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Criminal capital persistence: Evidence from 90,000 inmates' releases*

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Abstract

We study persistence in criminal capital by looking at the effects of inmates' releases on crime around prisons in Colombia. Leveraging detailed geographic and temporal information on the universe of releases from all prisons and crime reports, we find that property crimes are 16% higher around prisons on days inmates are released. Inmates specialized in property crimes drive the impacts. Improvements in non-criminal human capital or longer incarceration spells do not mitigate these effects. These results suggest the specific deterrence or rehabilitation effects of incarceration are weak for individuals with higher initial levels of criminal capital. We also document two externalities resulting from incarcerating specialized criminals. First, we find evidence of adverse peer effects. Second, in a back-of-the-envelope estimation, we document that crime incidence due to prison location drops property values and property tax revenues. Our results raise concerns about the usefulness of incarceration in its most widely adopted form.

JEL codes: D04, H41, J24, K14, K42
Keywords: crime, violence, recidivism, prisons, Colombia

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1 Introduction

The prison population in the world exceeds 10 million people.[1] Every day, tens of thousands of inmates are released. In the U.S. alone, more than 600 thousand people exit prison every year.[2] A central role of incarceration is to deter offenders who face prison from engaging in repetitive criminal behavior—a process known as 'specific deterrence', in contrast with the 'general deterrence' effect of the threat of punishment. Another role of incarceration is to improve non-criminal human capital by means of prison-based educational and occupational programs.[3] Nonetheless, recidivism rates seem dramatically high. Two-year re-conviction rates range from 20% to 60% in most of North America and Europe, where data is available.[4]

Incarceration could affect the probability of recidivism through several ways. Past literature focuses, for instance, on changes in the severity of punishment (e.g., Drago, Galbiati and Vertova 2009; Hansen 2015; Mueller-Smith and Schnepel 2021; Tobón 2020), changes in the probability of punishment (e.g., Anker, Doleac and Landersø 2017; Doleac 2017), the expansion of criminal capital (e.g., Aizer and Doyle Jr 2015; Mueller-Smith 2015), or changes in access to and earnings in the legal sector (e.g., Bhuller et al. 2020; Kling 2006; Schnepel 2018).[5] However, we know little about the specific deterrent and rehabilitation effects of incarceration on highly skilled criminals. We address this question by studying the short-term impacts of inmates' releases on crime around prisons in Colombia.

We leverage two features of the Colombian context. First, we use specific coordinates and event dates to link detailed information on the universe of inmates' releases with the universe of property crime reports. We build a panel dataset of up to 5km buffers around all 138 Colombian prisons over more than 1,300 days, from January 2013 through September 2016. This data aggregates information on roughly 90,000 releases and 250,000 reported crimes.

[1] See Walmsley (2013) for detailed statistics in 223 countries.

[2] See for instance Bronson and Carson (2019).

[3] These effects are rooted in the economic theory of crime introduced by Becker (1968) and Ehrlich (1973).

[4] See Fazel and Wolf (2015) for a systematic review on recidivism rates. The range is for Canada, Denmark, Finland, Iceland, the Netherlands, Norway, Sweden, the U.K. and the U.S.

[5] See Doleac (2019) for a thorough review of this literature.

Second, we address endogeneity concerns by leveraging the quasi-random selection of release days in each prison. Prison officials process releases on-demand, as convicted inmates meet the requirements of their sanction, or in-trial inmates are sentenced to time served or reach a non-guilty verdict. Judicial authorities make the requests as cases are solved and prison officials cannot delay the release date. Consistent with an exogenous selection process, we show that conditional on prison location and day fixed effects, prison characteristics are similar between days with or without releases.

This paper documents six sets of findings. First, property crime reports increase around prisons on days inmates are released. Using 5km buffers, people report 0.13 more crimes around prisons on release days than non-release days. The coefficient is statistically significant at conventional levels. This magnitude is equivalent to a 16% increase in crime reports relative to the average number of crimes reported in non-release days. Furthermore, one additional release is associated with a rise of 0.08 crime reports around prisons (10% relative to the control mean), also statistically significant at conventional levels.

Second, inmates specialized in property crimes drive the impacts. Relative to non-release days, people report 0.20 more property crimes when any inmate convicted or tried for property crimes is released—statistically significant—but remain unchanged on days other inmates exit prison. The difference between both treatment effects is statistically significant at conventional levels. Relative to the average number of property crimes reported on non-release days, crimes increase by 26% on days specialized criminals are released. Also, one additional release of a specialized inmate is associated with 0.09 more crime reports around prisons (12% relative to the control mean). When one additional, non-specialized inmate is released, crime reports increase by 0.06 (7% relative to the control mean). While both effects are precisely estimated, the difference between the two coefficients is statistically significant at conventional levels. In our interpretation, these results suggest the specific deterrent effect of incarceration is weak, at best, for offenders with higher initial levels of criminal capital.

Third, improvements in non-criminal human capital do not seem to mitigate these effects. We examine if the marginal effect of one additional release of a specialized criminal is lower whether the inmate participated in rehabilitation programs or reached a higher educational attainment at the time of release. We find that crime reports around prisons are not lower whether the specialized inmate was or was not enrolled in educational or occupational training programs while serving time. We also find that crime reports are roughly the same whether the specialized inmate had a higher or lower level of educational attainment at the time of release. We cannot argue we identify a causal relationship in this case, because inmates self-select to participate in rehabilitation programs or to pursue a higher educational attainment in prison. Yet, within the group of specialized criminals, those self-selecting to improve their non-criminal human capital are presumably less likely to recidivate. Hence, these results suggest criminal capital persists and can even outweigh improvements in non-criminal human capital.

Fourth, longer incarceration spells do not seem to mitigate the effects either. We also examine whether the marginal effect of one additional release of a specialized criminal diminishes if the inmate served more time. The results suggest crime reports around prisons remain unchanged whether the specialized inmate served time for a longer or shorter period. The selection problem in this case is more subtle because inmates serving more time might already be more willing to re-engage in crime. Nonetheless, these results further suggest the specific deterrent effect of incarceration is weak for specialized criminals.

Fifth, we document large adverse peer effects. We study whether confinement with specialized criminals is associated with higher levels of criminal activity on release days. We find that one additional release of an offender who was confined with inmates convicted or tried for property crimes is associated with 0.06 more crimes around prisons on release days (roughly 7%, relative to the control mean). This result is statistically significant at conventional levels. Non-specialized inmates drive these effects. This implies specialized inmates do not seem to develop additional criminal skills, but others do if they are in close contact with specialized criminals. Prison authorities assign inmates to prison wings and cells following several legal and availability constraints. While this process is not random, it is unlikely that inmates who do not have specialized criminal skills for property crimes but have potential are housed with specialized offenders. Hence we interpret this result as suggestive evidence of criminal capital production resulting from interaction with peers.

Finally, we further exploit our setting to examine one additional externality of criminal capital persistence: the effects of prison location on housing prices. We conduct a back-of-the-envelope analysis of the aggregate effect of inmates' releases on crime in the surroundings of prisons. Since specialized criminals exit prison in roughly 15% of all prison-days in our sample, we estimate release days account for about 4% of all property crimes in the 5km areas around the 138 Colombian prisons. Morales-Mosquera (2021) estimates that the elasticity of property values with respect to crime in the three largest Colombian cities is -0.24. This implies that property values in the surroundings of prisons lose about 1% of their value due to negative externalities of prison locations. Using official data on property values and urban density, we estimate that the aggregate effect of current prison locations on property values amounts to a loss of \$15.9 billion, adjusting for purchasing power parity. This also implies a loss of roughly \$78 million per year on property tax revenues, on average.

This paper contributes to a few strands of the literature. First, studies examining the specific deterrent effects of incarceration. Munyo and Rossi (2015) leverage the quasi-random selection of release days to study first-day criminal recidivism in Montevideo, Uruguay. They document that roughly one in four released offenders recidivates on the first day. Green and Winik (2010) use random judge assignments to estimate the deterrent effect of incarceration on recidivism in the District of Columbia, and find that incarceration does not deter subsequent criminal behavior. Mueller-Smith (2015) uses random courtroom assignment to study the effects of incarceration on recidivism in Harris County, Texas, and finds that incarceration generates net increases in recidivism. Rose and Shem-Tov (2021) use discontinuities in North Carolina's sentencing guidelines to examine the effects of incarceration on recidivism. They find sizable specific deterrent effects in the short term that decrease over time.

Their results also suggest that these effects diminish in incarceration length. Abrams (2011) leverages the randomization of public defenders to study the effects of incarceration on recidivism in Clark County, Nevada, and finds that incarceration has a mild deterrent effect that rapidly diminishes.[6] Our findings are consistent with Munyo and Rossi (2015), Green and Winik (2010) and Mueller-Smith (2015). They differ from Rose and Shem-Tov (2021) and Abrams (2011). We extend this literature by examining incarceration effects for highly skilled criminals, documenting that specific deterrence effects for this population seem weak or even non-existent.

Second, studies focusing on the effects of educational and training interventions on subsequent criminal behavior. Bhuller et al. (2020) use random judge assignment in Norway to examine the effects of imprisonment on recidivism. Their results suggest that incarceration reduced recidivism for inmates who were previously unemployed because they enrolled in employment programs in prison. Kuziemko (2013) exploits discontinuities in the parole guidelines in the state of Georgia to study the effects of incarceration and parole on recidivism. She finds that inmates who could not receive parole due to good behavior reduced their participation in rehabilitation programs and increased recidivism.[7] Our results suggest improvements in non-criminal human capital are less promising in other contexts. Prison conditions in Colombia are relatively harsh, and the quality of educational and occupational prison-based training programs is low. We find that these prison-based interventions have no effect on mitigating subsequent criminal behavior for specialized criminals.

Third, studies on peer effects in the production of criminal capital within prison. Bayer, Hjalmarsson and Pozen (2009) exploit within facility variation in exposure to peers to esti-

[6] Other studies focus on the deterrent effects of the threat of increasing sanctions. Drago, Galbiati and Vertova (2009) exploit the 2006 Italian clemency bill and find that one additional month in expected sentence reduces the probability of recidivism. Helland and Tabarrok (2007) use the fortuitous randomization of trial outcomes in California and find that the introduction of three-strikes legislation sanctions reduced recidivism for offenders with two strikes. Mueller-Smith and Schnepel (2021) leverage abrupt changes in the probability of diversion in the criminal justice system in Harris County, Texas, and find that increased sanctions for subsequent offenses reduced recidivism.

