

# Freedom in Oppressive Societies: How Emancipation Led to Imprisonment in Buenos Aires, 1820–1830

Valentín Figueroa  
Pontificia Universidad Católica de Chile

Guadalupe Tuñón  
Princeton University\*

## Abstract

We show that the emancipation of enslaved Black persons led to their subsequent imprisonment. To establish causality, we study a lottery of certificates of freedom in nineteenth-century Buenos Aires that randomly freed a small group of enslaved persons. Through archival research and digitization of the full count of the handwritten 1810 census, we link lottery winners and a set of eligible nonwinners to police records and use these data to assess the effect of emancipation on imprisonment. We find that emancipation increased the probability of imprisonment, on average, by 11.8 percentage points. Our results show that the link between emancipation and imprisonment predated abolition and was present in a non-labor-intensive economy, suggesting that punitive criminal justice systems were not crafted purely to cater to the labor market concerns of former slaveholders but were rather the reflection of a racist equilibrium that enforced the subjugation of Black persons.

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# 1 Introduction

After the abolition of slavery, societies had to adapt economically to formally free labor markets and socially to new forms of interaction with the recently emancipated groups.<sup>1</sup> Elites are thought to have responded to emancipation by turning to the state’s coercive apparatus—the police and criminal courts. The intense policing of racially oppressed groups after abolition was ubiquitous, taking place across societies from Mauritius (Allen, 2008) to the French Caribbean (Pluskota, 2020), the British Caribbean (Johnson, 1991), and Brazil (Huggins, 1985). Nowhere has this phenomenon been more studied than in the United States (Davis, 2006; Blackmon, 2009; Alexander, 2010).<sup>2</sup>

An open discussion in this literature is whether the former slaveholder elite crafted discriminatory laws and criminal justice systems after abolition to discipline the newly emancipated workforce (Blackmon, 2009; Oshinsky, 1996; Huggins, 1985; Johnson, 1991; Dippel, Greif and Treffer, 2020) or if the criminalization of free Black persons was just a reflection of a broader racist ideology prescribing their subjugation (Ulrickson, 2018; Davis, 2006; Alexander, 2010). In this article, we make an empirical contribution to this debate by estimating a positive effect of emancipation on imprisonment in the city of Buenos Aires—a nonplantation economy—prior to the abolition of slavery. Because labor market incentives to criminalize free Black individuals in this context were weak, our findings provide evidence of the role of a racist ideology in the imprisonment of free Blacks.<sup>3</sup>

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<sup>1</sup>For recent works on the political economy of slavery, abolition, and racial relations after abolition, see, e.g., Acharya, Blackwell and Sen (2016); Hall, Huff and Kuriwaki (2019); Suryanarayan and White (2021); Dippel, Greif and Treffer (2020); Jenkins and Peck (2021); Schwarz (2022); Mangonnet (2022).

<sup>2</sup>Scholars of American political development have argued that the harsh policing of emancipated Black persons in the US South allowed White elites to protect their dominant position in the racial hierarchy (e.g., Du Bois 1998; Acharya, Blackwell and Sen 2016, 2018) and procure cheap wage labor for their plantations (Alston and Ferrie, 1999; Blackmon, 2009).

<sup>3</sup>Our findings are consistent with Schwarz (2022)’s argument on the US South that the mass incarceration of emancipated Black persons preceded the development of the convict lease system—which only became dominant when penitentiaries became overwhelmed.

To estimate the effect of emancipation on imprisonment, we study a lottery conducted by colonial authorities in 1807 to distribute certificates of freedom to a small group of enslaved persons. The purpose of the lottery was to reward those who had served as combatants in the city’s defense during a prior invasion. The random assignment of enslaved persons to freedom through the lottery allows us to estimate the effect of emancipation by comparing emancipated individuals to the set of eligible individuals who were not selected in the lottery.<sup>4</sup> To assess the effect of emancipation on imprisonment, we manually linked individuals in our experimental sample with police records from the 1820–1830 period.

We find that emancipated individuals were, on average, 11.8 percentage points more likely to be imprisoned than those who remained enslaved (according to intent-to-treat estimates).<sup>5</sup> This result is robust to a host of alternative specifications and ways to define our study group. When we explore the mechanisms, we find no evidence that the effect is driven by the harvest months, when labor demand increased, which suggests that the effect is not explained by the need to discipline emancipated individuals into the labor force. We do find suggestive evidence of a positive impact of emancipation on imprisonment for robbery in particular —suggesting that emancipation could have produced material deprivation that pushed individuals towards property crime.

We also uncover a set of patterns that are consistent with the effect of emancipation on imprisonment being the result of a broader context of racial tension and a racist equilibrium that enforced the subjugation of Black persons. First, we show that emancipated

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<sup>4</sup>Even though the comparison is often implicit in qualitative research, the literature on racial oppression after emancipation compares free White persons with emancipated Black persons (or Black persons before and after abolition). While White/Non-White comparisons can, under certain assumptions, measure racial discrimination, they do not capture the effect of *emancipation* on imprisonment. A strength of our research design is that it directly assesses the effect of emancipation of Black persons by comparing freed and enslaved persons. To our knowledge, this is the first article to estimate quantitatively the effect of emancipation on incarceration with individual-level data. A similar paper to ours is Sacerdote (2005), which estimates the inter-generational effect of slavery on human capital by comparing the children and grandchildren of enslaved persons in the US to the children and grandchildren of non-enslaved African American persons.

<sup>5</sup>The estimated complier average causal effect is 15.1 percentage points.

persons were more likely to be involved in violent interactions, both as victims and perpetrators of violence, evidencing a context of racial tension and hostility towards free Blacks. Second, we show that emancipated persons were more likely to be imprisoned for discretionary charges that were easy to trump up (e.g., vagrancy and drunkenness), suggesting that they were arbitrarily targeted by the police. Finally, we discuss qualitative evidence that enslaved persons benefited from the protection of the person claiming ownership over them, suggesting that enslavement afforded Black persons protection against arbitrary punishment.

Our results confirm with quasi-experimental evidence the existence of a criminal system that discriminated against emancipated Black persons. The intense policing of newly emancipated individuals was part of a broader social ideology dictating the subjection of Black persons (Escott, 1979). Emancipated Blacks were more likely to be imprisoned than those enslaved not because of a quest for disciplining labor, but because emancipation did not erase the racist assumptions upon which slavery was based (Oshinsky, 1996). In essence, one could interpret our findings as suggesting that slavery and racially discriminatory criminal justice systems performed a similar function: controlling and regulating the behavior of racially oppressed groups. In the words of Angela Y. Davis (2006, p. 363), “the imprisonment of former slaves ... symbolically emphasized Black people’s social status continued to be that of slaves.”

## 2 Historical Context

In the late eighteenth century, Buenos Aires was a peripheral city of the Spanish Empire in America. Its commercial and political importance surged in 1776, when it became the capital of the newly created Viceroyalty of the River Plate. Enslaved persons were forcefully imported by Europeans from West Africa to the American colonies, and Buenos

Aires, unsuitable for labor-intensive agriculture, was the destination of a comparatively small number. Nonetheless, in 1810, a third of the population of Buenos Aires was Black, and approximately 80% of Blacks were enslaved (Goldberg, 1976). The abolition of slavery in Buenos Aires was a process that ended in the mid-nineteenth century. The slave trade was abolished in 1812. In 1813, a law declared that all children would be born free, even if the mother was enslaved—but established a 20-year period of mandatory apprenticeship for the “free-born” children. Slavery was fully abolished only in 1853 (Candiotti, 2016; Sobrevilla-Perea, 2022).

