The end of slavery in Brazil: Escape and resistance on the road to freedom*

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Abstract

A longstanding debate opposes two mechanisms by which labor coercion persists or changes to free labor: a *labor demand effect*, by which the elite coerces labor when supply is scarce, and an *outside option effect*, by which labor scarcity and better outside options for the workers undermine coercive arrangements. Using a novel data set of roll-call votes on 1884-1888 emancipation bills in the Brazilian legislature, we find that both mechanisms played a role in building the coalition that eventually abolished slavery.

Key words: labor coercion, Brazil, slavery, immigration, institutional change, intra-elite conflict.

JEL codes: J15, J47, N36, N46, O54.

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> Art. 1.º: É declarada extinta desde a data desta lei a escravidão no Brasil.
> Art. 2.º: Revogam-se as disposições em contrário.
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> Lei Áurea, May 13, 1888

A longstanding debate opposes two mechanisms by which labor coercion persists or ends: the elite's increased motivation to coerce scarce labor, and the growing difficulty of doing so as workers gain more outside options. We bring a unique perspective on the political dynamics that bring an end to officially sanctioned coercive systems, exploiting the case of Brazil. In 1888, Brazil was the last country to abolish slavery in the Western Hemisphere. Between 1884 and 1888, Brazilian legislators, each representing their own district in the *Câmara dos deputados*, the lower chamber of the Brazilian Parliament, voted on thirteen abolition-related bills.

Individual legislators voted in a way that reflects the economic interests of their district's landowners. The oligarchical political system of the Empire of Brazil enfranchised a limited elite, and within the elite, historians argue that the interests of slaveholders dominated. Conrad (1972, p. 16) argued that "much of the real power in the provinces was in the hands of the slaveholding landlord class." As a result, "politicians often represented in the Chamber, the Senate, or the Council of State the interests of plantation owners and merchants to whom they were tied by links of patronage and clientele" (Viotti da Costa, 1989, p. 179).

This article studies two main determinants of the abolitionist vote. The decision of a district's representative to vote for or against abolition-related bills was influenced, first, by the district's access to immigrant labor and, second, by the level of resistance put forth by the enslaved population within the district. These two factors correspond to two mechanisms influencing local elites' support for slavery: a *labor demand effect*, by which the elite coerces labor when supply is scarce, and an *outside option effect*, by which labor scarcity and better outside options for the workers undermine coercive arrangements through the agency of those who were coerced.

Our two proxies for labor demand and outside options are anchored in Brazilian historiography. Enforcement of the coercive institution was affected by the frequency of evasions by enslaved individuals (Needell, 2001, Graden, 2006, Machado, 2011, and Roberts, 2015), and landowners for whom enforcement was relatively more costly had

less to lose or more to gain from a switch to free labor. We capture the outside option of the enslaved by constructing a collection of measures of the proximity to *quilombos*, communities of maroons that were able to escape and hide in the hinterland ('distance to freedom'), and of how vast these quilombos were. At the same time, Conrad (1972) and Klein and Luna (2009) emphasized the importance of the substitution between enslaved workers and European immigrants at the end of the abolition period, but access to immigrant labor was heterogeneous across the land. Foreigners represented between 0% and 24% of a district's population in 1890. We use this heterogeneity to capture variation in the scarcity of labor, effective or anticipated by local landowners while abolition bills were being discussed in Parliament.

We find that, first, representatives from districts where it was easier for the enslaved to escape were less likely to oppose abolition, in line with an *outside option effect*. We estimate that an increase in the average distance to the nearest quilombo by one standard deviation [SD], i.e. 29km, is associated with a 10 percentage points decrease in the likelihood that a legislator representing a typical slavery-intensive district votes in favor of emancipation (an 18% increase relative to the sample mean). Alternatively, decreasing the average area of land occupied by quilombos in a district by one SD (45km^2) is associated with an 11 percentage points decrease in a legislator's likelihood to vote in favor of emancipation (a close to 20% increase from the sample mean). Second, representatives from districts that attracted more immigrants in the run up to abolition voted more in favor of abolition. This is in line with a *labor demand effect*: landowners with easier access to immigrant labor found a switch to free labor less costly than some of their competitors. We estimate that an increase in the share of immigrants in a district by one SD (4 percentage points) is associated with a 12 percentage points increase in the corresponding legislator's likelihood to vote in favor of emancipation bills (a 21% increase from the sample mean).

To find exogenous variation in the location and size of quilombos, we rely on the fact that fugitives tended to establish quilombos in remote places or rugged terrain. In the proximity of large settlement areas, a rugged terrain facilitated escape. Remoteness could substitute for ruggedness, less important at a safe distance from cities. The instrument we propose for the location of quilombos is only valid if within-province variation in this substitution effect does not affect legislators' voting decisions other than by its influence on enslaved workers' 'outside options' (e.g. via trade and transport costs). We show that this assumption is likely to hold. The variation driving the

instrument lies in this interaction, holding constant the direct effects of remoteness and ruggedness and flexibly controlling for nonlinearities in the un-interacted terms.

To find exogenous variation in the location of immigrants, we construct a shift-share instrument that predicts the inflows of immigrants between 1872 and 1890 based on preexisting migration networks in each district. Shift-share instrumental variables (SSIV) are common to address the endogeneity of immigrants' location (see e.g. Boustan, 2010, Alesina et al., 2016, Burchardi et al., 2019, Tabellini, 2020, Derenoncourt, 2022, Calderon et al., 2023, Bazzi et al., 2023). We build two versions of this instrument: a standard SSIV relying on origin country shocks, and a religion-based SSIV. In both cases, we verify that results are robust to alternative ways to construct and scale the instruments, the use of alternative estimators, and other identification checks outlined in the literature on SSIVs (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). Our IV strategy successfully isolates the labor demand and outside option effects: the Bartik instrument predicts 1890 immigration waves without influencing quilombo location, while our substitution instrument strongly predicts quilombos but not immigration. These approaches consistently support our OLS findings and yield comparable point estimates of the two effects.

We conduct a comprehensive series of tests to verify the robustness of our results. In particular, we confirm that our findings are unlikely to be driven by unobserved heterogeneity across units or selection on unobservables, and rule out selection related to specific bills or legislators. We do not find that absenteeism (nonvoting) may have been used strategically. Results are also robust to placebo checks exploiting effect heterogeneity, to various operationalizations of our voting outcome and measures of proximity to freedom, and to alternative ways of dealing with outliers. Overall, our alternative identification approaches and robustness checks suggest that the effects of outside options and labor demand on voting decisions can be interpreted as causal.

This work contributes to an important literature on the political economy of labor coercion. There is, in particular, a growing body of evidence that demonstrated that the shadow of coercive systems is particularly long. The persistent influence of labor coercion on development is now well established (Nunn, 2008, Dell, 2010, Nunn and Wantchekon, 2011, Acemoglu et al., 2012, Bertocchi and Dimico, 2014, Acharya et al., 2016, Markevich and Zhuravskaya, 2018, Dell and Olken, 2020, Buggle and Nafziger, 2021 and Lowes and Montero, 2021). In Brazil, Summerhill (2010) found little association between colonial institutions, including slavery, and contemporary

outcomes, but Naritomi et al. (2012) found that differences in sub-national colonial institutions did matter for development. More recently, Papadia (2019) investigated the influence of slavery on fiscal capacity in Brazil's main coffee provinces, Palma et al. (2021) its influence on industrialization, Laudares and Valencia Caicedo (2023) showed that slavery had a persistent influence on contemporary income inequality, and Seyler (2021) that support for slavery influenced long-run development.

This literature still largely lacks systematic evidence on the politics that ended such systems. Still, we are not first to study factors of abolition. Nieboer (1900) and Domar (1970) predicted the labor demand effect, i.e. that landowners are more likely to implement coercive labor institutions when the ratio of land to labor is high. As a country with an open agricultural frontier, with half of the province of São Paulo still branded as *terreno desconhecido*, uncharted land, on historical maps, and where slavery played such a defining role in social relations, nineteenth-century Brazil is an ideal case-study to verify how landowners used slavery to sustain their status and, among landowners, which ones found it in their best interest to hasten the transition to 'free labor.'[1] In contrast, the institutional consequences of the mobility of scarce labor, i.e. the outside option effect, were already noted in neo-Malthusian studies of the decline of coercive feudal arrangements in fourteenth- to sixteenth-century Western Europe (Postan, 1937, Le Roy Ladurie, 1969, North and Thomas, 1973, Brenner, 1976, and Cox and Figueroa, 2021). Recent contributions showed how variations in the outside option of enslaved individuals also affected human capital accumulation (Bobonis and Morrow, 2014), wages, formal employment, and incarceration rates (Dippel et al., 2020), but none tested the outside option effect on the institutions of coercion themselves. Scholars had long thought that the two corresponding mechanisms, both expressed in terms of the ratio of land to labor, contradicted each other (Aston and Philpin, 1985). Acemoglu and Wolitzky (2011) showed formally that the two effects should coexist. Previous empirical works, focusing on the single relationship between the land to labor ratio and coercive institutions, found support for the labor demand effect in Nigeria (Fenske, 2012), South Africa (Green, 2014), and in the Czech Republic (Klein and Ogilvie, 2017). Using other proxies of the demand for labor and for workers' outside options, we unpack these mechanisms, and find

[1] Ashraf et al. (2025) propose a refinement of the labor demand hypothesis during industrialization. They show that it is the rise of capital-skill complementarity that eventually made the employment of free skilled workers more profitable in Prussia.

support for both mechanisms in Brazil.

Our approach also relates to the literature that considers how legislative coalitions formed to back institutional change: democratization (Jha, 2015 and Aidt and Franck, 2015, 2019) as well as significant pieces of legislation (Mian et al., 2010). A common observation of this literature is that individual legislators are usually not pivotal: they are not deciding whether a policy passes, but what message they are sending to their party and voters. This is especially relevant here, for an issue that was sensitive and visible, to the point that the roll was called for each of the thirteen abolition-related bills in the period.

The remainder of the paper proceeds as follows. Section 1 provides additional elements of the historical context. Section 2 describes our data sources. Section 3 presents our main empirical strategy and baseline results. Section 4 details our identification strategy and presents the corresponding results. Section 5 concludes.

1 Historical context

With the benefit of hindsight, it is maybe easy to believe that slavery was doomed to disappear eventually.[2] And indeed, in its last moments, it faced a combination of intense international pressure (de Paiva Abreu and Aranha Corrêa do Lago, 2010 and de Bivar Marquese, 2015), and increasing domestic opposition (Conrad, 1972 and Izecksohn, 2021). This teleological view of history overlooks a unique combination of obstacles to abolition in nineteenth-century Brazil. The roots of slavery are deep in the Americas. After the sixteenth-century repression of the Tupi-Guaraní-speaking populations along the Brazilian coastline (Schwartz, 1978), the success of large-scale sugar production and the massive arrival of enslaved Africans marked the birth of a deeply coercive system that permeated through the New World. The Brazilian economy was entirely driven by agricultural exports and heavily dependent on captive labor. According to the latest estimates of the *Slave Voyages* online database (Eltis, 2007), 5,532,120 Africans arrived in Brazil between 1550 and 1866. In comparison,

[2] At least, legal slavery. In the Brazilian Amazon, tens of thousands of workers (mostly internal migrants from rural areas) continue to live under coercive labor arrangements. The exact number of coerced workers is not known, but according to Repórter Brasil (2015), nearly 50,000 workers were freed between 1995 and 2015. Illegal slavery is very much a modern problem, and not only in Brazil: from the *kafala* system in Qatar to debt-bondage in India, examples abound globally. The International Labour Organization and the Walk Free Foundation (2017) estimate that in 2016, 40 million people were victims of modern slavery, 71% of whom women and girls.

'only' 472,382 disembarked in mainland North America, nearly twelve times fewer. In 1872, an average of 15%, and up to 53% of a district's population was enslaved (see Figure 1 for municipality-level shares).

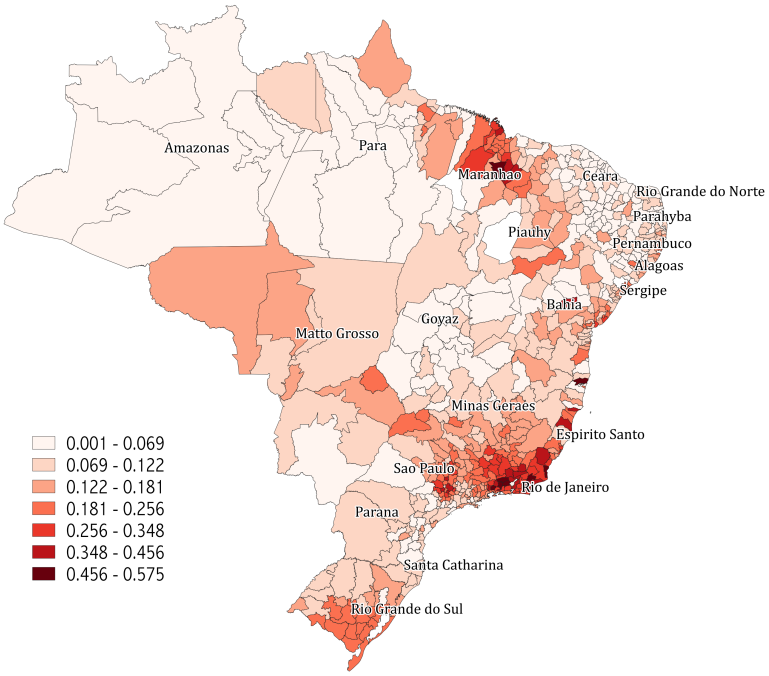


Figure 1: Enslaved population by municipalities in 1872 (Brazil, 1874). Municipality boundaries in 1872 come from IBGE (2010).

By 1807, the United Kingdom and the United States had abolished the Atlantic slave trade, and started pushing other countries to do the same. The young empire had accepted to ban the trade in 1831, but it took 20 years (and forceful action by the British crown) to get Brazil to effectively act against it (de Alencastro, 1979). Another 20 years later, the 1871 *Lei do Ventre Livre* temporarily placated abolitionist sentiments stirred by the American Civil War (Conrad, 1972), with little immediate effect (children born from enslaved mothers after 1871 would only be effectively free upon reaching majority at age 21). Despite the domestic and the international pressure (de Paiva Abreu and Aranha Corrêa do Lago, 2010 and de Bivar Marquese, 2015), the political context was hardly favorable to abolition.