[7] Similarly, Landersø (2015) leverages an exogenous increase in incarceration length in Denmark and finds that inmates who served more time had better employment outcomes likely because of rehabilitation.

mate the peer effects on subsequent criminal activity using data from Florida. Their results suggest that juveniles exposed to peers who committed the same offense recidivate more. Stevenson (2017) use a similar identification strategy and data from the same state to also examine the mechanisms. She finds that peer effects are not constrained to offenders with the same criminal history, and that social contagion of negative attitudes seem to explain the relation between peer exposure and subsequent crime. Chen and Shapiro (2007) exploit the discontinuity in risk scores that determines placement in higher-security facilities to examine peer effects on recidivism. They find that inmates who were just above the score recidivate more and argue that this is due to peer effects from hardened criminals. Our results further document the adverse effects of peer exposure in future criminal behavior.

Fourth, studies examining the urban determinants of criminal activity. Glaeser and Sacerdote (1999) use data from the U.S. and document that more female-headed households, higher pecuniary benefits for crime, lower probabilities of arrest and lower probabilities of recognition are important determinants of higher crime rates in large cities. Glaeser, Sacerdote and Scheinkman (1996) use data from the U.S. and find that social interactions—instances when one person's decision to engage in crime affects their neighbors' decision—account for a large share of non violent crimes. Geographic concentration of crimes has also been a subject of study of a growing literature in criminology and economics. A systematic review by Braga et al. (2019) includes a number of studies documenting crime hot spots within cities and studying interventions to address them.[8] Our study documents how the location of prisons can shape the patterns of criminal activity within cities.

The final contribution of this paper is to fill the large gap in incarceration studies from outside developed economies. In a recent review of this literature, Roodman (2017) identified 34 studies and only one was from a developing country. While the number of studies from developing economies is growing, there is still a dearth of evidence on the effects of incarceration in fragile contexts.[9]

[8]Blattman et al. (2021) and Collazos et al. (2020) document crime hot spots in Colombian cities.

[9]See Arteaga (2020) on the effects of parental incarceration on children's educational attainment and

Table 1: Prison characteristics, 2019

	Prisons (1)	Average prison population (2)	Average capacity (3)	Average occupation (4)	Share of prison population (5)	Average city population (6)
Large cities	12	3,497.8	1,954.6	179.0%	33.9%	1,867,080
Medium cities	60	1,024.5	718.3	142.6%	49.7%	165,558
Small cities	62	333.8	224.7	148.6%	16.5%	15,596
Total	134	930.8	603.5	154.2%	100%	43,011

Notes: The table presents the descriptive statistics for prison and location characteristics. Each city is located in one of the three groups by dividing the universe of municipalities in the country in terciles based on their population. There are four fewer prisons relative to our main sample because these prisons closed between our observation period and 2019.

2 Background and data

2.1 Background

The Colombian prison system is centralized, run by the National Prison Institute, technically a branch of the Ministry of Justice. It consists of 134 prisons that held about 125 thousand inmates by the end of 2019.[10] The system is divided into six regional offices, each with little managerial autonomy.[11] Regional offices are sub-divided into judicial districts that match the organization of the Colombian judicial power. Judicial authorities decide who enters or exits prison, following the Colombian penal code and further regulations. Prison authorities play a managerial role, e.g., deciding which prison to send each inmate. We describe the release process in detail in section 3.

Table 1 reports descriptive statistics on the 134 prisons and the characteristics of their locations. The average population per prison is 604 inmates, while the average capacity is

Tobón (2020) on the effects of prison conditions on recidivism. Both use data from Colombia.

[10] As we explain below in section 2.2, our sample includes 138 prisons. Four prisons closed between our observation period and 2019.

[11] In principle, the system should house only convicted offenders, as on-trial defendants should be placed at municipal or regional jails. However, with the exception of a handful of cities, the rest does not comply and but rather pays the National Prison Institute for housing their defendants. As a result, less than 2 thousand defendants are placed in municipal jails.

931. This implies that each prison has an occupancy level of roughly 154%. About 9% of the prisons are located in relatively large cities (holding 34% of all the prison population), 45% in middle-sized cities (with 50% of all inmates) and 46% in small cities (with 17% of all inmates). All prisons are located in urban areas.[12] Appendix Figure A.1 presents a map of with the distribution and location of all prisons run by the National Prison Institute.

High recidivism rates are one of the core problems of the criminal justice system in Colombia. About one in five convicted offenders is back in the prison system five years following release.[13] This figure is relatively low compared to recidivism rates in other countries, but being back to prison is a mechanical under-estimate of actual recidivism and impunity in Colombia is likely high. In 2019, for instance, the Office of the General Attorney charged three people per every ten homicides.[14] Since not all charges lead to guilty verdicts, this means fewer than three people per every ten homicides were convicted. Moreover, clearance rates for homicides are presumably larger than for petty crimes.

Furthermore, offenders in Colombia exhibit systematic patterns of specialization and persistence in criminal capital. Table 2 reports transition matrices on the type of crime committed in the first and second offenses for inmates convicted at least twice. Each row presents the type of new crimes committed by offenders originally convicted for the crime denoted in the row label. The diagonal depicts the share of inmates who recidivate in the same type of crime. Broadly, most inmates commit the same crime when they recidivate. This is especially true for offenders specialized in property crimes, as roughly 60% of property crime recidivists repeat the original offense. Other criminal specializations show similar patterns, such as drug crimes or criminal possession of weapons, where more than half the repeat offenders recidivate in the same crime.

[12] Only one facility is deemed as an agricultural prison. Yet, it is located within 2km of the city center of Acacías, Meta, a city with a population over 50 thousand.

[13] See Tobón (2017) for a detailed report on recidivism rates in Colombia, as measured with data from the National Prison Institute.

[14] See the official report by the Office of the General Attorney.

Table 2: Transition matrices for repeat offenders

First offense	Second offense (Re-entry crime)									Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
(1) Property	**59.32%**	2.66%	1.44%	0.57%	6.30%	4.51%	22.75%	0.65%	1.81%	100%
(2) Homicide	21.81%	**15.73%**	1.44%	0.56%	6.39%	8.15%	42.73%	0.96%	2.24%	100%
(3) Assault	36.04%	6.34%	**10.89%**	0.79%	9.11%	4.16%	27.92%	1.39%	3.37%	100%
(4) Sexual assault	21.49%	1.54%	3.29%	**43.20%**	8.11%	1.10%	16.45%	1.32%	3.51%	100%
(5) Drugs	24.87%	1.74%	0.87%	0.93%	**50.82%**	5.75%	12.80%	0.57%	1.65%	100%
(6) Conspiracy	19.14%	4.03%	0.38%	0.09%	6.94%	**29.36%**	36.68%	1.50%	1.88%	100%
(7) Weapons	25.67%	3.11%	0.95%	0.43%	5.75%	5.50%	**56.08%**	0.70%	1.81%	100%
(8) Other	39.92%	2.76%	1.06%	2.34%	7.22%	7.43%	25.48%	**11.46%**	2.34%	100%
(9) Other (violent)	25.25%	3.38%	1.10%	0.56%	5.81%	6.15%	51.19%	0.65%	**5.92%**	100%
All	47.72%	3.31%	1.46%	1.61%	13.56%	4.94%	23.88%	0.81%	2.71%	100%

Notes: The table presents the transition matrices for one year recidivism. We use our sample of all incarceration events in the 138 active prisons since January 2013 through September 2016. The column header corresponds to the numeration of the row labels. The diagonal of the matrix depicts the share of inmates who recidivate in the same type of crime.

2.2 Data and sample

In this paper, we use three data sources. First, we use data from the National Prison Institute. This database includes the universe of incarceration spells from January 2013 through September 2016. More specifically, we observe all incarceration spells that were active by January 2013 and every in or outflow onward until September 2016. For each incarceration spell we observe socioeconomic characteristics of the inmate such as their age, gender, place of birth or educational attainment at release. We also observe the prison, whether the inmates are on-trial or convicted, the crimes they committed or were charged with, the duration of their sentences, the location of the inmates within prison, participation in rehabilitation programs or authorization to receive visits. We also observe exact dates of entry and release. The data includes about 90,000 inmates' releases.

Second, we use data on crime reports from the National Police of Colombia. This database includes the universe of reported property crimes over the 2013-2016 period. Property crimes include violent or non-violent personal or motor-vehicle thefts. For each crime report we observe exact location coordinates as well as the date of occurrence. The data includes about 250,000 reported property crimes in the 5km area around the centroid of each prison. As in any other context, crime reports tend to be under-reported, especially for petty crimes. We do not expect this to be a problem, to the extent that under-reporting around prisons should not be systematically correlated with decisions to release inmates.

Finally, we use geolocated prison data form the National Prison Institute. This database contains the exact location coordinates of each of the 138 prisons run by the National Prison Institute during our study period. This does not include a few municipal prisons for which we do not have data, that we discuss in section 2.1.

We organize our data by first assembling a prison-day panel dataset from January 2013 through September 2016. We use the information on incarceration spells to create a set of treatment variables. For instance, we build a dummy variable that indicates whether any inmate was released from one prison on one day. We also build an intensive margin treatment measure with the count of inmates released from one prison on one day. Next, we use the police crime data and the geolocation of prisons to create a set of outcome variables. For each prison we build a buffer of 1-5km around the centroid of the facility. Using the location coordinates of reported crimes, we create count measures on the number of property crimes reported around prisons per day. We examine outcomes with both cumulative buffers and donuts around prisons.

2.3 Descriptive Statistics

Table 3 presents descriptive statistics for our analytical sample in Panel A, columns (1) and (2). These are average prison characteristics for each prison-day in our sample. Roughly 71% of all inmates were new offenders on any given prison-day. About 29% of all inmates were in prison for property crimes on any given prison-day. These figures are 14% for inmates incarcerated for homicide, 23% for inmates incarcerated for drug crimes, and 43% for inmates incarcerated for other crimes. The prison population was relatively uneducated, with most inmates having at most primary education. About half participated in occupational and educational prison-based programs. Most inmates were between 21 and 30 years old on any given prison-day. The average sentence of convicted inmates in any given prison-day was 343 months.