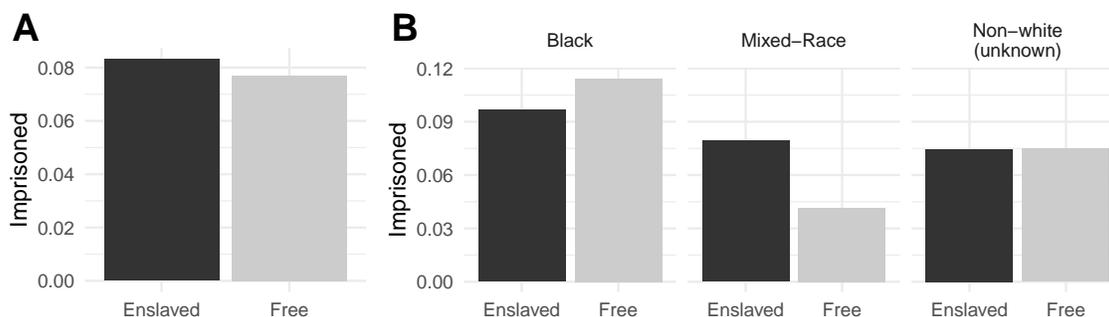
To characterize the relationship between slavery and imprisonment during the context of our study, we digitized the full count of non-White men over 15 years old in the 774 surviving pages of the handwritten 1810 census of the city. We then manually linked each of the 2,672 names to police records for 1820–1830 to assess whether they had ever been imprisoned during that period. As shown in Panel A of Figure 1, the overall rates of imprisonment among enslaved and free non-Whites were almost indistinguishable (8.3% and 7.7%, respectively). While these rates mask variation across racial groups (Panel B), there is no racial group for which the difference in the imprisonment rates across free and enslaved individuals is statistically different from zero.

The high rates of imprisonment of enslaved individuals may be surprising to readers unfamiliar with the context of our study.<sup>6</sup> Unlike the plantation economies of the US South, Brazil, and the Caribbean, the environs of Buenos Aires and other places in South America were not suited for growing labor-intensive crops, so enslaved persons were not bought for labor-intensive agriculture (Sobrevilla-Perea, 2022). Instead, the nature of urban slavery in Buenos Aires was “stipendary” (Saguier, 1989). Slave owners forced

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<sup>6</sup>The imprisonment rates that we report are not abnormal from a comparative perspective. Allen (2008), for instance, reports that in Mauritius, 4% of all Indian males were imprisoned, on average, for vagrancy and desertion each year from 1852 to 1863 and over 12% from 1861 to 1871.

**Figure 1:** Imprisonment rates among non-Whites, 1820–1830



Notes: The figures show the proportion in the 1810 census of individuals who were imprisoned at least once during the 1820–1830 period. Panel A shows imprisonment by slave status for all persons, while Panel B shows variation across both race and slave status. Black refers to individuals who are listed in the census as “negro” or “moreno” (N=919), mixed-race includes “mulato” and “pardo” (N=401), and the third category includes individuals classified as non-Whites but whose specific racial group was not available (N=1352).

enslaved persons to sell their labor outside the household as domestic servants and artisans, and enslaved persons were expected to pay a daily stipend to those claiming ownership over them (Goldberg, 1976). This form of slavery had two consequences regarding the interaction between enslaved persons and the police. First, enslaved persons in Buenos Aires were more integrated into colonial society and had more freedom of movement than those in plantation economies (Rebagliati, 2014). The typical day of an enslaved person in Buenos Aires took place “on the street, in public spaces, in taverns, in stores, in markets” (Bernand, 2000). As a result, both enslaved and free individuals were similarly exposed to law enforcement on a daily basis. Second, as enslaved persons were not forced to work in labor-intensive agriculture, there were no plantations with private jails like those in the US (Davis, 2006), and no slave patrols emerged to privately police enslaved persons, as in the US and Brazil (Wilson, 2022).

The police could not immediately observe the legal status of Black persons (i.e., whether

they were freemen or enslaved). Yet policemen could ask individuals about their legal status and Buenos Aires was small enough for the police, or those reporting to the police, to learn through repeated interaction who was free and who was enslaved. Most importantly, the minority of Black persons who were free were closely monitored by the state because of the paternalistic belief that they needed some degree of tutelage—if not from a master, from the state (Candiotti, 2010). For example, associations of free individuals of African descent existed in Buenos Aires during most of the nineteenth century; and since the 1820s these associations needed official authorization to function and were subject to police surveillance (Chamosa, 2003; Vaccaroni, 2021).

### 3 Research Design

#### 3.1 The *Plaza Mayor* Lottery

To identify the effect of emancipation on the imprisonment rates of formerly enslaved persons, we exploit a natural experiment from early nineteenth-century Buenos Aires. When the British invaded Buenos Aires in 1806–7, the city was recaptured with the help of urban militias that included enslaved persons. Municipal authorities decided to reward the enslaved combatants with certificates of freedom. Numerous historical sources describe the lottery (see, e.g., Lobo (1875) and Mitre (1877, 153)). We further verified and reconstructed its terms through research in Argentina’s National Archive—the *Archivo General de la Nación* (henceforth AGN). We provide evidence on the decision to hold the lottery and its public announcement in Appendix A, Figures A1, A2, and A3.

Three sets of previously enslaved individuals were liberated after the British invasions: (1) all of those who were physically impaired as a result of their cooperation in the defense efforts, (2) twenty-five individuals whose merit in defending the city was to be rewarded

with their freedom, and (3) forty-five of the remaining combatants. This final set of individuals was to be chosen through a public lottery performed in the *Plaza Mayor* on November 12th, 1807. This lottery provides us with random variation in the emancipation status of those eligible to participate in it.

### 3.2 Sample and Data

To identify the causal effect of emancipation, we would ideally have access to the comprehensive list of lottery participants and winners, which would allow us to directly compare the lottery winners from this list to those who entered the lottery and lost. The historical records available at the AGN allowed us to reconstruct the full list of lottery winners.<sup>7</sup> Unfortunately, however, the full list of participants has not survived. We obtained instead the official list of the surviving enslaved combatants who were members of the “Battalion of Slaves” during the British invasions, which includes their names and an indicator of whether they were wounded in combat.<sup>8</sup> Because all enslaved persons formally participating in the defense efforts were enlisted in the Battalion of Slaves (Cuadra Centeno and Mazzoni, 2011; Palombo, 2007), our list is the closest one can get to a comprehensive list of those eligible for the lottery, and we use this list to approximate the lottery participants. We then assume that all those eligible entered the lottery or at least are equivalent in their potential outcomes to those who entered and subsequently won.

We compare the imprisonment rate of the 45 individuals in our list of lottery winners to the imprisonment rate of the 150 individuals in our survivor list who did not win the lottery. It is important to note that our treatment group includes all lottery winners, only a

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<sup>7</sup>The historical records provide details on all individuals who were offered certificates of freedom, indicating whether they were chosen due to their merit or through the lottery. *Relación Circunstanciada de los Premios de Libertad que ha Concedido el M.I.C. de la Capital de Buenos Ayres a la Esclavatura de Ella, por Mérito que Contrajo en su Defensa del Día 5 de Julio del Presente año 1807*, Buenos Ayres en la Real Imprenta de los Niños Expósitos, Año de 1807, Academia Argentina de Letras (AAL): L Caja 75-12.

<sup>8</sup>Slave companies of the Viceroyalty of the River Plate (1808-1809), AGN, 26-7-5, Sala IX.

subset of whom could be matched by name to the list of combatants in the control group. The overlap between the two lists is small, due to the typical documentary difficulties surrounding historical research on enslaved persons (see, e.g., Logan and Pritchett (2018)). Little information on enslaved people was ever collected, even less has survived, and in the subset of surviving documents, the names of enslaved persons were sometimes unreliably listed. Reassuringly, however, the quality of names across the treatment and control groups is comparable (see Table 1).