The question was brought back to the forefront of the legislative agenda in the 1880s. By this time, the interprovincial slave trade had drained over two thirds of the enslaved from every region into the Sudeste (Conrad, 1972, pp. 285-290). In 1884, Emperor Pedro II – compelled to act after the rise of emancipation movements in the

North – charged the liberal senator Sousa Dantas to constitute a new cabinet and to move towards emancipation. The bill he presented, known as the *Dantas project*, rallied pro-slavery interest groups (Ridings, 1994). In the ensuing parliamentary crisis (Conrad, 1972 and Viotti da Costa, 1989), Sousa Dantas was ousted and replaced by a cabinet more amicable to slaveholders' interests.

The new cabinet proposed the *Saraiva-Cotegipe* bill that emancipated enslaved persons over 60 years old. Despite opposition from some liberals disappointed that the law did not go far enough, from some conservatives (mostly from Minas Gerais and Rio de Janeiro) opposed to any change in the existing institution, and from the hardcore slaveholders from the Paraíba valley, the bill was adopted with 81% of the votes in 1885. The *Lei dos Sexagenários* marked the point where abolition started gaining supporters in the Centro-Sul (see Figure 2, and Appendix A.5 for other bills). Scholars disagree about the fundamental causes of São Paulo's gradual conversion to abolitionism. Morse (1958), Graham (1968) and Dean (2012) proposed that new planters were progressive and keen on turning to immigrant workers as a substitute to coerced ones. However, Conrad (1972) observed that the growth of the enslaved population was largest in newly cultivated areas of São Paulo (see also Lowrie, 1938). Be that as it may, and although Paulista representatives were still cautious in 1885, their conversion played a fundamental role in the abolition of slavery.

In the late 1880s, emancipation movements radicalized, with frequent rebellions, scenes of violence and flights from plantation (Conrad, 1972 and Reis and Reis, 1988). Military aid was sent to (reluctantly) help persecute runaways (Toplin, 1969). At the same time, efforts to attract European immigrants to work on the plantations started paying off. 6,500 immigrants entered São Paulo in 1885, 32,000 in 1887, and 90,000 in 1888 (Conrad, 1972). It is under these circumstances that many Paulista planters converted to abolitionism (Luna, 1976), and liberated around 100,000 individuals in the first months of 1888. By early 1888, slavery was almost extinguished in the province (and coffee production was continuing almost unperturbed). Other provinces followed in the steps of São Paulo, leaving only planters in Rio de Janeiro and a few recalcitrant *latifundiários* from São Paulo and Minas Gerais to defend the coercion system.

The legislative session that opened in May 1888 had one priority: bring a definitive solution to the question of emancipation. A bill proclaiming the immediate abolition of slavery in two short articles was voted in the Chamber on May 9. As illustrated

in Figure 2, Rio de Janeiro was by the spring of 1888 the very last bastion of slavery in the Empire. Of the nine legislators that voted against the bill, eight were representatives from the province's electoral districts. At the time of abolition, Rio had remained unaffected by immigration and its planters were threatened by bankruptcy, with little more wealth than that represented by the enslaved labor they controlled (Conrad, 1972 and Viotti da Costa, 1989). The bill was sanctioned by over 90% of representatives and passed by the Senate a few days later. It was soon approved by the Princess Regent as the *Lei Áurea*, the Golden Law.

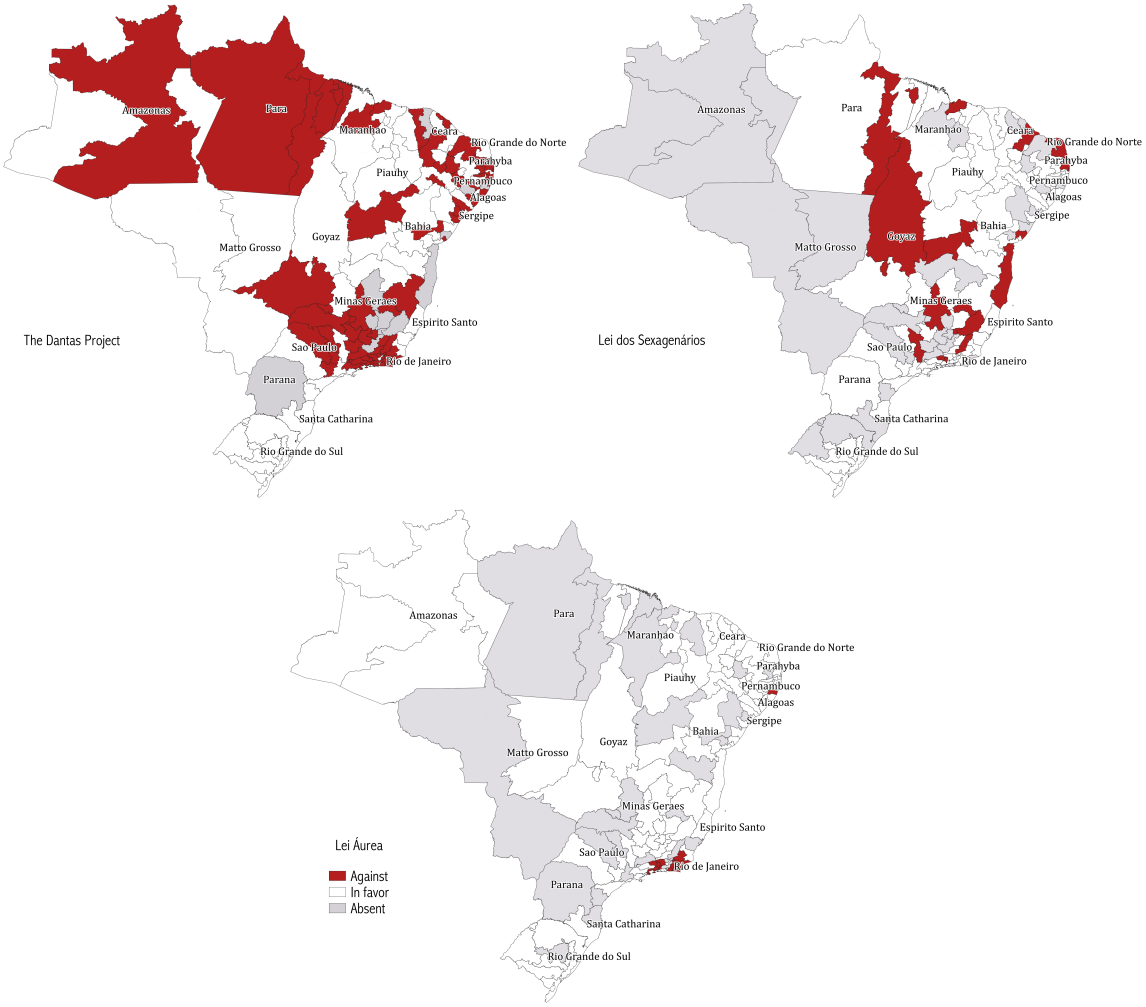


Figure 2: Vote by district on the 1884 Dantas Project, the 1885 *Lei dos Sexagenários*, and the 1888 *Lei Áurea*.

2 Data

We assembled a data set from a variety of archival records, census surveys, historical maps, and georeferenced sources. This section describes the sources and construction of the main variables of interest. Table 1 provides summary statistics for these variables. More details on these and additional data are given in Appendix A.1.

The 1881 Saraiva Law restructured the Empire's political system, introducing single-member electoral districts and direct elections. Political outcomes and covariates are therefore observed at the level of the district (122 districts from 1881 onward). Other variables, such as those drawn from the 1872 or 1890 censuses, are observed at a lower level, typically *paróquia* or *município* (e.g., 642 municipalities in 1872). To keep units consistent over time, we match administrative divisions with 1881 districts and aggregate variables at that level.

Political variables.—We collect the vote (or absence) of all legislators on each instance of the thirteen roll-call votes related to the emancipation of enslaved persons from the onset of the eighteenth legislature (1882) to the end of the twentieth legislature (1889) in the Câmara dos Deputados. Together, they constitute the universe of relevant votes,[3] starting with the first no-confidence vote against the Dantas Cabinet in 1884 and ending with the vote for the Lei Áurea in 1888. We use the 1881 electoral legislation reported in Jobim and Porto (1996) to identify the post-Lei Saraiva district-level electoral division and georeference each district using IBGE (2010). We then cross-reference the matching of each legislator to a unique district from Nogueira and Firmo's (1973) encyclopedia of parliamentarians. The baseline measure of support for slavery is coded as one whenever a legislator voted in favor of an emancipation-related bill, and zero if they voted against (we consider alternatives in Appendix A.4). Nogueira and Firmo's (1973) biographical records also provide controls for individual legislators' characteristics, such as party affiliation, reelection status, other government appointments, and occupation/education. Figure A.20 in Appendix A.4 provides an example of these biographical records.

Demographic variables.—We compute the share of enslaved persons in a district's

[3]Very few issues other than emancipation were subjected to nominal voting during the period, and no usable roll-call other than the thirteen we assemble. In particular, we identified only one roll-call vote on an issue related to immigration, on July 22, 1885. It concerned a levy to finance immigration, opposed both by moderate abolitionists and hard-core slaveholders. In Appendix A.4 we verify that our results are robust to both selection on bills and legislators (i.e. absenteeism).

Table 1: Summary statistics

Statistic	N*	Mean	St. Dev.	Min	Max
Political variables (by district × vote)					
Abolitionist vote	1,284	.56	.50	0	1
Absence on roll call day	1,586	.19	.39	0	1
Reelected	1,586	.32	.47	0	1
Other gov. appointment	1,586	.54	.50	0	1
Occupation/Education:†					
Law	1,586	.66	.47	0	1
Medicine	1,586	.15	.36	0	1
Science	1,586	.047	.21	0	1
Priesthood	1,586	.010	.10	0	1
Owner/Businessman	1,586	.033	.18	0	1
Military	1,586	.003	.05	0	1
Civil service	1,586	.057	.23	0	1
Demographic variables (aggregated by district)					
Share of enslaved in 1872	122	.148	.094	.018	.537
Share of free foreigners in 1872	122	.025	.047	.000	.267
Share of foreigners in 1890	122	.019	.040	.000	.237
Share of free colored in 1872	122	.476	.163	.104	.829
Share of literates in 1872	122	.161	.065	.080	.362
Outside option variables (aggregated by district)					
Av. number of quilombos	122	4.29	6.67	.0	52.0
Av. dist. to closest 1-quilombo municipality	122	21.93	29.00	.0	196.6
Av. dist. to closest 2-quilombos municipality	122	48.96	85.81	.0	737.1
Av. dist. to closest 3-quilombos municipality	122	67.67	92.42	.0	737.1
Av. dist. to closest 4-quilombos municipality	122	76.82	92.48	.0	737.1
Av. dist. to closest 5-quilombos municipality	122	86.58	92.94	.0	737.1
Av. dist. to closest 6-quilombos municipality	122	111.1	111.2	.0	861.4
Av. dist. to closest 7-quilombos municipality	122	117.8	112.8	.0	861.4
Av. dist. to closest 8-quilombos municipality	122	118.8	112.7	.0	861.4
Av. dist. to closest 9-quilombos municipality	122	138.3	121.5	.0	861.4
Av. area quilombola	122	112.7	729.3	.004	7,198
Geographical variables (aggregated by district)					
Average coffee suitability	122	26.35	12.24	.00	50.48
Average sugarcane suitability	122	24.86	9.70	5.16	59.85
Average cotton suitability	122	24.17	8.78	3.42	44.65
Topographic ruggedness index	122	.45	.37	.03	2.34
Average rainfall	122	1,390	366.5	733.7	2,506
Average latitude	122	−43.02	5.95	−65.87	−35.04
Average longitude	122	−14.54	7.84	−31.48	−2.44
Distance to the coast	122	242.8	312.1	12.25	1,828
Distance to closest river	122	31.59	21.82	2.051	93.69
Av. human mobility index	122	.292	.027	.235	.354
Av. travel time to nearest prov. capital	122	51.61	41.68	.0	192.3
Av. pop. density	122	19.53	41.41	.024	232.3

* 122 districts × 13 votes = 1586 obs. Dist. in km, surf. in km^2, time in hr, dens. in km^{-2}.
† These categories are not necessarily mutually exclusive.

total population in 1872 from the first nation-wide demographic census in the country's history (Brazil, 1874). From the 1872 census, we also compute the share of the non-captive colored population (defined as the sum of Black, brown-skinned and mixed-race persons), and the literacy rate. We compute the shares of (free) foreigners in a district from the 1872 and 1890 censuses, as well as the number of foreigners by nationality (26 nationalities with both catholic and noncatholic populations reported)

and religion (8 religions reported) (Brazil, 1895, is the second census).[4] Further description of these variables is provided in Appendix A.1.

Quilombos.—Quilombos were communities founded by maroons as early as the sixteenth century (most of them before the nineteenth), that could offer refuge to runaways (Anderson, 1996). For each district, we compute the number of quilombos and the area occupied by quilombos using data from Fundação Palmares (2020) on all certified quilombos, as well as shapefiles from INCRA (2020). We also consider the distance from each municipality's head town to the closest municipality with at least n quilombos, which we average at the level of the district to better account for the actual outside option of enslaved workers.[5]

Geographical variables.—We use IIASA/FAO (2012) to build measures of rainfall and land suitability for sugarcane, coffee, and cotton aggregated at the district level.[6] We compute each district's area, population density in 1872, latitude and longitude of districts' centroids, as well as several distances (notably the distance from each district's centroid to the coast and the average distance from each municipality to the closest river). We also use Nunn and Puga's (2012) data on terrain ruggedness at the 30"×30" grid level to construct a measure of each district's topographic ruggedness index (TRI) (Riley et al., 1999). Finally, we use the Human Mobility Index (HMI) developed by Özak (2010, 2018) to calculate the average travel time across a 1km × 1km cell within a district and the average minimum travel time between each municipality's head town and the closest provincial capital (computing the least-cost

[4]The distribution of the number of immigrants by district is skewed. Figure A.5 in Appendix A.4 plots the distribution of the number of foreigners by district in 1890. This number ranges from 18 to 73311 immigrants. The first district of Rio de Janeiro (Rio de Janeiro city) holds 73311 foreigners in 1890, whereas the second largest district in terms of immigrants only holds 12217. Dropping or including Rio de Janeiro does not alter the results.

