Oppression Beyond Plantations: The Effect of Emancipation on Incarceration in Urban Buenos Aires

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Abstract

We estimate the effect of the emancipation of formerly enslaved people on their probability of being incarcerated in an urban setting —a context that lacked the economic incentives for incarceration present in plantation economies like the U.S. South. Focusing on early nineteenth-century Buenos Aires, we document the incarceration histories of all inhabitants of the city in 1810. For this, we digitize the full count of a handwritten census and manually match individuals with police records until 1830. To establish causality, we study a lottery of certificates of freedom that randomly freed a small group of enslaved persons. We find that emancipation increased the probability of incarceration, on average, by 11.8 percentage points. Exploring mechanisms, we find no evidence that the effect was driven by rural labor shortages, but rather by the criminalization of petty offenses.

*The authors are grateful to Anna Callis, Gary W. Cox, Germán Feierherd, Vicky Fouka, Adrián Lucardi, Lucas Rebagliati, Catherine Reyes-Householder, Marcos Salgado, Luis Schiumerini, Alice Wang, and seminar participants at PolMeth Latin America and Stanford's Institute for Research in the Social Sciences for helpful comments. We thank Victoria Werner for excellent research assistance and the Liechtenstein Institute on Self-Determination at Princeton for generous funding.

1 Introduction

From the 16th to the 19th century, millions of African people were enslaved and transported to the Americas. When slavery was abolished, racially heterogeneous societies had to adapt economically to free labor markets, but also socially to new forms of interaction with the recently emancipated groups.[1] White elites often responded to emancipation by turning to the coercive apparatus of their nascent states —the police and criminal courts. The intense policing of racially oppressed groups after abolition was ubiquitous, taking place across societies from the French (Pluskota, 2020) to the British Caribbean (Johnson, 1991) to Brazil (Huggins, 1985). Nowhere has this phenomenon been more studied than in the United States (e.g., Davis, 2006; Blackmon, 2009; Alexander, 2010).

Most of the theoretical insights of this literature have been developed for plantation economies —where slavery supported labor-intensive agriculture. The dominant argument is that landed elites crafted discriminatory laws and criminal justice systems to discipline the emancipated workforce, using coercion to increase the labor supply while suppressing wages (e.g., Blackmon, 2009; Huggins, 1985; Dippel, Greif and Trefler, 2020).[2] The policing of racial minorities upon emancipation in urban settings, which differed from plantations in numerous ways, is much less understood (cf. Wade, 1964). We contribute to this literature estimating the effect of emancipation on incarceration in nineteenth-century Buenos Aires —a city where enslaved persons were mainly semiskilled artisans, not agricultural workers.

[1] 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 Trefler (2020); Jenkins and Peck (2021); Schwarz (2022); Mangonnet (2022).

[2] Penitentiaries in the U.S. South established the "convict lease" system, which allowed them to lease prisoners to private contractors for a fee (Blackmon, 2009); and more indirectly, the threat of arbitrary conviction gave emancipated persons incentives to entrust themselves to a landowner who could protect them (Alston and Ferrie, 1999; Huggins, 1985; Dippel, Greif and Trefler, 2020). Other scholars have challenged this view, arguing that the mass incarceration of Black people after abolition had racist ideological roots (e.g., Alexander, 2010; Davis, 2006); or that the practice of leasing convicts for a fee was not designed to increase the labor supply, but was rather a pragmatic state response to limited carceral capacity (Schwarz, 2022).

To do this, (i) we digitized the full-count of a handwritten census from 1810 and incarceration records until 1830; and (ii) we did archival research to obtain the details of a small lottery of certificates of freedom in 1807 that affords us random variation in emancipated status.

To assess the effect of emancipation on incarceration, we manually linked randomly emancipated people and sets of non-emancipated people who were eligible for the lottery with police records, and compare the incarceration rates between these groups. The validity of this empirical strategy hinges on a crucial aspect of slavery in urban settings: that enslaved persons could be incarcerated.[3] For Buenos Aires, we show below that 8.3 percent of enslaved people listed in the 1810 census were incarcerated at least once before 1830. We estimate that, on average, emancipation increased the probability of incarceration by 11.8 percentage points. When we turn to exploring mechanisms, we find that the effect was somewhat stronger in months when the demand for rural labor was low (off the wheat harvest season), which is the opposite of what labor-market theories of oppression in plantation systems predict. Furthermore, studying all the incarcerations in this period, we show that the proportion of free Black people incarcerated did not grow during months of high demand for rural labor. This suggests that the effect of emancipation on incarceration in this context was unlikely explained by the need to discipline emancipated people into the labor force. Yet our analysis also reveals commonalities with plantation systems: emancipated people were incarcerated at higher rates regardless of the criminal charge considered (e.g., robbery and assault) but most notably for discretionary charges that targeted the urban poor, such as vagrancy and drunkenness.

The main contribution of our paper is to estimate the effect of emancipation on incarceration using plausibly exogenous variation in emancipated status. Existing quantitative

[3]Were this not the case, then any differences in incarceration rates between enslaved and emancipated persons would arise mechanically —reflecting that enslaved persons were privately punished by their owners.

studies have examined the link between emancipation and incarceration either comparing the incarceration rates of White and Black persons after the abolition of slavery (e.g., Myers, 1990); or documenting the swelling of the prison population shortly after abolition (e.g., Mazumder, 2019; Huggins, 1985). We estimate the effect of emancipation comparing free Black people with enslaved Black people —a research design that has not been possible in contexts such as that of the rural U.S. South because enslaved people were privately punished by slave owners and rarely incarcerated.[4]

Whereas the literature on emancipation and incarceration has focused on plantation economies, we find support for a causal link between these variables in an urban setting. Despite this commonality, our theoretical discussion and exploration of mechanisms suggest that the motive behind incarceration in urban settings was not, like it has been argued for plantation economies, to coerce free individuals into the work force. Given the small sample of our paper and the unreliable nature of data on racial minorities who lived 200 years ago, our findings should be taken as suggestive but indicative of the need for further research on the comparative political economy of slavery regimes and post-emancipation racial relations in the Americas.

The rest of our paper is organized as follows. In the next section, we describe the similarities and differences between slavery in plantations and in urban settings; and discuss the implications of these differences for the incarceration of racial minorities. In the following section, we describe the historical context of our study, with a focus on slavery and racial relations, patterns of state formation after independence, and the development of police institutions. The third section shows descriptive data on race and incarceration from the full count of the 1810 census. The fourth section describes our research design.

[4]The closest 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.

The fifth and sixth sections present our results and examine potential causal mechanisms, respectively. The last section concludes.

2 Urban Slavery and Incarceration

2.1 Differences between Urban and Plantation Slavery

The word "slavery" typically evokes groups of enslaved people segregated in plantations, picking cotton in the U.S. South or cutting sugar cane in Brazil and the Caribbean. These preconceptions are not a coincidence —plantation economies accounted for the vast majority of enslaved persons. Yet the institution of slavery featured significant variation over time and across the Americas. Without exhausting the heterogeneity of slavery institutions, one can distinguish between two stylized types: plantation and urban slavery (see, e.g., Wade, 1964; Bernand, 2000). In this section, we discuss some significant differences that existed between the plantation model, with which readers are much more familiar, and the urban model that existed in Buenos Aires. These models should be seen as two distinct forms that slavery institutions could take, not ends of a spectrum.[5]

Plantation economies specialize in labor-intensive agriculture, and in particular, in the production and export of cash-crops, such as cotton, sugar cane, and coffee. Historical examples include the northeast of Brazil, the British Caribbean, Cuba, and the U.S. South. The purpose of slavery in plantation economies was to ensure a cheap supply of rural labor. The size of slave populations in these places was large. In 1790, there were about 3.6 million enslaved persons in the Americas, and 1.5 million were in Brazil, 1.3 million in the French and British Caribbean, 694 thousand in the United States, and 64 thousand in Cuba

[5] As categories, "plantation slavery" and "urban slavery" represent ideal types, to which actual historical cases therefore only imperfectly corresponded. Careful historical inspection reveals significant differences within examples of the same category of cases. See, for instance, Bergad (2006)'s discussion of the differences in the history, evolution, and institutional characteristics of slavery in Cuba, Brazil, and the U.S. South; and Klein (1986) on the differences between slavery in Brazil and the Caribbean.

(Bergad, 2006, p. 63).

The enslaved population in plantation economies was segregated from the free population —their lives took place inside plantations. In Brazil, for example, the first national census of 1872 reveals that 67% of the 1.2 million enslaved persons were employed in agriculture —and many of the remaining enslaved people were also indirectly connected with plantation life (Klein, 1986, p. 126). Partly because of the size of the enslaved population and their segregation, planters privately provided some of the coercion needed to extract forced labor from those whom they enslaved. In the rural U.S. South, there were plantations with their own private jails (Davis, 2006); and both in the U.S. and Brazil, "slave patrols" emerged to privately police enslaved persons (Wilson, 2022).

Urban areas developed a significantly different type of slavery (Goldin, 1972; Bernand, 2000; Dantas, 2008). Enslaved persons in urban settings were frequently forced to work in the services and crafts sectors —as domestic help, shoemakers, tailors, bakers, builders and barbers (Goldberg and Mallo, 2005). An important feature of urban slavery was that it was common for it to be "stipendiary", meaning that instead of forcing enslaved persons to work for a given number of hours, slave owners were entitled to a periodical payment from their slaves (Saguier, 1989). Historians of the United States have described this type of arrangement as "hiring out" enslaved people (e.g., Goldin, 1972), which was illegal but widespread in cities (Wade, 1964); and in urban Brazil it was known as owning *escravos de ganho* (Klein, 1986, p. 129).[6]

The specific details of an enslaved person's obligations towards their enslavers were "negotiated" on a case-by-case basis (Saguier, 2012). In Spanish America, municipal legislation established limits on the sums that slave owners could demand (Andrews, 1980, p. 34). A typical agreement was that, for example, enslaved shoemakers could live outside

[6]See Dantas (2008) for an in-depth case study of urban slavery in Sabará, Minas Gerais, Brazil and in Baltimore, MD.

of their masters' households but had to pay the equivalent of, say, two pairs of shoes per month. Arrangements sometimes involved non-monetary payments, like the obligation of enslaved persons to serve their masters once or twice a day in their household (Saguier, 2012). Any income above the value of the required payment was often for the enslaved person to keep (Wade, 1964; Saguier, 1989); and the police could intervene in the cases in which enslaved persons did not honor their obligations (Saguier, 2012). Enslaved persons could also persuade their masters that they be sold to another master who offered better economic conditions (Saguier, 2012).