To measure individual-level imprisonment, we manually matched the individuals in our sample with police records from the *Archivo General de Policía* for the 1820–1830 period.<sup>9</sup> We considered exact phonetic matches because the spelling of names was not standardized.<sup>10</sup> Unfortunately, there is no information on imprisonment for the years 1807–1820—a period of political instability with the Wars of Independence and the strife that followed. Police records indicate which individuals were imprisoned each year and provide a short description of the violation that they were accused of—e.g. robbery, assault, and public intoxication—and information on whether an individual was victimized.

### 3.3 Estimation

We are interested in estimating the effect of emancipation on the probability that an individual is imprisoned. We first estimate the following reduced-form equation using OLS:

$$\text{Imprisoned}_i = \alpha + \beta \text{Lottery Winner}_i + \epsilon_i \quad (1)$$

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<sup>9</sup>Our source is the *Indice del Archivo del Departamento General de Policía desde el Año 1812*, Buenos Aires: La Tribuna, 1858. When individuals did not have a surname, we assigned them the surname of their (former) owner.

<sup>10</sup>It is possible that our matching strategy produces false positives—i.e., individuals in our sample could be matched to individuals with the same name in police records. For our results to be valid, we need to assume that the false positive rate is similar across groups. Table 1 shows that individuals in the treatment and control group had on average the same number of matches to names in the 1810 census.

where  $i$  indexes individuals,  $\text{Imprisoned}_i$  is an indicator measuring whether  $i$  was imprisoned between 1820 and 1830, and  $\text{Lottery Winner}_i$  indicates that  $i$  was randomly selected to be freed through the public lottery. The coefficient of interest is  $\hat{\beta}$ , which measures the intent-to-treat (ITT) effect of being assigned to be emancipated on the probability of imprisonment.

We focus on the ITT in our baseline estimates because our experimental sample has non-compliance: ten of the 45 winners were ex post denied liberation by their owners (see additional discussion below).<sup>11</sup> We extend the results from Equation 1 by estimating the complier average causal effect (CACE) through an instrumental variables strategy where we use the random assignment to treatment by the public lottery as an instrument for freedman status. We estimate the following equation:

$$\text{Imprisoned}_i = \gamma + \lambda \widehat{\text{Emancipated}}_i + \mu_i \quad (2)$$

where  $\widehat{\text{Emancipated}}_i$  indicates the predicted values for individual  $i$  from a regression of the indicator for emancipation from slavery on the outcome of the lottery.<sup>12</sup>

Our natural experiment affords us a relatively small sample. Therefore, we use randomization inference and test the sharp null hypothesis of no effect for any units. The exact p-values that we report quantify the probability of observing, if emancipation has no effect on the probability of imprisonment, an effect of the size that we observe (or larger) purely by chance in the process of random assignment of the treatment.

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<sup>11</sup>Lobo (1875). See the Appendix for more details.

<sup>12</sup>For the IV strategy to work, it is crucial that a strong first-stage relationship exists between lottery winners and emancipation. The results from a simple regression of the binary indicator of emancipation on the binary indicator for lottery winners (the first stage) provides evidence that this is the case: the main estimate is .78, and the F statistic from this regression is 154.

### 3.4 Identifying Assumptions

The validity of the ITT strategy rests on two further identifying assumptions. The first assumption is that all eligible slaves entered the lottery, or at least that owners did not strategically influence which of the surviving slaves did. If owners were more likely to allow men who were more prone to violence to participate in the lottery, we would incorrectly detect a larger effect of emancipation on imprisonment. We believe this assumption is sensible in the context of our study because all the eligible slaves had already been allowed by their owners to serve in the defense of the city against the British invasion. This event was akin to the lottery in that it entailed a non-trivial probability of “losing” that enslaved person by death or physical impairment. As a result, none of the individuals eligible for the lottery was a person whom his master was unwilling to surrender.

The second assumption is that the certificates of freedom were indeed as good as randomly assigned among individuals in the Battalion of Slaves and that the potential outcomes of the individuals in our treatment group are equivalent to those of the individuals in our control group. Crucially, the historical record suggests that the lottery took place and that the physical randomization of the treatment was met (Lobo, 1875; Mitre, 1877).<sup>13</sup> The emancipated gained their freedom purely by chance and not because of any personal characteristic that distinguished them from those who remained enslaved and that could also affect imprisonment—such as their combat experience, assimilation into colonial society, deference towards Whites, or work ethic.

In addition to physical randomization, it is desirable that the treatment and control groups are balanced with respect to pre-treatment characteristics. In addition to sex and combat experience, which are identical across groups by design, both groups are balanced, on average, with respect to other covariates. The proportion of individuals named after a

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<sup>13</sup>We provide more details of the lottery process in Section B in the Appendix.

**Table 1:** Balance table

	<b>Treatment</b>	<b>Control</b>	<b>Difference</b>	<b>N</b>	<b>Exact p-value</b>
Male	All	All	Equal by design	195	
Combatant	All	All	Equal by design	195	
Apostolic name	0.156	0.207	-0.051	195	0.436
Name length	14.978	14.24	0.738	195	0.162
Nr matches (5%)	0.333	0.6	-0.267	195	0.436
Nr matches (10%)	1.178	1.547	-0.369	195	0.644
Age (5% matches)	28.17	26.811	1.358	36	0.713
Age (10% matches)	30.792	30.782	0.01	68	0.998

Notes: Exact p values are from Fisher’s randomization inference tests of the sharp null hypothesis of no individual treatment effect.

Catholic apostle is similar across groups (15.6% vs 20.7%),<sup>14</sup> and so is the average name length (14.98 vs 14.24 letters). Individuals in the treatment and control are also balanced regarding the number of names with which they are matched in the 1810 census, considering Levenshtein distances of 5 and 10%. Finally, in the subset of individuals whom we could match to the census, the average ages in 1810 of those assigned to the treatment and control groups are similar when we consider Levenshtein distance thresholds of 5% (28.17 vs 26.81 years old) and 10% (30.79 vs 30.78 years old).<sup>15</sup>

The validity of the IV estimates require two further assumptions. The first one is the exclusion restriction, which requires the instrument (the lottery) to influence the outcomes of interest only through the emancipation. Given that lottery winners only accessed emancipation, we believe this to be a reasonable assumption. The second additional assumption is that noncompliance is limited to the one that we observe—noncompliance coming from lottery winners whose masters did not agree to their emancipation. That is, we are assuming no additional noncompliance stemming from lottery winners who were freed and

<sup>14</sup>Apostolic names are Pedro, Santiago, Juan, Andrés, Bartolomé, Judas, Mateo, Felipe, Simón, and Tomás.

<sup>15</sup>When we matched an individual in our sample with multiple persons in the census, we assigned that individual the average age of his census matches.

later enslaved again or individuals who were emancipated despite losing the lottery. Given the need for these additional assumptions, we report the CACE estimates but focus the description of our results on the ITT results.

