, increasing the probability that durable social ties emerged. If these ties persist, the return of a formerly incarcerated peer may reactivate prior networks, facilitating criminal collaboration.

To measure early-life connections, I combine information on individuals' neighborhood of residence during adolescence and their birth cohort. I proxy childhood location using the first neighborhood in which an individual is observed residing, which corresponds to age 16. Then, for each released offender, I identify residents in the reentry neighborhood who both (i) resided in the same neighborhood as the offender at age 16 and (ii) belong to the same birth cohort. By focusing on individuals who share both the same neighborhood of origin and the same birth cohort, the measure increases the likelihood of capturing individuals who may have interacted directly, for instance through attendance at the same local schools. In reentry neighborhoods, approximately 48% of residents share the same childhood neighborhood as the released offender, and about 15% of all residents share both the same childhood neighborhood and birth cohort.

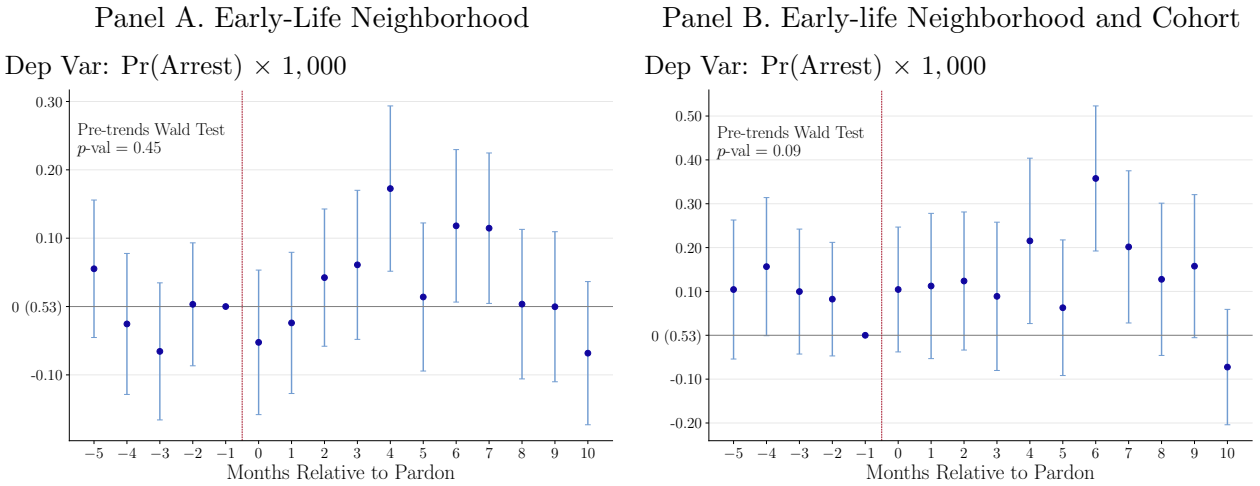
To assess whether spillovers operate through these early-life connections, I estimate equation 4 using two definitions for $Same\ Network_i$: (i) an indicator equal to one if the individual and the released offender resided in the same neighborhood at age 16, and (ii) an indicator equal to one if they both resided in the same neighborhood at age 16 and belong to the same birth cohort. Figure 7 presents the estimated event-study coefficients separately for each variable.

Figure 7 examines whether neighborhood spillovers are stronger among individuals who share early-life exposure with the released offender. Panel A shows that residents who shared the same neighborhood of residence at age 16 as the released offender experience a larger increase in arrest probability following reentry relative to other residents in the same treated neighborhood. At the peak of the response, these individuals are approximately 0.015 percentage points more likely to be arrested (21% of the baseline mean). Panel B further restricts the definition of early-life exposure to individuals who share both the same neighborhood at age 16 and the same birth cohort. Under this narrower definition, the differential effect is larger: at its peak, arrest probabilities increase by approximately 0.02 percentage points (38% of the baseline mean) relative to other residents. Further, in both panels, the dynamic pattern mirrors the main results, displaying an inverted U-shape with effects emerging shortly

after reentry and peaking between four and seven months after the release.

To quantify the importance of these early-life connections, I decompose the overall post-release increase in arrests within treated neighborhoods into the component attributable to early-life connected residents and the remainder attributable to other residents. Using the share of individuals who share both the same neighborhood of origin and birth cohort with the released offender (15%), early-life connections account for approximately 20% of the cumulative post-release increase in arrests. Focusing on the peak response window (months 4 to 7), this share rises slightly above 20%. Despite representing a relatively small fraction of neighborhood residents, individuals connected through early-life exposure explain a disproportionate share of the post-release increase in arrests. Taken together with the previous evidence on criminal networks, these results reinforce the interpretation that social ties formed prior to incarceration structure the transmission of post-release spillovers.