As a consequence of the stipendiary nature of urban slavery, enslaved persons were not segregated in plantations. On the contrary, the typical day of an enslaved person in urban settings took place "on the street, in public spaces, in taverns, in stores, in markets" (Bernand, 2000, p. 5). As long as enslaved people "supplied their owners with the required wages, the masters cared little how or where they spent their time" (Andrews, 1980, p. 34). In some cities of the U.S. South, such as Charleston, SC, some hired-out "slaves moved about at will, made their own contracts, and even rented houses" (Martin, 2004, p. 26). This has led some scholars to consider urban slavery more benign than plantation slavery (Wade, 1964; Rebagliati, 2014). Because enslaved persons cohabitated with the free population (they were not segregated) and because the average urban slave owner did not have a number of enslaved persons large enough to justify the creation of a private police force (Bernand, 2000), the coercion needed to control the enslaved population was mostly provided by the state's coercive apparatus (Mallo, 2005; Rebagliati, 2014; Jean, 2019).

2.2 The Political Economy of Incarceration in Urban Settings

The differences between plantation and stipendiary slavery had significant consequences for the link between race, legal status, and incarceration. One of these differences is that

in plantation economies enslaved persons were rarely incarcerated. Slave owners privately produced the coercion needed to control those whom they enslaved —with private slave patrols and jails in plantations— and punishment was primarily corporal (Davis, 2006, p. 362). In urban settings with stipendiary slavery, in contrast, slave owners relied on official police forces to discipline enslaved people, and could send them to jail. Enslaved persons who were accused and found guilty of crimes were not returned to their owners but served time in jail (Fernandez Plastino, 2001; Jean, 2019).[7]

Another difference between plantation and stipendiary systems concerns the economic incentives to incarcerate emancipated Black persons. In plantation economies, the emancipation of enslaved persons damaged planters by decreasing the labor supply. A large historical literature has argued that after plantation slavery was abolished, landowners promoted the creation of a punitive criminal system that discriminated against emancipated Black persons (Blackmon, 2009; Huggins, 1985; Dippel, Greif and Trefler, 2020). According to a large body of scholarship, the goal of this coercive system was to increase rural labor supply (cf. Schwarz, 2022).[8] It is telling that in the British Caribbean, the laws constituting police forces after abolition stated that planters should appoint the rural police (Dippel, Greif and Trefler, 2020, p. 1683). A direct way in which the criminal persecution of emancipated Black persons in the U.S. increased rural labor supply was through the convict lease system —which allowed renting prisoners to private contractors for a fee (e.g., Blackmon, 2009). There was also an indirect complementary channel: a racially discriminatory legal system gave emancipated Black persons incentives to entrust themselves

[7]Newspapers from Richmond, VA, often listed enslaved people who were arrested (Goldin, 1972, p. 21). See Jean (2019) for data on the imprisonment of enslaved people in nineteenth-century Rio de Janeiro, Brazil, where enslaved people represented 22 percent of the prisoners in 1883.

[8]Schwarz (2022) shows through a case study of Georgia, that the convict lease system in the U.S. South was a consequence of limited state capacity after the American Civil War, and not a programmatic attempt to re-enslave African Americans. The number of convicted African Americans rose rapidly after Emancipation as a result of new punitive laws, and only later, when Southern prisons became overwhelmed, legislatures instituted the convict lease system with bipartisan support.

as employees of a White patron who could "protect" them from arbitrary punishment, and this allowed landowners to recruit rural labor offering below market wages (Huggins, 1985; Alston and Ferrie, 1999).

In cities, in contrast, slave labor represented a smaller fraction of the workforce, so the abolition of slavery plausibly did not damage the economic and political elite to the same extent. The magnitude of the labor market shock of emancipation was also ameliorated by the fact that "the occupational structure of free Afro-Argentines tended to replicate that of slaves" (Andrews, 1980). Therefore, any pressure for the incarceration of emancipated Black persons was most likely not intended to guarantee a larger supply of labor, but rather to suppress it because emancipated persons competed with White semi-skilled laborers.[9]

The distinctive political economy of incarceration in stipendiary systems affords us an analytical advantage for studying the causal effect of emancipation and incarceration: unlike enslaved people in plantation economies, those in stipendiary systems could be incarcerated, which allows to estimate the effect of emancipation on incarceration comparing the incarceration rates of emancipated and enslaved persons. In addition to this methodological advantage, urban slavery and its differences with plantation slavery has not been sufficiently examined. Studying the incarceration of emancipated people in an urban context allows us to determine whether the theories developed in plantation systems can travel to settings with different institutions, actors, and preferences.

[9]For instance, in cities of the U.S. South, White workers often lobbied for prohibiting "hiring out" enslaved people (Martin, 2004, p. 161); and White artisans in Buenos Aires attempted to ban enslaved people from specific crafts (Andrews, 1980, p. 33).

3 Historical Context: 19th Century Buenos Aires

3.1 Race, Slavery and Abolition

The context of our study on emancipation and incarceration is early nineteenth-century Buenos Aires, an urban setting with stipendiary slavery, and its rural hinterland. Buenos Aires was located in a region with a climate that was unsuitable for the production of cash crops, which limited the profitability of forced labor. The city was the destination of a comparatively small number of enslaved people. Nonetheless, in 1810, a third of the population of Buenos Aires was Black, of which about 80% were enslaved (Goldberg, 1976).

Like other Spanish American colonial societies, Buenos Aires had a system of racial stratification known as the caste regime. At the top of the hierarchy were White inhabitants; at the bottom, African and Afro-American enslaved persons, and Native Americans; and between these two groups were free persons of African or mixed racial ancestry (Andrews, 1985, p. 114). Most enslaved and free Black persons worked as domestic servants or in semi-skilled crafts and trades (Goldberg and Mallo, 2005), which were dominated by non-White practitioners (Andrews, 1980).

The abolition of slavery in Buenos Aires was a gradual process that began in 1812 with the abolition of the slave trade, and ended in 1853 with the full abolition of slavery (Candioti, 2016; Sobrevilla-Perea, 2022). 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. All enslaved persons were declared free in 1853 (Candioti, 2016).

3.2 The Post-Independence State in Buenos Aires, 1810-1830

The historical period of our study is one of major political upheaval: it coincides with the convoluted process of state formation in South America.[10] Until 1810, Buenos Aires, as capital of the Viceroyalty of the River Plate, was subject to Spanish colonial rule. Spain's collapse under Napoleon in 1808 unleashed a sudden cascade of revolutions for independence across Spanish America (Lynch, 1986). The Wars of Independence were destructive and also led to domestic political instability. The urban elite of Buenos Aires began the struggle for independence in 1810 by deposing the royal administration and forming an autonomous assembly. Even though a Revolutionary State initially attempted to consolidate control over the territory of modern day Argentina and Uruguay, civil war against warlords from the interior ensued, and by 1820 the dominions of the Buenos Aires revolutionary elite was confined to the city of Buenos Aires and its surrounding countryside (Mazzuca, 2021).

From 1820 to 1830, Buenos Aires alternated between periods of political order and strife. In the early 1820s, the elite in Buenos Aires established political order as a rudimentary port-state dependent on trade that included the city and its rural hinterland. This inaugurated a period of liberal reforms that fueled economic growth. In 1826, Bernardino Rivadavia, a politician from Buenos Aires, intended to create a unitary state that included provinces in the interior of what is now Argentina. Rivadavia's unitary constitution — which centralized power in Buenos Aires but also shared income from the city's port with the provinces— alienated both *caudillos* from the interior and landed elites from Buenos Aires. The result was another period of civil war that ended in 1829 with the institution of a dictatorship in Buenos Aires under landowner Juan Manuel de Rosas, who ruled the province until 1852 (Halperín Donghi, 2002; Ternavasio, 2009).

[10] See Mazzuca (2021) for a comparative-historical analysis of this process in the Americas.

3.3 Policing Afro-Americans in 1820s Buenos Aires

When civil strife diminished after 1820, re-establishing political order in Buenos Aires involved disarming individuals who had participated in the wars (Casagrande, 2014, p. 281). All factions of the elite were preoccupied with the establishment of law and order, which involved re-injecting a tone of respectability into social relations —something that had been lost after independence (Szuchman, 1984). We study the relationship between race, slavery, and incarceration in this context.

Because this was also a period of state formation in Latin America (Mazzuca, 2021), we study police forces as police institutions were themselves developing. Law enforcement was a priority of the elite at the time, at least budget-wise (Barreneche, 2006). In 1821, a modern *Departamento de Policía* replaced the more rudimentary *Indentencia de Policía* (Vaccaroni, 2021). The new police department adopted features of a professional bureaucracy. The jurisdictions of the modern police commissars became territorial —with each commissar exclusively in charge of a specific geographic area. From 1822 to 1824, the number of officers in managerial roles incrementally increased: each police section, in addition to a commissar, received medical doctors in 1822, two junior commissars in 1823, and eight watchmen in 1824. These agents were hierarchically organized. Watchmen had to carry a shield that identified them as such, and had to adhere to a formal code of conduct. Officers did not own their own horses and weapons; they were centrally supplied.

The police monitored the activities of racial minorities because of the paternalistic belief that they needed some degree of tutelage —if not from a master, from public authority (Candioti, 2010). For example, since 1820 the associations of free Afro-Americans needed official authorization to function (Chamosa, 2003), and were subject to police surveillance (Vaccaroni, 2021). The handbook for policemen printed in 1825 included a section on Afro-American dance (Vaccaroni, 2020, p. 150). In 1822, the government required that

enslaved persons carry registration papers with the names of their owners to prevent them from posing as free in parts of the city where they were not known (Andrews, 1980, p. 34).

4 The Incarceration of Free and Enslaved Afro-Americans

There are no quantitative historical studies on the extent to which Afro-Americans (either free of enslaved) were incarcerated in Buenos Aires.[11] To study this phenomenon, we rely on the 774 surviving pages of the handwritten census of the city, which have complete information for 15 out of 20 of the city's neighborhoods.(Goldberg, 1976) We digitized the full count of non-White men over 15 years old (N = 2,672) as well as a random sample of White men (N = 685).[12] We then manually linked each person to police records from the *Archivo General de Policía* for the 1821–1830 period, which allows us to compute incarceration rates for different demographic groups.[13] We show the results in Figure 1.