Panel B, columns (1) and (2), reports descriptive statistics on our treatment variables. On about 24% of all prison-days in our sample, at least one inmate was released. Specialized inmates were released on roughly 15% of all prison-days in our sample. On average, 0.5 inmates were released per prison per day. About 0.3 specialized offenders were released per prison per day.

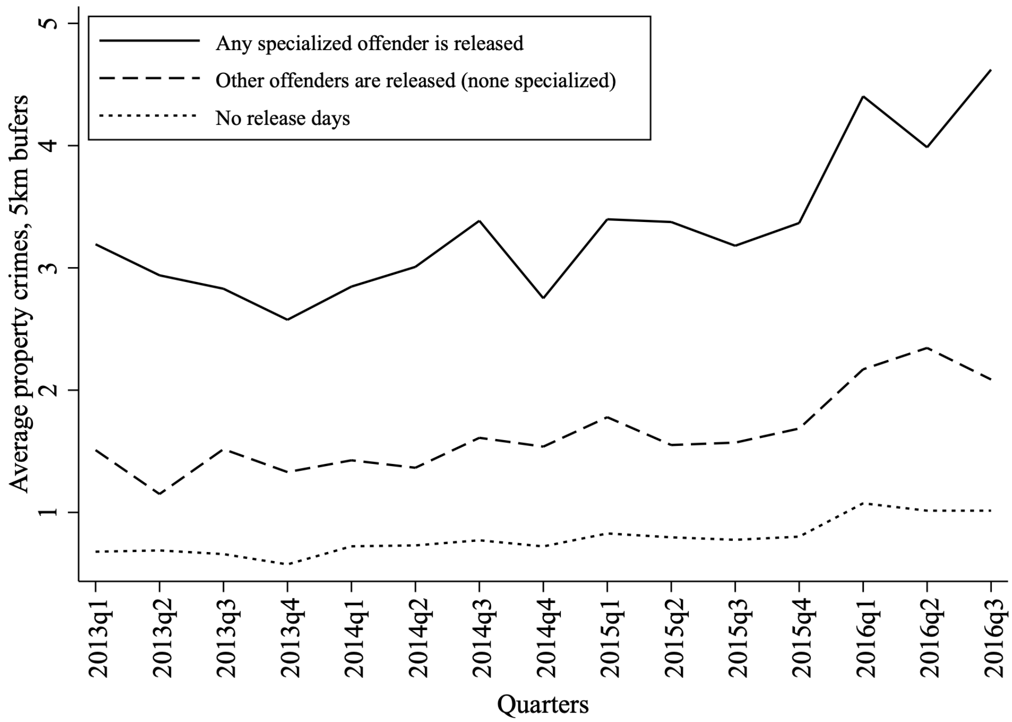
Figure 1 depicts one stylized fact in our data. The figure plots the average number of reported property crimes per quarter, within 5km buffers of all prisons in our sample. The three lines split these averages by type of day: days when no inmates are released, days when inmates specialized in property crimes are released, and days when other offenders (none

Table 3: Descriptive statistics and balance tests

	Summary statistics		Balance tests: Individual tests		Balance tests: Joint test	
	Control mean	S.D.	Coeff.	S.E.	Coeff.	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)
A. Prison characteristics (proportion of inmates meeting each condition unless noted otherwise)						
Is a new offender	0.711	0.284	0.003	[0.002]	-0.049	[0.140]
Is in prison for property crime	0.290	0.167	0.002	[0.001]	-0.006	[0.109]
Is in prison for homicide	0.137	0.135	-0.001*	[0.001]	-0.431**	[0.176]
Is in prison for drug crime	0.232	0.172	0.002	[0.001]	0.096	[0.158]
Is in prison for other crime	0.429	0.221	0.001	[0.001]	0.003	[0.112]
Has below primary ed.	0.041	0.036	0.000	[0.000]	-0.276	[0.328]
Has at most primary ed.	0.569	0.231	0.002	[0.002]	-0.221	[0.289]
Has at most secondary ed.	0.230	0.112	0.001	[0.001]	-0.098	[0.314]
Has tertiary education	0.028	0.039	0.000	[0.000]	-	[-]
Is enrolled in occup. prog.	0.559	0.269	0.001	[0.001]	0.055	[0.075]
Is enrolled in educ. prog.	0.485	0.245	0.001	[0.001]	-0.020	[0.063]
Is authorized to receive visits	0.819	0.329	0.003	[0.003]	-0.039	[0.0.094]
Has minor children	0.697	0.289	0.001	[0.002]	-0.154	[0.124]
Is male	0.808	0.376	0.004	[0.003]	0.029	[0.198]
Is Colombian	0.868	0.339	0.004	[0.003]	-	[-]
Is less than 20 years old	0.079	0.045	0.001	[0.000]	-0.355	[0.745]
Is between 21-30 years old	0.344	0.154	0.002	[0.001]	-0.578	[0.724]
Is between 31-40 years old	0.220	0.099	4.52e-04	[0.001]	-0.587	[0.734]
Is between 41-50 years old	0.127	0.067	4.12e-04	[4.24e-04]	-0.312	[0.719]
Is between 51-60 years old	0.069	0.046	2.10e-04	[1.77e-04]	-0.308	[0.722]
Is between 61-70 years old	0.022	0.020	-3.39e-05	[7.28e-05]	-0.572	[0.836]
Is more than 71 years old	0.007	0.011	5.02e-05	[5.46e-05]	-	[-]
Sentence (in months)	343.001	289.261	3.544	[3.173]	8.55e-06	[8.01e-06]
B. Treatment variables						
Any inmate released	0.235	0.424	-	-	-	-
Specialized inmate released	0.148	0.355	-	-	-	-
No. of released inmates	0.480	1.237	-	-	-	-
No. of released spec. inmates	0.253	0.796	-	-	-	-
C. Additional tests of balance						
p-value of F test	-		-		0.196	

Notes: Columns (1) and (2) report summary statistics. Columns (3) and (4) report the coefficients and standard errors for a regression of each variable on an indicator for release day and fixed effects (the results from each row come from an independent regression). Columns (5) and (6) report the coefficients and standard errors for a regression of an indicator variable for release day on all variables and fixed effects (the results for the complete column come from one regression). Tertiary education and above 71 years old are the omitted categories for education and age, respectively. The Colombian indicator is omitted due to collinearity. The reported p-value of F test refers to a joint test of the null hypothesis for all the covariates (excluding fixed effects). Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Each estimation includes prison and day fixed effects. Robust standard errors clustered at the prison level in brackets.

Figure 1: Average property crimes and inmates exit days



Notes: The figure depicts the evolution of the average property crime reports in the 5km buffer around all 138 prisons in our sample, by type of day: days where any inmate convicted for property crimes is released; days where any inmate is released but none of them was convicted for property crimes; days with no releases.

specialized in property crimes) are released. Crime rates are highest when specialized inmates are released. There are, however, potential confounders that prevent us from interpreting this figure as causal evidence of inmates' release on crime rates. For instance, weekends have probably fewer crimes and, as we show in section 3, inmates are released mostly on week days. We elaborate on the causality argument in the following sections.

3 Research design

In this paper, we are interested in the effects of inmates releases on crime around prisons on release days. The key selection problem for the identification of causal effects is related to how prison authorities choose one day and not others to release prisoners. For instance, if prison authorities select low-crime days or times to release inmates, a simple comparison between release and non-release days would underestimate a positive effect of release days on crime incidence. Conversely, if prison authorities select high-crime periods, the comparison would overestimate a positive relation.

3.1 How are inmates released from prisons?

The formal procedure to release an inmate is specified in the Colombian legislation.[15] For convicted offenders, the release process consists of two steps. First, judicial authorities issue a court order requesting the prison authority to release an inmate. These court orders are an outcome of the criminal trial and certify that the inmates complied with their sentence. Judicial authorities process cases of inmates assigned to different prisons—those within their jurisdiction—hence these outcomes are produced independently of prison authorities at each prison. Once the judicial authority issues the order, prison authorities verify whether the inmates have any outstanding court orders from other judicial authorities. If not, the inmates undergo a medical examination to certify their exit conditions and are released. With few exceptional cases, the release process happens within the same day the order arrives at the prison authority.

On-trial defendants follow a similar process, whereby judicial authorities issue court orders requesting their release. This happens due to either a non-guilty verdict, a guilty verdict sentencing the offender to time served, or pretrial release. With the court orders, prison authorities release the inmate following the same procedure as for convicted offenders.

Table 4 reports descriptive statistics on the days of the week and the months of the year inmates are released. We split the summaries per regional office of the National Prison Authority, to reflect differences across prisons. Broadly, we see the same patterns across regional offices—which also suggests patterns are relatively stable across prisons. Inmates are released mostly on week days rather than weekends. This reflects working days in

[15] Specifically, the Prison Code is in Law 65/1993. Articles 52-78 describe the prison regime in detail, including how inmates are released.

Colombia, that are followed by judicial authorities as well as most national bureaucracies. Also, January is the month of the year with the lowest share of release days across regional offices of the prison authority. Collective holidays of judicial authorities in Colombia explain this pattern.[16]

3.2 Is the selection of release days quasi-random?

We examine if the selection of release days is quasi-random by studying whether the decision to release inmates in a given day is correlated with observable prison characteristics of that day. We perform three tests. First, columns (3) and (4) of Table 3 report the coefficients and standard errors of different regressions of each variable on an indicator for release day, prison fixed effects and day fixed effects. Each row presents the results of an independent regression. These tests are equivalent to a test of balance in the context of a randomized controlled trial. We find that prison characteristics are similar between release and non-release days. That is, these results show the expected degree of balance one would anticipate under random treatment assignment.

Second, columns (5) and (6) of Table 3 report the coefficients and standard errors for a regression of an indicator variable for release day on all variables and fixed effects simultaneously, i.e., the results for the complete columns come from one regression only. The unreported coefficients for tertiary education and being above 71 years old are the omitted categories for education and age. The nationality indicator is omitted due to collinearity, as some prisons house only Colombian inmates. We also report the p-value of a joint test of the null hypothesis for all the covariates in the regression, excluding the prison and day fixed effects. These results also suggest there is no systematic correlation between the decision to release inmates and current prison characteristics of the day.

We do observe a more precise negative estimate of the share of inmates who are in prison for homicide in a given day and the release decision. Broadly, however, we deem this as

[16] While this may seem rather unusual, the judicial authorities in Colombia have a mandated collective holiday season at the end of the year. These holidays are specified in Lay 270/1996.