Figure 7: Spillovers on Early-Life Connected Peers



Notes: The figure displays the event-study coefficients corresponding to the interaction between neighborhood-level exposure to a returning releasee and an indicator for sharing the same childhood neighborhood (Panel A) or sharing both the same childhood neighborhood and birth cohort (Panel B) on the probability of arrest times 1,000 (i.e., the θ_k from Equation 4). The sample includes men aged 18-40 at risk of offending (without a degree or professional occupation), 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.

5.4 Family Networks

The final potential source of peer effects I examine is family ties of released offenders within their neighborhood. Similar to early-life peers and prior criminal associates, family members

constitute a set of social relationships through which spillovers may propagate. Because family ties often involve repeated interaction, and shared resources, the return of an incarcerated relative may directly alter the opportunities or incentives for criminal activity among close kin. To identify family relationships, I rely on administrative records containing parental information for all individuals in the sample, and is maintained by the Ecuadorian Civil Registry. This registry is created at the time of birth registration and, for each individual, records the identity of both parents.

Using this information, I reconstruct the complete family tree of each released offender and determine whether individuals residing in the releasee's reentry neighborhood belong to their family network, as well as the degree of kinship between them. Degrees of kinship are calculated following the civil-law convention, which counts the number of vertical steps separating two individuals in the family tree. For example, siblings are two degrees apart, while first cousins are four degrees apart.

Despite the comprehensive reconstruction of family trees, relatives of released offenders represent only a small fraction of neighborhood residents. Even when extending the definition of kinship to include relatives up to five degrees of separation, there are on average ten relatives living in the releasee's reentry neighborhood, representing less than 1% of the neighborhood population. Most of these relatives correspond to distant kin such as cousins and extended family members, while immediate relatives represent a substantially smaller subset of neighborhood residents.

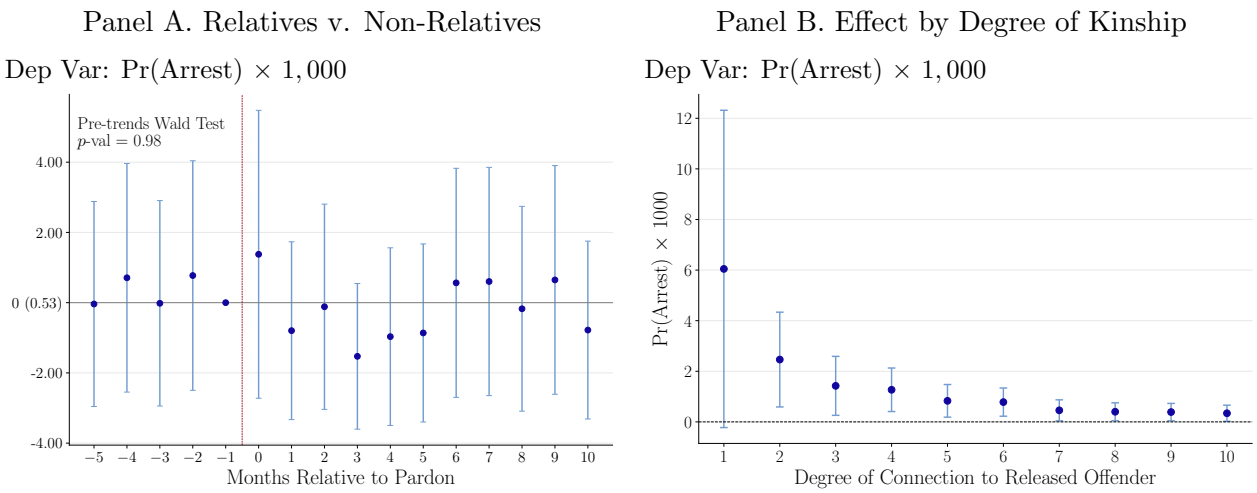
To examine whether relatives residing in the same neighborhood as the released offender are affected by their return, I present two pieces of evidence. First, I estimate Equation 4 using an indicator equal to one for residents within five degrees of kinship of the released offender and zero for non-relatives. This specification focuses the comparison on individuals most likely to maintain active contact with the releasee. Second, I estimate a series of difference-in-differences specifications in which I vary the degree cutoff used to define the treatment group. For each cutoff $d \in \{1, \ldots, 10\}$, the sample includes residents whose degree of kinship to the releasee is at most d and non-relatives, with residents at degrees strictly above d excluded. Each specification recovers the aggregate post-release differential in arrest rates between family members within d degrees and non-relatives, within the same treated neighborhoods.