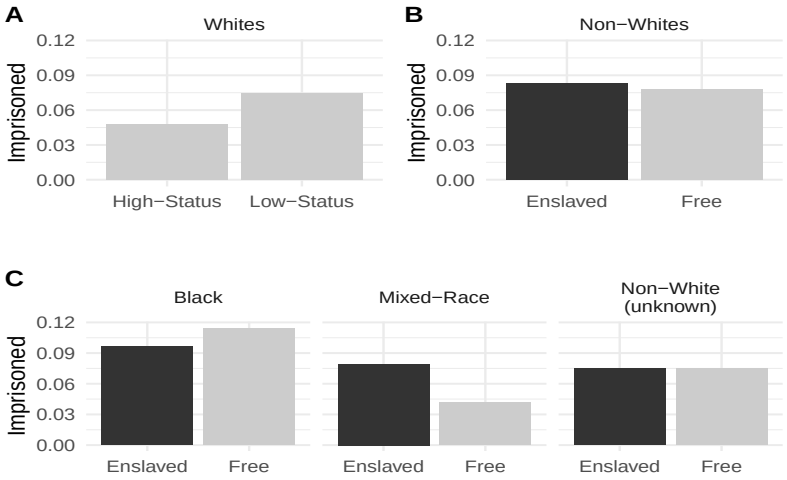
To provide a benchmark for the incarceration rates of enslaved Afro-Americans, we begin showing in Panel A of Figure 1 the incarceration rates of high-status (*patricios*) and low-status (not *patricios*) White individuals in our sample. These rates were 4.8% and 7.5%, respectively. Turning to non-White people, Panel B shows that the overall rates of incarceration among enslaved and free Afro-Americans were both slightly higher than the incarceration rate for low-status White people and substantially higher than the same rate for high-status White people. Nonetheless, the rates for enslaved and free Afro-Americans

[11] For a qualitative historical discussion of crime and race in Buenos Aires, see Mallo (2005).

[12] The names, surnames, and races of individuals in the census were sometimes abbreviated, so we relied on the dictionary of Hispanic abbreviations from the Instituto de Investigaciones Filológicas at the Universidad Nacional Autónoma de México (see https://www.iifilologicas.unam.mx/dicabenovo/). To obtain a random sample of White men, we sampled the third White person in each page of the census.

[13] 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. We considered exact phonetic matches because the spelling of names was not standardized.

Figure 1: Incarceration rates, 1820–1830



Notes: The figures show the proportion of individuals in the 1810 census who were incarcerated at least once during the 1821–1830 period. Panel A shows incarceration by slave status for a random sample of whites (N=685), Panel B shows incarceration by slave status for all non-whites, and Panel C shows variation across both race and slave status among non-whites. 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).

were almost indistinguishable from each other (8.3% and 7.7%, respectively).[14] As shown in Panel C, the small overall difference in incarceration rates masks variation across racial groups within the "non-White" camp —even though there is no racial group for which the difference in incarceration rates across free and enslaved individuals is statistically different from zero.

These results are, of course, purely observational and cannot be interpreted causally.

[14]The incarceration 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 incarcerated, on average, for vagrancy and desertion each year from 1852 to 1863 and over 12% from 1861 to 1871. The overall incarceration rates that we estimate are consistent with previous studies, such as Szuchman (1984, p. 86), which finds that in 1831 approximately 2 percent of the city's population were serving time in jail.

Enslaved and free people were different in many other ways besides their legal status. This was especially the case in stipendiary systems in which enslaved persons could buy or negotiate their release from slavery.

5 Research Design

5.1 The *Plaza Mayor* Lottery

To identify the effect of emancipation on the incarceration 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 distributed through a lottery. 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.[15] This lottery provides us with random variation in the emancipation status of those eligible to participate in it.[16]

To identify the causal effect of emancipation, we would ideally have access to the com-

[15]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.

[16]One might wonder why the municipal government would decide to free enslaved people and subsequently seek to imprison them. Although this might appear as a contradiction, the government freed only a small number of enslaved people through the lottery. Emancipating a small set of enslaved combatants served the dual purpose of, first, ensuring that the enslaved population would participate in the defense of the city upon future invasions; and second, rewarding the enslaved for their military efforts in order to reduce the probability of rebellion of this oppressed group that now had weapons and military experience.

prehensive 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. Historical documents allowed us to reconstruct the full list of lottery winners from official sources.[17] The treatment group in our quasi-experiment is a set of 45 people who were emancipated at random. 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 incarceration—such as their combat experience, assimilation into colonial society, deference towards Whites, or work ethic.

Unfortunately, however, the full list of participants in the lottery has not survived. Secondary sources claim that 686 enslaved people participated in the lottery (e.g., Lobo, 1875; Mitre, 1877; Cuadra Centeno and Mazzoni, 2011)—but no official source confirms this number. The main challenge of our research design, therefore, is to construct a valid control group for this natural experiment. While the typical identification problem in the literature is to account for selection into emancipation, we deal with the problem of selection into the lottery of certificates of freedom. We explain our estimation strategy below, and then describe our strategies to construct a valid control group, all of which take into account the eligibility criteria for the lottery.

5.2 Estimation

We are interested in the average causal effect of emancipation on incarceration defined as

$$\tau = \frac{1}{N}\sum_{i=1}^{N} T_i - \frac{1}{N}\sum_{i=1}^{N} C_i \tag{1}$$

[17]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. We provide more details of the lottery process in Section B in the Appendix.

where N is the total number of units in the lottery, T_i is the potential outcome of unit i under treatment and C_i is the potential outcome of unit i under control. The first term of the equation is the average outcome that would occur if all units were assigned to the treatment group and the second is the outcome that would occur if all units were assigned to the control group.

To estimate this quantity with the lottery, we would need

$$\hat{\tau} = \frac{1}{n}\sum_{i \in A} T_i - \frac{1}{m}\sum_{i \in S} C_i \tag{2}$$

where $A = \{1, \cdots, N\}$ denotes the units randomly assigned to treatment and $S = \{1, \cdots, M\}$ those randomly assigned to control.[18] In our setting, we effectively have a lottery which ensured random assignment of individuals to treatment and control. Moreover, we have full information on the randomly selected lottery winners, which allows us to calculate the mean relevant to the first term. Our main challenge is that we do not have complete information on the control group. We take two complementary approaches to construct a control group. While both are imperfect, we believe that the combined results from these strategies provide a strong basis for our claims.

Baseline Estimates: Using the "Battalion of Slaves" as the Control Group In our baseline estimates, we aim to approximate the control group for the natural experiment using an official list of enslaved individuals who were eligible for the lottery. We derived this list from an official record containing the names of 152 surviving enslaved combatants

[18]For a complete treatment of the Neyman model in the context of natural experiments, see Dunning (2012). We focus on the ITT because our experimental sample has non-compliance: ten of the 45 winners were ex post denied liberation by their owners (Lobo, 1875); and historical records do not allow us to measure whether individuals who appear as enslaved in our sample eventually earned manumission. Many enslaved persons received manumission in exchange for military service during the Wars of Independence; and after 1820, which is the period in which we study incarceration, the *alcabala* tax on the sales of slaves was eliminated, which also increased manumissions (Rosal, 2004).

who belonged to the "Battalion of Slaves" during the British invasions.[19] Additionally, the record includes an indication of whether they sustained combat-related injuries. Given that all enslaved individuals who officially participated in the defense efforts were enlisted in the Battalion of Slaves (Cuadra Centeno and Mazzoni, 2011; Palombo, 2007), our list represents the most comprehensive compilation of those eligible for the lottery available in the archive.

The size of the Battalion is significantly smaller than the number of 686 lottery participants mentioned in secondary sources. This suggests that the lottery included individuals beyond those in the Battalion. Supporting this possibility is the comparison we made between our comprehensive list of lottery winners and the names of combatants in the Battalion. We could only find two winners who were also listed as combatants. While the scarcity of such overlap could partly be attributed to the inherent challenges of historical research on enslaved persons (see, e.g., Logan and Pritchett, 2018), it is evident that the overlap between the two groups is quite limited. This confirms that the enslaved men comprising the Battalion represented only a fraction of the individuals eligible for the lottery. It is highly likely that an additional group of men, perhaps those who informally participated in defending the city, also took part in the lottery.

The validity of using the men listed within the "Battalion of Slaves" as our control group hence rests on two crucial identifying assumptions. First, that the Battalion is representative of the full group of those eligible to participate in the lottery. And second, that all those eligible entered the lottery, or at least that owners did not strategically influence which of the surviving slaves did. Together, these assumptions would imply that the potential outcomes of those in the Battalion of slaves is equivalent in potential outcomes to those in the full control group.

[19]Slave companies of the Viceroyalty of the River Plate (1808-1809), AGN, 26-7-5, Sala IX.

While these assumptions are impossible to test directly, we provide some suggestive evidence through a balance test comparing the individuals in our control group—those in the Battalion of Slaves—to the lottery winners. In addition to sex and combat experience, which are identical across groups by design, both groups are balanced, on average, with respect to other available covariates. The proportion of individuals named after a Catholic apostle is similar across groups (15.6% vs 20.7%),[20] 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).[21] While these tests only provide evidence of balance across observables, they include some important prognostic covariates.[22] For example, numerous studies highlight age as one of the main determinants of the economic value of slaves (Riella, 2021).

Even though we consider the baseline strategy detailed above to be the best approximation of the true control group of the natural experiment, skeptical readers might object to some of the assumptions required for the validity of our estimates. Specifically, one might worry that slaves in the Battalion differ from those who unofficially defended the city in important ways that could bias our results. We offer additional evidence for our claims through a complementary strategy which we detail next.

[20] Apostolic names are Pedro, Santiago, Juan, Andrés, Bartolomé, Judas, Mateo, Felipe, Simón, and Tomás.

[21] 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.

[22] On tests for causal inference see Eggers, Tuñón and Dafoe (2023).

Table 1: Balance table

	Treatment	Control	Difference	N	ri p-value
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: Randomization inference (ri) p-values are from Fisher's randomization inference tests of the sharp null hypothesis of no individual treatment effect.

Establishing Plausible Bounds: Using the Full-Count of Enslaved Individuals as the Control Group In our second approach, we complement our baseline estimates using the full-count of enslaved people in the 1810 census to construct alternative control groups. We first consider the full population of enslaved men in the census as the control group (as well as more narrowly defined sets of enslaved men from various age cohorts). For the potential outcomes of the full enslaved population to be equivalent to those of the real control group we need to assume that (1) the full population of enslaved men was involved in the defense of the city and hence eligible to participate in the lottery, and (2) that all the enslaved men entered the lottery (or there was no strategic selection on who did).

Finally, using the full count of the census we propose an additional empirical exercise. We take the claim that 686 enslaved participated in the lottery at face value, and compare the 45 lottery winners with different permutations of 641 (686 minus 45) enslaved people in the census. This allows us to construct upper and lower bounds for the estimated effect of emancipation and imprisonment.

Hypothesis Testing Our natural experiment affords us a relatively small sample. Therefore, we report results from both the simple difference-in-means as well as those from Fisher's exact test (also denoted as randomization inference), looking at the sharp null hypothesis of no effect for any units. The exact p-values that we report quantify the probability of observing an effect at least as large as the one that we observe if emancipation has no effect on the probability of incarceration.