Table 4: Descriptive statistics on days of the week and months of the year inmates are released

	Regional offices of the National Prison Authority											
	Central		Western		North		Eastern		Northwestern		Viejo Caldas	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Day of the week												
Monday	0.21	0.41	0.24	0.43	0.23	0.42	0.19	0.40	0.16	0.37	0.19	0.39
Tuesday	0.31	0.46	0.35	0.48	0.34	0.47	0.30	0.46	0.24	0.43	0.26	0.44
Wednesday	0.33	0.47	0.38	0.48	0.35	0.48	0.32	0.47	0.26	0.44	0.26	0.44
Thursday	0.34	0.47	0.39	0.49	0.38	0.49	0.34	0.48	0.27	0.44	0.27	0.44
Friday	0.39	0.49	0.44	0.50	0.42	0.49	0.40	0.49	0.29	0.45	0.28	0.45
Saturday	0.10	0.30	0.07	0.26	0.09	0.29	0.08	0.28	0.09	0.28	0.07	0.26
Sunday	0.05	0.21	0.03	0.17	0.01	0.12	0.03	0.18	0.04	0.20	0.04	0.20
B. Month of the year												
January	0.21	0.41	0.22	0.41	0.21	0.41	0.19	0.39	0.15	0.36	0.16	0.37
February	0.28	0.45	0.30	0.46	0.28	0.45	0.28	0.45	0.21	0.40	0.22	0.41
March	0.25	0.43	0.28	0.45	0.24	0.43	0.23	0.42	0.20	0.40	0.20	0.40
April	0.26	0.44	0.28	0.45	0.25	0.43	0.25	0.43	0.21	0.40	0.21	0.41
May	0.25	0.43	0.26	0.44	0.26	0.44	0.25	0.43	0.21	0.40	0.20	0.40
June	0.24	0.43	0.27	0.44	0.25	0.43	0.24	0.43	0.19	0.39	0.20	0.40
July	0.26	0.44	0.29	0.46	0.29	0.45	0.25	0.43	0.20	0.40	0.20	0.40
August	0.24	0.43	0.28	0.45	0.28	0.45	0.23	0.42	0.18	0.39	0.19	0.39
September	0.25	0.43	0.29	0.46	0.27	0.45	0.24	0.43	0.19	0.40	0.18	0.39
October	0.24	0.42	0.25	0.44	0.28	0.45	0.26	0.44	0.20	0.40	0.19	0.40
November	0.22	0.41	0.23	0.42	0.22	0.42	0.21	0.41	0.19	0.39	0.18	0.38
December	0.25	0.43	0.28	0.45	0.28	0.45	0.24	0.43	0.21	0.40	0.20	0,.40
Total	0.25	0.43	0.27	0.44	0.26	0.44	0.24	0.43	0.19	0.39	0.19	0.40

Notes: The table reports descriptive statistics on the days of the week inmates are released (Panel A) and the months of the year inmates are released (Panel B), for each regional office of the National Prison Authority. Each reported mean refers to the proportion of each days of the week or months of the year that inmates are released.

consistent with these results showing the expected degree of balance one would anticipate under random assignment.

Third, since we are also specifically interested on days in which inmates specialized in property crimes are released, Appendix Table A.1 reports the coefficients and standard errors of different regressions of each variable on indicators for days when any specialized inmate was released and days when any others inmate—but none specialized—were released. The results further suggest selection of release days is not based on prison characteristics.

3.3 Estimating Equation

To study the effects of inmates' releases on crime around prisons we estimate equation (1) using ordinary least squares:

$$Y_{i,t} = \beta T_{i,t} + \delta_i + \gamma_t + \varepsilon_{i,t} \tag{1}$$

where Y is the number of property crimes reported on day t around prison i within a given buffer. T is our treatment variable. In our extensive margin analysis, the treatment variable is an indicator that takes the value 1 if any inmates were released from prison i on day t. In our intensive margin analysis, the treatment variable is the count of inmates who were released from prison i on day t. δ is a vector of prison fixed effects. This vector allows us to control for unobserved prison characteristics that do not vary over time, such as the managerial capacity of the director, the quality of the rehabilitation programs, or the criminogenic environment of the prison's surroundings. γ is a vector of day fixed effects. This vector allows us to control for unobserved day characteristics that are common for all prisons, such as the crime trends per day of the week or month of the year. ε represents an error term. Our coefficient of interest is β. In all estimations, we cluster standard errors at the prison level.

Note our specification of equation (1) resembles a two-way fixed effects difference-in-differences estimator. As Goodman-Bacon (2021) documents, a causal interpretation of these estimates requires both parallel trends and treatment effects that are constant over time. In our setting, the quasi-randomness of the selection process suggests the parallel trend assumption holds. Moreover, due to the nature of the process, with thousands of inmates being released over thousands of days from all prisons, we do not expect treatment effects to vary over time.

In section 4 below, we also report results from estimations of alternative versions of equation (1). For instance, in the extensive margin analysis, we split T into two indicators: days when any specialized inmate was released and days when any other inmates—but none specialized—were released. In our intensive margin analysis, we also split T into two counts: number of released specialized inmates and number of released non-specialized inmates.

4 Criminal capital persistence

4.1 Effects of inmates' releases on crime

Table 5 reports the baseline results for the effects of inmates' releases on crimes around prisons, estimating equation (1). The dependent variable is the count of reported property crimes around the centroid of the prison, within buffers of 1-5km. The size of the buffer entails a bias-precision trade-of: a smaller buffer captures the effect more directly but covers lower levels of variation in the outcome variable. We are agnostic on the specification and report all buffer sizes in most of our estimations.

Panel A presents the extensive margin effects. That is, the treatment variable is an indicator that takes the value 1 if any inmates were released from prison i on day t. Broadly, the results suggest that property crime reports are higher around prisons on days inmates are released. The relevant coefficient is positive and statistically significant at conventional levels in all regressions. The magnitude of the coefficient grows larger as we extend the size of the buffer. At 5km buffers, we observe 0.13 additional reported property crimes around prisons. The average number of reported property crimes on non-release days is 0.79. This implies crime reports are roughly 16% higher on release days.

Panel B reports the intensive margin effects. In this case, the treatment variable is the count of inmates released from prison i on day t. Similar to the results on the extensive margin, the estimates suggest that property crime reports are higher around prisons for every additional inmate released. The relevant coefficient is positive and statistically significant at conventional levels in all regressions except for the 1km buffer. This is expected due to the smaller magnitude of the effect and the bias-precision trade-of we describe above. The magnitude of the coefficient also grows larger as we extend the size of the buffer. At 5km buffers, we observe an increase in 0.08 reported property crimes around prisons for each additional inmate released. Relative to the average number of reported property crimes on non-release days, the magnitude of the coefficient is equivalent to a 10% increase in crime.

4.2 Effects focusing on specialized inmates

We now examine whether persistence in criminal capital drives the effects by focusing on specialized inmates—that is, offenders who were originally incarcerated for property crimes. Table 6 reports the results. We estimate alternative versions of equation (1), where we split the relevant treatment variable into two types of treatments. Columns (2) and (3) present the coefficient and standard errors for the treatment variable related to specialized inmates. Columns (4) and (5) present the corresponding coefficient and standard errors for the treatment variable related to non-specialized inmates. We also test whether these two coefficients are different, and report the corresponding p-value in column (6).

Panel A presents the extensive margin effects. Here, we split the treatment variable into two indicators. One for days when any inmate specialized in property crimes was released, and one for days when any other inmates—but none specialized in property crimes—were released. The results suggest the increase in property crime around prisons is mainly driven by the release of specialized inmates. The coefficients for the treatment variable related to

Table 5: Effects of inmates' releases on crime, with cumulative buffers

	Control mean	Coeff.	S.E.	R^2	Obs.
	(1)	(2)	(3)	(4)	(5)
A. Treatment is an indicator for release days					
Property crimes, 1km buffers	0.114	0.012**	[0.005]	0.247	186,576
Property crimes, 2km buffers	0.345	0.047***	[0.016]	0.556	186,576
Property crimes, 3km buffers	0.537	0.074***	[0.027]	0.687	186,576
Property crimes, 4km buffers	0.689	0.101**	[0.040]	0.760	186,576
Property crimes, 5km buffers	0.789	0.128***	[0.046]	0.793	186,576
B. Treatment is the number of inmates who exit					
Property crimes, 1km buffers	0.114	0.004	[0.003]	0.247	186,576
Property crimes, 2km buffers	0.345	0.027***	[0.009]	0.557	186,576
Property crimes, 3km buffers	0.537	0.042***	[0.012]	0.687	186,576
Property crimes, 4km buffers	0.689	0.059***	[0.020]	0.760	186,576
Property crimes, 5km buffers	0.789	0.076***	[0.024]	0.793	186,576

Notes: The table presents results for ordinary least squares regressions of the count of crimes within buffers around each prison on the treatment variables and fixed effects. Column (1) reports the control prison-day mean. Column (2) reports the regression coefficient for the treatment variable. Column (3) reports the standard errors. Column (4) reports the R-squared of the regression. Column (5) reports the number of observations. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Each estimation includes prison and day fixed effects. Robust standard errors clustered at the prison level in brackets.

specialized inmates are positive and statistically significant in all regressions. At 5km buffers, we observe 0.2 additional property crime reports on days specialized inmates are released. This is equivalent to a 26% increase relative to the average number of reported property crime reports on non-release days. Moreover, the coefficient for the treatment variable related to non-specialized inmates show positive but rather imprecise effects. These coefficients are systematically smaller than those for the alternative treatment. The differences between these coefficients are statistically significant, as reported in column (6).

Panel B presents the intensive margin effects. In this case, we split the treatment variable into two counts. One for the number of released specialized inmates, and one for the number of released non-specialized inmates. The results suggest that the number of reported property crimes increase when any additional inmate—regardless of their specialization—is released. The magnitude of the coefficients, however, suggest that increases in property crime reports are larger when specialized inmates are released. At 5km buffers, for example, we observe an increase in 0.09 crime reports for each additional specialized inmate exiting prison (12% relative to the control mean). The increase in property crime reports for each additional non-specialized inmate exiting prison is 0.06 (7% relative to the control mean). The difference between these coefficients is statistically significant at conventional levels, as we report in column (6). These pattern is broadly consistent across all buffer specifications.