Figure 8 presents the results. Panel A reports the event-study estimates from Equation 4. The post-release coefficients are imprecisely estimated, reflecting the small number of relatives residing in treated neighborhoods, and do not display a clear or systematic increase

following reentry. The confidence intervals are large relative to the baseline arrest rate, which reflects the limited number of relatives living in treated neighborhoods. This absence of precision should be interpreted with caution. Because relatives represent only a small share of neighborhood residents, the event-study specification has limited statistical power to detect month-to-month changes in arrest rates for this subgroup.

Panel B addresses this limitation by aggregating evidence across post-release periods. The figure plots the estimated difference-in-differences coefficient as a function of the kinship-degree cutoff used to define the treatment group, together with 95% confidence intervals. The estimates display a clear gradient: the effect is largest when the treatment group is restricted to first-degree relatives and gradually declines as more distant relatives are included, approaching zero as the cutoff extends to higher degrees of kinship. For instance, relatives within one degree of the releasee are 0.6 percentage points more likely to be arrested than non-realtives on treated neighborhoods, while the corresponding number for those with 5 degrees of separation is approximately 0.1 percentage points. This gradient is consistent with the influence associated with the releasee declining as kinship distance increases.

Figure 8: Spillovers on Relatives



Notes: Panel A displays the event-study coefficients corresponding to the interaction between neighborhood-level exposure to a returning releasee and an indicator for being within five degrees of kinship of the released offender on the probability of arrest times 1,000 (i.e., the θ_k from Equation 4). Residents more distantly related than five degrees are excluded from the sample. Panel B plots the difference-in-differences estimate from a separate specification for each degree cutoff d, where the treatment group includes residents within d degrees of kinship and the control group consists of non-relatives; residents at degrees strictly above d are excluded. In both panels the sample includes men aged 18-40 at risk of offending (without a degree or professional occupation), excluding released offenders. In Panel A, the coefficient at $t = -1$ is normalized to zero and the mean of the dependent variable at $t = -1$ is shown in parentheses on the y-axis. Bars and brackets represent 95% confidence intervals, with standard errors clustered at the neighborhood level.

Taken together, the two panels suggest that family ties constitute a transmission channel for post-release spillovers, albeit a relatively narrow one. The gradient by kinship distance indicates that individuals more closely connected to the releasee experience stronger exposure associated with their return. At the same time, the small share of relatives residing in the neighborhood, even under broader definitions, implies that this mechanism operates through a concentrated subgroup of residents and cannot account for the aggregate neighborhood effects documented earlier.

5.5 Discussion

Taken together, the evidence in this section establishes that neighborhood spillovers from released offenders are not a diffuse neighborhood-level phenomenon but are instead concentrated among individuals with pre-existing social proximity to the releasee. The formation of criminal partnerships confirms that the spillover involves direct interaction rather than a parallel individual response to shared environmental conditions. Across all three dimensions of social ties examined (criminal networks, early-life connections, and family networks) the same pattern recurs: effects are disproportionate to the population share of each subgroup. This structure has implications for how these results relate to the broader literature on peer effects in crime. That literature has established, primarily using within-prison variation, that exposure to criminal peers affects behavior (Bayer et al., 2009; Stevenson, 2017; Damm and Gorinas, 2020). The mechanism posited there is accumulation of criminal capital or norm diffusion during shared confinement. The evidence here points to a distinct but complementary process: the reactivation of ties formed before incarceration. Pre-incarceration co-offending networks and shared early-life environments (not prison-based co-exposure) structure the transmission of post-release spillovers. For instance, the relative weakness of the family channel, despite its potential for repeated interaction and shared resources, suggests that the criminogenic quality of the tie, rather than its social closeness, is what drives heterogeneity in spillover exposure.

What the results presented in this section do not resolve is the precise behavioral channel through which reactivated ties translate into criminal activity. The literature identifies at least three candidates: information and skill transmission, which lowers the costs of offending (Bayer et al., 2009; Damm and Gorinas, 2020); normative or attitudinal influence, which shifts the perceived legitimacy of crime (Stevenson, 2017; Drago and Galbiati, 2012; Perez-Truglia and Troiano, 2018); and network reactivation, which expands the opportunity set through new or renewed co-offending partnerships (Mastrobuoni and Rialland, 2020). The concentration of effects within close social networks and the increase in joint arrests are

most directly consistent with the first and third channels. However, identifying which of these specific mechanisms drives the peer effects is beyond the scope of this paper. What the evidence does make clear, however, is that the spillovers generated by prisoner reentry operate through existing social structures rather than through neighborhood exposure alone.

6 The Role of Prisons

In this section, I present evidence that the prison experience of released offenders shapes the magnitude of the neighborhood crime spillovers. I examine two dimensions of incarceration: the time an offender served, and the availability of rehabilitation programs during their prison stay. Both mechanisms are motivated by a growing literature linking favorable prison conditions to a decrease in post-release criminal behavior (Alsan et al., 2025; Arbour et al., 2024; Tobón, 2022; Bhuller et al., 2020).