5.3 Data

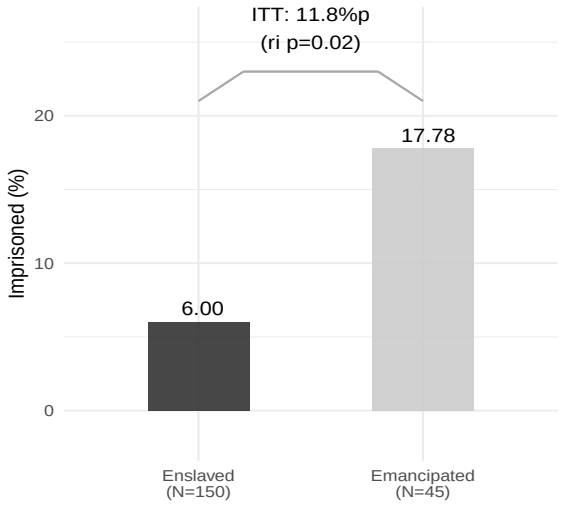
Working with centuries-old historical data, particularly when dealing with information related to racial minorities, introduces inherent challenges (Cirone and Spirling, 2021) and requires a comprehensive description of the nature of our data sources. The list of lottery winners was produced by colonial authorities after the lottery in 1807, plausibly in order to keep a record of the owners that ought to be compensated for their lost property. The list of enslaved combatants was produced after the British Invasions that same year. The purpose of creating such a record was likely to keep track of individuals who could be enlisted in the military in the event of future invasions. This was even more likely the case for the census of 1810, which was conducted by local elites after a coup that ousted the viceroy Baltasar Cisneros earlier that year. The military motivations behind both the list of 1807 and the 1810 census make them adequate control groups for our study —the lists include either former or potential combatants. For our analysis, we matched all individuals to the same police records, which were produced more than a decade later by Buenos Aires' police forces. While the police records, as we discussed above, were created during a period of internal strife, any issues regarding the quality of record-keeping should have affected both experimental groups to the same extent.

6 Main Results

6.1 Baseline Strategy

Did emancipation increase incarceration? Figure 2 displays our baseline results comparing the lottery winners to the enslaved combatants from the Battalion of Slaves. The point estimate of the impact of emancipation indicates that, on average, emancipated individuals were 11.8 percentage points more likely to be incarcerated than enslaved individuals—almost a threefold 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 (Model 1) and extends them in several important ways.

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



Notes: The figure shows the difference in means in incarceration rates for the comparison between lottery winners and individuals in the Battalion of Slaves.

The results in Model 1 focus on the extensive margin of incarceration (whether individuals were incarcerated 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 incarceration. However, in Model 2, we report results for the intensive margin (the number of times an individual was incarcerated). We show that emancipation significantly affected incarceration on the intensive margin. According to our baseline estimates, emancipation, on average, led to a 0.17 increase in the number of episodes of incarceration (exact p value = 0.017).

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

	(1)	**(2)**	**(3)**	**(4)**
Mean in Control	0.060	0.073	0.060	0.057
Difference in Means	0.118+	0.171+	0.131*	0.121+
	(0.061)	(0.094)	(0.061)	(0.060)
ri p-value	0.022	0.017	0.020	0.014
Num.Obs.	195	195	173	220
Outcome	Binary	Count	Binary	Binary
Sample	Full	Full	No Wounded	Full+Merit

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

Notes: The table shows the difference in means in incarceration outcomes for the comparison between lottery winners and individuals in the Battalion of Slaves.

The announcement of the lottery stated that those physically impaired during combat would all be eligible to be liberated *prior* to the lottery, and would thus be excluded from it. Yet our control group includes all individuals in the Battalion of Slaves. This could bias our results if, for example, those wounded were less likely to commit crimes or died shortly after the lottery. To address this, 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 excluding wounded individuals from our sample (Model 3). The results are closely in line with those in our main specification and confirm the effect of emancipation on incarceration.

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. Model 4 reports the result of this analysis and shows our results are robust to the inclusion of this group.

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; Andrews, 1980)—which would bias our results upwards—even though emancipated persons also joined the military (Andrews, 1980). Yet, on the other hand, emancipated persons were unbound to a master and had the freedom to move out of Buenos Aires—which would bias our results downwards.[23] Furthermore, data on mortality from Goldberg (1976, p. 88) shows that, around 1820, enslaved men over 15 years old had lower mortality rates than emancipated Black men (24.95 vs 30.27 per thousand).

Our data allow us to address concerns about selective attrition in four ways. First, in Table 1, we show that emancipated and enslaved persons were equally likely to be

[23]Indeed, quantitative research on the post-emancipation period in the U.S. South reveals significant migration (Thomas, Haeley and Cottingham, 2017).

matched to records from the 1810 census—suggesting that, at least over this short period, there was no differential attrition. We also show that the ages of those who were matched were similar across groups. Second, in Model 3 of Table 2, we show that our results hold when we remove all individuals who were wounded—and thus had a higher risk of mortality—from the control group. Third, 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 = 0.59).[24] Finally, a fourth way of accounting for selective attrition is to perform a rough back-of-the envelope calculation to quantify how much more likely to die before 1820 enslaved persons should have been in order to make our point estimate in Model 1 (Table 2) equal to zero. This equality ensues only after we drop 100 enslaved persons from our sample; which would imply a mortality rate of 66 percent for enslaved persons and zero mortality for emancipated persons.

Small Sample Issues A second concern is that our baseline sample includes only 195 individuals. We employ 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 A7). We then further probe our results by iteratively dropping each possible pair of observations (Appendix Figure A8). Together, these robustness checks show that our results are not driven by any single observation or pair of observations.

[24] A limitation of this result is that enslaved persons could have been more likely to join private *caudillo* armies.

6.2 Using the Full-Count of Enslaved Men as the Control Group

A weakness of our research design is that we lack full information about the individuals who participated in the lottery of certificates of freedom but lost —which would constitute the ideal control group for our study. Instead, we relied on an official list of enslaved individuals who were eligible for the lottery: combatants in the official militias that resisted the British invasion in 1807. In this section, we consider alternative control groups based on the full count of the 1810 census.

All enslaved men as the control group. As a first approximation, we compare the lottery winners with the set of all enslaved men over 18 in the census (i.e. those above 15 in 1807). 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 incarceration between our treatment group and the enslaved men in the census in Table 3. The result in Column 1 shows that the randomly emancipated individuals were, on average, 9.6 percentage points more likely to be incarcerated than the average enslaved person in the census (exact p value = 0.03). Columns 2-4 compare the lottery winners with the enslaved population in different age cohorts. Age was one of the main determinants of the economic value of a slave in this period (see, e.g., Riella, 2021), so the subsetting allows us to compare lottery winners with enslaved people of different levels of "quality." The results mirror those in Column 1. The magnitude of the estimated effects ranges from 8.4 to 9.6 percentage points. The exact p-values are below conventional

significance levels for all samples except for that comparing lottery winners to men who were 15-20 years old at the time of the British Invasions (exact p value = 0.11).

Table 3: Enslaved people in the census as the control group

	(1)	**(2)**	**(3)**	**(4)**
Mean of control group	0.082	0.084	0.089	0.096
Difference in Means	0.096	0.094	0.089	0.082
	(0.058)	(0.058)	(0.058)	(0.060)
ri p-value	0.03	0.04	0.06	0.11
Num. obs.	1363	1224	946	368
Control group	15 and older	15–40 years	15–30 years	15–20 years

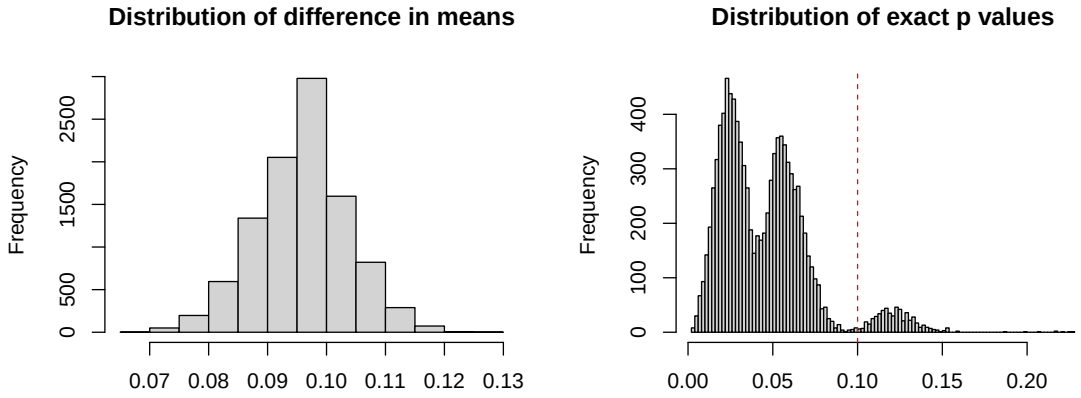
+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Notes: The table shows the difference in means in incarceration outcomes for the comparison between lottery winners and enslaved individuals in the full count of the 1810 census.

Multiverse analysis. A more agnostic approach is to use the names of enslaved people in the census to compare lottery winners with all the possible control groups. For this robustness check, we take the claim that 686 enslaved individuals participated in the lottery at face value. We know with certainty that 45 enslaved people were emancipated through the lottery, which implies that the list of missing participants should include 643 additional people (686 participants minus 45 winners). If we assume that the full-count of non-White individuals in the census is complete, then the "missing" 643 participants must be some set of people in the census. In this robustness check, we use the census to construct 10,000 synthetic control groups, each made up of 643 randomly sampled enslaved people without replacement. We then use each of these synthetic control groups to compute our baseline estimates.

Figure 3 shows the distribution of estimated effects and randomization inference exact

Figure 3: Comparing the Winners to Randomly Sampled Control Groups



The figures report estimates from the comparison of the lottery winners to 10,000 randomly sampled control groups of 643 enslaved men from the full-count of enslaved men from the census. The panel on the right shows the distribution of the estimated difference in means for the incarceration dummy variable, while the figure on the left present the distribution of the associated p-values. The mean of the estimates is .1. The average randomization inference p-value is 0.05, and $p < 0.1$ in 94% of the simulations.

p values across synthetic control groups. We estimate a positive effect of emancipation on incarceration in all 10,000 simulations —the estimates are never below 0.07 and have a mean of 0.1. The average exact p value is 0.05, and $p < 0.1$ in 94% of the simulations. These results are robust to sampling from the narrower age cohorts reported in Table 3.

7 Exploring Mechanisms

We have shown that emancipation made individuals in early nineteenth-century Buenos Aires more likely to be incarcerated. Although the data are too sparse to confidently pin down causal mechanisms, in this section we explore potential mechanisms analyzing: (i) temporal patterns in the incarceration data, and (ii) heterogeneous treatment effects by type of criminal charge.

7.1 Labor Market Demands

Studies of the incarceration of racial minorities after abolition in plantation economies have argued that incarceration was, to an important extent, motivated by labor market considerations. These theories claim that the intense policing of racial minorities gave emancipated people incentives to entrust themselves to employers who could protect them (Alston and Ferrie, 1999), artificially depressing the wages that landowners needed to pay to recruit agricultural workers. Supporting this mechanism, Huggins (1985) shows that the police in Brazil became more punitive during periods of labor shortage.