[5]The distribution of quilombos across districts is also skewed. The largest quilombo in the Fundação Palmares (2020) data set is Tambor, in the municipality of Manaus, first district of Amazonas, measuring an impressive 7197 km^2. The second one, Kalunga, in the municipality of Cavalcante, second district of Goiás, is significantly smaller, still measuring 2618 km^2. Following that, they become much more limited in size, ranging from roughly 3600 m^2 to 250 km^2. Figure A.6 in Appendix A.4 plots the distribution of the total area occupied by quilombos by district. A number of other works use quilombos, albeit in a different context. Laudares and Valencia Caicedo (2023) and Papadia (2019) use quilombos to measure slavery. Closer to our interpretation (and in line with Schwartz, 1992), Lambais (2020) uses quilombos as a measure of the resistance of enslaved persons, focusing on long-run effects on economic development.

[6]IIASA/FAO (2012) produces a grid cell-level normalized suitability index for different crops, notably based on inputs, water supply, soil type, and terrain slope. Ramankutty et al. (2002) also provides an alternative similar measure.

path according to the HMI).

When testing the robustness of our results, we mobilize several other variables (colonial settlements, insurrections, abolitionist clubs and newspapers etc.) that we describe in more detail in Appendix A.1.

3 Empirical strategy and baseline results

In this section, we present our baseline empirical strategy and results, setting the stage for a detailed discussion of identification in the next section.

3.1 Baseline specification

When casting their vote, legislators aggregate the preferences of their constituents, possible pressure from special interest groups, and their own political views. To the extent that elected representatives respond to their constituents' interests—and extensive evidence suggests that this is the case (see e.g. Mian et al., 2010; Calderon et al., 2023)—the economic interests of a district's voters will be reflected in their representative's vote.

Then, in districts where slavery is still relevant in the 1880s, enforcement costs should predict votes more favorable to abolition according to the outside option mechanism, while the availability of immigrant labor should have a similar effect according to the labor demand mechanism. To test this, we use the following baseline estimating equation:

$$P_{ijv} = \alpha_{jv} + \theta Q_{ij} + \phi(Q_{ij} \times S_{ij}) + \lambda F_{ij} + \mu(F_{ij} \times S_{ij}) + \mathbf{x}'_{ijv}\boldsymbol{\gamma} + \varepsilon_{ijv}, \qquad (1)$$

where the dependent variable P_{ijv} is equal one when the legislator representing district i of province j adopts a pro-abolition stance on vote v (and $P_{ijv} = 0$ otherwise). The main variables of interest are Q_{ij} and F_{ij}. Q_{ij} captures the 'proximity to freedom' in district i, a measure of outside options for the enslaved population. F_{ij} captures the share of foreigners in district i in 1890, a measure of the availability of immigrant labor during the abolition period. Both of these variables are interacted with S_{ij}, the share of enslaved individuals in district i in 1872. We are mainly interested in the signs and magnitudes of ϕ and μ: conditionally on slavery intensity, these respectively capture

the influence of outside options and availability of immigrant labor on a legislator's decision to support or oppose emancipation.

Our favorite measures of proximity to freedom Q_{ij} are built as a function of the location and size of quilombos. Our preferred measures are *i)* the geographic distance to the closest quilombo (the district average of the distance from each *município*'s head town to the closest quilombo), and *ii)* the spatial extent of lands controlled by maroons (quilombolas).[7] We also consider alternative measures, in particular distances to varying numbers of quilombos, or the number of quilombos within a district. The presence of quilombos not only suggests that some runaways successfully joined them but also reflects local geographic and social conditions that facilitated durable escapes. In this sense, quilombos serve as a proxy for the likelihood of successful escapes in a given area.

We capture variation in the scarcity of labor with the share of immigrants F_{ij} in 1890. There had been efforts to attract immigrant labor since the 1830s, but these efforts only started to pay off in the second half of the 1880s, with immigration flows increasing severalfold then. A large fraction of these were destined to supply labor to Southeastern coffee plantations. We have only two snapshots of the immigrant population, in the 1872 and 1890 censuses. Although the second snapshot is taken as of 1890—after abolition—it plausibly captures variation in immigrant labor availability during the 1884–88 voting period, since immigration patterns observed in 1890 largely reflect settlement and labor market conditions established in the preceding years (Conrad, 1972).

A challenge in our context is that, given existing data, our independent variables of interest only vary cross-sectionally. Equation 1 therefore cannot include district fixed effects. Instead, α_{jv} denotes province-by-vote fixed effects (20 provinces by 13 votes). In turn, $\mathbf{x}_{ijv}$ includes two distinct vectors of controls. The first is a vector of cross-sectional controls interacted with vote fixed effects to flexibly allow geographic and demographic covariates to influence different votes differently. This vector notably includes: the share of enslaved workers in a district's population in 1872, average soil

[7] This measure effectively captures two dimensions of enslaved individuals' outside options. First, the total area of maroon-controlled territory naturally increases with the number of quilombos, which captures an extensive margin: more quilombos nearby implies a better escape success rate. At the same time, this measure accounts for the average size of quilombos, which captures an intensive margin: larger quilombos within a district likely reflects greater safety, stability, and resource availability (as well as better defenses and military strength).

suitability to coffee, sugar and cotton, average rainfall, longitude and latitude, distance to the coast and to the closest river, average Human Mobility Index, population density, share of free foreigners in 1872, share of free colored in 1872, and literacy rate in 1872. Second, $\mathbf{x}_{ijv}$ also includes a vector of legislator controls (which, therefore, vary across legislatures). This includes a number of dummies for: legislator reelection status (whether they previously served as legislator), other government appointment (whether they simultaneously hold another office), and occupation/education (law, medicine, science, civil service, business, priesthood, and military). In baseline regressions, we report both two-way clustered standard errors by district and vote (allowing observations to be serially correlated over votes within districts, and spatially correlated across districts within votes), and spatial HAC standard errors (Conley, 1999, 2010; Bester et al., 2011) allowing both serial and spatial correlation within 250 kilometers radii. Results are robust to other variance-covariance matrix assumptions, in particular varying the radius of the spatial kernel from 50 to 2500 kilometers and one-way clustering.

3.2 Baseline results

Table 2 presents the results of estimating equation 1, measuring the effects of proximity to freedom and 1890 immigration on legislators voting decisions.[8] Panel A uses the distance to the closest quilombo as a measure of proximity to freedom, whereas Panel B uses the size of land occupied by quilombos within a district. The different columns add controls progressively. Column 1 includes province-by-vote fixed effects and controls for the share of enslaved workers interacted with vote fixed effects. Column 2 adds geographic controls interacted with vote fixed effects (soil suitability to coffee, cotton and sugarcane, average rainfall, longitude and latitude coordinates, distance to the coast and to the closest river, and average Human Mobility Index). Column 3 adds demographic controls interacted with vote fixed effects (population density, share of foreigners, share of literates, and share of free colored). Finally, column 4 adds (time-varying) legislator controls (reelection status, other government appointment, and occupation/education dummies).

In slavery-intensive areas, a higher cost of enforcing slavery (better outside options for the workers) is associated with lower support for slavery, in line with an *outside*

[8] In this section, we focus on heterogeneous effects. We discuss analogous regressions excluding interaction terms later in the text and in Appendix A.4 Table A.16.

Table 2: Outside option and labor demand effects – baseline OLS

	𝟙(Abolition vote)			
	(1)	(2)	(3)	(4)
Panel A: Outside option is distance to closest quilombo				
Dist. to closest quilombo	0.0294	0.0328	0.0322	0.0310
	(0.0107)***	(0.0124)***	(0.0124)***	(0.0117)***
	{0.0088}***	{0.0099}***	{0.00913}***	{0.0091}***
Dist. to closest quilombo × Share of enslaved	-0.234	-0.245	-0.246	-0.251
	(0.0708)***	(0.0724)***	(0.0738)***	(0.0701)***
	{0.0538}***	{0.0553}***	{0.0541}***	{0.0546}***
1890 share of foreigners	-8.493	-8.098	-6.296	-5.097
	(3.197)***	(3.414)**	(3.487)*	(3.495)
	{2.208}***	{2.460}***	{2.549}**	{2.511}**
1890 share of foreigners × Share of enslaved	48.06	48.43	44.60	35.85
	(16.03)***	(17.26)***	(17.21)***	(18.45)*
	{11.63}***	{12.41}***	{13.88}***	{14.46}**
N	1284	1284	1284	1284
Adj. R^2	0.259	0.246	0.239	0.260
Panel B: Outside option is area occupied by quilombos				
Quilombos occupied area	-0.00151	-0.00152	-0.00223	-0.00213
	(0.000999)	(0.000996)	(0.00116)*	(0.00123)*
	{0.00079}*	{0.000868}*	{0.000943}**	{0.00100}**
Quilombos occupied area × Slavery prevalence	0.0184	0.0180	0.0202	0.0205
	(0.00568)***	(0.00643)***	(0.00759)***	(0.00837)**
	{0.004014}***	{0.00530}***	{0.00592}***	{0.00645}***
1890 share of foreigners	-6.250	-6.428	-4.424	-3.214
	(3.202)*	(3.393)*	(3.516)	(3.489)
	{2.378}***	{2.689}**	{2.767}	{2.827}
1890 share of foreigners × Slavery prevalence	39.78	42.14	37.12	29.51
	(16.64)**	(17.80)**	(18.44)**	(19.48)
	{12.64}***	{13.91}***	{15.80}**	{16.72}*
N	1263	1263	1263	1263
Adj. R^2	0.255	0.255	0.236	0.264
Mean dep. var	0.56	0.56	0.56	0.56
Share of enslaved × Vote FE	✓	✓	✓	✓
Province × Vote FE	✓	✓	✓	✓
Geographic controls × Vote FE	✗	✓	✓	✓
Demographic controls × Vote FE	✗	✗	✓	✓
Legislator controls	✗	✗	✗	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses, and spatial HAC standard errors (with a 250km window) are reported in curly brackets. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE.

option effect. Similarly, higher numbers of immigrant workers in 1890 are associated with lower support for slavery, in line with our *labor demand effect* (controlling for 1872 immigration). The results for proximity to freedom are statistically significant for both measures (Panels A and B) and across all specifications (columns 1 to 4). The results for immigrant arrivals are also statistically significant in both panels, although less precisely estimated when legislator controls are added. Figure A.8 in

Appendix A.4 illustrates the average marginal effect of proximity to freedom and immigration for varying levels of slavery intensity, using the estimates in column 4. In each case, the effect is near zero for low levels of slavery, and increases in magnitude as the share of enslaved workers within a district increases. In the average district of the Sudeste region (the coffee growing region, with an average 22.6% enslaved workers in 1872), a one percentage point decrease in distance to the closest quilombo is associated with a $-[0.0310 + (-0.251) \times 0.226] = 0.026$ increase in the likelihood that a district's representative votes in favor of emancipation, a 4.7% increase from the full sample mean (or 6.1% from the Sudeste mean). Similarly, a one percent increase in the share of immigrants in 1890 is associated with a $[(-5.097) + 35.85 \times 0.226]/100 = 0.030$ increase in the likelihood that a district's representative votes in favor of emancipation, a 5.37% increase from the full sample mean (or 7% from the Sudeste mean).[9]

Control variables.—As the interpretation of control variables may be of intrinsic interest, Tables A.7 and A.8 in Appendix A.4 respectively present the results of regressions analogous to Panels A and B of Table 2 but without interacting controls with vote fixed effects. Across all specifications, slavery intensity is the major driver of voting decisions, as well as, within slavery-intensive areas, proximity to freedom and immigrant arrivals. Geographic and demographic covariates are consistently statistically insignificant, to the exception of the coefficient associated with longitude coordinates in Table A.7, which is positive and marginally significant when all other covariates are added. Turning to legislator controls, certain occupations appear to significantly predict voting behavior. Legislators classified as owners and businessmen by Nogueira and Firmo (1973) are significantly (albeit not very strongly, p-value of 0.086, see Table A.7) less likely to support emancipation. This effect is sizeable, as

[9]The regression analogous to the one presented in Panel A column 4 but with distance to the closest quilombo not being logged yields 0.00270 (standard error 0.00113) for the uninteracted term and −0.0276 (standard error 0.0106) for the interaction. A 1km decrease in the distance to the closest quilombos induces a $-[0.0027+(-0.0276)\times 0.226] = 0.0035$ increase in the likelihood to vote in favor of emancipation. Therefore, a standard deviation increase in the distance to the closest quilombo translates into a $28.9 \times 0.0035 = 0.10115$ increase in the likelihood to vote in favor of emancipation (18.0625 change from the sample mean). Alternatively, according to Panel B, a square kilometer increase in the land occupied by quilombos is associated with a $(-0.00213)+0.0205\times 0.226 = 0.0025$ increase in the likelihood that a legislator votes in favor of emancipation. A standard deviation increase of 44.62 square kilometers therefore translates into a 0.11 change. Comparatively, a standard deviation increase in the share of immigrants within a district in 1890 translates into a $0.030\times 3.95 = 0.119$ increase in the likelihood that a legislator votes in favor of emancipation.

the probability that a district's representative votes in favor of abolition decreases by 0.24 when a legislator is classified as an owner/businessman, a 42.9% decrease from the sample mean. Legislators classified as priests and military are also respectively more (0.27) and less (−0.62) likely to support emancipation, but only 0.95% and 0.25% legislators are reported to be priests and military respectively.

3.3 Robustness checks

To interpret equation 1 as identifying the causal effects of proximity to freedom and immigrant arrivals on voting decisions, the two main concerns are that, within province-by-votes and conditioning on controls, *i)* there are no omitted variables, such as within province variation in norms and culture, driving both the existence of quilombos and legislators' willingness to support abolition; and *ii)* immigrants did not self-select into their district of destination based on that district's emancipation prospects. Although these assumptions are not directly testable, several validation exercises and robustness checks support a causal interpretation of the baseline estimates. Before turning to instrumenting immigration and quilombo location in section 4, we first conduct a range of robustness tests to assess the credibility of our identifying assumptions.

Table 3 reports some of these checks. First, we estimate a version of eq. 1 without interacting Q_{ij} and F_{ij} with S_{ij}, but considering only the Sudeste (column 1) and the Nordeste regions (column 2). Second, in column 3, we consider a specification in which S_{ij} is replaced with a time-varying proxy for the local importance of slavery, which allows to introduce district fixed effects. Finally, in columns 4 and 5, we assess the possibility that our results are driven by omitted variable bias. We detail each of these robustness tests below.