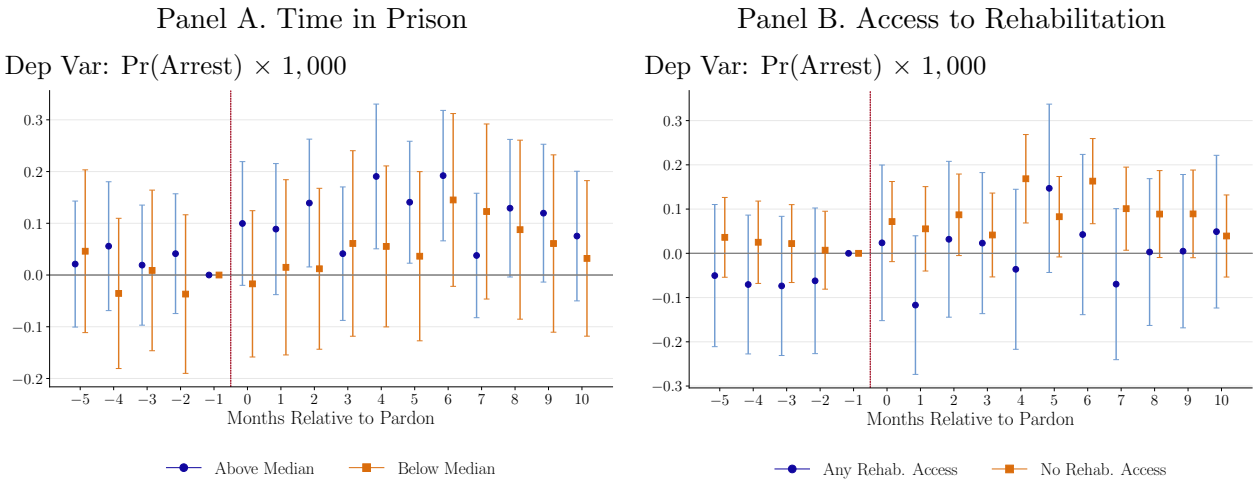
Time in Prison: Longer exposure to incarceration can amplify the criminogenic effects of prison through deeper integration into inmate criminal networks, stronger internalization of criminal norms, and greater barriers to reintegration into legal labor markets upon release. To examine whether this mechanism is relevant for neighborhood spillovers, I estimate whether treated neighborhoods that received offenders with longer prison sentences exhibit larger post-pardon increases in crime.

A key challenge in identifying this effect is that, in the general population of released offenders, sentence length conflates the severity of the underlying crime with individual criminal ability, both of which can predict post-release behavior independently. I address this concern by restricting the treated sample to neighborhoods that received *pardoned* offenders only. Among pardoned individuals, time served is determined solely by the timing of their arrest relative to the pardon date: pardoned releases were granted uniformly, so the variation in time served across pardoned offenders reflects when they entered prison, not their behavior or rehabilitation inside. This restriction isolates the incarceration channel from selection on criminal ability. Appendix Figure A4 displays the distribution of time served among this sample of pardoned individuals.

Panel A of Figure 9 presents stratified event-study estimates for two groups of treated neighborhoods: those that received a pardoned offender who served *above* the sample median ($\sim$ 24 months) in prison time (blue circles), and those that received one who served *below* the median (orange squares). Both regressions use the same pool of control neighborhoods. The results reveal that the post-pardon rise in neighborhood crime is concentrated entirely among neighborhoods whose returning offenders had longer sentences. In these neighborhoods, the

probability of arrest rose by an average of 0.008 percentage points (16% of the mean). Neighborhoods whose returnees served shorter sentences show no statistically significant change in arrest rates. This pattern suggests that incarceration itself, rather than the intrinsic criminal characteristics of offenders, is a driver of the spillover effects.

Figure 9: Incarceration Conditions



Notes: Both panels display event-study coefficients for the difference in the probability of arrest (multiplied by 1,000) between individuals in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). Panel A stratifies treated neighborhoods by the time served in prison of the pardoned offenders they received: circles represent neighborhoods where the longest-serving pardoned offender was *above* the sample median in prison time; squares represent those *below* the median. The treated sample is restricted to neighborhoods receiving only pardoned (conditional) releases. Panel B stratifies treated neighborhoods by whether the pardoned offenders they received came from a provincial prison with *any* inmate participation in formal rehabilitation programs (circles) or *no* recorded participation (squares), based on the 2022 Prison Census. Both panels pool control neighborhoods into each stratified regression. All confidence intervals are at the 95% level, with standard errors clustered at the neighborhood level.