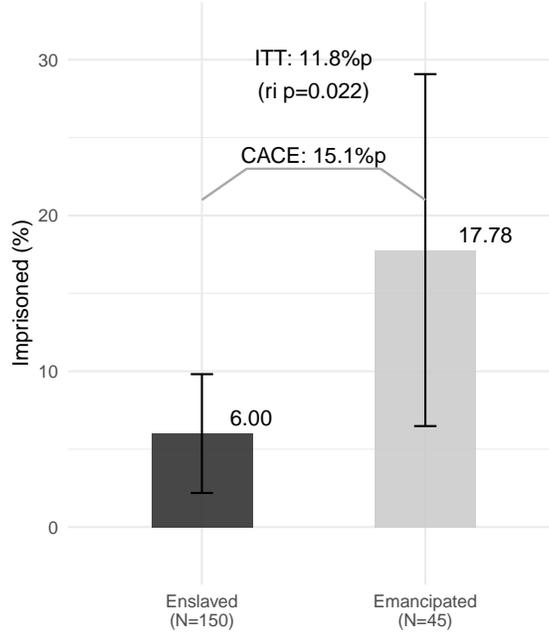
## 4 Results

Did emancipation increase imprisonment? Figure 2 displays our baseline results comparing the lottery winners to the enslaved combatants. The point estimate of the impact of emancipation indicates that, on average, emancipated individuals were 11.8 (ITT) percentage points more likely to be imprisoned than enslaved individuals—more than a 195% increase from the control group mean of 6 percentage points. The exact p-value indicates that the probability of observing a difference this large purely by chance if emancipation had no effect would be 2.2%. Table 2 reports these results in detail (Column 1) and extends them in several important ways.

The results presented above focus on the extensive margin of imprisonment (whether individuals were imprisoned at least once). We are more interested in this outcome because jail spells might affect future criminality (see, e.g., (Aizer and Doyle, 2015)), introducing bias in our estimates of the effect of emancipation on imprisonment. However, in Column 2, we report results for the intensive margin (the number of times an individual was imprisoned). We show that emancipation significantly affected imprisonment on the intensive margin. According to our baseline estimates, emancipation, on average, led to a 0.17 (ITT) increase in the number of episodes of imprisonment (exact p value  $<0.01$ ).

While the announcement of the lottery stated that those physically impaired during combat would all be eligible to be liberated prior to the lottery, the list of emancipated individuals that we retrieved in the archive did not include anyone emancipated explicitly under this clause. This would bias our results if a subset of the individuals in our con-

**Figure 2:** The effect of emancipation on imprisonment, 1820–1830



trol group, currently coded as lottery losers, was actually emancipated due to their health condition. To address this issue, we leverage the fact that the list of survivors from the Battalion of Slaves indicated the individuals who were wounded in combat, and assume this group to be that of those physically impaired. We then evaluate the robustness of our main results to analyzing this group in two alternative ways. First, we consider this issue as a compliance problem and simply code all of those described as wounded as automatically emancipated (Column 3). We alternatively consider that the liberation of this group followed the order planned in the event announcement and effectively took place prior to the lottery and re-estimate our main analysis excluding wounded individuals from our sample (Column 4). Both sets of results are closely in line with those in our main specification

**Table 2:** The effects of emancipation on imprisonment, 1820–1830

	(1)	(2)	(3)	(4)	(5)
<b>Mean in Control</b>	0.060	0.073	0.060	0.060	0.057
<b>Panel A: Reduced Form (ITT)</b>					
Lottery Winner	0.118+ (0.061)	0.171+ (0.094)	0.118+ (0.061)	0.118+ (0.061)	0.121* (0.060)
<b>Panel B: 2SLS</b>					
Emancipated	0.151+ (0.078)	0.220+ (0.120)	0.187+ (0.096)	0.151+ (0.078)	0.190* (0.095)
<b>Exact p-value</b>	0.022	0.017	0.019	0.020	0.014
Num.Obs.	195	195	195	173	195
Outcome	Binary	Count	Binary	Binary	Binary
Sample	Full	Full	Full	Lottery	Full+Merit
Wounded as Emancipated	No	No	Yes	N/A	No

+  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

All specifications are estimated using ordinary least squares (ITT) and two-stage least squares (CACE).

and confirm the effect of emancipation on imprisonment.

Finally, our main sample excludes those emancipated due to their performance in combat because all contemporary accounts state that their liberation took place before the lottery. We consider the alternative that their liberation did not precede the lottery and include these additional 25 individuals in the sample. Column 5 reports the result of this analysis. The results are robust to their inclusion.

#### 4.1 Additional Robustness Checks

**Differential attrition.** A potential threat to the validity of our results is that they might arise mechanically under differential attrition, for example if enslaved persons were

more likely to die before 1820 than those emancipated, making us less likely to find them in administrative records like police files. Theoretically, however, it is unclear how differential attrition would affect our results. On one hand, enslaved persons are thought to have been more likely to join the military and die during the wars of independence (Mallo, 1991)—which would bias our results upwards. Yet, on the other hand, emancipated persons could also have been more likely to migrate out of Buenos Aires because they were unbound to a master—which would bias our results downwards. Our data allow us to address concerns about selective attrition in two ways. In Table 1, we show that emancipated and enslaved persons were equally likely to be matched to records from the 1810 census—suggesting there was no selective attrition in this period. Furthermore, we can examine empirically whether enslaved persons in our sample were more likely to join the military. Police files often mention when individuals were conscripted. We find that in the period 1820–1830 enslaved persons were no more likely than those emancipated to be conscripted (exact  $p$ -value = 0.592).

**Small sample issues.** A second concern is that our baseline sample includes only 195 individuals. We use randomization inference in the analysis to avoid potential inference issues. We further explore the robustness of our results to this issue by analyzing how the results change when we iteratively drop observations. We first show that the main results are robust to dropping any single observation from the study group (Appendix Figure A6). We then further probe our results by iteratively dropping each possible pair of observations (Appendix Figure A7). Together, these robustness checks show that our results are not driven by any single observation or pair of observations.

**Alternative control groups.** Finally, we complement our main results by comparing the lottery winners with an alternative control group—the set of all enslaved men over

15 in the full count of the 1810 census. This comparison may suffer from bias, among other reasons because not all individuals in the census had combat experience, which prior research links to subsequent criminality (Galiani, Rossi and Schargrodsky, 2011; Hjalmarsson and Lindquist, 2019), and to factors that might affect criminality like earnings (Angrist, 1990) and organizational skills (Jha and Wilkinson, 2012). However, it provides an additional way to benchmark our results. We report the differences in imprisonment between our treatment group and the enslaved men in the census in Table A2 in the Appendix. The results largely reflect those in our main analysis. The ITT effect of winning the lottery on imprisonment is positive across specifications, and the magnitude ranges from 8.2 to 9.6 percentage points.

## 5 Mechanisms

We have shown that emancipation made individuals in early nineteenth-century Buenos Aires more likely to be imprisoned. In this section, we consider four plausible channels linking emancipation to imprisonment: a mechanical increase in crime as a result of more free time, a system of oppression responding to elites' labor market demands, an increase in crime in response to increasing material needs, and an environment of racial tension and racist control.

### 5.1 Opportunity

When people think about slavery, the first thing that comes to mind is typically the plantation system of the US South, Brazil, or Cuba. In plantation economies, enslaved persons were segregated in rural estates and had almost no autonomy of movement. Therefore, some readers might believe that the pattern that we detect is mechanical—that enslaved persons were less likely to run afoul of the law simply because they were less free. Never-

theless, as we have mentioned before, the institution of slavery in Buenos Aires (and many other Latin American cities) was nothing like slavery in plantation economies. Enslaved persons in Buenos Aires were not forced to work in the countryside; instead, people claiming ownership over them demanded a daily payment, known as a “stipend,” and enslaved persons worked as artisans or construction workers or in other low-skill jobs outside of their masters’ households (Saguier, 1989). Indeed, we showed in Figure 1 that enslaved persons were imprisoned just as frequently as free non-White individuals.

Even if unlikely, it is possible that enslaved persons spent less time in public spaces than emancipated persons. Given that lottery winners were imprisoned with a probability of 0.17 and nonwinners with a probability 0.06, the ratio of time spent in public across the liberated and enslaved would have to be at least 2.96 for this difference to fully explain our results. For the reasons previously discussed, it would be unlikely to see such a large difference in the context of Buenos Aires.