This mechanism was developed for plantation systems and is unlikely in urban contexts, such as Buenos Aires, which did not engage in labor-intensive agriculture and where enslaved people worked as artisans. The rural hinterland of Buenos Aires engaged mostly in ranching for export, and only one rural worker was needed per 10,000 head of cattle (Halperín Donghi, 2002, p. 45). Nonetheless, to test the labor-demand mechanism, we examine whether the effect of emancipation on incarceration was higher during the harvest season for wheat: between December and February (Galarza, 2015). The harvest season for wheat was the point of highest labor demand of the year (Garaviglia and Gelman, 1998). It was "the most feverish and agitated time of the agricultural calendar" and when "a hive of migrants arrived to the countryside to offer their labor" (Mayo, 1995, p. 124).

If landowners used incarceration as a tool to coerce emancipated people into the rural workforce, then we should observe that the effect of emancipation on incarceration was bigger during the harvest season. Table 4 displays the results, which suggest that the effect of emancipation on incarceration was positive and statistically significant outside of the harvest season (exact p value = 0.033) and smaller and not significant during the harvest season (exact p value = 0.367).[25] Note, however, that the difference between

[25]In Table A2 in the Appendix, we report the results of this analysis when using the census control groups, which also show no effect during the harvest season.

Table 4: Effect of emancipation, harvest vs. nonharvest months

	Harvest Months	**Nonharvest Months**
Mean in Control	0.027	0.04
Difference in Means	0.04 (0.039)	0.093+ (0.054)
ri p-value	0.367	0.033
Num.Obs.	195	195

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

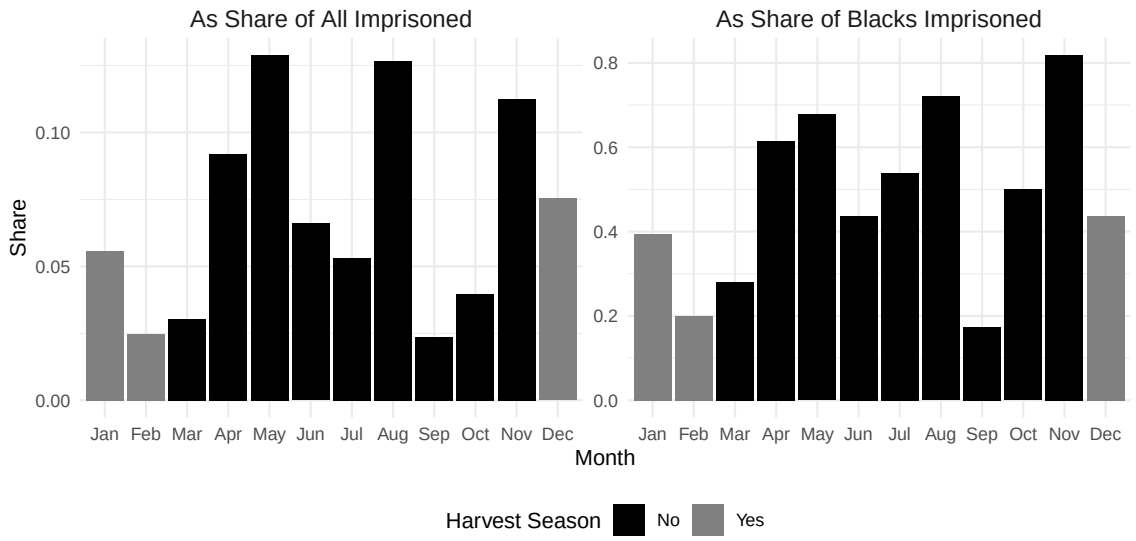
these effects is not itself statistically significant (exact p value = 0.352). This pattern is the opposite of what labor-market theories of racial oppression in plantation economies predict.

To better understand seasonal patterns in incarceration beyond our small natural experiment, we digitized all the available police records for 1827-1830. These entries describe the incarceration of 2,073 people and include information on race and whether the person was enslaved, which allowed us to code the proportion of free Blacks imprisoned per month. Figure 4 reports the free Blacks imprisoned as a share of the total number of individuals imprisoned (left panel) and the total number of Blacks imprisoned (right panel).[26] Both figures show similar patterns, with incarcerations increasing in April-May, August, and November. In both cases, we fail to see a pattern suggestive of incarcerations increasing during the wheat harvest period.

All told, the results from both the natural experiment and the descriptive analysis strongly suggest that the incarceration of emancipated Black people in Buenos Aires was not intended to coerce them into the rural workforce.

[26]Figure A6 in the Appendix reports the raw monthly quantities for all these categories.

Figure 4: Monthly Incarceration Rates of Free Blacks



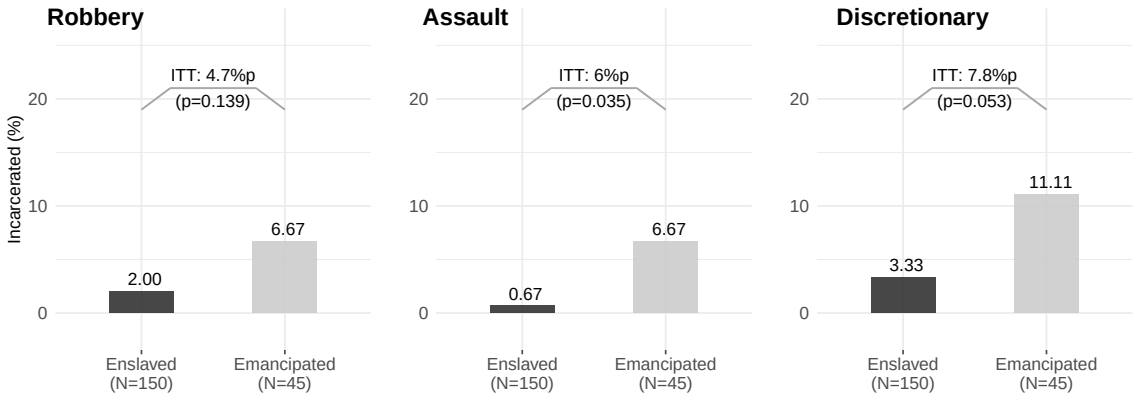
Notes: The figures show the rates of incarceration of free blacks during throughout the year as a share of the total number of incarcerations (left panel) and as a share of the total number of black individuals incarcerated (right panel) for 1827-1830. Black individuals include "negro", "moreno", "pardo" and "mulato". Data comes from the *Indice del Archivo de Policía desde 1812*. Months corresponding to the wheat harvest season are shaded in grey.

7.2 Heterogeneous Effects by Type of Criminal Charge

Analyzing heterogeneous effects by type of criminal charge allows us to form conjectures about potential mechanisms that might explain our main result. For this, we used the text of police records to classify criminal charges into three types: robbery, assault, and discretionary charges —like profanity, vagrancy, and drunkenness. We study the effect of emancipation on incarceration for each of these charges. These results, however, should be interpreted with caution given the small number of observations and the possibility of misclassification —e.g., an attempted robbery can fail and become assault.

Figure 5 shows that emancipation has a positive average causal effect across criminal charges. The effect is not statistically significant for the case of robbery (Panel A; exact

Figure 5: Effect of emancipation on incarceration by type of criminal charge



Randomization inference exact p-values in parentheses.

p = 0.139); and it is statistically significant for assault (Panel B; exact p = 0.035) and discretionary charges (Panel C; exact p = 0.053).[27]

A possible interpretation for these results is that with emancipation, formerly enslaved persons gained liberty but lost the "safety net" afforded by their owners. The fact that emancipated people transitioned from one type of marginality (slavery) into another one (poverty) has been considered by some scholars to be "the central paradox of the postemancipation period" (Jean, 2019, p. 696). Our results show that emancipation had a positive affect on robbery and assault (albeit not significant for robbery), which could reflect a higher degree of material deprivation that might have pushed emancipated people towards criminal activity. This finding is consistent with, Goldberg (1976, p. 88)'s view that the higher mortality rates of emancipated people relative to enslaved people reflected the "only possible reality" that "the living conditions of people of color worsened when

[27]In Table A3 in the Appendix, we report the results of this analysis when using the census control groups. While the effect is positive for all types of criminal charges, it is only statistically significant for discretionary charges.

they obtained freedom." Likewise, the results show that emancipated people were mainly incarcerated for petty offenses that targeted the urban poor. This could reflect both discrimination by police officers (if emancipated people were intentionally targeted or lost the possibility of being defended by their enslavers) or a higher incidence of poverty among emancipated people. This pattern might also reflect the concern of political authorities with re-establishing social control during a period of disorder and internal strife, targeting the lower social strata to which emancipated person belonged (Szuchman, 1984).

8 Discussion

In this paper, we study the relationship between slavery, emancipation, and incarceration in an urban setting. Most work on this topic has focused on plantation economies, where scholars have widely documented that abolition led landed elites to craft discriminatory legal systems to coerce recently emancipated persons back into the rural workforce in exchange for below-market wages. Urban environments posed different incentives for the incarceration of free racial minorities. Unlike plantation economies, where enslaved people were a rural labor force, enslaved persons in urban settings worked mostly as artisans and were "hired out." Because of the particularities of slavery in urban contexts, emancipation did not jeopardize the profitability of agriculture.

Focusing on early nineteenth-century Buenos Aires, we show that emancipation also increased the incarceration of non-whites in urban contexts. We bring a wealth of new data to describe the relationship between race, slavery, and incarceration. We digitized the full count of Afro-Americans in the 1810 census and manually linked their names to police records from the pre-1830 period. This allowed us to document for the first time the incarceration rates of free and enslaved Black persons in the urban context of Buenos Aires. We also exploited a natural experiment to estimate the effect of emancipation on

incarceration. Comparing individuals who won a lottery of certificates of freedom with different samples of individuals who were eligible for the lottery but did not win, we show that emancipation, on average, increased the probability that a person was subsequently incarcerated.

Determining mechanisms is challenging because emancipation from slavery is a bundled treatment. The components of this bundle depend on the slavery institutions of each particular place. In plantation economies, emancipation entailed at least six significant changes. Emancipated people (1) became legally susceptible to incarceration, (2) gained freedom of movement, (3) lost the economic "safety net" afforded by their enslavers, (4) could no longer be forced to work in agriculture, (5) lost protection from their enslavers against arbitrary treatment by law enforcement agents, and (6) gained a legal status that threatened the racial hierarchy and motivated hostility from Whites people. In urban settings, the first two components of the bundle were absent because enslaved people could be incarcerated and had substantial freedom of movement; and the third component of the bundle was arguably less prominent than in plantation systems because enslaved people in urban settings owed their enslavers periodic payments, so freedom entailed liberation from this "debt." If one accepts these claims, then the link between emancipation and incarceration that we detect must stem from some of the last three components of the bundled treatment. Crucially, our ability to conclusively detect causal mechanisms is limited by the size of our study, so our tests to parse out which of these mechanisms is at play should be interpreted with caution.