Access to Rehabilitation Programs: I next examine whether access to rehabilitation inside prison moderates the spillover effects. If programs that prepare inmates for productive reintegration, such as vocational training and formal education, are effective, neighborhoods whose returnees participated in such programs should exhibit smaller criminal spillovers.

To measure rehabilitation access I use the 2022 Ecuadorian Prison Census (*Censo Penitenciario*), which records individual-level self-reported participation in institutional programs. I construct a prison-level rehabilitation index as the share of inmates in each prison who participated in either vocational labor training or educational activities. Then, for each treated neighborhood, I take the median of this index across all pardoned offenders released there during the pardon window. I then classify treated neighborhoods into two groups: those whose returnees came from a prison with *any* recorded rehabilitation participation (index > 0), and those from a prison with *no* recorded participation (index $= 0$).

Panel B of Figure 9 presents the stratified event-study estimates for each of the two groups. Blue circles correspond to treated neighborhoods whose pardoned returnees came from prisons with at least some rehabilitation programming, and orange squares correspond to neighborhoods whose returnees came from prisons with no recorded rehabilitation access. The two series tell a different story. Neighborhoods receiving offenders from prisons with no rehabilitation access exhibit a statistically significant and persistent increase in arrest rates following the pardon. By contrast, neighborhoods whose returnees had access to rehabilitation programs inside prison show no significant post-pardon effect, with estimates undistinguishable from to zero throughout the post-period.

Taken together, the two panels of Figure 9 point to incarceration experience as an important mediating factor in the transmission of criminal behavior from returning offenders to their communities. Longer time in prison amplifies the spillover, consistent with the hypothesis that extended incarceration deepens criminal human capital and weakens ties to legal employment. Access to rehabilitation programs inside prison attenuates the spillover, suggesting that structured reintegration programming can meaningfully limit the spillovers that returning offenders impose on their neighborhoods.

7 Conclusions

Incarceration has long been the default crime control tool in Latin America, where "Mano Dura" policies have driven incarceration rates to record levels without a commensurate investment in rehabilitation (The Economist, 2024). This paper argues that the social cost of this approach extends beyond the released offender. Each prisoner release generates spillover effects on the surrounding community, and approximately 60% of those effects fall on individuals with no prior criminal record. Standard assessments of recidivism, which focus on the likelihood that released offenders reoffend, thus capture only part of the cost. The evidence presented here implies that the total increase in arrests attributable to a prisoner release is 0.49 per 1,000 residents per month, with spillovers to non-released individuals accounting for 84% of this total. Policies designed around recidivism will therefore miss the majority of the social cost of prisoner reentry.

Spillovers are concentrated among former criminal partners and individuals who shared early-life social environments with the released offender, while family ties generate no statistically significant effects. This pattern suggests that what is being transmitted is not social deviance in a diffuse sense, but criminal opportunity and co-offending coordination through pre-existing networks. Two implications follow. First, the temporal pattern of the spillovers

(emerging gradually, peaking between four and six months after release, and fading by the ninth month) is consistent with a process of network reactivation followed by disruption as joint offenders are re-arrested. This argues against persistent neighborhood-level changes, such as shifts in gang territory or long-run norm change, as the primary driver. Second, it is unlikely that these spillovers compound into more persistent changes. The share of arrests resulting in conviction and incarceration is less than 10%, making it unlikely that newly drawn individuals are themselves incarcerated, released, and generate a second round of spillovers.

The evidence on prison conditions points to a direct policy lever. Offenders released from prisons with vocational training or educational programs generate no statistically significant neighborhood spillovers, while those from prisons without such programming do. This finding implies that rehabilitation programs carry external benefits that extend beyond the participating inmates. Combined with the result that longer prison spells amplify spillovers, the evidence suggests that the criminogenic effect of incarceration is not immutable: it depends on what happens inside prison. A longer sentence in a rehabilitative environment may generate smaller community spillovers than a shorter sentence in one that is not.

Several questions remain open. This paper establishes that co-offending networks and early-life connections are channels of transmission, but it cannot separately identify the underlying mechanism: whether the transmission operates through information and criminal skill transfer, through the reactivation of co-offending partnerships, or through shifts in perceived norms and returns to crime (Bayer et al., 2009; Stevenson, 2017; Drago and Galbiati, 2012). Distinguishing among these channels would sharpen the policy implications, since each calls for a different form of intervention.

The broader lesson is that the effectiveness of incarceration as a crime control policy depends on what happens at both ends of the sentence. A criminal justice system that incarcerates without rehabilitating and releases without supporting does not simply fail to reduce crime; it actively generates it, by drawing in individuals who would not otherwise have offended. These external costs should be incorporated into evaluations of incarceration policy, and the evidence here suggests that investments in prison rehabilitation programs represent one of the more direct ways to reduce them.