## 5.2 Labor market demands

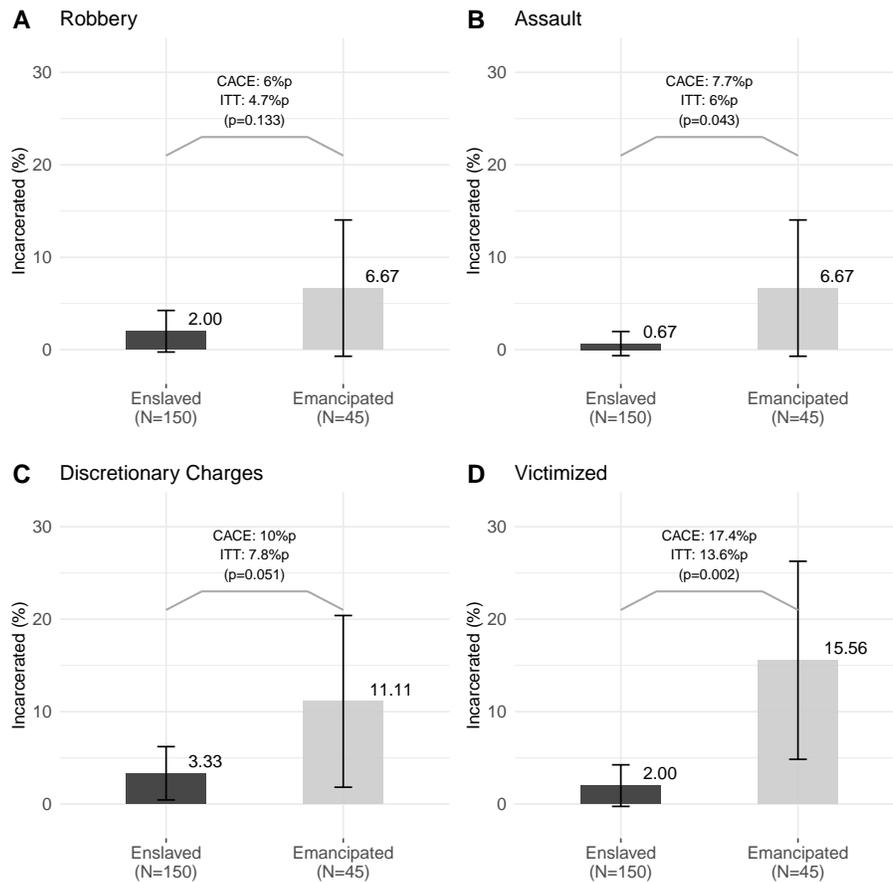
A common explanation for the intense policing of previously enslaved persons after abolition is that abusive criminal systems gave emancipated persons incentives to entrust themselves to employers who could protect them (Alston and Ferrie, 1993, 1999)—in other words, that the intense policing of the emancipated was motivated by labor market considerations. This mechanism is also unlikely in our context because Buenos Aires did not have a labor-intensive economy. Nevertheless, to test this mechanism, we examine whether the effect of emancipation on imprisonment was higher during the harvest season for wheat (from October to January), when the demand for labor was at its highest. Table 3 displays the results, which suggests that the effect of emancipation, if anything, was lower during the harvest season (0.03 vs. 0.06) and not significantly different from zero.

**Table 3:** Effect of emancipation, harvest vs. nonharvest months

	Harvest Months	Nonharvest Months
ITT Effect	0.033	0.062+
Exact p-value	0.403	0.081

+ p < 0.1, \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

**Figure 3:** Effect of emancipation on imprisonment by type of criminal charge, 1820–1830



### 5.3 Economic necessity

Another possible explanation for the higher imprisonment rate of emancipated individuals is that freedom also implied a higher incidence of poverty, which might have led emancipated individuals to commit more crimes (Curtin, 2000). In fact, the positive effect of emancipation on imprisonment during nonharvest months reported in Table 3 could be read as suggestive evidence for this mechanism, as poverty may have increased in months with lower labor demand. A second observable implication of this explanation is that we should expect the effect of emancipation on imprisonment to be driven by robbery crimes in particular. In Panel A of Figure 3, we report the results of the effect of emancipation on imprisonment due to robbery. We detect a small positive effect, suggesting that economic necessity is a potential mechanism—even though the coefficient is not statistically significant.

### 5.4 Racial tension and oppression

Finally, we show evidence of a context of racial tension and arbitrary punishment against emancipated persons. We also discuss qualitative evidence that Black persons could avoid arbitrary punishments by subjecting themselves to a White owner—suggesting a trade-off between private oppression by a master and public oppression by the state.

**Hostile environment.** We first assess whether emancipated individuals were more likely to be involved in violent interactions with other persons as both victims and perpetrators of violence. In Panel B of Figure 3, we show that emancipation had a positive and significant effect on the probability of being imprisoned for assault. Panel D complements these findings, showing that emancipated persons were also more likely to be victimized than their enslaved counterparts. These patterns evidence a context of social hostility to-

wards emancipated Black persons. Studies on the US have documented similar patterns of systematic violence against emancipated Black persons, known as lynching (e.g., Acharya, Blackwell and Sen (2018))—violence that rarely targeted enslaved persons (Clarke, 1998).

**Arbitrary punishment.** In addition, we also find evidence of the criminalization of behaviors that do not obviously constitute criminal offenses—such as vagrancy, insulting gestures or acts, and drunkenness—which disproportionately targeted emancipated persons. Engagement in this type of behavior by Whites was often tolerated but led to conviction when it was instead carried out by emancipated Black individuals (Davis, 2006). In Panel C, we show that emancipated persons were significantly more likely to be imprisoned for these discretionary charges, suggesting arbitrary punishments against them. This finding is consistent with Curtin (2000), who in the context of Alabama argues that the fabrication of charges against emancipated Black persons was common; and with Mallo (2005), who describes how Black persons in colonial Buenos Aires were presumed to be criminals and discriminated against by law enforcement.

**Paternalistic protection.** Studying the US South, Alston and Ferrie (1993) argue that such a context of racial hostility and arbitrary punishment gave Black persons incentives to entrust themselves to a White patron who could protect them. While our data does not allow us to test this quantitatively using our research design, qualitative data from the period confirms this dynamic was also in place in the context of our study. We examined a set 33 criminal cases compiled by Fernandez Plastino (2001) involving a Black defendant accused of physical assault in Buenos Aires in the period 1776-1810.<sup>16</sup> Whereas emancipated persons were always found guilty, only 30% of the enslaved defendants were convicted. The paternalistic protection that slave owners provided to those whom they

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<sup>16</sup>In 23 of these cases, the defendant was enslaved, and in 10 cases the defendant was a free person.

enslaved was acknowledged by the public defender (*Defensor de Pobres*), who argued in 1777 that “slaves cannot be convicted without hearing their masters first.”<sup>17</sup> For example, when the enslaved person Alberto Azevery was accused of stabbing a man, he was set free by the judge, who ordered his owner to donate 1500 bricks to the jail and pay the full cost of the judicial process.<sup>18</sup> Emancipated Black persons in criminal court faced a completely different fate, including convictions even against evidence. Consider the case of Francisco Jayme who was accused of wounding his neighbor in the thigh with a gun while intoxicated. Even though the wounded neighbor declared that the situation was an accident and that Francisco was not drunk, Francisco was nonetheless sentenced to six months in prison.<sup>19</sup> These criminal cases show that enslaved persons were actively defended by the persons claiming ownership over them.

## 5.5 Discussion

Our interpretation of the evidence presented in the discussion of mechanisms is that Black persons in Buenos Aires were oppressed regardless of labor market considerations. Enslaved persons were privately oppressed by slaveholders who guaranteed them basic living conditions and could protect them from arbitrary imprisonment. It was in a slaveholder’s best interest to protect those whom they enslaved. Emancipated persons, on the other hand, were unbound to a master but were nevertheless socially oppressed: there is some evidence that poverty pushed them into property crime, they were more likely to be involved in violent interactions (both as victims and perpetrators of violence), and were also more vulnerable to arbitrary imprisonment than enslaved persons.