4.3 Effects with exclusive donuts instead of cumulative buffers

Our setting allows us to examine how the effects of inmates' releases on crime change with distance to the prisons. Appendix Table A.2 reports the results from estimating equation (1), but rather than using cumulative buffers around the centroid of each prison to build our outcomes, we use exclusive donuts. Panel A reports the results for the extensive margin effects. Broadly, we observe the same pattern as with the cumulative buffers. Relative to the average number of reported property crimes at each distance, the effects grow monotonically, reaching 26% in the 4-5km donut. Panel B reports the results for the intensive margin effects.

Table 6: Criminal capital persistence, with cumulative buffers

Variable	Control mean	Specialized inmates are released		Other inmates are released		p-value	R^2	Obs.
		Coeff.	S.E.	Coeff.	S.E.	diff.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Treatments are indicators for release of specialized inmates and release of other inmates								
Property crimes, 1km buffers	0.114	0.020***	[0.007]	4.41e-04	[0.005]	0.007	0.247	186,576
Property crimes, 2km buffers	0.345	0.078***	[0.022]	0.005	[0.011]	0.001	0.557	186,576
Property crimes, 3km buffers	0.537	0.122***	[0.039]	0.009	[0.016]	0.001	0.687	186,576
Property crimes, 4km buffers	0.689	0.165***	[0.058]	0.016	[0.020]	0.003	0.760	186,576
Property crimes, 5km buffers	0.789	0.204***	[0.068]	0.024	[0.024]	0.002	0.793	186,576
Panel B: Treatments are counts of specialized inmates and other inmates who exit								
Property crimes, 1km buffers	0.114	0.007**	[0.003]	0.001	[0.004]	0.172	0.247	186,576
Property crimes, 2km buffers	0.345	0.034***	[0.010]	0.018**	[0.008]	0.021	0.557	186,576
Property crimes, 3km buffers	0.537	0.054***	[0.015]	0.026***	[0.011]	0.008	0.687	186,576
Property crimes, 4km buffers	0.689	0.073***	[0.023]	0.041**	[0.018]	0.023	0.760	186,576
Property crimes, 5km buffers	0.789	0.093***	[0.027]	0.056**	[0.023]	0.029	0.793	186,576

Notes: This table presents the ordinary least square regression of crimes, separated by property crimes and others, within the buffers around each prison on the treatment variables and fixed effects. Column (1) contains the control prison-day mean/ Column (2) reports the coefficient for property crimes and column (3) the standard error. Column (4) contains the regression coefficient for non-property crimes, with column (5) containing the standard errors. Column (7) reports the p-value of the difference, with column (7) reporting the R-suqared and column (8) the number of observations. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Each estimation includes prison and day fixed effects. Standard errors clustered at prison level.

We also observe a similar pattern, with property crime reports increasing in all donut zones with each additional inmate released.

Similarly, Appendix Table A.3 reports the results from estimating the alternative version of equation (1) with two treatments, using exclusive donuts. Panel A reports the results for the extensive margin effects. We also observe a similar pattern. The effects of releasing specialized inmates grow monotonically with distance—relative to the control means. We see no effects on days other inmates—and none specialized—are released. In the 4-5km area around prisons, the crime reports are 39% higher on days specialized inmates are released. Panel B reports the results for the intensive margin effects. The results suggest that property crime reports increase in all donut zones as more inmates are released. These effects are larger when specialized inmates exit prison.

4.4 Additional sensitivity checks

We check whether our results are robust to alternative specifications. Table 7 presents the sensitivity checks using 5km buffers.[17] In our baseline specification we treat all release events equal, whether the inmates who exit prison were convicted or not. Inmates who are not convicted are less likely to have criminal skills.[18] Panel A reports results considering only convicted inmates. That is, we build our different treatment variables by overlooking inmates with no conviction. The four rows in the panel resemble our main specifications: extensive and intensive margins, looking at any release and those of any specialized inmate. In the regressions focusing on specialized inmates, we control for other releases—we run the same regressions as in tables 5 and 6, changing only the definition of the treatment variable. The results are broadly similar to our main specification.

In panels B and C we examine whether our results are robust to regional or temporal

[17]Appendix tables A.4 to A.7 report the same sensitivity tests for other buffer zones. The results are similar to the ones we report with 5km buffers.

[18]Criminal trials are subject to type one and type two errors. See for instance Kleinberg et al. (2018). Hence we expect some inmates who are not convicted to have criminal capital, and some who are convicted to not have any criminal capital.

patterns. Panel B reports our baseline results excluding one regional office of the National Prison Authority at a time. We are interested in looking at whether one prison or regional office explains the effects. The results suggest this is not the case, as we observe positive and precise estimates in all regressions. Similarly, Panel C reports the baseline results excluding one quarter at a time. In this case, we are interested in looking at whether seasonal trends explain the effects. We also find positive and precise estimates in all regressions.

5 Heterogeneous treatment effects

To further study persistence in criminal capital, we also examine whether improvements in non-criminal human capital or longer incarceration spells mitigate these effects.

5.1 Non-criminal human capital

Non-criminal human capital increases the opportunity cost of crime. Hence rational criminals would be less likely to engage in crime if they improve non-criminal human capital during incarceration. We examine if this is the case by looking at heterogeneous treatment effects based on participation in prison-based rehabilitation programs and educational attainment at the time of release.

Panel A in Table 8 reports the results of a regression of the count of crimes within buffers around each prison on the count of released specialized inmates (reported in columns 2-3), the count of released inmates who enrolled in prison-based educational programs (reported in columns 4-5), the count of released inmates meeting both conditions (reported in columns 6-7), and prison and day fixed effects. The coefficients in column (6) are all positive, and broadly precise across buffer sizes. This implies that crime rates are even higher when the specialized inmates who are released participated in educational programs.

Panel B in Table 8 reports the results from analogous regressions that differ only in the type of program in which the released inmates enrolled. In this case, we focus on inmates

Table 7: Sensitivity checks

	Control mean	Coeff.	S.E.	R^2	N
	(1)	(2)	(3)	(4)	(5)
Panel A: Only convicted inmates, treatment definition in each row (5km buffers)					
Any inmate is released	0.872	0.120**	[0.048]	0.793	186,576
No. of released inmates	0.872	0.068***	[0.023]	0.793	186,576
Any specialized inmate is released	0.931	0.194***	[0.062]	0.793	186,576
No. of released spec. inmates	0.931	0.105***	[0.031]	0.793	186,576
Panel B: Excluding one regional office of the prison authority at a time (5km buffers)					
Excluding Central region	0.805	0.087**	[0.043]	0.770	129,792
Excluding Western region	0.745	0.128**	[0.051]	0.776	154,128
Excluding North region	0.686	0.145***	[0.053]	0.805	164,944
Excluding Eastern region	0.808	0.120**	[0.050]	0.795	167,648
Excluding Northwestern region	0.885	0.137***	[0.052]	0.798	158,184
Excluding Viejo Caldas region	0.814	0.141***	[0.052]	0.801	158,184
Panel C: Excluding one quarter at a time (5km buffers)					
Excluding Quarter 1	0.774	0.126***	[0.045]	0.795	136,758
Excluding Quarter 2	0.782	0.134***	[0.045]	0.791	136,344
Excluding Quarter 3	0.786	0.134***	[0.047]	0.787	138,138
Excluding Quarter 4	0.812	0.117***	[0.048]	0.800	148,488

Notes: The table presents results for ordinary least squares regressions of the count of crimes within buffers around each prison on the treatment variables and fixed effects. We use 5km buffers in all regressions. Panel A considers only releases of convicted inmates. The regressions when the treatment definition is "any specialized inmate is released" and "No. of released spec. inmates" include controls for a secondary treatment variable (either indicator or count). Panel B checks for robustness by excluding one regional office of the prison authority at a time. Panel C excludes one quarter of the year at a time. Column (1) reports the control prison-day mean. Column (2) reports the regression coefficient for the relevant treatment variable. Column (3) reports the standard errors. Column (4) the R-squared and column (5) the number of observations. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Each estimation includes prison and day fixed effects. Standard errors clustered at prison level.

participating in prison-based occupational programs. The coefficients in column (6) are all positive, however imprecise. This suggests that crime rates are not lower when the specialized inmates who are released participated in occupational programs.

Finally, Panel C in Table 8 reports the results of a regression of the count of crimes within buffers around each prison on the count of released specialized inmates, the count of released inmates who have higher levels of educational attainment at the time of release, the count of released inmates meeting both conditions, and prison and day fixed effects. In this case, the coefficients in column (6) do not point to any systematic pattern, with the signs changing from one regression to another. We interpret these results as further evidence that non-criminal human capital does not seem to reduce the adverse effects on crime following the release of specialized inmates.

The coefficients in Column (2) confirm our baseline results: reported property crimes increase each time one additional specialized inmate is released. Focusing on 5km buffers, we observe an increase between 0.07 to 0.8 in crimes with each additional release (equal to roughly 9% to 10% relative to non-release days). These results are similar to our baseline findings on the intensive margin effects reported in Panel B in Table 6.

To discuss causality we need to address an additional selection problem. That is, inmates self-select to participate in prison-based rehabilitation programs or to reach higher levels of education. However, if offenders who self-select to improve their non-criminal human capital are the least criminogenic, we would expect the coefficients in column (6) to be negatively biased. That is, in our regressions, we are overestimating the beneficial effects of education and training. Hence, in our interpretation, these results further suggest criminal capital persists and can even outweigh improvements in non-criminal human capital.

5.2 Severity of punishment

The experience of a more severe punishment increases the expected cost of crime for former inmates. Again, rational offenders would be less likely to engage in crime if they expect a higher cost in case they are arrested. We examine this by looking at heterogeneous treatment effects based on sentence length.

Panel D in Table 8 reports the results of a regression of the count of crimes within buffers around each prison on the count of released specialized inmates (reported in columns 2-3), the count of released inmates who served above-median incarceration spells (reported in columns 4-5), the count of released inmates meeting both conditions (reported in columns 6-7), and prison and day fixed effects. The coefficients in column (6) are negative but rather imprecise. This suggests crime rates around prisons are roughly the same whether or not specialized inmates served more time.