Nonparametric heterogeneity.—Specification 1 captures the influence of proximity to freedom and immigrant arrivals within slavery-intensive areas by interacting Q_{ij} and F_{ij} with S_{ij}, the share of enslaved workers in 1872. An alternative is instead to split the sample and directly focus on the slavery-intensive region. In column 1 of Table 3, we run specification 1 without the interaction term on a subsample comprised only of the Sudeste (provinces of Espírito Santo, Minas Gerais, São Paulo, and Rio de Janeiro). Based on Panel A, within the Sudeste, a percentage point decrease in distance to the closest quilombo is associated with a 0.024 increase in

Table 3: Outside option and labor demand effects – baseline robustness

	𝟙(Abolition vote)				
	Sudeste only	Nordeste only	W/in district	AET test	Oster test
	(1)	(2)	(3)	(4)	(5)
Panel A: Outside option is distance to closest quilombo					
Dist. to closest quilombo	-0.0236***	-0.0070			
	(0.0083)	(0.0094)			
1890 share of foreigners	2.362*	0.0687			
	(1.396)	(5.467)			
Dist. to closest quilombo × Owner/Businessman			-2.368***		
			{0.194}		
1890 share of foreigners × Owner/Businessman			142.68***		
			{20.75}		
Predicted dist. to closest quilombo				-0.00846	
				{0.0169}	
Predicted dist. to closest quilombo × Share of enslaved				0.0612	
				{0.0817}	
Predicted 1890 share of foreigners				-2.081	
				{2.398}	
Predicted 1890 share of foreigners × Share of enslaved				16.79	
				{11.39}	
δ for outside option interaction = 0					1114.9%
δ for labor demand interaction = 0					235.4%
N	453	632	1284	1284	1284
Panel B: Outside option is area occupied by quilombos					
Quilombos occupied area	0.00287**	0.0010			
	(0.00140)	(0.0013)			
1890 share of foreigners	3.756***	0.3162			
	(1.012)	(5.4827)			
Quilombos occupied area × Owner/Businessman			0.0913***		
			{0.0115}		
1890 share of foreigners × Owner/Businessman			100.44***		
			{17.87}		
Predicted quilombos occupied area				-0.00060	
				{0.00137}	
Predicted quilombos occupied area × Share of enslaved				0.0147	
				{0.0095}	
Predicted 1890 share of foreigners				-2.032	
				{2.489}	
Predicted 1890 share of foreigners × Share of enslaved				17.04	
				{13.09}	
δ for outside option interaction = 0					1042.0%
δ for labor demand interaction = 0					245.9%
N	453	632	1263	1263	1263
Mean dep. var	0.43	0.59	0.56	0.56	0.56
Province × Vote FE	✓	✓	✗	✓	✓
Vote FE	✗	✗	✓	✗	✗
District FE	✗	✗	✓	✗	✗
Legislator controls	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✗	✓	✓
Demographic controls × Vote FE	✓	✓	✗	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered at the district level are reported in parentheses, and district-vote two-way clustered standard errors are reported in curly brackets. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: share of enslaved population, population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects.

the likelihood that a representative votes in favor of emancipation, a 5.6% increase from the subsample mean. A percentage point increase in the 1890 share of foreigners

(holding constant that of 1872) is similarly associated with a 0.024 increase in the probability that a representative votes in favor of emancipation.

In column 2, we replicate this exercise on the Nordeste (Northeast, comprised of the provinces of Alagoas, Bahia, Ceará, Maranhão, Paraíba, Piauí, Pernambuco, Rio Grande do Norte and Sergipe), where slavery had been all but effectively abolished several years before emancipation. As expected, neither proximity to freedom nor 1890 immigration are statistically significant predictors of voting decisions there. The results presented in Panel B, where we use our second measure of proximity to freedom, are qualitatively identical.

Within-district variation.—In specification 1, unobserved heterogeneity across districts is likely captured to a large extent by province-by-vote fixed effects. However, some residual variation may still correlate with Q_{ij} and/or F_{ij} within provinces and voting periods, such as local differences in cultural and normative attitudes toward emancipation, or pull factors driving endogenous location choices. A wide range of controls further helps capture this variation in equation 1, but some concerns may persist. Instead of interacting Q_{ij} and F_{ij} with S_{ij} (which only varies cross-sectionally) as in specification 1, in column 3 of Table 3 we estimate the following specification

$$P_{iv} = \beta_i + \delta_v + \zeta(Q_i \times L_{iv}) + \eta(F_i \times L_{iv}) + \mathbf{w}'_{iv}\boldsymbol{\xi} + \epsilon_{iv}, \tag{2}$$

where L_{iv} is an indicator variable switching on if the legislator representing district i during vote v is described as an owner or businessman by Nogueira and Firmo (1973). This allows us to introduce district fixed effects. The logic of equation 2 mirrors that of specification 1, with L_{iv} a time-varying proxy for slavery-intensity—since slavery-intensive districts are more likely to elect plantation owners and businessmen as legislators. While only 52 legislators are reported to be owners/businessmen over the period, they are significantly more likely to oppose emancipation bills: owners/businessmen voted in favor of emancipation in only about 22% of cases, as opposed to 56% for the full sample. We find that a newly elected owner/businessman is comparatively less reluctant to vote for emancipation when maintaining slavery is costly (when the enslaved have better outside options as evidenced by the presence and size of quilombos) and when his district can more easily move away from enslaved labor (and to immigrant labor). This suggests that our baseline results cannot be explained merely by unobserved variation across districts, another step in the direction

of interpreting the effects as causal.

Regression sensitivity and selection on unobservable variation.—In columns 4 and 5 of Table 3, we also investigate whether unobserved variation is likely to drive the results. Column 4 presents the results of a test à la Altonji et al. (2005). We first regress Q_{ij} and F_{ij}, our measures of proximity to freedom and 1890 immigration, on all observables and fixed effects (province-by-vote fixed effects, legislators characteristics, and vote dummies interacted with soil suitability measures, rainfall, geographical coordinates, remoteness measures, HMI, population density, and 1872 shares of foreigners, free colored, and literates) and gather predicted values $\widehat{Q}_{ij}^{O}$ and $\widehat{F}_{ij}^{O}$ (where the O superscript indicates prediction from observables). We then regress voting decisions on $\widehat{Q}_{ij}^{O}$ and $\widehat{F}_{ij}^{O}$, as well as province-by-vote fixed effects. If the results presented in Table 2 are indeed not driven by unobservable variation, and under the assumption that observables are informative about unobservables, one would expect that $\widehat{Q}_{ij}^{O}$ and $\widehat{F}_{ij}^{O}$ do not significantly predict voting decisions. We find that this is indeed the case: predicted-from-observables proximity to freedom and 1890 immigration do not predict voting decisions.

In column 5 of Table 3, we report Oster's (2019) δ, the value of the relative degree of selection on unobserved versus observed variables that would explain away the results (expressed as a percentage). Following Oster (2019), we set the value of $R_{\max}$, the theoretical coefficient of determination of a regression of voting decisions on the full vector of observables and unobservables, at $1.3\tilde{R}$ (the bounding value provided by randomized control trials), where $\tilde{R}$ is the R-squared from the regression with controls. We find that in each case, δ would need to be much larger than one for the identified set to include zero. This implies that selection on unobservable variation would need to be implausibly strong for omitted variables to explain the results.

Alternative measures of proximity to freedom, outliers and nonlinearities.—In Appendix A.4 Table A.10, we extend our baseline measure of distance to freedom by considering the distance to the K closest quilombos, with $K \in \{1; 9\}$. These results are illustrated in Figure A.9. The magnitude of the estimates increases with K, suggesting that a greater number of quilombos captures better prospects of freedom. As K increases, a decrease in proximity to freedom has a larger effect on the likelihood that a representative votes in favor of emancipation, holding slavery-intensity constant. Alternatively, maintaining distance constant, the promise of freedom is greater when success rate is higher, as captured by the number of quilombos.

Moreover, figures A.10, A.11, A.12, and A.13 in Appendix A.4 show that the results are robust to inclusion or exclusion of extreme values in immigration, slavery and quilombos, to modeling nonlinearities and to various other measures of outside options (number of quilombos, complete cases of areas and distances with or without logs).

Other checks.—In Table A.9, we confirm that our results hold under a probit model instead of a linear probability model. Figure A.14 in Appendix A.4 further illustrates the consistency of the estimates by separately estimating a version of equation 1 for each of the thirteen roll calls, showing broadly similar coefficients across votes.

Table A.11 addresses potential selection concerns, particularly strategic absenteeism, by verifying that results hold when accounting for absences and alternative interpretations of voting behavior. We also ensure robustness by flexibly controlling for a district area polynomial to rule out boundary-driven effects and by testing the sensitivity of interacted model estimates to omitted variable bias under conditional exogeneity following Feigenberg et al. (2023).

In Tables A.12 and A.13, we examine alternative mechanisms and potential confounders, including the influence of capitalists and industrialists, insurrections, abolitionist and anti-abolitionist organizations, newspapers, and quilombo destruction. Our findings suggest that, while a higher share of capitalists in a district's population in 1872 correlates negatively with pro-emancipation voting, the presence of industrialists within slavery-intensive areas is positively associated with abolitionist support—consistent with a capital-skill complementarity argument (Ashraf et al., 2025) and evidence from Britain on industrialists' relative detachment from slavery-derived income (Figueroa and Fouka, 2023). Moreover, using data on destroyed quilombos in Goiás, Mato Grosso, Minas Gerais, and São Paulo (Guimarães, 1996 and Silva, 2003), we find that, within slavery-intensive areas, a higher number of destroyed quilombos is positively associated with pro-emancipation votes, suggesting that direct conflict with quilombolas may have influenced the perceived cost of enforcing slavery.

Finally, Tables A.14 and A.15 assess whether the influence of 1890 immigration on voting decisions merely reflects cultural transmission rather than substitution possibilities. Tables A.7 and A.8 show that immigration prior to 1872 is negatively but insignificantly associated with abolitionist voting, making a purely cultural influence explanation unlikely. Additionally, given that most 1890 immigrants had arrived only in the preceding years and were largely disenfranchised, their direct political influ-

ence would have been minimal. Our estimates remain robust when controlling for key cultural determinants, including the shares of foreigners from main sender countries in 1872, state-sponsored settlements by origin between 1750 and 1870, and minority religion shares in 1890.

4 Identification

In the previous section, we have taken several steps to address the concern that an omitted variable, such as a culture of progressiveness or a 'taste for freedom,' could be endogenously driving the location of quilombos and immigrants. To further assess whether our results can be interpreted as causal, we use *i)* the existence of a substitution effect between topographic determinants that predict the location and size of quilombos – hereafter SubIV, and *ii)* heterogeneous exposure to common immigration shocks – hereafter SSIV. These respectively provide exogenous variation in proximity to freedom, Q_{ij}, and 1890 immigration, F_{ij}.

4.1 Instrumenting quilombos and immigration

Specification 1 includes four endogenous variables, as the interaction terms $(Q_{ij} \times S_{ij})$ and $(F_{ij} \times S_{ij})$ also need to be accounted for. Natural – and, under some conditions, optimal – IVs for these interactions are respectively provided by interacting S_{ij} with SubIV and SSIV.[10] The first stages of the TSLS counterpart of equation 1 are therefore

[10] Another option is to run 'zero stage' regressions of each original endogenous regressor (Q_{ij} and F_{ij}) on their respective instrument with no exclusion restrictions, and then use the resulting fitted values interacted with slavery as instruments for the interaction terms (instruments and not regressors, as that would yield the infamous 'forbidden regression'; see e.g. Wooldridge (2010)). This yields qualitatively identical results (available upon request).

as follows:

$$
\begin{aligned}
Q_{ij} &= o_{1jv} + \iota_1 \text{SubIV}_{ij} + \kappa_1(\text{SubIV}_{ij} \times S_{ij}) + \pi_1 \text{SSIV}_{ij} + \rho_1(\text{SSIV}_{ij} \times S_{ij}) \\
&\quad + \mathbf{x}'_{ijv}\boldsymbol{\tau_1} + \nu_{1ijv}, \\
(Q_{ij} \times S_{ij}) &= o_{2jv} + \iota_2 \text{SubIV}_{ij} + \kappa_2(\text{SubIV}_{ij} \times S_{ij}) + \pi_2 \text{SSIV}_{ij} + \rho_2(\text{SSIV}_{ij} \times S_{ij}) \\
&\quad + \mathbf{x}'_{ijv}\boldsymbol{\tau_2} + \nu_{2ijv}, \\
F_{ij} &= o_{3jv} + \iota_3 \text{SubIV}_{ij} + \kappa_3(\text{SubIV}_{ij} \times S_{ij}) + \pi_3 \text{SSIV}_{ij} + \rho_3(\text{SSIV}_{ij} \times S_{ij}) \\
&\quad + \mathbf{x}'_{ijv}\boldsymbol{\tau_3} + \nu_{3ijv}, \\
(F_{ij} \times S_{ij}) &= o_{4jv} + \iota_4 \text{SubIV}_{ij} + \kappa_4(\text{SubIV}_{ij} \times S_{ij}) + \pi_4 \text{SSIV}_{ij} + \rho_4(\text{SSIV}_{ij} \times S_{ij}) \\
&\quad + \mathbf{x}'_{ijv}\boldsymbol{\tau_4} + \nu_{4ijv},
\end{aligned}
$$

where we include cross-sectional controls interacted with vote fixed effects, time-varying controls, and province-by-vote fixed effects as before. We also extend the list of covariates as described below. The second stage is estimated with the fitted values $\widehat{Q_{ij}}$, $\widehat{(Q_{ij} \times S_{ij})}$, $\widehat{F_{ij}}$, and $\widehat{(F_{ij} \times S_{ij})}$ replacing their counterparts in equation 1.