Even though we can rule out labor market considerations as a mechanism, a limitation

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<sup>17</sup>AHPBA. JC. 34-1-9-57 / 1777

<sup>18</sup>AHPBA. JC. 34-2-23-32 / 1798.

<sup>19</sup>AHPBA. JC. 34-1-12-4 / 1783.

of our quantitative approach is that we cannot distinguish between two slightly different (and not mutually exclusive) processes of racial oppression that could underlie our results. One possibility is that the police targeted emancipated Black persons *because* of their legal status in a conscious attempt to subjugate them. An alternative possibility is that the police criminalized Black persons regardless of legal status but treated enslaved persons more leniently because they were enslaved—so, for example, their masters could advocate on their behalf. In any case, these two processes have a common core feature: a racist equilibrium made subjugation effectively impossible to escape. Those who were not privately oppressed by a master became publicly oppressed by the state.

## 6 Conclusion

This article offers quasi-experimental evidence that emancipated Black persons were imprisoned more frequently than enslaved individuals—a contention that previous studies have supported only with qualitative evidence (Du Bois, 1998; Blackmon, 2009; Oshinsky, 1996; Alexander, 2010).<sup>20</sup> We show that incarceration rates increased among emancipated individuals and that these effects are best explained by a context of racial tension and the arbitrary use of the police for racial control (Ulrickson, 2018; Davis, 2006; Alexander, 2010).

While the problem of labor discipline may have been a key driver of the racist punitive turn after abolition in the United States (Blackmon, 2009; Oshinsky, 1996) and the British Caribbean (Dippel, Greif and Treffer, 2020), our evidence shows that emancipated persons were also more likely to be imprisoned in Buenos Aires, a non-labor-intensive economy—and even before abolition and off harvest season. Our findings indicate that

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<sup>20</sup>Eubank and Fresh (2021) study econometrically the related question of the impact of the extension of voting rights on the imprisonment rates of African Americans.

the discrimination in the criminal justice system was not driven exclusively by economic incentives to discipline a newly free workforce. Instead, the context of racial tension was enough to motivate the use of the state as a tool of racial control (Ulrickson, 2018; Davis, 2006; Alexander, 2010) even in the absence of economic incentives.

Our results may also be interpreted as suggesting that slavery and police control were, at least to some extent, functional substitutes. Similar substitution patterns have been detected in other contexts, as well. For instance, in the US South, slave patrols were replaced by state-sanctioned police officers (Wilson, 2022), and the lynching of emancipated Black persons became common after abolition (Pfeifer, 2009), until mob violence targeting Black persons was supplanted by capital punishment (Clarke, 1998). In a similar vein, as Davis (2006, p. 363) has argued, upon emancipation “Black people entered into a relationship with the state unmediated by a master, they were divested of their status as slaves in order to be accorded a new status as criminals.”

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# Appendix

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2. Section B illustrates the mechanics of how the lottery worked (p. A7).
3. Section C provides details on how we digitized the 1810 census and matched the individuals in our experimental groups to names in the census (p. A8-A9).
4. Section D reports the results of Figure 3 in table format (p. A10).
5. Section E reports the results obtained when we iteratively exclude individuals (and pairs of individuals) from our sample (p. A11).
6. Section F reports the results obtained when we use the enslaved persons in the 1810 census as the control group (p. A12).

## A Archival Material on the Lottery

This appendix provides archival evidence on the decision to hold the lottery (A1 and A2) and its public announcement (A3). Note that, originally, only 25 slaves were to have been freed (20 by lottery and 5 by selection). On the day of the lottery, municipal authorities announced the liberation of an additional 45 slaves (25 by lottery and 20 on grounds of individual merit) sponsored by the Spanish king, which we document in Figure A4. As a result, 45 individuals were liberated by the lottery and 25 by selection, making the total number of enslaved persons liberated on the ceremony equal to 70.

While the original terms also required evidence that their masters would agree to liberate them if they were selected, there is no archival evidence indicating that slaves were asked to present the evidence of their masters' agreement to liberate them before the lottery took place. Instead, a subset of winners could not be freed because their masters did not agree, suggesting that the proof of agreement was required only after the lottery.<sup>21</sup>

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<sup>21</sup>See Acuerdo del Cabildo del 15 de Noviembre de 1807, AGN, Sala IX, Argentina. This number is confirmed in the Acuerdo del 28 de Abril de 1808, AGN, Sala IX.





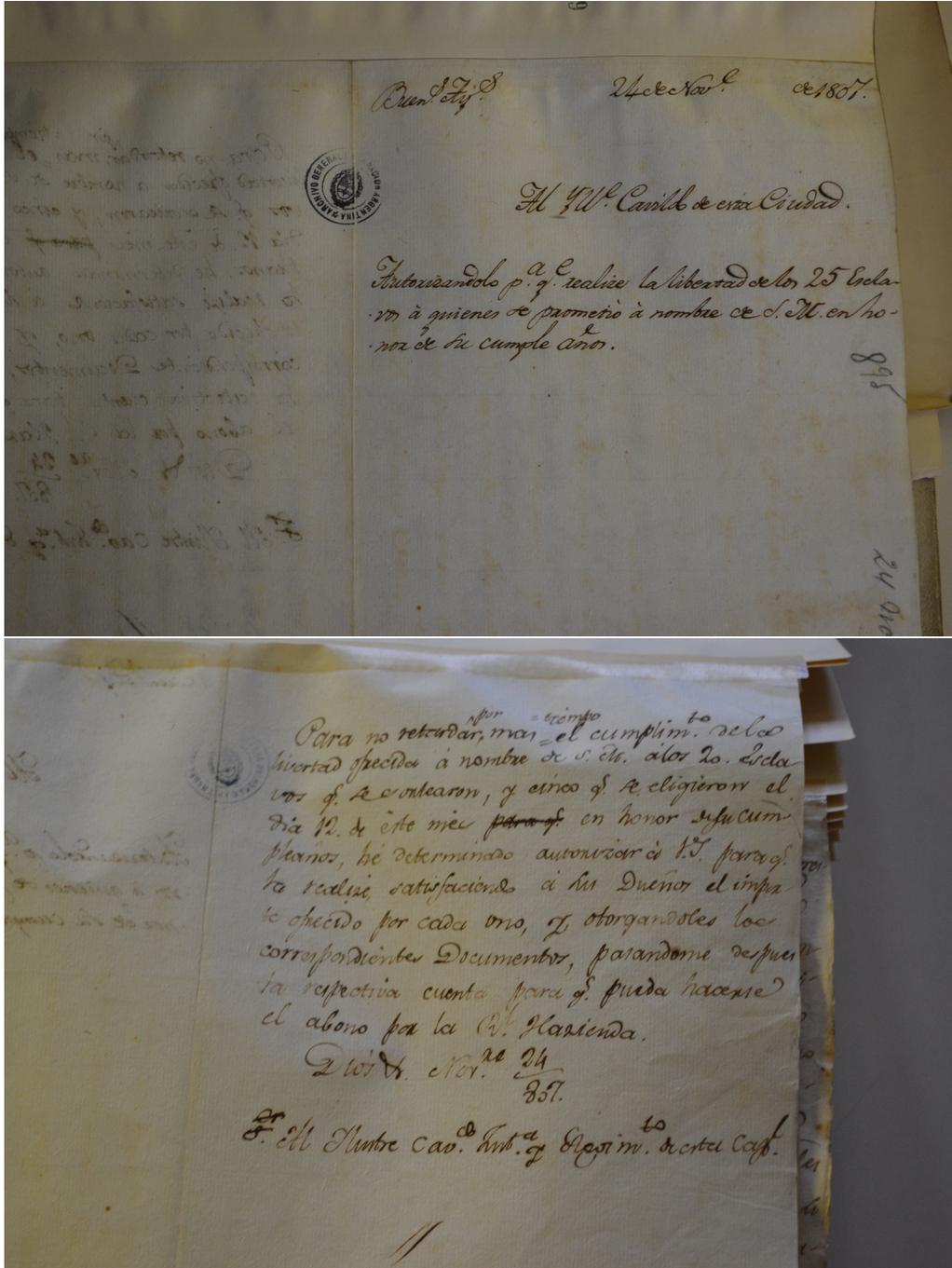
Figure A3: Public announcement of the lottery. October 22nd, 1807