The selection problem is more subtle in this case. If the sentencing process leads to the expected outcomes, offenders with a higher probability of recidivism would serve longer incarceration spells. Hence, if offenders serving more time are the most criminogenic, we would expect the coefficients in column (6) to be positively biased. In other words, we are underestimating the beneficial effects of harsher punishments. Nonetheless, these results are consistent with the specific deterrent effects of incarceration being weak for specialized criminals.

6 Further unintended consequences

Criminal recidivism has obvious negative externalities, the main being welfare losses for the victims and their families. In this section we examine two additional externalities we can link to the incarceration (and subsequent release) of specialized offenders.

6.1 Peer effects

We first examine whether confinement with criminals specialized in property crimes is associated with higher levels of property crime reports on release days. A large literature documents adverse peer effects caused by incarceration (e.g., Bayer, Hjalmarsson and Pozen

Table 8: Criminal capital persistence: Heterogeneous effects

	Control Mean	Count of released specialized inmates		Count of released inmates meeting the panel condition		Count of released inmates meeting both conditions		R^2	Obs.
		Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Heterogeneous effects for inmates who enrolled in prison-based educational programs									
Pr. crimes, 1km buffers	0.114	0.005	[0.004]	-0.004	[0.002]	0.020**	[0.009]	0.247	186,576
Pr. crimes, 2km buffers	0.345	0.023*	[0.012]	0.008	[0.007]	0.034**	[0.017]	0.557	186,576
Pr. crimes, 3km buffers	0.537	0.037**	[0.017]	0.010	[0.012]	0.055**	[0.027]	0.687	186,576
Pr. crimes, 4km buffers	0.689	0.056*	[0.029]	0.013	[0.015]	0.061*	[0.037]	0.760	186,576
Pr. crimes, 5km buffers	0.789	0.070*	[0.035]	0.025	[0.018]	0.059	[0.044]	0.793	186,576
B. Heterogeneous effects for inmates who enrolled in prison-based occupational programs									
Pr. crimes, 1km buffers	0.114	0.008	[0.005]	-0.001	[0.005]	0.001	[0.009]	0.247	186,576
Pr. crimes, 2km buffers	0.345	0.030**	[0.013]	0.006	[0.008]	0.021	[0.016]	0.557	186,576
Pr. crimes, 3km buffers	0.537	0.047***	[0.017]	0.006	[0.010]	0.040*	[0.023]	0.687	186,576
Pr. crimes, 4km buffers	0.689	0.068**	[0.027]	0.014	[0.011]	0.028	[0.027]	0.760	186,576
Pr. crimes, 5km buffers	0.789	0.084**	[0.031]	0.026**	[0.012]	0.032	[0.032]	0.793	186,576
C. Heterogeneous effects for inmates with higher educational attainment at the time of release									
Pr. crimes, 1km buffers	0.114	0.008**	[0.003]	-0.002	[0.006]	-0.005	[0.009]	0.247	186,576
Pr. crimes, 2km buffers	0.345	0.036***	[0.009]	0.006	[0.012]	-0.025	[0.022]	0.557	186,576
Pr. crimes, 3km buffers	0.537	0.051***	[0.013]	0.021	[0.014]	-0.024	[0.046]	0.687	186,576
Pr. crimes, 4km buffers	0.689	0.066***	[0.017]	0.036	[0.024]	0.005	[0.048]	0.760	186,576
Pr. crimes, 5km buffers	0.789	0.081***	[0.021]	0.048**	[0.026]	0.032	[0.079]	0.793	186,576
D. Heterogeneous effects for inmates who served above-median incarceration spells									
Pr. crimes, 1km buffers	0.114	0.009*	[0.005]	-0.002	[0.005]	-0.001	[0.009]	0.247	186,576
Pr. crimes, 2km buffers	0.345	0.041***	[0.014]	0.010	[0.010]	-0.019	[0.018]	0.557	186,576
Pr. crimes, 3km buffers	0.537	0.067***	[0.019]	0.020	[0.015]	-0.040	[0.027]	0.687	186,576
Pr. crimes, 4km buffers	0.689	0.088***	[0.025]	0.030	[0.021]	-0.050	[0.036]	0.760	186,576
Pr. crimes, 5km buffers	0.789	0.108***	[0.030]	0.047*	[0.025]	-0.065	[0.048]	0.793	186,576
E. Heterogeneous effects for inmates who served time along with offenders specialized in property crime									
Pr. crimes, 1km buffers	0.114	0.007	[0.006]	0.001	[0.004]	-0.002	[0.009]	0.247	186,576
Pr. crimes, 2km buffers	0.345	0.022*	[0.012]	0.017**	[0.008]	0.010	[0.025]	0.557	186,576
Pr. crimes, 3km buffers	0.537	0.035*	[0.020]	0.025**	[0.011]	0.017	[0.043]	0.687	186,576
Pr. crimes, 4km buffers	0.689	0.039	[0.029]	0.040**	[0.018]	0.036	[0.067]	0.760	186,576
Pr. crimes, 5km buffers	0.789	0.058*	[0.035]	0.055**	[0.022]	0.023	[0.084]	0.793	186,576

Notes: The table presents results for ordinary least squares regressions of the count of crimes within buffers around each prison on the treatment variables and fixed effects. Treatment variables are the count of released specialized inmates, the count of inmates meeting the condition depicted in the panel header, and the count of inmates meeting both conditions. Column (1) reports the control prison-day mean. Column (2) reports the regression coefficient for the count of specialized inmates released. Column (3) reports the standard errors. Column (4) reports the regression coefficient for the count of released inmates meeting the panel condition. Column (5) reports the standard errors. Column (6) reports the regression coefficient for the count of released inmates meeting both conditions. Column (7) reports the standard errors. Column (8) reports the R-squared of the regression. Column (9) reports the number of observations. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Each estimation includes prison and day fixed effects. Robust standard errors clustered at the prison level in brackets.

2009; Chen and Shapiro 2007; Stevenson 2017). Our setting allows us to examine peer effects resulting from the interaction with specialized criminals.

Panel E in Table 8 reports the results of a regression of the count of crimes within buffers around each prison on the count of released specialized inmates (columns 2-3), the count of released inmates who served time along with specialized offenders (columns 4-5), the count of released inmates meeting both conditions (columns 6-7), and the fixed effects we describe in equation (1). The coefficients in Column (2) confirm our baseline results: reported property crimes increase each time one additional specialized inmates is released. Focusing on 5km buffers, we observe an increase in 0.06 crimes with each additional release (equivalent to roughly 7% relative to non-release days). This figure is similar to our baseline findings on the intensive margin effects reported in Panel B in Table 6.

The coefficients in Column (4) point to adverse peer effects: reported property crimes increase each time one additional inmate confined with a specialized offender is released. In the specification using 5km buffers, we also observe an increase of roughly 0.06 crimes around prisons with each additional release (also equivalent to 7% relative to non-release days).

Finally, the coefficients in Column (6) suggest these effects do not reinforce one another. We do not observe an increase in crimes each time one additional inmate specialized in property crimes who was also exposed to peer effects reaches freedom. The coefficients are positive, but small in magnitude and imprecisely estimated.

To discuss causality, the selection problem in this case is how prison authorities assign inmates to one cell and wing and not to other. Broadly, prison authorities assign inmates to prison wings and cells following several legal and supply constraints. On the one hand, each prison has an assignment board that decides the cell and wing of each inmate.[19] These decisions follow broad guidelines, considering information provided by each inmate in an interview, their personal and medical conditions, security and risk assessments, and their

[19] This board is called the *Junta de Patios y Asignaciones*, and has five members: the prison director, a legal advisor, the health and sanitation chief, the security chief and a social advisor (usually a psychologist). Details are in the Normative Framework of the National Prison Institute.

legal status. On the other hand, assignment also follows space availability. However, by the end of 2019, only 6 out of 138 prisons had enough space to house all their inmates (see Table 1). Hence, while the assignment process is not random, it is unlikely that inmates who do not have criminal skills for property crimes but have potential are put together with specialized offenders. We interpret these results as suggestive evidence of criminal capital production due to peer effects during incarceration.

6.2 Property values

Second, we examine the effects of prison location on housing prices. Previous studies document the relationship between crime incidence and urban characteristics of cities (e.g., Glaeser and Sacerdote 1999; Glaeser, Sacerdote and Scheinkman 1996). We leverage our setting to conduct a back-of-the-envelope estimation of the externalities of prison location on housing prices in the context of criminal capital persistence.

We focus on the 5km buffers. Absent releases, we observe an average of 0.79 crimes in each prison-day in our sample. There are roughly 28 thousand prison-days with specialized criminals released—about 15% of all prison-days—and we estimate that crimes rise by 0.13 on these days. Hence, broadly, we estimate that release days account for 4% of all property crimes around prisons. Morales-Mosquera (2021) estimates that the elasticity of property values with respect to crime in the three largest Colombian cities—Bogotá, Medellín and Cali—is -0.24. Assuming this elasticity is constant across urban areas in Colombia, this implies that properties around prisons lose 1% of their value as a result of negative externalities of prison locations.

Official cadastral data for urban areas in Colombia suggests there are roughly 68 thousand properties in an average 5km buffer. Also, that the average commercial price per property is about $169 thousand, adjusting for purchasing power parity.[20] Hence, 1% of all properties within 5km buffers around all prisons in our sample adds up to $15.9 billion. Property tax

[20] We use data from the National Cadastral Authority and the four decentralized authorities (Bogotá, Medellín, Cali and Antioquia) to retrieve these estimates.

rates for properties of the average value are generally between 9.4 to 12.3 per 1,000, with a tax base of 45% of the commercial value. Hence, we estimate that property tax revenues drop between $67 and $88 million per year for the cities housing the 138 prisons.

7 Discussion and conclusions

In this paper, we document that incarceration's specific deterrence and rehabilitation effects seem weak—even in the very short-term—for specialized offenders. We examine this question in Colombia by looking at crimes nearby prisons on days inmates are released.

Relative to non-release days, we find that property crime reports increase by 16% around prisons on release days. Specialized offenders drive the overall effect—those convicted initially for property crimes—as the rise in property crime reports is 26% higher on days any specialized inmate is released. Furthermore, improvements in non-criminal human capital do not mitigate the effects. This includes participation in either educational or occupational prison-based programs or even observed higher educational attainment at the time of release. Finally, through longer prison spells, harsher punishments do not seem to mitigate these effects either.