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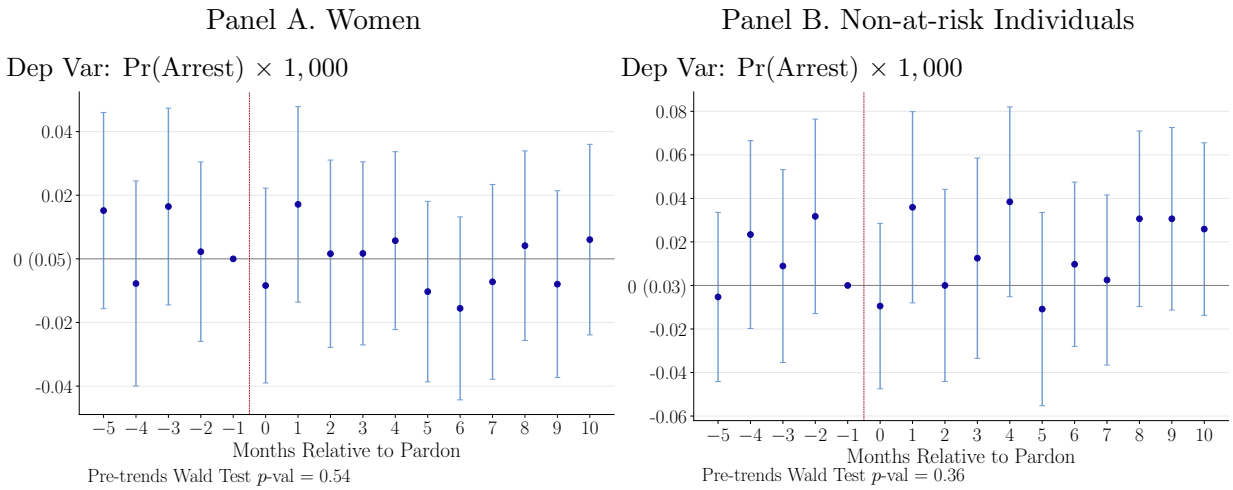
A Additional Results

Table A1: Changes in Exposure to Released Offenders

	Release Rate (1)	Number of Releases (2)	Any Release (3)
Treated × Post Pardon = 1	0.171***	0.106***	0.098***
	(0.015)	(0.012)	(0.009)
N. Neighborhoods	1,786	1,786	1,786
Mean Dep. Var.	0.165	0.185	0.151
Observations	28,576	28,576	28,576

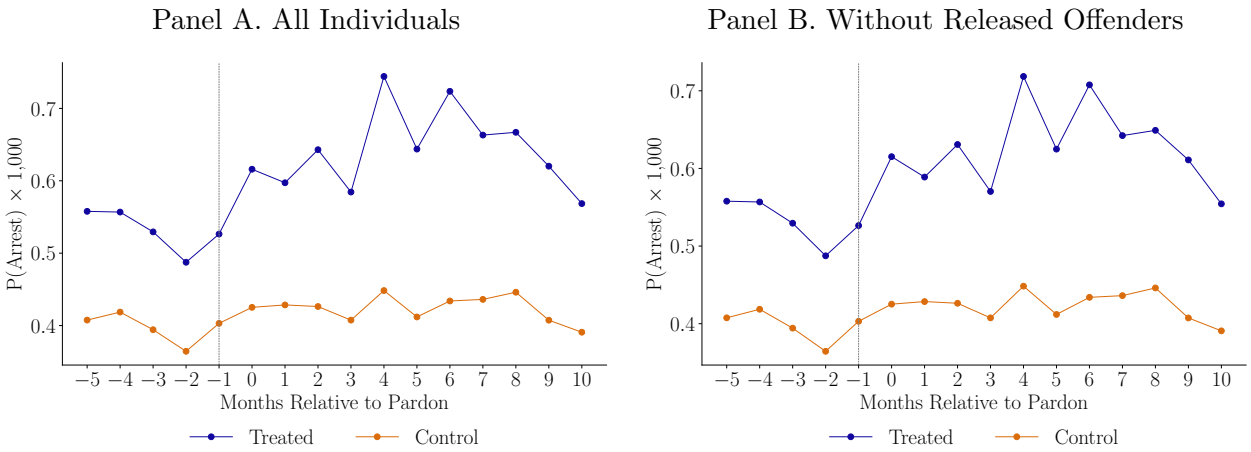
Notes: The table shows the difference-in-difference coefficients of the effect of the pardon between treated and control neighborhoods. The unit of observation is a neighborhood-by-month pair. Column 1 uses as outcome the release rate per 1,000 inhabitants, Column 2 uses the number of releases, and Column 3 uses an indicator for receiving at least one releasee. The sample includes all neighborhoods that received at least one released offender between 2016 and 2021. The time frame of reported is between September 2021 ($t = -5$) and December 2022 ($t = 10$). Standard errors clustered by neighborhood in parentheses. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Figure A1: Spillovers for Non-Risk Populations