## AVISO AL PUBLICO.

**E**L extraordinario entusiasmo con que la esclavatura de esta ciudad se dedicò à defenderla en los dias 1 hasta el 6 de Julio pasado : el generoso ardimiento con que se prestò à toda clase de fatigas y riesgos ; y la parte que tuvo en el memorable triunfo conseguido contra las armas britànicas, han sido motivos muy poderosos para excitar en el Cabildo de Buenos-Ayres los mas vivos deseos de hacer demostrable su reconocimiento. No ha perdido de vista, ni por un solo instante el mèrito que contrageron esos esclavos , ni los medios de compensarlo. Pero exausto de fondos , y apurados sus recursos por los ingentes extraordinarios desembolsos que ha sufrido , no puede dar desahogo à sus ideas en los tèrminos que quisiera. No obstante para que esos defensores de la Patria, vean en algun modo premiados sus servicios , y conozcan el singular aprecio que han merecido sus acciones ; ha dispuesto el Cabildo , de acuerdo con el Señor Gobernador y Capitan General , à pesar de su escasez de fondos , y de ser constantes sus crecidos empeños , dar la libertad à los esclavos que resultaron ò resulten inutilizados , ò inútiles para el servicio , asignandoles para su subsistencia la pension mensual de seis pesos. Ha determinado ademas , que la gozen tambien hasta el numero de veinte y cinco, sorteados entre los que concurrieron al servicio y defensa de la Ciudad en los indicados dias. Para ello han de acreditar sus servicios con certificaciones de los Comandantes de los puestos y avanzadas , à cuyas ordenes sirvieron , y visto bueno , ò la conformidad de sus amos , cuyos documentos deberán presentar al Cabildo antes del dia siete de Noviembre proximo: en la inteligencia de que pasado , no seràn admitidos. Calificado el derecho de los precedentes por la inspeccion de dichos documentos , se hará el sorteo el 12 de dicho mes de Noviembre , cumple años del Rey N. Sr. que Dios guarde , baxo los balcones de las casas Capitulares con asistencia del Sr. Gobernador y Capitan General ; debiendose tambien tener entendido que concluida la operacion del sorteo , elegirà el Cabildo à su arbitrio cinco esclavos mas de aquellos que entraron en càntaro , y no les tocò la suerte , à quienes juzgue acreedores por su conducta y servicios , para franquearles la libertad , pagando à sus amos el precio de todos. Lo que se avisa al público para inteligencia de los interesados.

Sala Capitular de Buenos-Ayres, Octubre 22 de 1807. = *Martin de Alzaga.* = *Estevan Villanueva.* = *Manuel Mansilla.* = *Antonio Piran.* = *Manuel Ortiz de Basualdo* = *Miguel Fernandez de Agüero.* = *Joseph Antonio Capdevilla.* = *Juan Bautista de Iruarte.* = *Martin de Monasterio.* = *Benito de Iglesias.*

**Figure A4:** Note from the Governor confirming the 25 additional liberations on behalf of the Spanish king.



## **B The Mechanics of the Lottery**

The lottery of certificates of freedom was performed using two urns. One urn had the names of the  $N$  participants of the lottery. The other urn had 45 white balls and  $N - 45$  black balls. Two children sequentially drew one name from urn A and one ball from urn B. If a name matched a white ball, that person was freed. If a name matched a black ball, that person remained enslaved.

## C Matching Participants in the Lottery with the 1810 Census

In this appendix, we provide details on how we digitized the full count of the 1810 census and matched the individuals in our experimental groups to names in the census.

**The 1810 census.** The census of 1810 is handwritten, available online through FamilySearch, and has never been transcribed before.<sup>22</sup> Significant parts of this census have not survived to the present day, so we could rely on only the 774 surviving pages.

We manually transcribed the names of all the non-White persons over 18 years of age. For illustration, Figure A5 shows a page from the census. The box in the lower-right quadrant displays the name of a 20-year-old non-White slave (a *mulato*) called Gabriel. He was a slave of the household of Don Mariano Sanchez. In total, the available pages of the census contained the names of 2,766 non-White individuals. When individuals did not have a surname, we assigned them the surnames of their masters or the heads of their households.

**The matching algorithm.** We used an automatic routine in R to search for matches in the census to the names of the individuals in the list of lottery winners and combatants in the Battalion of Slaves. To match participants in the lottery to individuals in the census, we relied on the Levenshtein measure of the distance between two words. The Levenshtein distance is the minimum percentage of single-character edits required to convert one name into another. We defined matches as names in the census within a Levenshtein distance of 5 or 10 percentage points.

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<sup>22</sup><https://www.familysearch.org/search/catalog/498602?availability=Family%20History%20Library>