Substitution instrument.—Two topographic and geographic features strongly predict the existence and size of surviving quilombos: ruggedness and remoteness. There is little doubt that the establishment of quilombos was driven by considerations of security and remoteness (Pardelli and Kustov, 2022). Nunn and Puga (2012) showed that in Africa, terrain ruggedness discouraged slave trades and facilitated escapes. In Brazil, a permanent escape depended on "the existence of dense forests or inaccessible mountains within a short distance from their [the runaways'] homes" (Klein and Luna, 2009, p. 195). Remoteness also made repression harder, and the establishment of successful (and long-lasting) quilombos more likely. However, ruggedness and remoteness are substitutes: in the proximity of large settlement areas, a rugged terrain was critical to provide a defensive advantage and allow to remain hidden. But at a safe distance from settlements, a rugged terrain was unnecessary, as a community's isolation already ensured its safety. Our instrument leverages this substitution effect, *controlling for actual levels of ruggedness and remoteness*. We define SubIV_{ij} as follows.

$$\text{SubIV}_{ij} = \text{Ruggedness}_{ij} \times [1 - \text{Remoteness}_{ij}], \tag{3}$$

where Ruggedness_{ij} and Remoteness_{ij} respectively denote the min-max normalized topographic ruggedness index (TRI_{ij}) and the min-max normalized average travel

time (ATT_{ij}) to the closest provincial capital (based on the Human Mobility Index and computed using a least-cost path algorithm), i.e.

$$\text{Ruggedness}_{ij} = [\text{TRI}_{ij} - min(\text{TRI})]/[max(\text{TRI}) - min(\text{TRI})], \text{ and}$$
$$\text{Remoteness}_{ij} = [\text{ATT}_{ij} - min(\text{ATT})]/[max(\text{ATT}) - min(\text{ATT})].$$

The intuition behind this instrument is illustrated in Figure 3. Within highly rugged districts, more numerous and/or larger quilombos are found when remoteness is relatively low (left panel). Conversely (right panel), within highly remote districts, more numerous and/or larger quilombos are found when ruggedness is relatively low. A possible concern is that ruggedness has been found to be negatively associated with economic outcomes at the country level, as it may increase transport costs and makes trading more difficult (Nunn and Puga, 2012). To bypass this issue, we control for levels of ruggedness and ruggedness to only exploit variation in the substitution effect between the two. We interact levels with vote fixed effects to allow them to vary from vote to vote. We also interact them with the share of enslaved population (S_{ij}) to make sure that our instruments are not picking up topographic or geographic correlates of slavery. The identification assumption is therefore that, conditional on the levels of ruggedness and remoteness and all other covariates and fixed effects, the substitution effect between ruggedness and remoteness (i.e. how much the effect of ruggedness [remoteness] decreases as remoteness [ruggedness] increases) is orthogonal to omitted variables influencing voting decisions. Appendix A.2 discusses this instrument's intuition in more detail. In alternative specifications, we also ensure that the instrument is not picking up nonlinearities in ruggedness and remoteness by flexibly controlling for polynomials in both variables.

Shift-share instrument.—We build two different shift-share instruments: a conventional immigration SSIV, and a religion-based leave-out SSIV. A difficulty specific to our context is that the 1890 census does not provide immigration data disaggregated by nationality. It does, however, provide detailed information on religion (eight religions reported), which we leverage in the religion-based SSIV. The 1872 census, in turn, provides detailed nationality information that is also disaggregated between catholics and noncatholics (that is, for each of the twenty-six nationalities represented, both the catholic and noncatholic populations are reported).

To construct the origin country SSIV, we collect data on the aggregate number

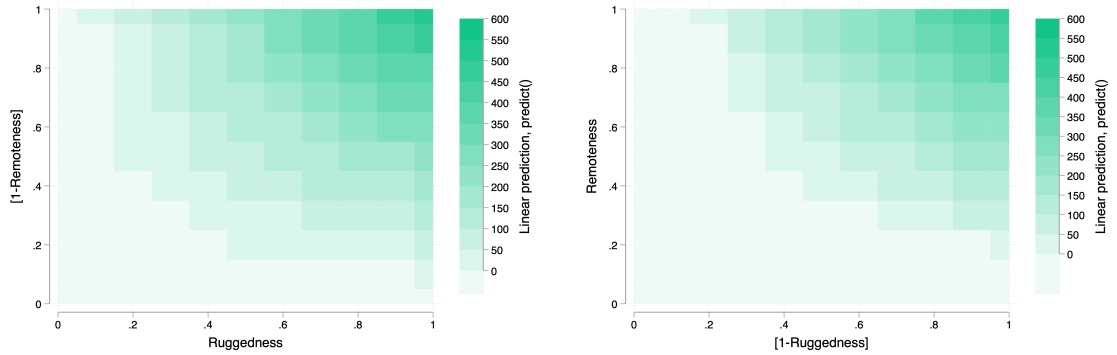


Figure 3: Heatmaps – substitution between ruggedness and remoteness

Notes: Linear predictions of the area occupied by quilombos based on the substitution instrument. This figure shows the margins from linear regressions of Q_{ij} (in this case, the size of the lands occupied by quilombos) on either $\text{Ruggedness}_{ij} \times [1 - \text{Remoteness}_{ij}]$ (left panel) or $\text{Remoteness}_{ij} \times [1 - \text{Ruggedness}_{ij}]$ (right panel), controlling for Remoteness_{ij}, Ruggedness_{ij}, interactions of Remoteness_{ij} and Ruggedness_{ij} with S_{ij}, as well as all other controls and fixed effects in equation 1.

of foreigners that entered the country between 1884 and 1893 from IBGE (1996). This provides inflows disaggregated according to twenty-two origins and an 'others' residual category, which we match with the twenty-six origins accounted for in the 1872 census. This yields the conventional shift-share:

$$\text{SSIV}_{ij} = \sum_{o} \alpha_{ijo1872} I_{o'84-93}, \tag{4}$$

where $\alpha_{ijo1872}$ is the share of foreigners from origin o in district i in 1872, and $I_{o'84-93}$ is the inflow of immigrants from origin o that entered Brazil between 1884 and 1893. A drawback of merely observing foreigners' arrival by origin country at the aggregate level is that it precludes employing a leave-out strategy in the construction of the shocks. Excluding the contribution of a unit in the construction of its shock can improve identification notably by preempting reverse causation.

As an alternative to the origin country SSIV, we build an origin religion SSIV, employing the rich religion information provided in the 1890 census. We hypothesize that chain migration is likely to occur along religious lines, just as immigrants are more likely to settle where other migrants originating from the same location have previously settled. Formally, the baseline version of this instrument is constructed as follows

$$\text{SSIV}_{ij} = \sum_r \widehat{\alpha}_{ijr1872} \widehat{I}^{-i}_{r1890}, \tag{5}$$

where $\widehat{\alpha}_{ijr1872}$ is the predicted share of foreigners of religion r in district i (in province j) in 1872, constructed by matching catholic and non-catholic foreign populations by nationality from the 1872 census with the corresponding dominant (catholic and noncatholic) religions in each origin country. Specifically, $\widehat{\alpha}_{ijr1872} = \frac{\sum_o \widehat{n}_{ijor}}{\sum_o \sum_i \widehat{n}_{ijor}}$, where $\widehat{n}_{ijor}$ is the (predicted) number of foreigners from origin o of religion r in district i in 1872.[11] In turn, $\widehat{I}^{-i}_{r1890}$ is the predicted total number of immigrants of religion r that entered the country between 1872 and 1890, net of those that settled in district i. To construct this, we use the empirical regularity that the native population in 1890 was largely Roman Catholic.[12] For simplicity, in baseline specifications we approximate the number of immigrants of religion r in district i by the number of individuals of religion r in district i for every religion except Catholicism, and get the number of catholic foreigners by taking the difference between the number of catholics and the native population in each district. This can also be constructed leaving out provinces rather than districts, with $\widehat{I}^{-j}_{r1890}$ the predicted inflow of immigrants that practice religion r, net of those that settled in province j. Finally, the instrument can be normalized by P_{ij1890}, the 1890 population of district i, or $\widehat{P}_{ij1890}$, the predicted 1890 population of district i, constructed using 1872 population scaled with a predicted national population growth rate leaving out district i (or, alternatively, province j). Appendix A.3 discusses the construction of this instrument and the intuition underlying it in more detail.

A sufficient condition for the validity of SSIV_{ij} is that, conditional on fixed effects and controls—including the 1872 share of foreigners by district—the foreign

[11] A possible refinement of the matching procedure could instead be based on assigning 1872 foreigners to *minority* religions in origin countries, on the grounds that individuals that practice minority religions may be more likely to emigrate. In practice, this would not significantly change the instrument, as having two religions per origin typically already covers the main minority religion in each origin country.

[12] Roman Catholicism held a hegemonic position during the period, losing terrain only slowly and mostly after the advent of the First Republic. Article 5 of the 1824 constitution established Roman Catholicism as the state religion of the Empire of Brazil. Although Protestantism was introduced in the country only shortly after Independence, with the establishment of Swiss and German colonial settlements, these first waves of Protestant immigration remained both culturally and geographically isolated, and did not threaten the position of the Church (Mendonça, 2003). North-American missions posed a larger threat, with only relative success initially, although increasingly toward the end of the period.

composition of districts by origin country/religion in 1872 is orthogonal to omitted variables driving 1884-1888 votes on abolition-related bills. In other words, within province-by-votes and maintaining constant the 1872 level of immigration in each district (among other controls), SSIV_{ij} constitutes a valid instrument if the influence of a district having a higher share of immigrants of a given origin country (or from a given origin religion) instead of another in 1872—i.e. the differential exposure effect, but not the variation in initial immigration levels—only influences voting decisions by how it influenced subsequent immigration (Goldsmith-Pinkham et al., 2020).

To assess the plausibility of this identifying assumption, we *i)* evaluate whether geographic and 1872 economic and demographic covariates are significant predictors of country/religion shares and establish robustness to excluding origin countries/religions with the largest Rotemberg weights, following Goldsmith-Pinkham et al. (2020), *ii)* verify that results are robust to using origin country/religion shares as separate instruments (running overidentification tests), including with LIML and bias-corrected TSLS estimators, and *iii)* consider various alternative approaches to the construction of both instrument versions and ensure that they all yield similar estimates. For the religion-based SSIV, we also consider alternative approaches to match origins with religions. Under the exogenous shares view, this simply comes down to selecting a different GMM weight matrix, and does not alter the identification assumption. We find that our different matching approaches yield qualitatively identical results. Should the assumption of exogeneity of country/religion shares fail, however, the instrument can also be valid if aggregate shocks are exogenous to destination districts' prospects on emancipation votes (Borusyak et al., 2022). To account for possible pull factors for specific origin countries/religions in destination districts, we extend our baseline vector of controls to including the number of colonial settlements by country and district.

4.2 Piecewise instrumentation and discussion

We start with specifications analogous to the OLS estimates presented in Panel B of Table 2. Baseline TSLS estimates are reported in Table 4. Panel A presents the results of a specification where proximity to freedom (here, the land area occupied by quilombos within a district) and its interaction with the share of enslaved workers are instrumented with SubIV_{ij} and $\text{SubIV}_{ij} \times S_{ij}$. First stages are reported in columns

1 and 2, and the second stage is reported in column 3. In turn, Panel B reports the results of a specification where immigration and its interaction with the share of enslaved workers are instrumented with SSIV_{ij} and $\text{SSIV}_{ij} \times S_{ij}$ (here, SSIV_{ij} is the origin country-based unscaled shift-share defined in equation 4). First stages are reported in columns 4 and 5, and the second stage is reported in column 6.

Second stage estimates are quite similar to their OLS counterparts; slightly larger, but also (unsurprisingly) slightly less precisely estimated. According to Panel A, in an average district of the Sudeste region, a standard deviation increase in the land occupied by quilombos is associated with a $44.62 \times [(-0.00536) + 0.0428 \times 0.226] = 0.19$ increase in the likelihood that a legislator votes in favor of emancipation (compared to 0.11 for OLS estimates). Similarly, according to Panel B, a standard deviation increase in the share of immigrants within an equivalent district translates into a $3.95 \times [(-6.724) + 50.71 \times 0.226]/100 = 0.19$ increase in the likelihood that a legislator votes in favor of emancipation (compared to 0.12 for OLS estimates).

In both panels, Sanderson and Windmeijer (2016) F-statistics, appropriate for setups with multiple endogenous variables, are consistently above conventional thresholds. In both panels, endogeneity tests are unable to reject the null hypothesis that TSLS and OLS estimates are equal, so that one might prefer the latter for efficiency reasons. Moreover, in both cases, weak IV-robust Anderson-Rubin intervals on the coefficient associated with the interaction exclude zero (and include OLS estimates).[13] Overall, the evidence reported here is strongly consistent with a causal interpretation of the influence of proximity to freedom and immigration on legislators voting decisions, in line with the coexistence of the outside option and labor demand effects.

[13]Given that endogeneity tests fail to reject the null, and given the size of standard errors, we believe that one should not over-interpret the size difference between OLS and TSLS estimates. Nonetheless, this difference can also be easily explained by heterogeneous treatment effects (Bisin and Moro, 2021). To see this, recall that the ban of the Atlantic slave trade in 1851 induced an intensive inter-provincial slave trade, which was still ongoing when we observe the prevalence of slavery in 1872. This implies that some (mostly northern) areas continued to have their captive population depleted after 1872, and conversely for some (mostly south-eastern) areas. Keeping constant the 1872 prevalence of slavery, some districts had much less to lose from emancipation than others, simply because their reliance on captive workers continued to decrease until the abolition period in the 1880s. Therefore, 'take-up' of part of 'the treatment' – i.e. acquisitions and transfers of enslaved individuals – continued after we actually observe slavery intensity. Take-up is likely to have evolved differently in different locations based on the profitability of slavery (as translated by the inter-provincial slave trade), and the treatment effect is likely to be higher in places where the returns to slavery were higher and slaveholders had more to lose. In this context, it is therefore unsurprising for an instrument capturing the effect of treatment on compliers to predict a relatively larger impact.

Table 4: Substitution and shift-share instruments introduced separately

	Panel A: instrument is Quilombos area [*Instrument*]: SubIV_{ij}			*Panel B: instrument is 1890 share of foreigners* [*Instrument*]: SSIV_{ij}		
	1st stage		2nd stage	1st stage		2nd stage
	Quil. area	Quil. area × Sh. of enslaved	𝟙(Abo. vote)	Sh. foreigners	Sh. foreigners × Sh. of enslaved	𝟙(Abo. vote)
	(1)	(2)	(3)	(4)	(5)	(6)
Quilombos occupied area			-0.00536 (0.00414) {0.00337}	0.000001 (0.00005) {0.00003}	-0.000001 (0.000011) {0.000005}	-0.00187 (0.00159) {0.00111}*
Quilombos occupied area × Share of enslaved			0.0428 (0.0221)* {0.0148}***	-0.00014 (0.00038) {0.00022}	-0.000027 (0.000088) {0.000051}	0.0207 (0.01111)* {0.00697}***
1890 share of foreigners	-9.143 (329.9) {157.3}	6.281 (58.69) {22.07}	-4.261 (3.408) {3.087}			-6.724 (6.468) {3.655}*
1890 share of foreigners × Share of enslaved	-904.0 (1880.7) {927.1}	-180.4 (322.4) {135.8}	34.11 (19.75)* {18.24}*			50.71 (33.26) {22.09}**
[*Instrument*]	920.4 (387.6)** {213.1}***	58.38 (50.37) {28.47}**		0.00134 (0.000284)*** {0.000159}***	0.000200 (0.0000567)*** {0.0000244}***	
[*Instrument*] × Share of enslaved	-1302.1 (1748.4) {966.8}	304.8 (331.1) {188.7}		-0.00160 (0.133) {0.0674}	0.0792 (0.0266)*** {0.0250}**	
N	1263	1263	1263	1263	1263	1263
Mean dep. var	23.80	2.88	0.56	0.019	0.004	0.56
Endog test p-value			0.576			0.832
SW F-stat	17.25	16.91		44.64	35.64	
Under-id test p-value			0.032			0.062
Weak IV-robust AR interval			[0.0111, 0.0767]			[3.910, 97.51]
Province × Vote FE	✓	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses, and Conley standard errors (with a 250km window) are reported in curly brackets. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average HMI, average Topographic ruggedness index (ruggedness), and travel time to closest provincial capital (remoteness). Demographic controls include: population density, share of foreigners in 1872, number of settlement colonies by origin country, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE.