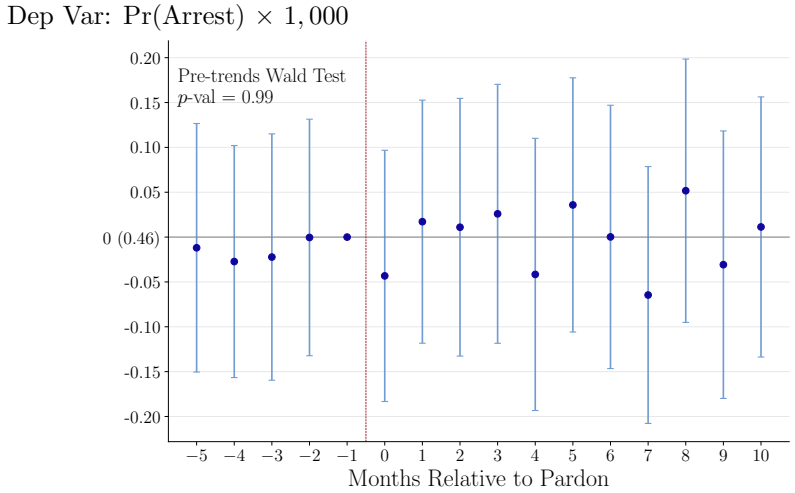
Notes: The figure plots the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) between individuals living in treated and control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). Panel A reports estimates for all women, and Panel B reports estimates for individuals not at risk, i.e., those with post-secondary education or a professional or managerial occupation. Released offenders are excluded from both panels. The coefficient at $t = -1$ is normalized to zero. Bars represent 95 percent confidence intervals, with standard errors clustered at the neighborhood level.

Figure A2: Probability of Arrest - Raw Means



Notes: The figure plots raw mean arrest probabilities (multiplied by 1,000) for men aged 18 to 40 at risk of offending (without a degree or professional occupation). The data covers the period from September 2021 ($t = -5$) to December 2022 ($t = 10$). The sample includes all neighborhoods that received at least one released offender since 2016.Treated neighborhoods are defined as those corresponding to the pre-incarceration place of residence of offenders released between February and April 2022. Panel A reports means for the full sample, including released offenders, while Panel B excludes released offenders from the sample.

Figure A3: Differential Effects Between Pardoned and Non-Pardoned Releasees



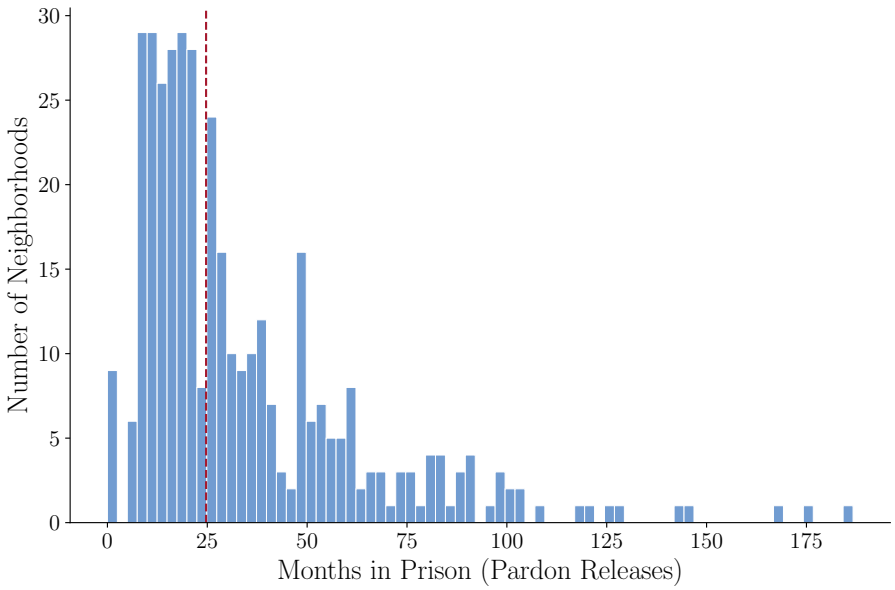
Notes: The figure plots the event-study coefficients for the difference in the effect of the pardon on the probability of arrest (multiplied by 1,000) between neighborhoods where the returning offender was pardoned and those where the offender completed the sentence (i.e. the θ_k from equation 3). The sample includes men aged 18-40 at risk of offending (without a degree or professional occupation), excluding those released from prison. Neighborhoods that received both a pardon and non-pardoned criminals are excluded from the sample. 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.