We show that the positive effect of emancipation on incarceration in this urban setting is not driven by labor market incentives from the country side —suggesting that the emancipated were not more likely to be incarcerated because they could not be forced to join the rural workforce. We find that the effect of emancipation on incarceration is higher during

periods of *low* agricultural labor demand, and differences across seasons are not themselves statistically significant. We corroborate this result digitizing the full police records from the period that we analyze, and show that the proportion of free Black people incarcerated did not grow, on average, during periods of high demand for rural labor. While our results do not refute the validity of the labor-coercion mechanism in plantation economies, they cast doubt on its generalizability to the context of urban slavery —which was common throughout the Americas.

We find evidence that emancipated persons were more likely to be imprisoned for discretionary charges that typically targeted the urban poor —vagrancy, drunkenness, loitering, etc. This finding is consistent with emancipated people losing the protection against arbitrary incarceration afforded by their enslavers; and also with a context of hostility from Whites against their emancipated status, could have been seen as a threat to the racial hierarchy. Our evidence, nonetheless, is insufficient to distinguish between these alternative mechanisms.

Even though ethnoracial inequalities abound across Latin America, research on how such inequalities have mapped onto state formation has been scarce. Our paper has documented a link between emancipation and incarceration, which suggests that the abolition of slavery must have stressed the coercive apparatus of societies. In rural settings, labor coercion had to some extent be transferred from planters to the state. In urban settings, where the nascent states supplied most of the coercion needed to sustain slavery, abolition contributed to the growth and professionalization of the existing coercive apparatus. Studying how slavery regimes in the Americas influenced state development is an interesting avenue for further research.

In addition to estimating the effect of emancipation on incarceration, we have also provided the first quantitative micro-historical description of incarceration rates across

socio-demographic groups. An interesting pattern that arises from our data, and that we have not explored in-depth, is that the observed incarceration rates of non-White and low-status White persons was similar between 1820 and 1830. Other scholars have discussed the difficulties that arise when making comparisons across racial groups (Sen and Wasow, 2016). Yet this similarity might suggest an intersectionally oppressive context that discriminated by race, but also by social class. We hope our paper will motivate further research that delves into the effects of other personal traits on incarceration rates, such as social class and gender.

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Appendix

Table of contents:

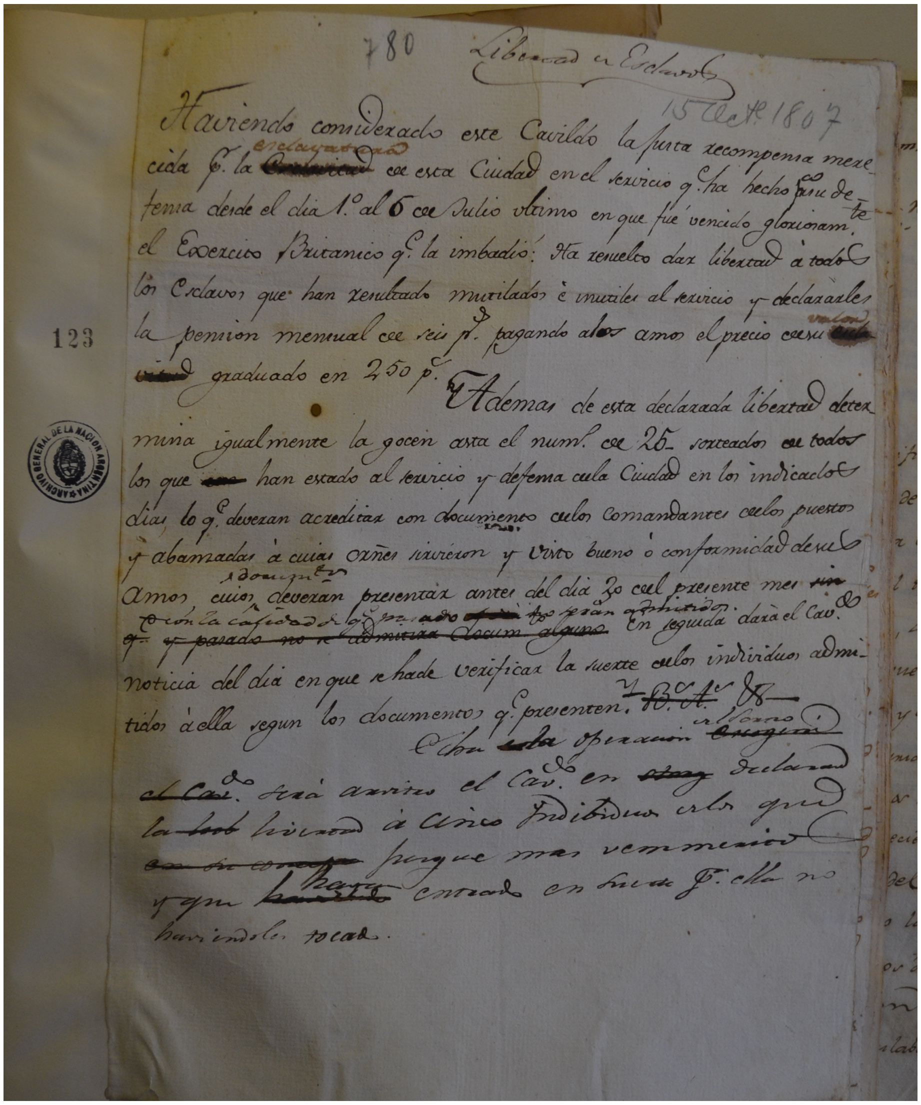
A Archival Material on the Lottery

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.[28]

[28]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 A1: Cabildo's decision to reward slaves with emancipation. October 15th, 1807



780 Libertad de Esclavos

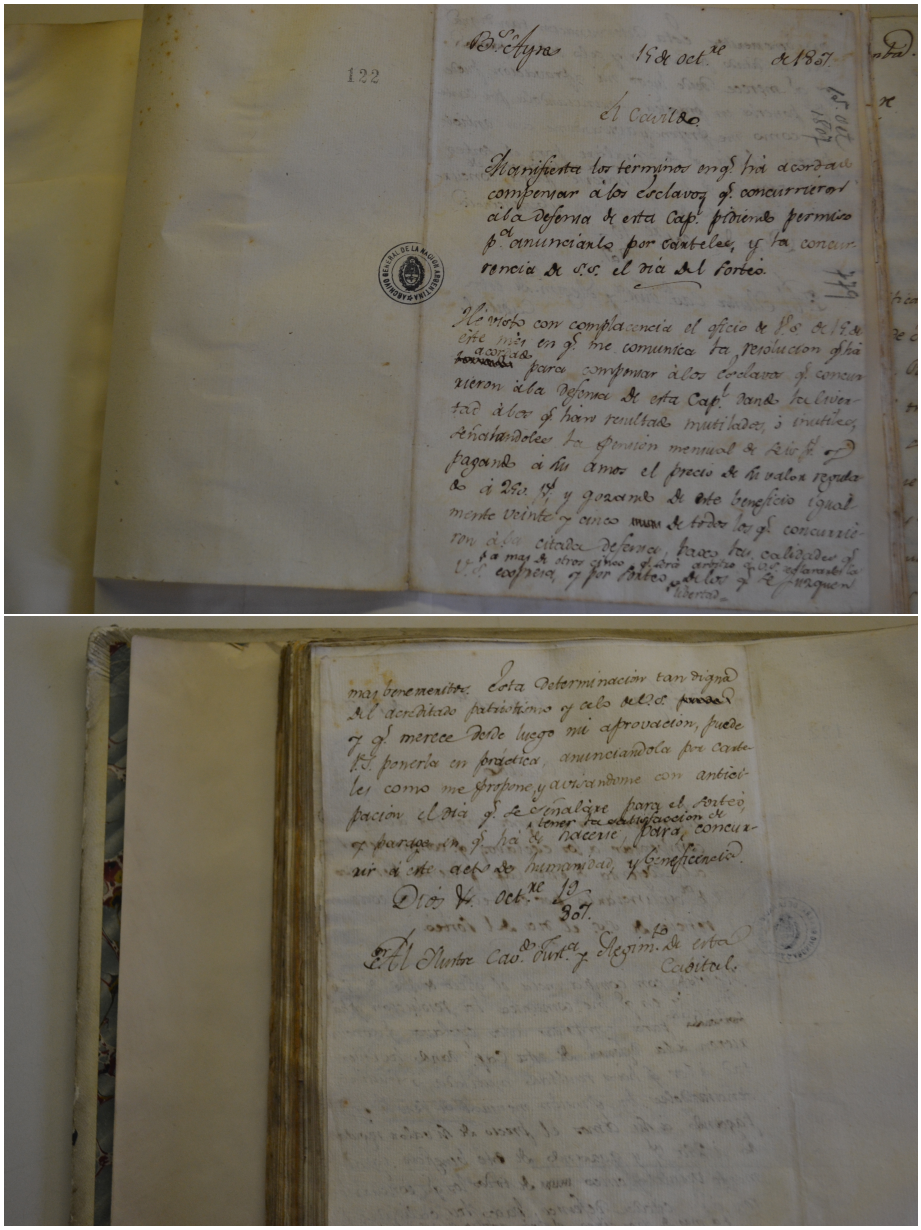
15 Oct.e 1807

Haviendo considerado este Cavildo la justa recompensa merecida q. la esclavatura de esta Ciudad en el servicio q. ha hecho en su defensa desde el dia 1.º al 6 de Julio ultimo en que fue vencido gloriosam.te el Exercito Britanico q. la imbadio: Ha resuelto dar libertad à todos los Esclavos que han resultado mutilados è inutiles al servicio y declararles la pension mensual de seis p.s pagando a los amos el precio de su valor graduado en 250 p.s

Ademas de esta declarada libertad determina igualmente la gocen asta el num.o de 25 sorteados de todos los que han estado al servicio y defensa de la Ciudad en los indicados dias, lo q. deveran acreditar con documentos de los Comandantes de los puestos y avanzadas à cuias ordenes sirvieron y visto bueno ó conformidad de sus Amos, cuios documentos deveran presentar antes del dia 30 del presente mes [illegible]. En seguida dará el Cav.do noticia del dia en que se ha de verificar la suerte de los individuos admitidos à ella segun los documentos q. presenten.