Alternative measures of proximity to freedom—Table A.17 in Appendix A.4 reports the results of specifications analogous to Table 4 Panel A, but using different proximity to freedom measures (number of quilombos and proximity to closest quilombo). SubIV_{ij} and $\text{SubIV}_{ij} \times S_{ij}$ are weaker instruments of these alternative measures, but in both cases TSLS estimates are similar in magnitude to OLS analogs and corroborate the conclusions of Table 4.

Alternative SSIV versions—Table A.18 in Appendix A.4 reports the results of specifications analogous to Table 4 Panel B, but using different SSIV designs and/or controls. Panel A uses the same SSIV as in Table 4 (unscaled origin country-based SSIV defined in equation 4), but additionally controls for the total number of colonial settlements by origin and district to account for possible pull factors for specific origins in destination districts. Panel B uses the leave-out origin religion-based SSIV defined in equation 5. Panel C uses the SSIV defined in equation 4, but scaled with total district population. Results are similar to those reported in Table 4 Panel B.

We also verify that these results are robust to: scaling either instrument with the predicted district population (constructed leaving out either districts or provinces),[14] as well as building the religion-based Bartik (scaled or unscaled) by leaving out provinces instead of districts (results available upon request).

Additional SSIV checks—Tables A.19 and A.20 in Appendix A.4 report the results of specifications analogous to Table 4 Panel B, but instead using the overidentified TSLS estimator obtained by using each share as a separate instrument (first origin country shares, and second predicted origin religion shares). These results are again similar to those reported in Table 4 Panel B. In all cases, we cannot reject the null hypothesis that over-identifying restrictions are valid. We also re-compute these regressions using estimators less-sensitive to using many instruments: LIML Fuller and Bias-corrected TSLS. In all cases, resulting estimates are numerically very similar. Again, under conventional thresholds, we cannot reject the null hypothesis that instruments are valid.

We also compute Rotemberg weights for both the origin country-based SSIV defined in equation 4 and the origin religion-based SSIV defined in equation 5.[15] For

[14] Define predicted district population $\widehat{P}_{ij1890} = P_{ij1872} \times (1 + \frac{P^{-i}_{1890} - P^{-i}_{1872}}{P^{-i}_{1872}})$, where P_{ij1872} is district i's population in 1872, and P^{-i}_{1890} and P^{-i}_{1872} are the total population leaving out district i in 1890 and 1872 respectively. This can also be constructed leaving out province j instead of district i.

[15] As these specifications include a large number covariates, it is impractical to report correlates of origin country/religion shares. We instead show that results are robust to constructing alternative

the former, Italy, Portugal and Spain receive the largest weights (0.537, 0.267, and 0.157 respectively, although some countries are negatively weighted). For the latter, Roman Catholics are overwhelmingly more influential (Rotemberg weight close to 1, although again several religions are negatively weighted). In Table A.21 we report the results of specifications for which influential shares are iteratively excluded in the construction of the SSIV. Again, in all cases the results are similar to the results presented in Table 4 and consistent with a causal interpretation of the influence of immigration on voting decisions.

Reduced form evidence— Table A.22 in Appendix A.4 reports the results of reduced form regressions documenting the influence of *i)* SubIV_{ij} and $\text{SubIV}_{ij} \times S_{ij}$ (column 1) and *ii)* SSIV_{ij} and $\text{SSIV}_{ij} \times S_{ij}$ (column 2) on voting decisions. In both cases, these results mirror closely their OLS and TSLS counterparts. As the multiple interactions and controls makes the effect of SubIV and its interaction not trivial to interpret, Figure A.15 provides an illustration of these results using three-dimensional margins plots. Analogously to first stage results, reduced form regressions show that as slavery intensity increases, the substitution effect between ruggedness and remoteness becomes a significant predictor of legislators voting decisions. That is, within slavery intensive areas, the combination of low remoteness and high ruggedness is a strong predictor of pro-emancipation voting decisions, but not within low slavery intensive districts.

4.3 Joint instrumentation and robustness checks

We now turn to specifications in which proximity to freedom, immigration, and their respective interactions are simultaneously treated as endogenous variables. Panel A of Table 5 reports the results of the just-identified model using SubIV_{ij}, $\text{SubIV}_{ij} \times S_{ij}$, SSIV_{ij}, and $\text{SSIV}_{ij} \times S_{ij}$ as instrumental variables. The first four columns report first stage estimates. F-statistics are consistently above conventional thresholds and, reassuringly, endogenous variables appear cleanly identified, each instrument being a significant predictor of at most one variable. Second stage estimates, reported in column 5, are again similar to their OLS counterparts and to TSLS estimates reported in Table 4. Again, we cannot reject the null hypothesis on endogeneity tests.

In Panel B, we augment our original TSLS model with a heteroskedasticity-based

SSIV versions that exclude influential shares.

Table 5: All instruments together

	Panel A: SubIV and SSIV only					*Panel B: With heteroscedasticity-based identification*				
	1st stage				2nd stage	1st stage				2nd stage
	Quil. area	Quil. area × Sh. enslaved	Sh. foreigners	Sh. foreigners × Sh. enslaved	𝟙(Abolition vote)	Quil. area	Quil. area × Sh. enslaved	Sh. foreigners	Sh. foreigners × Sh. enslaved	𝟙(Abolition vote)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Quilombos occupied area					-0.00601 (0.00429) {0.00411}					-0.00612 (0.00249)** {0.00247}**
Quilombos occupied area × Share of enslaved					0.0453 (0.0231)** {0.0160}***					0.0445 (0.0138)*** {0.0119}***
1890 share of foreigners					-8.319 (4.868)* {3.990}**					-8.560 (3.484)** {2.784}***
1890 share of foreigners × Share of enslaved					57.89 (22.23)*** {19.37}***					56.72 (21.66)*** {15.59}***
$SubIV_{ij}$	904.9 (398.2)** {293.3}***	55.32 (51.96) {37.80}	-0.0146 (0.127) {0.0856}	0.0153 (0.0234) {0.0147}		598.6 (317.2)* {223.9}***	15.08 (37.90) {27.93}	0.0416 (0.107) {0.0699}	0.0251 (0.0197) {.011496}**	
$SubIV_{ij}$ × Share of enslaved	-1345.5 (1788) {1398}	311.6 (341.1) {260.4}	0.367 (0.594) {0.377}	-0.0322 (0.141) {0.0766}		-956.8 (1379.4) {970.9}	283.7 (217.4) {144.9}**	-0.0344 (0.444) {0.194}	-0.103 (0.114) {0.03662}***	
$SSIV_{ij}$	-0.685 (0.805) {0.514}	-0.00642 (0.142) {0.0764}	0.00147 (0.000595)** {0.000483}***	0.0000756 (0.000114) {0.0000888}		-0.758 (0.780) {0.500}	-0.00790 (0.101) {0.0595}	0.00154 (0.000469)*** {0.000358}***	0.0000387 (0.000107) {0.0000875}	
$SSIV_{ij}$ × Share of enslaved	2.08 (3.830) {2.641}	-0.0702 (0.611) {0.369}	-0.000589 (0.00282) {0.00269}	0.00162 (0.000508)*** {0.000500}***		2.75 (3.870) {2.358}	-0.0109 (0.500) {0.274}	-0.00175 (0.00199) {0.00138}	0.00133 (0.000460)*** {0.000451}***	
Het.-based instruments	✗	✗	✗	✗		✓	✓	✓	✓	
N	1263	1263	1263	1263	1263	1263	1263	1263	1263	1263
Mean dep. var	23.80	2.88	0.019	0.004	0.56	23.80	2.88	0.019	0.004	0.56
Endog test p-value					0.673					0.410
SW F-stat	15.49	17.06	34.15	43.34		169.8	2262	269.9	1530	
Hansen J test p-value					N/A					0.301
1st stage BP test p-value						0.000	0.000	0.0013	0.000	
Province × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses, and Conley standard errors (with a 250km window) are reported in curly brackets. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average Human Mobility Index, average Topographic ruggedness index (ruggedness), and travel time to closest provincial capital (remoteness). Demographic controls include: population density, share of foreigners in 1872, number of settlement colonies by origin country, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE.

identification approach, following Lewbel (2012). We build on this method by using a machine learning approach for instrument selection, in the spirit of Belloni et al. (2010, 2012). Assuming errors are heteroskedastic in the first stages (which we verify with Breusch-Pagan tests: largest p-value is 0.013), we generate instruments from the second order association between observables and the variance of the errors. Since we have many controls (time-invariant observables interacted with vote fixed effects in each of the four first-stage regressions), there is a correspondingly high number of possible internally generated instruments. We select the instruments that most strongly predict each of the four endogenous variables using a cross-fit partialing-out lasso. From the superset of instruments selected by all four lassos, we select those generated from the covariates that we deem most unlikely to be colliders: rainfall and soil suitability measures to coffee, sugarcane, and cotton cultivation. The resulting (107) instruments are added to the first stage regressions—reported in columns 6 to 9—alongside SubIV_{ij}, SSIV_{ij}, and their respective interactions.

In a context with multiple instrumental variable strategies and, given the limitations of historical data, limited possibilities to run placebos, we find it useful to compare estimates resulting from strategies relying on different identification assumptions (Baum and Lewbel, 2019), and reassuring that they provide numerically similar results: second stage estimates (reported in column 10) are almost numerically identical to those reported in Panel A. This also allows to over-identify the model, and we find that we cannot reject over-identifying restrictions. Overall, the evidence reported here is strongly consistent with both the alternate TSLS and least squares previously reported, and suggests that the influence of proximity to freedom and immigration on voting decisions can be interpreted as causal.

Accounting for the endogeneity of slavery intensity.—In Appendix A.4 Table A.23 we report the second stage results of alternative specifications in which slavery intensity (this time not interacted with vote fixed effects) is included in the list of endogenous variables. Column 1 uses the same instruments as in Table 5 column 10 (with a fifth endogenous variable, and thus a fifth first stage), and shows that point estimates are comparable to those previously reported (even though this specification is slightly different, as previous regressions included interactions between slavery intensity and vote fixed effects) and again, we cannot reject the null hypothesis on endogeneity tests. As a form of sensitivity check, column 2 uses heteroskedasticity-based instruments only. While this unsurprisingly yields slightly weaker first stages, point estimates are again of similar magnitude.

Regressions without interaction terms.—To investigate the first stage association

between our instruments and Q_{ij} and F_{ij}, we consider similar 2SLS specifications without interaction terms. This also allows us to run a placebo check that our instruments do not predict voting behavior in the reduced form. Table A.16 in Appendix A.4 summarizes baseline no-interaction regressions with the usual set of controls, fixed effects, and operationalizations of the proximity to freedom in Panels A and B. First, this shows that neither proximity to freedom nor 1890 immigration have a direct impact on voting decisions in the full sample. Second, the TSLS estimations show that, while SubIV is a significant predictor of proximity to freedom, and SSIV is a significant predictor of immigration, proximity to freedom and immigration do not predict one another. Figure A.7 in Appendix A.4 illustrates these first stage relationships. Similarly to their OLS counterparts, second stage TSLS estimates do not significantly predict voting decisions in no-interaction regressions.[16] Finally, A.16 shows that neither SubIV nor SSIV have direct effects on voting decisions in the reduced form. This is a placebo test: if either SubIV or SSIV influenced voting decisions through other channels than proximity to freedom and immigration, one might expect these variables to have non-heterogeneous effects. This is reassuringly not the case.

5 Conclusions

In this paper, we investigate the determinants of the persistence and change of labor coercion institutions. We consider nineteenth-century Brazil, the last Western nation to abolish legal slavery, the largest importer of enslaved Africans through the Atlantic slave trade, and a country with (at the time) large areas of unexplored land. These three observations distinguish the Brazilian experience from any other; they also make it the ideal case study to unpack the interests of slaveholding elites in the presence of an open agricultural frontier. This article also makes an original contribution to the historiography of nineteenth-century Brazil. From the archival records of the Imperial parliament, we build a district-vote-level data set documenting political

[16]Overall, this evidence suggests that TSLS results are comparable to their OLS analogs in no interaction regressions. The Montiel Olea and Pflueger (2013) F-statistic (which tests for weak instruments) is 8.075 in Panel A and 22.24 in Panel B. The former is slightly below conventional thresholds, but this is not a concern as: *i)* with $\alpha = 0.05$ this corresponds to a percent of worst case bias (τ) of roughly 40%, which would not make the parameter statistically significant; *ii)* an under-identification test rejects the null hypothesis that the model is under-identified under $\alpha = 0.05$; and *iii)* the weak IV-robust AR interval includes zero.

decision-making on emancipation-related bills during the last decade of the Empire. We rely on census surveys, historical maps, and geo-referenced data sources to identify local variations in slaveholders' interests.

Our results establish the coexistence of two historically important drivers of the demand for coercive institutions from slaveholding elites. First, slaveholders' opposition to abolition was alleviated by the local availability of immigrant labor as a substitute to coerced labor. This supports the importance of the *labor demand effect*. Second, slaveholders' interests differed across districts depending on the local cost of enforcing the slavery. Representatives from slavery intensive districts where escaping slavery was easier were more likely to vote in favor of abolition. Even if—according to the Nieboer-Domar hypothesis—the elite as a whole should favor coercion when land is abundant, the frontier planters, who apparently benefited the most from the abundance of land, turned out to be less inclined to oppose emancipation because restricting workers' mobility proved costly under the prevailing arrangement. This result is in line with the *outside option effect* (Acemoglu and Wolitzky, 2011), and in our opinion, is the main contribution of this paper. As an important corollary, this result emphasizes the role of coerced workers themselves in precipitating the collapse of the legal coercion system in Brazil. Insurrections and flights raised the costs of coercion to the planter class, which contributed to undermining the institution.