We also document two negative externalities resulting from incarcerating specialized offenders. First, we find sizeable adverse peer effects. We observe that property crime reports rise by 7% each day one additional non-specialized offender, who was confined along with a specialized criminal, is released. Second, we examine the adverse effects of prison location on property values due to increased crime incidence. Our estimates suggest that the aggregate impact of current prison locations on property values amounts to a loss of $15.9 billion nationwide, adjusting for purchasing power parity.

Setting up a prison system to work comes at a high cost for taxpayers. For instance, yearly expenditures in corrections in the U.S. are about $80 billion or $35 thousand per inmate (20% of the total justice system expenditures).[21] In Colombia, yearly expenditures

[21] See for instance Hyland (2019).

in corrections amount to \$1.9 billion or \$15 thousand per inmate.[22] Hence it is essential, from a policy perspective, to ensure the prison system works as intended.

For incarcerated offenders, the prison system should decrease crime—and all its social costs— through three main mechanisms: (i) deter the offender from re-engaging in criminal behavior; (ii) rehabilitate the offender by improving valuable skills for the legal sector; and (iii) prevent the offender from committing crimes while incarcerated. Our results suggest that the specific deterrence and rehabilitation mechanisms do not seem to work consistently. More broadly, the prison system should work through one additional mechanism: (iv) deter the general population from engaging in crime due to the threat of punishment. In a recent review on the effects of incarceration on crime that includes 34 different studies—most of them U.S.-based, Roodman (2017) concludes that the specific and general deterrence effects are minimal. Also, that the adverse criminogenic impacts of prison most likely cancel out the beneficial incapacitation and rehabilitation effects. Thus, putting these conclusions along with our results, it is far from clear that the use of incarceration—the way countries such as the U.S. or Colombia adopt it—leads to aggregate welfare gains.

Nonetheless, in some contexts, incarceration does lead to improved outcomes. There is evidence favoring all the mechanisms we outline above: specific deterrence (e.g., Rose and Shem-Tov 2021), rehabilitation (e.g., Bhuller et al. 2020), incapacitation (e.g., Buonanno and Raphael 2013), and general deterrence (e.g., Katz, Levitt and Shustorovich 2003). Our study, however, focuses on a rather unexplored population: specialized offenders with high baseline levels of criminal capital. More than 80% of released prisoners in the U.S. are re-arrested within nine years, and almost 70% are re-arrested within just three years (Alper, Durose and Markman 2018). In Colombia, 21% of released prisoners are re-arrested within five years, but for those with prior convictions, this figure rises to 73% (Tobón 2017). Hence, arguably, specialized criminals represent the most relevant segment of the prison population. Our results suggest that we should test current incarceration practices further. This

[22]See the budgets for the National Prison Authority and the Prison Service Agency.

includes, but should not be limited to, the attributes of incarceration we examine in this study: sentences, inmate assignment to specific prisons, wings and cells, and prison-based rehabilitation programs.

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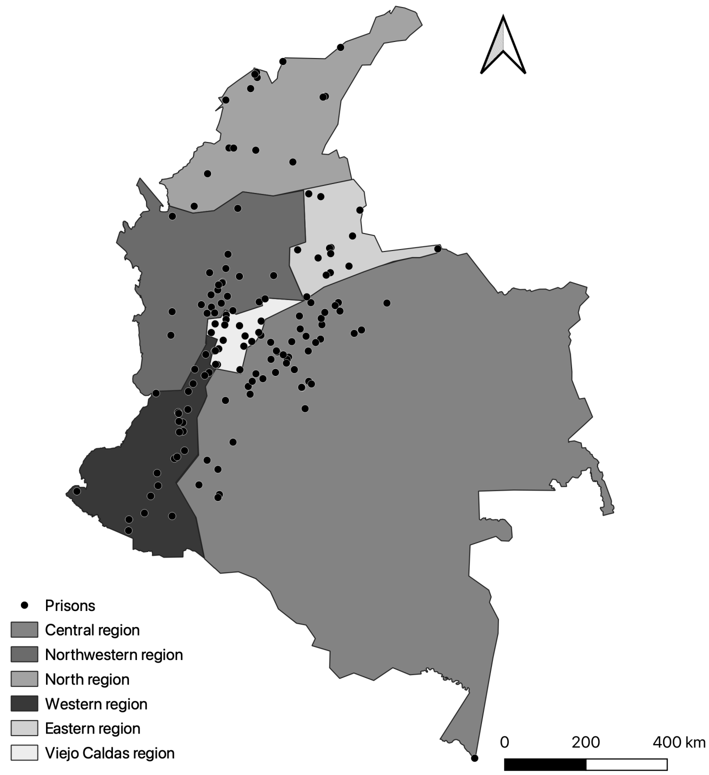
Appendix

A Supplementary figures and tables

Figure A.1: Prison location and INPEC regions



Notes: The figure depicts the location of the 138 prisons and the regional offices of the National Prison Authority.

Table A.1: Descriptive statistics and balance tests: specialized inmates

	Summary statistics		Balance tests			
			Specialized inmates are released		Other inmates are released	
	Con. mean	S.D.	Coeff.	S.E.	Coeff.	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)
A. Prison characteristics (proportion of inmates meeting each condition unless noted otherwise)						
Is a new offender	0.711	0.284	0.003	[0.002]	0.003	[0.002]
Is in prison for property crime	0.290	0.167	0.002	[0.001]	0.001	[0.001]
Is in prison for homicide	0.137	0.135	-0.001*	[0.001]	-0.001*	[0.001]
Is in prison for drug crime	0.232	0.172	0.002	[0.001]	0.002	[0.001]
Is in prison for other crime	0.429	0.221	0.001	[0.001]	0.001	[0.001]
Has below primary ed.	0.041	0.036	2.64e-05	[1.52e-04]	4.37e-05	[1.70e-04]
Has at most primary ed.	0.569	0.231	0.002	[0.002]	0.003	[0.002]
Has at most secondary ed.	0.230	0.112	0.001	[0.001]	0.001	[0.001]
Has tertiary education	0.028	0.039	6.72e-05	[1.70e-04]	1.18e-04	[1.57e-04]
Is enrolled in occup. prog.	0.559	0.269	0.001	[0.002]	0.002	[0.001]
Is enrolled in educ. prog.	0.485	0.245	0.001	[0.002]	0.002	[0.001]
Is authorized to receive visits	0.819	0.329	0.004	[0.003]	0.003	[0.003]
Has minor children	0.697	0.289	0.001	[0.002]	0.002	[0.002]
Is male	0.808	0.376	0.004	[0.003]	0.003	[0.003]
Is Colombian	0.868	0.339	0.004	[0.003]	0.004	[0.003]
Is less than 20 years old	0.079	0.045	0.001*	[4.94e-04]	0.001	[0.001]
Is between 21-30 years old	0.344	0.154	0.002	[0.001]	0.002	[0.001]
Is between 31-40 years old	0.220	0.099	4.38e-04	[0.001]	4.71e-04	[0.001]
Is between 41-50 years old	0.127	0.067	2.73e-04	[4.53e-04]	0.001	[4.38e-04]
Is between 51-60 years old	0.069	0.046	2.25e-04	[1.80e-04]	1.91e-04	[2.22e-04]
Is between 61-70 years old	0.022	0.020	-3.49e-05	[8.42e-05]	-3.27e-05	[9.13e-05]
Is more than 71 years old	0.007	0.011	5.75e-05	[5.27e-05]	4.05e-05	[6.61e-05]
Sentence (in months)	343.001	289.261	3.746	[3.158]	3.215	[4.577]

Notes: Columns (1) and (2) report summary statistics. Columns (3) to (6) report the coefficients and standard errors for an ordinary least square regression of each variable on two treatments (type of release day) and fixed effects (the results from each row come from an independent regression). Columns (3) and (4) report the coefficients and standard errors for an indicator variable for days on which any specialized inmate was released. Columns (5) and (6) report the coefficients and standard errors for an indicator variable for days on which other inmates—but none specialized in property crimes—were released. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Robust standard errors clustered at the prison level in brackets.

Table A.2: Effects of inmates' releases on crime, with exclusive donuts

	Control mean (1)	Coeff. (2)	S.E. (3)	R^2 (4)	Obs. (5)
A. Treatment is an indicator for release days					
Property crimes, 0-1km donut	0.114	0.012**	[0.005]	0.247	186,576
Property crimes, 1-2km donut	0.231	0.035***	[0.012]	0.530	186,576
Property crimes, 2-3km donut	0.192	0.026**	[0.012]	0.597	186,576
Property crimes, 3-4km donut	0.152	0.028*	[0.014]	0.638	186,576
Property crimes, 4-5km donut	0.100	0.026***	[0.008]	0.593	186,576
B. Treatment is the number of inmates who exit					
Property crimes, 0-1km donut	0.114	0.004	[0.003]	0.247	186,576
Property crimes, 1-2km donut	0.231	0.022***	[0.006]	0.530	186,576
Property crimes, 2-3km donut	0.192	0.015***	[0.005]	0.597	186,576
Property crimes, 3-4km donut	0.152	0.017**	[0.008]	0.638	186,576
Property crimes, 4-5km donut	0.100	0.017**	[0.005]	0.593	186,576

Notes: The table presents results for ordinary least squares regressions of the count of crimes within donuts around each prison on the treatment variables and fixed effects. Column (1) reports the control prison-day mean. Column (2) reports the regression coefficient for the treatment variable. Column (3) reports the standard errors. Column (4) reports the R-squared of the regression. Column (5) reports the number of observations. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Each estimation includes prison and day fixed effects. Robust standard errors clustered at the prison level in brackets.