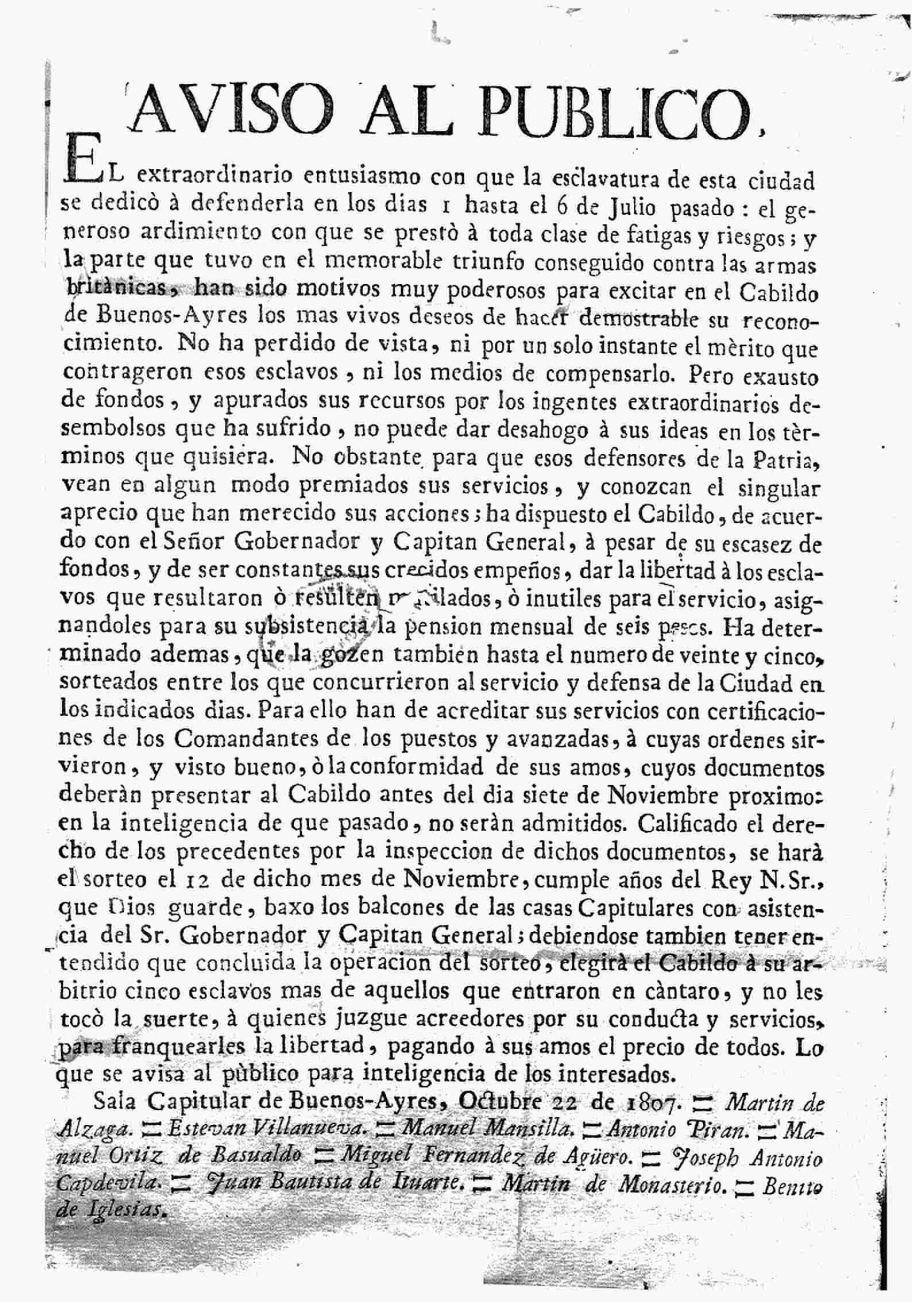
[illegible]

Figure A2: Governor's agreement to reward slaves with emancipation. October 19th, 1807



"Manifiesta los términos con que ha acordado compensar a los esclavos que concurrieron a la defensa de esta Capital pidiendo permiso para anunciarlo por carteles y la concurrencia de S.S. el día del sorteo."

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

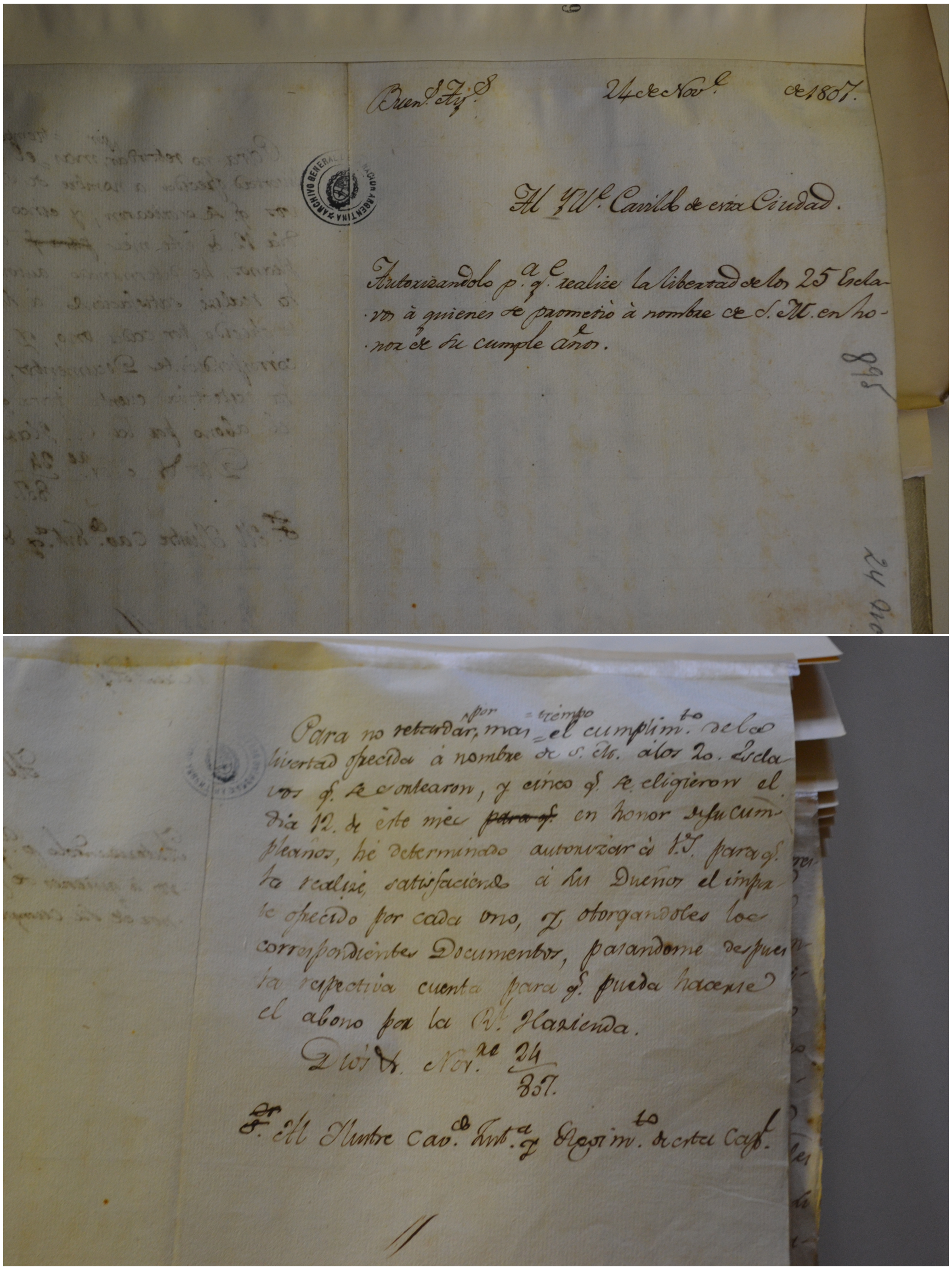


AVISO AL PUBLICO.

EL 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 inutilados, ò inutiles 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 deberàn 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 harà 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 pùblico 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 Capdevila.* = *Juan Bautista de Iturte.* = *Martin de Monasterio.* = *Benito de Iglesias.*

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



Buen.s Ay.s 24 de Nov.e de 1807.

Al Y. Il.e Cavildo de esta Ciudad.

Autorizandolo p.a q.e realize la libertad de los 25 Esclavos à quienes se prometio à nombre de S. M. en honor de su cumple años.

Para no retardar, por mas tiempo el cumplim.to de la libertad ofrecida à nombre de S. M. à los 20. Esclavos q.e se sortearon, y cinco q.e se eligieron el dia 12. de este mes ~~para q.e~~ en honor de su cumpleaños, he determinado autorizar à V.S. para q.e la realize, satisfaciendo à sus Dueños el importe ofrecido por cada uno, y otorgandoles los correspondientes Documentos, pasandome despues la respectiva cuenta para q.e pueda hacerse el abono por la R.l Hacienda.

Dios &c. Nov.re 24 807.

S.res del Ilustre Cav.do Just.a y Regim.to de esta Cap.l

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 Digitizing the 1810 census

In this appendix, we provide details on how we digitized the full count of the 1810 census.