Together, these two hypotheses lay some foundations of a more general theory of institutional change in a democracy dominated by oligarchic interests. Following the definition of Stasavage (2014, p. 338), "a political regime results in the provision of property rights for a specific group, accompanied by significant barriers to entry." Individual elite members compare how they would profit under the two alternative institutional arrangements, that imply different patterns of ownership of productive assets—in this paper, a claim to owning the labor of enslaved workers. This article reveals the importance of the *mobility* of labor (the ability of enslaved individuals to withdraw from the institutional arrangement) and of the *substitution* between enslaved and immigrant labor. Unbundling the interests of the elite looks like a promising way to expand this analysis to other aspects of institutional—and technological—changes.

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A Appendix – For online publication

A.1 Data appendix

This section provides additional details on the construction of our legislative voting database and variables of interest.

Abolitionist voting behavior. Our main dependent variable captures the voting behavior on emancipation-related bills of legislators in the Câmara dos Deputados. To build this variable, we explored the archival records of parliamentary debates from the onset of the eighteenth legislature (1882) to the end of the twentieth legislature (1889) and identified every occurrence of roll-call vote relative to slavery bills. Across these three legislatures, we identified thirteen such events of nominative voting. Occurrences of nominative voting had to be *i)* clearly related to the emancipation of enslaved individuals and *ii)* discussed in the parliamentary records in a way that allowed clear-cut identification of how the vote related to emancipation.[17] The votes retained in the final data set are the following:

- 03/06/1884: First motion of no confidence against the new *Presidente do Conselho*, nominated by the Emperor with the known objective to push forward the gradual emancipation of slaves. (Abolitionists vote against.)
- 30/06/1884: The roll is called on the decision to delay discussions on the Empires' finances until after the government's project related to labor emancipation is presented to the Chamber. (Abolitionists vote against.)

[17] In other words, the annals must have made clear that voting in favor/against the bill meant being in favor/against the emancipation of enslaved persons. In Appendix A.4, Figure A.14 illustrates separate regressions on each vote, and Table A.11 examines robustness to alternative interpretations of these votes.

- 15/07/1884: Dantas (*Presidente do Conselho*, in charge to build a proposal for the gradual abolition of slavery) finally presents his project to the chamber. This occasions a large upheaval, in particular because the proposal fails to mention any compensation to slaveholders. This leads the President of the Chamber (Moreira de Barros) to offer his resignation, on which the roll is called. (Abolitionists vote in favor.)

- 28/07/1884: New motion of no confidence against Dantas. The Chamber rejects the government's proposal on the emancipation of enslaved persons and withdraws its confidence from Dantas. (Abolitionists vote against.)

- 13/04/1885: New motion of no confidence, this time ending in a tie. (Abolitionists vote against.)

- 13/07/1885: The roll is called on a bill designed to increase the State valuation of enslaved individuals aged between 60 and 65 years old. (Abolitionists vote against.)

- 14/07/1885: The roll is called on an amendment related to the unconditional emancipation of enslaved persons aged 60 years or older. (Abolitionists vote in favor.)

- 27/07/1885 (1): The roll is called on a project regarding the prices of enslaved persons. (Abolitionists vote against).

- 27/07/1885 (2): The roll is called on a project regarding the manumission of disabled slaves. (Abolitionists vote in favor).

- 13/08/1885: The roll is called on the final version of the bill that would later become the *Lei dos Sexagenários*. (Abolitionists vote in favor.)

- 04/05/1885: New motion of no confidence. (Abolitionists vote against.)

- 05/05/1887: First project regarding the complete abolition of slavery. The roll is called on allowing it to proceed without hindrance. (Abolitionists vote in favor.)

- 09/05/1888: The roll is called on the project later known as the *Lei Áurea*. (Abolitionists vote in favor.)

To the best of our knowledge, the thirteen occurrences of nominative voting outlined above constitute, for the three legislatures considered, the universe of roll call votes with clear cut interests related to emancipation. In each of these cases, the annals of the Câmara dos Deputados provide the nominative list of legislators voting in favor or against (yeas and nays). We record these names and votes and match each legislator with the electoral district they represent. This is mostly done using

records of *Juntas Verificadoras de Poderes* (special councils that occur during the early months of each legislature), cross referenced using the encyclopedia of Imperial parliamentarians in Nogueira and Firmo (1973). We then link each district with the municipalities and *paróquias* (the lowest administrative unit) that comprise them using the transcript of the 1881 electoral reform in Jobim and Porto (1996), which details the Empire's electoral division after the 1881 Saraiva law. Ultimately, we are thus left with a mapping of 1881 *paróquias* and municipalities into 1882-1889 district-legislator-vote observations.

Demographic variables. Baseline demographic variables come from Brazil's first nation-wide demographic census in 1872 (Brazil, 1874). The main challenge here comes from matching 1872 municipalities (*municípios*) with 1881 municipalities. We implement a first matching procedure at the *paróquia* level. Although less than ten years separate the census from the legislation, a significant number of *paróquias* were created in this time frame. From the 1441 *paróquias* the country counted in 1872, there were 1662 enumerated in the 1881 legislation. Most of these 221 *paróquias* are added to existing municipalities, but several municipalities were also created in the meantime and several 1872 *paróquias* had become municipalities by 1881. To improve the precision of this matching, we exploit the IBGE (2010) Evolução da Divisão Territorial to (painstakingly) manually trace the genealogy of municipalities. Matched *paróquias* are then aggregated to the level of municipalities and then districts.

Geo-coded data. We exploit several sources of geo-coded data, in particular from IIASA/FAO (2012), Nunn and Puga (2012), Özak (2010, 2018) and INCRA (2020). Information provided by these sources are used in combination with the IBGE's municipality-level boundaries for the year 1872. This allows us to compute zonal statistics and distance measures at the municipality and district levels. For each administrative unit, we compute: land suitability, climatic and topographic measures, major towns' and centroids' geographical coordinates, remoteness measures (distance to the coast, to provincial capitals and to Rio de Janeiro), population density (using Brazil (1874)), and 'distance to freedom' proxies (number and size of quilombos and, in combination with Fundação Palmares (2020), average distance to the closest municipalities with at least k quilombos, $k \in \{1; 9\}$).

Linking our matched 1872-to-1881 municipalities-to-districts data to the 1872 municipality grid allows to draw district-level maps of voting patterns on the bills we consider. Aggregating municipality boundaries into districts offers an additional matching challenge, as 1872 municipalities do not perfectly map into 1881 districts. Two issues may arise: *i)* Some municipalities are actually comprised of several districts. This occurs for some large cities, e.g. Salvador (two districts, BA1-2) and Rio de Janeiro (three districts, RJ1-3). *ii)* Some 1881 districts include municipalities that were created/whose territory was altered after 1872, in which case apparent inconsistencies may occur. For example, the province of Amazonas is comprised of two districts in 1881, but the second district maps into two non-contiguous polygons

based on the 1872 territorial division. This most likely occurs because the territorial division of the two (very large) municipalities that comprise the first ditrict of Amazonas (Manaus and Barcellos) shrunk (the former in particular) before 1881 as newer municipalities expanded. Hence, the resulting maps do not offer a perfect representation of the 1881 electoral division, but they do illustrate the geographical configuration of abolition votes.

Finally, we also geo-reference a number of existing maps, in particular from CPDOC (2016). This allows to approximate the location of state-sponsored settlements in a succession of waves and to assess proximity to insurrection sites, slavery/abolitionist interest groups and abolitionist journals.

Appendix A.4 references a number of results using additional variables to the ones included in Table 1. Summary statistics for these additional variables are provided in Table A.6.

Table A.6: Additional summary statistics

Statistic	N*	Mean	St. Dev.	Min	Max
Pro-abolition vote alternative interpretation*	1,284	.59	.49	0	1
Colonial settlements during 1748-1800	122	.11	.59	0	4
Colonial settlements during 1800-1870	122	.16	.66	0	4
Colonial settlements during 1870-1930	122	.94	3.64	0	36
Colonial settlements during 1930-1970	122	.76	3.47	0	28
District area	122	68,471	184,391	979.6	1,138,963
Share of capitalists	122	.00369	.00415	.0000138	.0237
Share of industrialists	122	.00259	.00567	0	.0433
Distance to closest insurrection	122	214.7	278.6	3.871	1783.6
Distance to closest *Lavoura* club	122	396.3	395.9	6.513	2405.8
Distance to closest abolitionist club	122	278.1	229.3	10.78	1165.1
Distance to closest abolitionist newspaper	122	468.7	450.2	16.73	2980.4
Number of destroyed quilombos**	33	2.242	3.212	0	11
18722 share of foreigners from top sender countries:					
Germany	122	.095	.172	0	.859
Austria	122	.00185	.00508	0	.0382
Argentina	122	.00454	.0244	0	.244
Belgium	122	.00135	.00319	0	.0242
France	122	.0312	.0327	0	.258
Greece	122	.000409	.00232	0	.0196
Spain	122	.0187	.0190	0	.0875
Netherlands	122	.00298	.00995	0	.0930
England	122	.0255	.0549	0	.464
Italy	122	.0812	.064	0	.283
Japan	122	.000	.000105	0	.000801
Mexico	122	.000	.000218	0	.00140
U.S.A.	122	.00748	.0336	0	.336
Paraguay	122	.0247	.0487	0	.322
Russia	122	.000441	.00222	0	.0227
1890 share of individuals from minority religions:					
Catholic orthodox	122	.000	.000137	0	.00132
Evangelical	122	.00102	.00514	0	.0490
Presbyterian	122	.000	.000165	0	.00111
Other protestants	122	.00538	.0132	0	.0982
Islam	122	.000	.0000616	0	.000354
Positivist	122	.000	.000147	0	.000722
None	122	.000594	.00126	0	.00847

* 122 districts × 13 votes = 1586 obs.

** 33 districts in 4 provinces: Goías, Mato Grosso, Minas Gerais, and São Paulo.

Dist. in km, surf. in km^2.

A.2 Substitution instrument interpretation

Consider the simplified econometric model

$$Y_i = \alpha Rug_i(1 - Rem_i) + \Sigma_v^{V-1}\beta_v^{Rug} Rug_i \times Vote_v + \Sigma_v^{V-1}\beta_v^{Rem} Rem_i \times Vote_v + \gamma_1 Rug_i \times S_i + \gamma_2 Rem_i \times S_i + \epsilon_i,$$

where Rug_i denotes a measure of ruggedness, and Rem_i a measure of remoteness. The variables $Vote_v$ are $V-1$ vote dummies, and S_i captures slavery intensity. $Rug_i(1 - Rem_i)$ is the excluded instrument, and the other interactions are included as controls.

Then, marginal effects of the instrument's components are:

$$\frac{\partial Y_i}{\partial Rug} = \alpha(1 - Rem_i) + \Sigma_v^{V-1}\beta_v^{Rug} Vote_v + \gamma_1 S_i$$

and

$$\frac{\partial Y_i}{\partial Rem} = -\alpha Rug + \Sigma_v^{V-1}\beta_v^{Rem} Vote_v + \gamma_2 S_i$$

The identifying variation comes from $\alpha(1 - Rem_i)$ and $-\alpha Rug$, i.e. how much less does ruggedness affect outcome Y as a result of remoteness, and how much less does remoteness affect Y as a result of ruggedness. The remainder of these marginal effects comes from interacted terms included in both first and second stages. In other words, the exclusion restriction—null covariance between second stage error term and $Rug_i(1 - Rem_i)$—is notably conditionning explicitly on: the direct effect of ruggedness on Y for each vote given low levels of remoteness, the direct effect of remoteness on Y for each vote given low levels of ruggedness, and how much the effects of ruggedness and remoteness vary with slavery (in addition to all other controls). That is, if ruggedness (remoteness) has an effect on voting decisions (allowed to be different for each vote) independently from the level of remoteness (ruggedness), or an effect that varies with slavery intensity, then the exclusion restriction is conditioning on these effects.

In robustness checks, these regressions also include quadratic terms:

$$Y_i = \alpha Rug_i(1 - Rem_i) + \Sigma_v^{V-1}\beta_v^{Rug} Rug_i \times Vote_v + \Sigma_v^{V-1}\beta_v^{Rem} Rem_i \times Vote_v + \gamma_1 Rug_i \times S_i + \gamma_2 Rem_i \times S_i + \delta_1 Rug_i^2 + \delta_2 Rem_i^2 + \epsilon_i$$

In this case, marginal effects are:

$$\frac{\partial Y_i}{\partial Rug} = \alpha(1 - Rem_i) + \Sigma_v^{V-1}\beta_v^{Rug} Vote_v + \gamma_1 S_i + 2\delta_1 Rug_i$$

and

$$\frac{\partial Y_i}{\partial Rem} = -\alpha Rug + \Sigma_v^{V-1}\beta_v^{Rem} Vote_v + \gamma_2 S_i + 2\delta_2 Rem_i.$$

This implies that the the direct effect of ruggedness (remoteness) is allowed to vary with the level of ruggedness (remoteness).

A.3 Shift-share instrument interpretation

Figure A.4 illustrates the construction of the shares used in the SSIV, taking the example of five districts randomly chosen within five provinces. Each row of the figure focuses on a different district, from top to bottom: the thirteenth district of Bahia, the second district of Espírito Santo, the second district of Goías, the first district of Rio Grande do Sul, and the seventh district of São Paulo. The subfigures in the first and second columns respectively display the share of catholic and noncatholic foreigners from each origin country within the district population. The subfigures in the third and fourth columns respectively display the share of catholic and noncatholic foreigners from each origin country, out of all (catholic and noncatholic) foreigners from each origin country in 1872. The subfigures in the final column display the predicted shares of foreigners of each religion in 1872, constructed by matching the shares displayed in columns 3 and 4 with the dominant religion of each type in each origin country, and then summing over origins.

In 1872, the thirteenth district of Bahia had a remarkably small, mostly Portuguese, immigrant population (small both relative to the district's population and to populations from each origin country), and no immigrant with a noncatholic religion. Based on this information, the matching procedure predicts that in 1872, about 0.035% of the foreign Roman Catholic population are located in this district. The second district of Goiás (third row) had a similarly small immigrant population in 1872, but a nonzero share of noncatholic foreigners of English origin (about 0.0014% of the population was English, which represented a bit under 0.3% of the English population in the country). Because of this, the matching procedure predicts that a bit under 0.02% of foreign Presbyterians were located in this district in 1872. Hence, these two districts may e.g. be differently exposed to a wave of Presbyterian immigrants reaching Brazil in 1890.