Table A2: Formation of Criminal Partnerships

	Dep. Var.: Pr(Arrest with Releasee) × 1,000		
	All Individuals (1)	No Prior Arrests (2)	Prior Offenders (3)
$\text{Treated}_n \times \text{Pardon}_t$	0.0034	0.0017	0.0331
	(0.0022)	(0.0015)	(0.0429)
N. Neighborhoods	1,786	1,786	1,780
Mean Dep. Var.	0.009	0.005	0.135
Observations	30,868,670	29,773,567	1,095,103

Notes: The table reports difference-in-difference estimates of the effect of the pardon on the probability of arrest with a released offender, multiplied by 1,000. The unit of observation is the individual-by-month pair. Column (1) includes all men aged 18-40 at risk of offending (without a degree or professional occupation). Column (2) restricts the sample to individuals without prior arrest records, while Column (3) includes only prior offenders. All three columns focus only on spillovers, excluding released offenders from the analysis. The time frame spans from September 2021 ($t = -5$) to December 2022 ($t = 10$). Standard errors clustered by neighborhood in parentheses. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

Figure A4: Distribution of Time Served



Notes: The figure plots the distribution of the effective time served in months for the pardoned offenders. In case there was more than one pardoned offender released to the same neighborhood, the figure plots the maximum time served among all released offenders. The vertical dashed line represent the median of the distribution ($\sim$ 24 months).

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

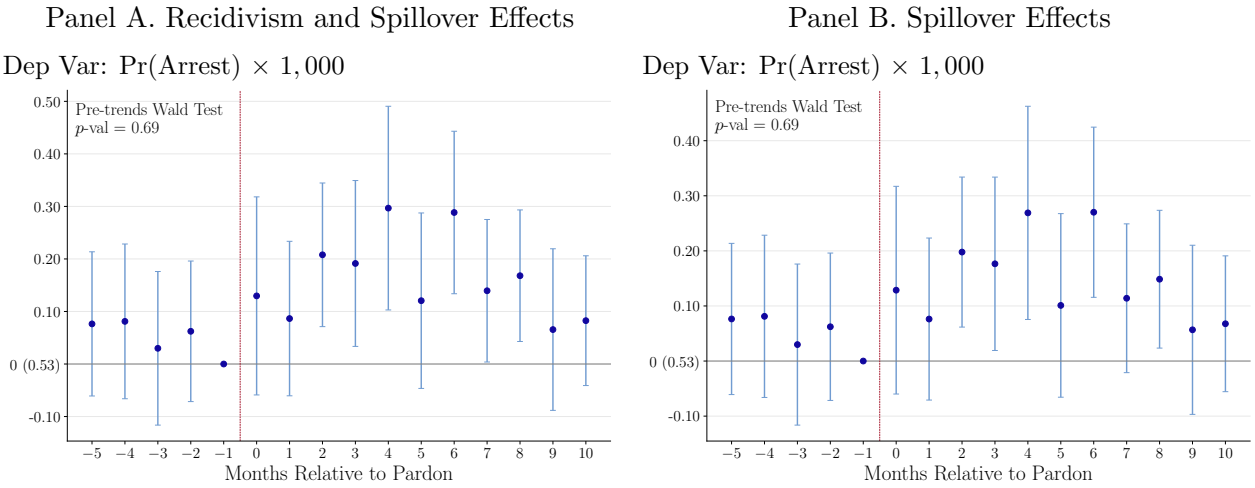
I use nearest-neighbor propensity score matching to pair each of the 706 neighborhoods that received a released offender during the time the pardon was active with a control neighborhood. The possible control group comprises all neighborhoods that did not receive a released offender within three months of the pardon (N = 1,080). 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 and 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 share of people with formal employment, average years of education, and measures of public services provision.

Using the predicted values (propensities) from this model, I matched each treated neighborhood with the untreated neighborhood with the closest propensity score without replacement, with a maximum caliper of 0.05. The final matched sample comprises 1,252 events, representing 626 treated neighborhoods and 354 unique control neighborhoods. On average, each control neighborhood appears 1.7 times in the sample. Figure B1 replicates the main estimation of the paper.

B.2 Results

Figure B1: Effects of Released Offenders on Arrests



Notes: The figure plots the regression coefficients for the difference in the probability of arrest (multiplied by 1,000) between people living in treated and matched control neighborhoods, relative to the month before the pardon (i.e., the β_k from Equation 1). Treated neighborhoods are those where offenders released under the pardon resided at the time of their arrest. Panel A reports estimates for men aged 18-40 who are at risk of offending (without a degree or professional occupation), including released offenders. Panel B excludes released offenders from the sample. 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. Bars denote 95 percent confidence intervals, with standard errors clustered at the neighborhood level.