Nombre	Nat	Edad	Sexo	Estado	Profes	Libro	Nombre	Nat	Edad	Sexo	Estado	Profes	Libro
Canudos													
Maria negra	Guineo	30		Viuda			Manuel Monter	Castilla	70		Canudo	+	
Ante. Vido	P. d. d.	12					Manuela Maximo	P. d. d.	50		Canudo	+	
Dr. Mo las Montinos	P. d. d.	35		Soltero Canudo	+		Agustin de la Laguna	Castilla	40		Canudo	+	
Dr. Pedro Merino	P. d. d.	30		Canudo			Juan de la Cruz	P. d. d.	45		Canudo	+	
Dr. Maria de la Cruz	P. d. d.	50		Viuda			Lorenzo de la Laguna	P. d. d.	19		Canudo		
Dr. Maria Mercedes Sanchez	P. d. d.	27		Viuda			Estimado de la Laguna	P. d. d.	14		Canudo		
Dr. Catalina Rey	P. d. d.	2					Manuel de la Laguna	P. d. d.	12		Canudo		
Dr. Maria Credero	P. d. d.	30		Soltero Canudo	+		Monte	P. d. d.	20		Canudo		
Dr. Maria Trine	P. d. d.	110		Canudo			Dr. Jose de la Laguna	P. d. d.	36		Canudo	+	
Dr. Maria de la Cruz	P. d. d.	16		Canudo			Dr. Manuela Camero	P. d. d.	45		Canudo	+	
Dr. Maria de la Cruz	P. d. d.	25		Soltero Canudo	+		Dr. Maria de la Cruz	P. d. d.	10		Canudo		
Dr. Maria de la Cruz	P. d. d.	8					Dr. Maria de la Cruz	P. d. d.	5		Canudo		
Dr. Pedro José Credero	P. d. d.	5					Concepcion	P. d. d.	30		Canudo		
Dr. Mariano Credero	P. d. d.	25		P. d. d.			Catalina	P. d. d.	70		Viuda		
Dr. Ignacio Trubista mulato	Francés	30		id			Jose	P. d. d.	18		Soltero		
Dr. Pedro Rodriguez Credero	P. d. d.	40		Canudo			Dr. Manuel Trubista	P. d. d.	21		Canudo	+	
Dr. Mariano Castillo Credero	P. d. d.	40		Canudo			Dr. Juan Carlos	P. d. d.	20		Canudo	+	
Dr. Maria Trubista	P. d. d.	20		Soltero			Dr. Manuel José de la Cruz	P. d. d.	50		Canudo	+	
Dr. Maria de la Cruz	P. d. d.	30		id			Dr. Maria Trubista	P. d. d.	50		Canudo	+	
Dr. Maria Trubista	P. d. d.	45		Viuda			Como negro	P. d. d.	20		Canudo	+	
Dr. Maria Trubista	P. d. d.	42		Viuda			Francisco	P. d. d.	8		Canudo		
Dr. Pedro Castillo	P. d. d.	12					Fita	P. d. d.	50		Canudo		
Dr. Constan	P. d. d.	3					Ignacia	P. d. d.	20		Canudo		
Dr. Maria Trubista	P. d. d.	30		Canudo			Maria	P. d. d.	11		Canudo		
Dr. Jose Trubista	P. d. d.	20		Soltero			Dr. Maria Trubista	P. d. d.	72		Canudo		
Dr. Jose Maria Trubista	P. d. d.	35		Canudo	+		Dr. Maria Trubista	P. d. d.	25		Canudo		
Dr. Concepcion Trubista	P. d. d.	19		Canudo			Dr. Jose Trubista	P. d. d.	20		Canudo		
Dr. Juan Maria Trubista	P. d. d.	1					Dr. Isabel Trubista	P. d. d.	60		Viuda		
Dr. Catalina Trubista	P. d. d.	22		Soltero			Dr. Ana Compara	P. d. d.	40		Soltero		
Dr. Juana Maria Trubista	P. d. d.	27		Soltero			Juan mulato	P. d. d.	20		Canudo	+	
Dr. Maria Trubista	P. d. d.	12					Manuel	P. d. d.	55		Soltero		
Dr. Juan de la Cruz	P. d. d.	35		Canudo	+		Manuel	P. d. d.	50		Canudo		
Dr. Maria Trubista	P. d. d.	35		Canudo			Juan	P. d. d.	20		Canudo		
Dr. Maria Trubista	P. d. d.	30		Soltero			Esco lastica	P. d. d.	22		Canudo		
Dr. Maria Trubista	P. d. d.	28		id			Manuel	P. d. d.	19		Soltero		
Dr. Maria Trubista	P. d. d.	26		Canudo									
Dr. Maria Trubista	P. d. d.	24		Soltero									
Dr. Maria Trubista	P. d. d.	25		id									
Dr. Maria Trubista	P. d. d.	70		Canudo	+								

Figure A5: An example from the census

## D Figure 3 Results in Table Form

**Table A1:** Effect of emancipation on imprisonment by type of criminal charge, 1820–1830

	<b>Robbery</b>	<b>Assault</b>	<b>Discretionary</b>	<b>Victimized</b>
<b>Mean in Control</b>	0.020	0.007	0.033	0.020
<b>Panel A: Reduced Form (ITT)</b>				
Lottery Winner	0.047 (0.039)	0.060 (0.038)	0.078 (0.050)	0.136* (0.056)
<b>Panel B: 2SLS</b>				
Emancipated	0.060 (0.051)	0.077 (0.049)	0.100 (0.063)	0.174* (0.072)
<b>Exact p-value</b>	0.133	0.043	0.051	0.002
Num.Obs.	195	195	195	195
Outcome	Binary	Binary	Binary	Binary
Sample	Full	Full	Full	Full

+  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

All specifications are estimated using ordinary least squares (ITT) and two-stage least squares (CACE).

## E Results Iteratively Excluding Observations

Figure A6: Leave-one-out results

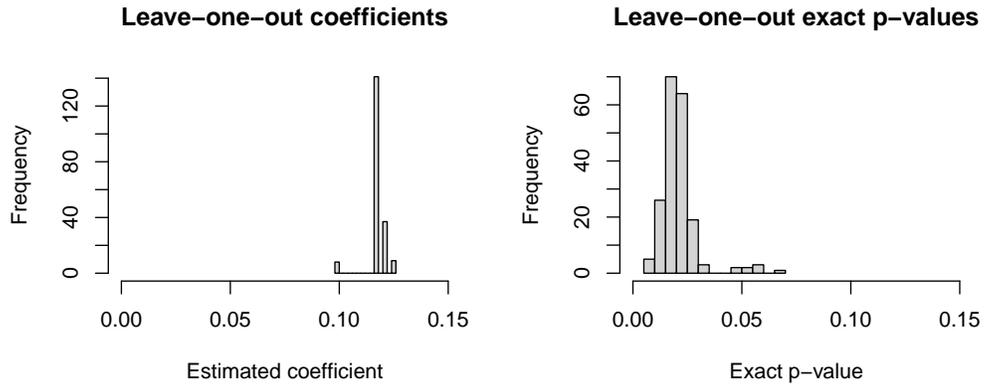
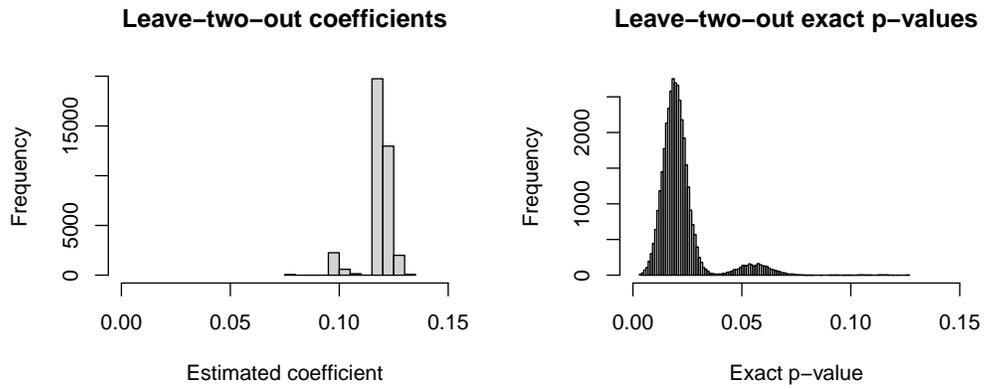


Figure A7: Leave-two-out results



## F Control Group Based on the 1810 Census

Our baseline research design compares the winners of the lottery of certificates of freedom with the nonwinners who were listed as combatants in the Battalion of Slaves. As a robustness check, in this appendix, we use an alternative control group: the full count of enslaved men in the 1810 census. This control group is not experimental—for example, because not all enslaved men in the census had combat experience.

**Table A2:** Alternative control groups using the 1810 census

Lottery winner	0.096 (0.058)	0.082 (0.060)	0.089 (0.058)	0.094 (0.058)
Mean of control group	0.082 (0.008)	0.096 (0.016)	0.089 (0.009)	0.084 (0.008)
<b>Exact p-value</b>	0.03	0.11	0.06	0.03
Num. obs.	1363	368	946	1224
Control group	All enslaved	15–20 years	15–30 years	15–40 years
R2	0.004	0.008	0.004	0.004

Table A2 displays the results. The first column compares lottery winners with all the enslaved men over 15 in the census. The second column compares winners to enslaved men between 15 and 20 years old. The third column compares lottery winners to enslaved men between 15 and 30 years old. The fourth column compares lottery winners with enslaved men between 15 and 40 years old. Across specifications, we estimate a positive effect of emancipation on the probability of imprisonment. This effect ranges from 8.2 to 9.6%. The exact p values are typically below conventional significance levels. The p value for the difference between lottery winners and enslaved men between 15 and 20 years old is slightly above the threshold of statistical significance ( $p=0.11$ ). Overall, the results with these alternative control groups support the baseline estimates: emancipated persons were, on average, more likely to be imprisoned than enslaved persons.