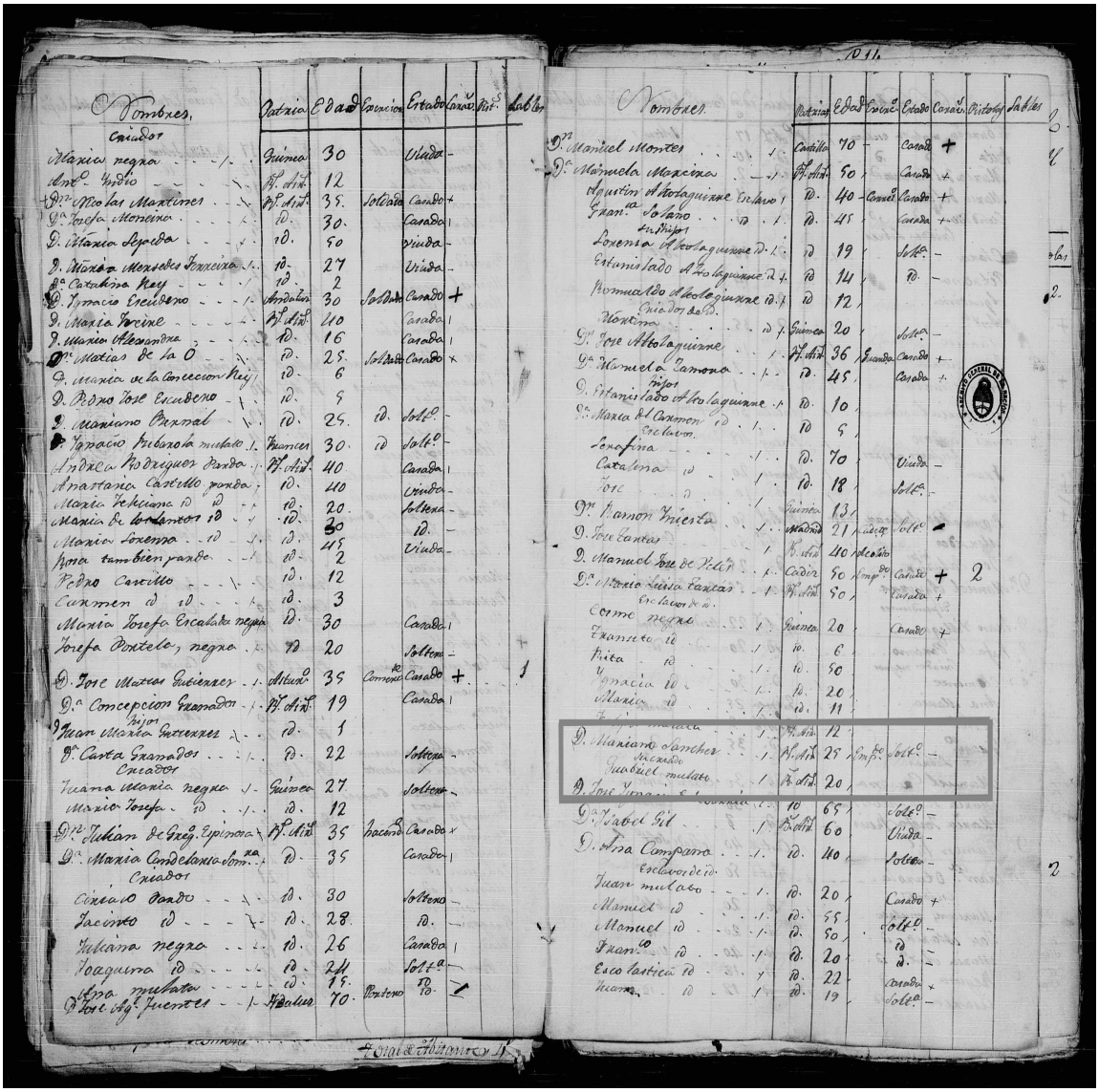


Figure A5: An example from the census

The 1810 census. The census of 1810 is handwritten, available online through FamilySearch, and has never been transcribed before.[29] Significant parts of this census have not survived to the present day, so we could rely on only the 774 surviving pages.

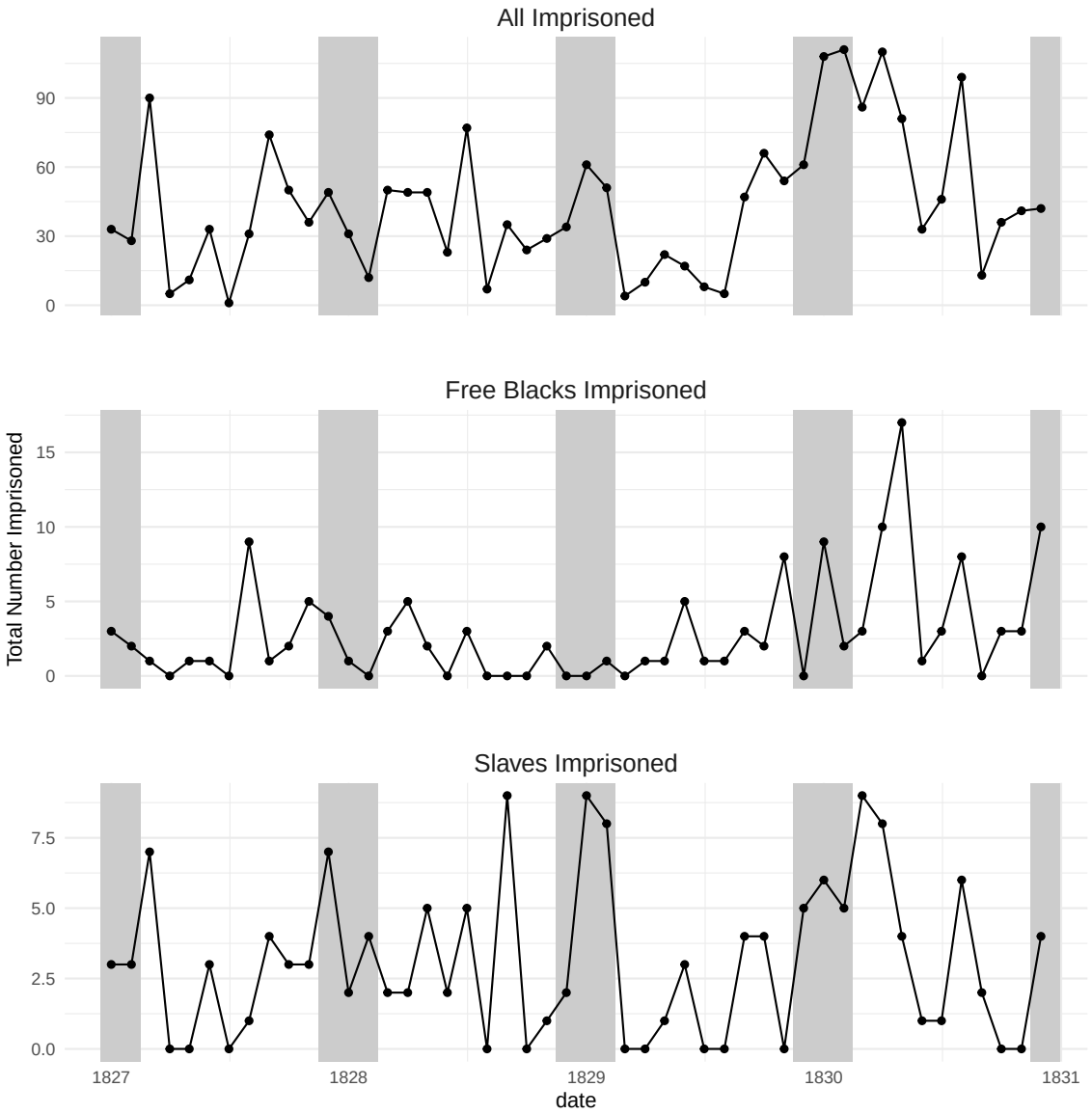
We manually transcribed the names of all the non-White persons over 18 years of age. For illustration, Figure A5 shows a page from the census. The box in the lower-right quadrant displays the name of a 20-year-old non-White slave (a *mulato*) called Gabriel. He was a slave of the household of Don Mariano 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.

We also digitized a random sample of 685 White persons. For this, we recorded the name of the third White person in each page of the census.

[29] https://www.familysearch.org/search/catalog/498602?availability=Family%20History%20Library

D Monthly Data on Incarcerations

Figure A6: Incarcerations 1827–1830



Notes: The figure shows the total number of individuals imprisoned each month during the 1827-1930 period. Data comes from the police archives. Wheat harvest season is shadowed in greay.

E Additional Results

E.1 Figure 3 Results in Table Form

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

	Robbery	**Assault**	**Discretionary**
Mean in Control	0.020	0.007	0.033
Difference in Means	0.047	0.060	0.078
	(0.039)	(0.038)	(0.050)
Exact p-value	0.133	0.043	0.051
Num.Obs.	195	195	195
Outcome	Binary	Binary	Binary
Sample	Full	Full	Full

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

Notes: The figure shows the difference in means in incarceration rates by type of criminal charge for the comparison between lottery winners and individuals in the Batallion of Slaves.

F Results Iteratively Excluding Observations

Figure A7: Leave-one-out results

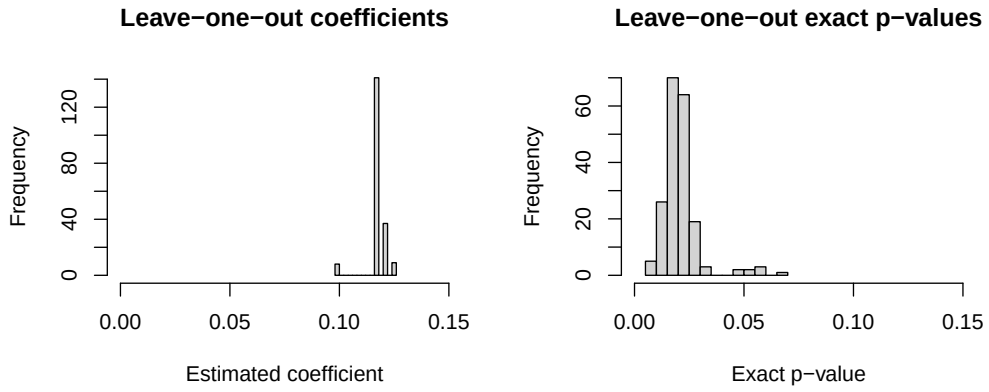
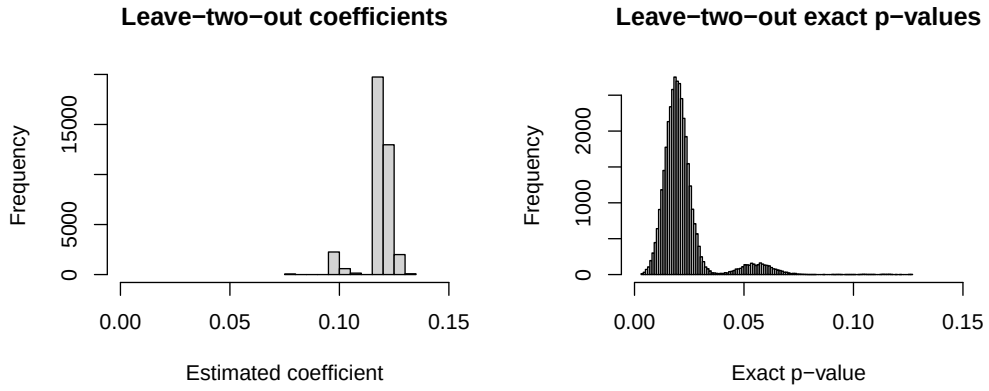


Figure A8: Leave-two-out results



G Additional Results Using the Census as the Control Group

Table A2: Effect of emancipation, harvest vs. nonharvest months

Control Group		Harvest Months	Nonharvest Months
15 and older	Diff. in Means	0.034	0.069
	ri p-value	0.39	0.11
15-40	Diff. in Means	0.036	0.053
	ri p-value	0.39	0.25
15-30	Diff. in Means	0.037	0.059
	ri p-value	0.17	0.15
15-20	Diff. in Means	0.034	0.065
	ri p-value	0.39	0.13

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

Table A3: Effect of emancipation by type of criminal charge

Control Group		Robbery	Assault	Discretionary
15 and older	Diff. in Means	0.032	0.027	0.065+
	ri p-value	0.40	0.43	0.05
15-40	Diff. in Means	0.026	0.036	0.052
	ri p-value	0.43	0.39	0.19
15-30	Diff. in Means	0.028	0.026	0.061+
	ri p-value	0.42	0.44	0.09
15-20	Diff. in Means	0.032	0.027	0.064+
	ri p-value	0.41	0.43	0.07

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