Table A.3: Criminal capital persistence, with exclusive donuts

Variable	Control mean	Specialized inmates are released		Other inmates are released		p-value	R^2	Obs.
		Coeff.	S.E.	Coeff.	S.E.	diff.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Treatments are indicators for release of specialized inmates and release of other inmates								
Property crimes, 1km donuts	0.114	0.020***	[0.007]	4.41e-04	[0.005]	0.007	0.247	186,576
Property crimes, 2km donuts	0.231	0.058***	[0.017]	0.005	[0.008]	0.001	0.530	186,576
Property crimes, 3km donuts	0.192	0.043***	[0.018]	0.009	[0.016]	0.001	0.687	186,576
Property crimes, 4km donuts	0.689	0.165***	[0.058]	0.004	[0.007]	0.029	0.638	186,576
Property crimes, 5km donuts	0.100	0.039***	[0.012]	0.009	[0.006]	0.011	0.593	186,576
Panel B: Treatments are counts of specialized inmates and other inmates who exit								
Property crimes, 1km donuts	0.114	0.007**	[0.003]	0.001	[0.004]	0.172	0.247	186,576
Property crimes, 2km donuts	0.231	0.027***	[0.008]	0.016*	[0.006]	0.063	0.530	186,576
Property crimes, 3km donuts	0.192	0.020***	[0.006]	0.008	[0.005]	0.118	0.597	186,576
Property crimes, 4km donuts	0.152	0.019**	[0.010]	0.015*	[0.008]	0.575	0.638	186,576
Property crimes, 5km donuts	0.100	0.019***	[0.005]	0.014**	[0.006]	0.316	0.593	186,576

Notes: This table presents the ordinary least square regression of crimes, separated by property crimes and others, within the donuts around each prison on the treatment variables and fixed effects. Column (1) contains the control prison-day mean/ Column (2) reports the coefficient for property crimes and column (3) the standard error. Column (4) contains the regression coefficient for non-property crimes, with column (5) containing the standard errors. Column (7) reports the p-value of the difference, with column (7) reporting the R-suqared and column (8) the number of observations. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Each estimation includes prison and day fixed effects. Standard errors clustered at prison level.

Table A.4: Sensitivity checks (1km buffers)

	Control mean	Coeff.	S.E.	R^2	N
	(1)	(2)	(3)	(4)	(5)
Panel A: Only convicted inmates, treatment definition in each row (1km buffers)					
Any inmate is released	0.121	0.014**	[0.006]	0.247	186,576
No. of released inmates	0.121	0.003	[0.003]	0.247	186,576
Any specialized inmate	0.126	0.022***	[0.007]	0.247	186,576
No. of released spec. inmates	0.126	0.006	[0.004]	0.247	186,576
Panel B: Excluding one regional office of the prison authority at a time (1km buffers)					
Excluding Central region	0.115	0.008	[0.005]	0.224	129,792
Excluding Western region	0.107	0.012**	[0.005]	0.235	154,128
Excluding North region	0.107	0.012**	[0.006]	0.260	164,944
Excluding Eastern region	0.116	0.013**	[0.005]	0.252	167,648
Excluding Northwestern region	0.120	0.013**	[0.005]	0.251	158,184
Excluding Viejo Caldas region	0.118	0.013**	[0.006]	0.257	158,184
Panel C: Excluding one quarter at a time (1km buffers)					
Excluding Quarter 1	0.111	0.012**	[0.005]	0.552	136,758
Excluding Quarter 2	0.113	0.014**	[0.006]	0.554	136,344
Excluding Quarter 3	0.113	0.011**	[0.005]	0.553	138,138
Excluding Quarter 4	0.118	0.011**	[0.005]	0.566	148,488

Notes: The table presents results for ordinary least squares regressions of the count of crimes within buffers around each prison on the treatment variables and fixed effects. We use 1km buffers in all regressions. Panel A considers only releases of convicted inmates. The regressions when the treatment definition is "any specialized inmate is released" and "No. of released spec. inmates" include controls for a secondary treatment variable (either indicator or count). Panel B checks for robustness by excluding one regional office of the prison authority at a time. Panel C excludes one quarter of the year at a time. Column (1) reports the control prison-day mean. Column (2) reports the regression coefficient for the relevant treatment variable. Column (3) reports the standard errors. Column (4) the R-squared and column (5) the number of observations. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Each estimation includes prison and day fixed effects. Standard errors clustered at prison level.

Table A.5: Sensitivity checks (2km buffers)

	Control mean (1)	Coeff. (2)	S.E. (3)	R^2 (4)	N (5)
Panel A: Only convicted inmates, treatment definition in each row (2km buffers)					
Any inmate is released	0.375	0.042***	[0.016]	0.556	186,576
No. of released inmates	0.375	0.021**	[0.008]	0.556	186,576
Any specialized inmate	0.394	0.071***	[0.021]	0.556	186,576
No. of released spec. inmates	0.394	0.034***	[0.011]	0.556	186,576
Panel B: Excluding one regional office of the prison authority at a time (2km buffers)					
Excluding Central region	0.353	0.035*	[0.018]	0.531	129,792
Excluding Western region	0.318	0.042***	[0.016]	0.539	154,128
Excluding North region	0.312	0.052***	[0.018]	0.579	164,944
Excluding Eastern region	0.355	0.046***	[0.016]	0.555	167,648
Excluding Northwestern region	0.386	0.052***	[0.018]	0.563	158,184
Excluding Viejo Caldas region	0.348	0.053***	[0.018]	0.563	158,184
Panel C: Excluding one quarter at a time (2km buffers)					
Excluding Quarter 1	0.338	0.045***	[0.015]	0.552	136,758
Excluding Quarter 2	0.341	0.055***	[0.017]	0.554	136,344
Excluding Quarter 3	0.343	0.047***	[0.016]	0.553	138,138
Excluding Quarter 4	0.357	0.042***	[0.016]	0.566	148,488

Notes: The table presents results for ordinary least squares regressions of the count of crimes within buffers around each prison on the treatment variables and fixed effects. We use 2km buffers in all regressions. Panel A considers only releases of convicted inmates. The regressions when the treatment definition is "any specialized inmate is released" and "No. of released spec. inmates" include controls for a secondary treatment variable (either indicator or count). Panel B checks for robustness by excluding one regional office of the prison authority at a time. Panel C excludes one quarter of the year at a time. Column (1) reports the control prison-day mean. Column (2) reports the regression coefficient for the relevant treatment variable. Column (3) reports the standard errors. Column (4) the R-squared and column (5) the number of observations. Significance levels: $***p < 0.01$, $**p < 0.05$, $*p < 0.1$. Each estimation includes prison and day fixed effects. Standard errors clustered at prison level.

Table A.6: Sensitivity checks (3km buffers)

	Control mean	Coeff.	S.E.	R^2	N
	(1)	(2)	(3)	(4)	(5)
Panel A: Only convicted inmates, treatment definition in each row (3km buffers)					
Any inmate is released	0.589	0.070**	[0.027]	0.687	186,576
No. of released inmates	0.589	0.037***	[0.013]	0.687	186,576
Any specialized inmate is released	0.624	0.118***	[0.036]	0.687	186,576
No. of released spec. inmates	0.624	0.059***	[0.017]	0.687	186,576
Panel B: Excluding one regional office of the prison authority at a time (3km buffers)					
Excluding Central region	0.547	0.050*	[0.027]	0.662	129,792
Excluding Western region	0.500	0.073**	[0.029]	0.671	154,128
Excluding North region	0.473	0.084***	[0.030]	0.706	164,944
Excluding Eastern region	0.554	0.069**	[0.027]	0.685	167,648
Excluding Northwestern region	0.608	0.080***	[0.030]	0.692	158,184
Excluding Viejo Caldas region	0.545	0.080***	[0.030]	0.696	158,184
Panel C: Excluding one quarter at a time (3km buffers)					
Excluding Quarter 1	0.527	0.073***	[0.025]	0.688	136,758
Excluding Quarter 2	0.532	0.078***	[0.028]	0.685	136,344
Excluding Quarter 3	0.534	0.076***	[0.028]	0.680	138,138
Excluding Quarter 4	0.553	0.067***	[0.026]	0.696	148,488

Notes: The table presents results for ordinary least squares regressions of the count of crimes within buffers around each prison on the treatment variables and fixed effects. We use 3km buffers in all regressions. Panel A considers only releases of convicted inmates. The regressions when the treatment definition is "any specialized inmate is released" and "No. of released spec. inmates" include controls for a secondary treatment variable (either indicator or count). Panel B checks for robustness by excluding one regional office of the prison authority at a time. Panel C excludes one quarter of the year at a time. Column (1) reports the control prison-day mean. Column (2) reports the regression coefficient for the relevant treatment variable. Column (3) reports the standard errors. Column (4) the R-squared and column (5) the number of observations. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Each estimation includes prison and day fixed effects. Standard errors clustered at prison level.

Table A.7: Sensitivity checks (4km buffers)

	Control mean (1)	Coeff. (2)	S.E. (3)	R^2 (4)	N (5)
Panel A: Only convicted inmates, treatment definition in each row (4km buffers)					
Any inmate is released	0.760	0.098**	[0.042]	0.760	186,576
No. of released inmates	0.760	0.053***	[0.020]	0.760	186,576
Any specialized inmate is released	0.809	0.162***	[0.054]	0.760	186,576
No. of released spec. inmates	0.809	0.084***	[0.027]	0.760	186,576
Panel B: Excluding one regional office of the prison authority at a time (4km buffers)					
Excluding Central region	0.705	0.067**	[0.040]	0.739	129,792
Excluding Western region	0.647	0.098**	[0.043]	0.742	154,128
Excluding North region	0.597	0.117***	[0.045]	0.773	164,944
Excluding Eastern region	0.709	0.096**	[0.042]	0.761	167,648
Excluding Northwestern region	0.776	0.110**	[0.045]	0.764	158,184
Excluding Viejo Caldas region	0.708	0.113***	[0.044]	0.768	158,184
Panel C: Excluding one quarter at a time (4km buffers)					
Excluding Quarter 1	0.677	0.101***	[0.038]	0.761	136,758
Excluding Quarter 2	0.683	0.105***	[0.039]	0.757	136,344
Excluding Quarter 3	0.686	0.107***	[0.041]	0.754	138,138
Excluding Quarter 4	0.709	0.093***	[0.041]	0.768	148,488

Notes: The table presents results for ordinary least squares regressions of the count of crimes within buffers around each prison on the treatment variables and fixed effects. We use 4km buffers in all regressions. Panel A considers only releases of convicted inmates. The regressions when the treatment definition is "any specialized inmate is released" and "No. of released spec. inmates" include controls for a secondary treatment variable (either indicator or count). Panel B checks for robustness by excluding one regional office of the prison authority at a time. Panel C excludes one quarter of the year at a time. Column (1) reports the control prison-day mean. Column (2) reports the regression coefficient for the relevant treatment variable. Column (3) reports the standard errors. Column (4) the R-squared and column (5) the number of observations. Significance levels: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Each estimation includes prison and day fixed effects. Standard errors clustered at prison level.