In contrast, the first district of Rio Grande do Sul had a considerable immigrant population in 1872 (this district's headtown was Porto Alegre, also capital of the province). Over 3% of the population was noncatholic German, which corresponds to roughly 1% of the noncatholic German population. The district also had a very large noncatholic Greek minority, which represented a small fraction of the district population, but a very large share of the noncatholic Greek population in the country. The predicted shares of foreigners by religion reflects this plurality, and the matching procedure predicts that a nonzero share of immigrants of each religion are located in Porto Alegre in 1872 (e.g. roughly 6% of the foreign Orthodox Catholic population).

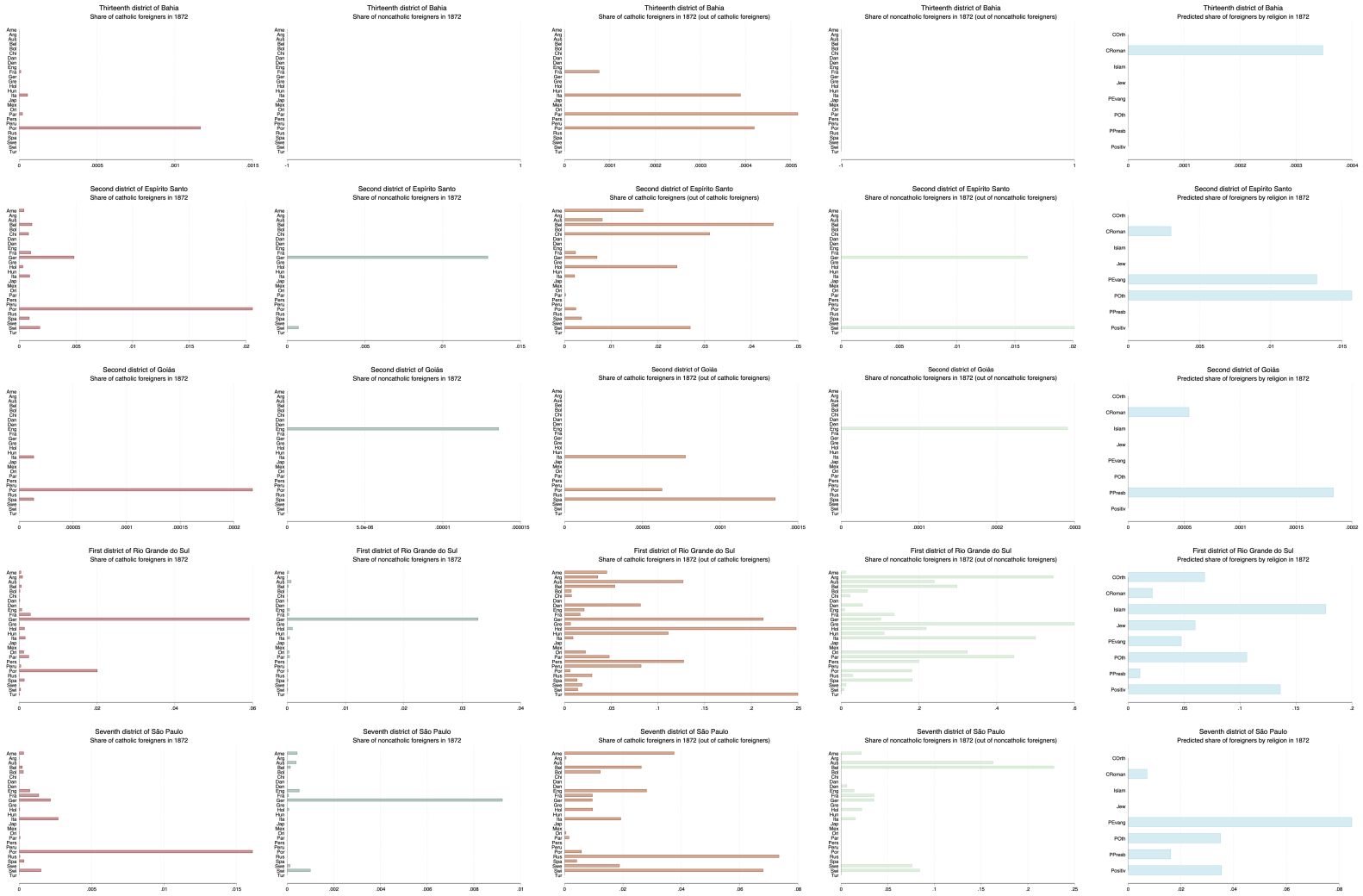


Figure A.4: Religion SSIV
Immigration and predicted religion of foreigners in 1872 – five random districts in five random provinces

A.4 Supplementary tables and figures

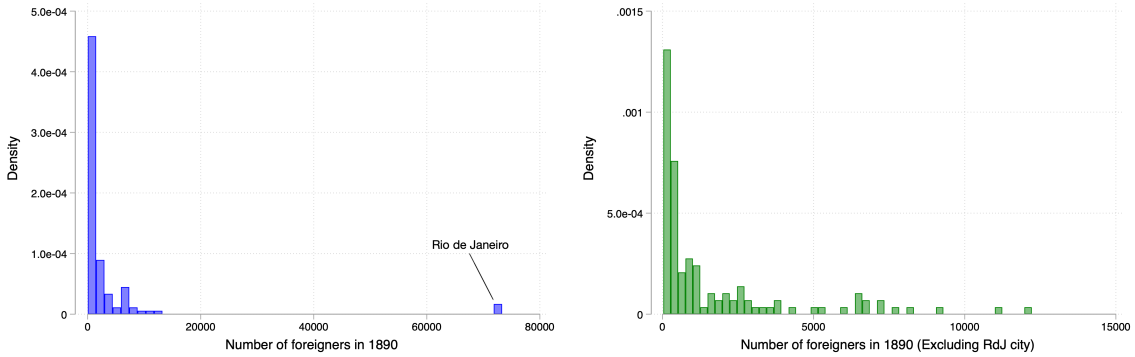


Figure A.5: 1890 immigration

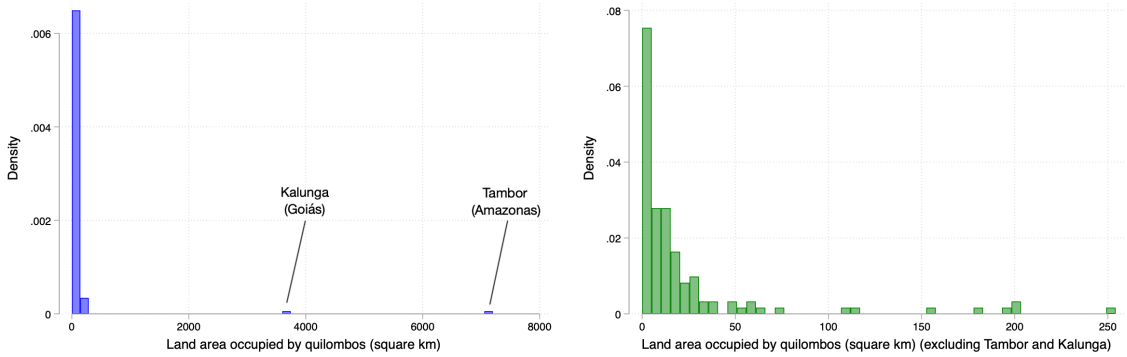


Figure A.6: Area occupied by quilombos

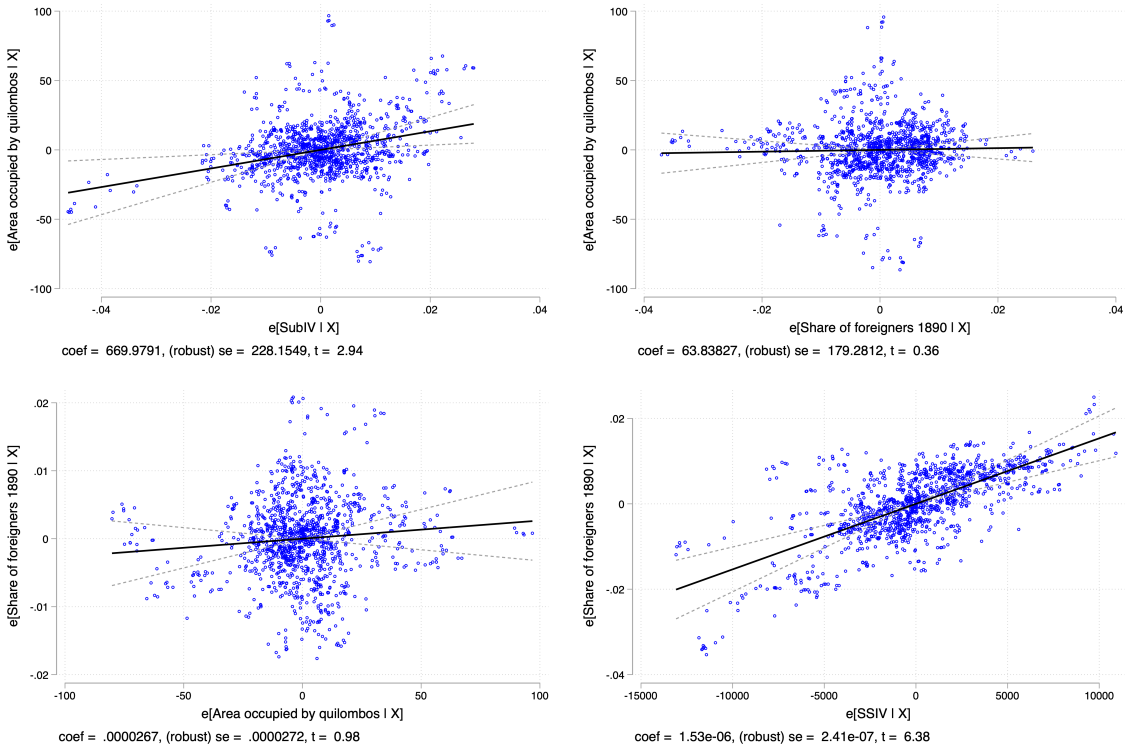


Figure A.7: First stage plots

Notes: partial regression plots of first stage relationships. Clock-wise from top left to bottom left: residualized area occupied by quilombos on residualized SubIV instrument, residualized area occupied by quilombos on residualized 1890 share of instruments, residualized share of foreigners on residualized SSIV, and residualized share of foreigners on residualized area occupied by quilombos. Regressions include province-by-vote fixed effects, legislator controls, and vote fixed effects interacted with both geographic and demographic controls (coffee, sugar, and cotton suitability, average rainfall, longitude and latitude, distance to closest river, population density, share of foreigners in 1872, share of free colored and literacy) as well as ruggedness and remoteness measures interacted with both vote fixed effects and slavery intensity. Standard errors are two-way clustered by district and vote.

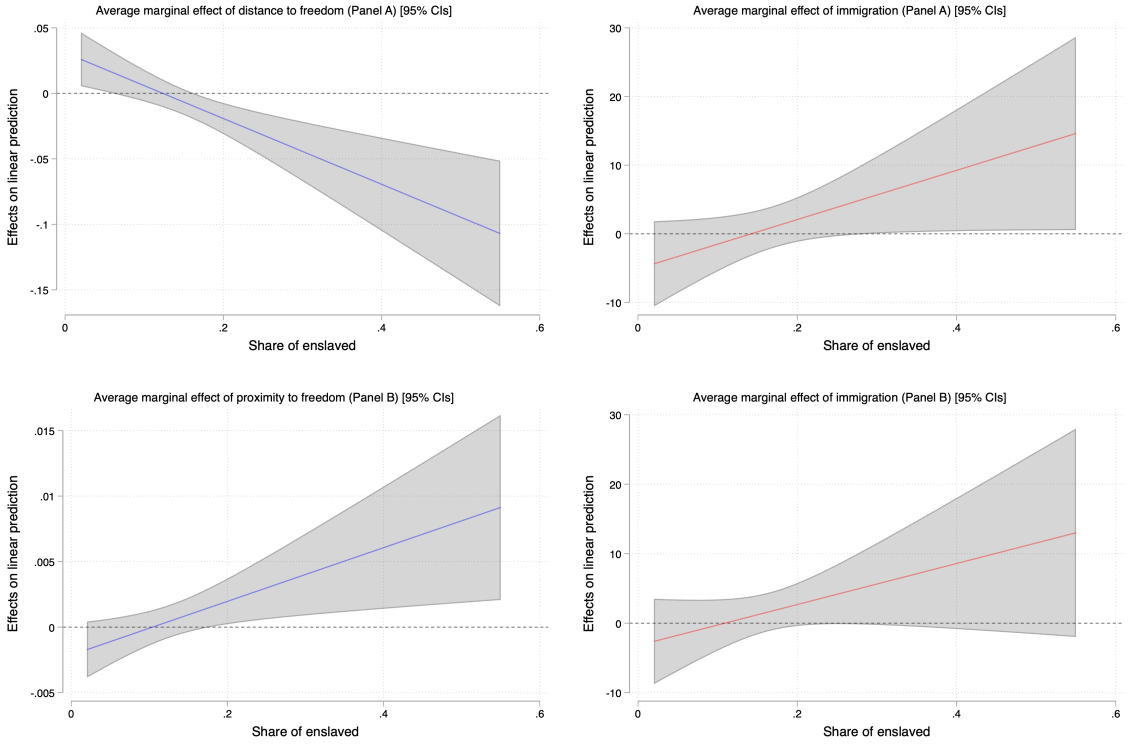


Figure A.8: Marginal effects of outside option and labor demand effects

Notes: margins from the fourth column of panels A and B of table 2. Panel A uses distance to the closest quilombo as a measure (inverse) of outside options. Panel B instead uses the quantity of land occupied by quilombos. These regressions include province-by-vote fixed effects, legislator controls, and vote fixed effects interacted with both geographic and demographic controls (coffee, sugar, and cotton suitability, average rainfall, longitude and latitude, distance to coast and to closest river, and average Human Mobility Index, population density, share of foreigners in 1872, share of free colored and literacy). Standard errors are two-way clustered by district and vote.

Table A.7: Outside option and labor demand effects (baseline OLS with un-interacted controls) — Outside option is distance to closest quilombo

	𝟙(Abolition vote)			
	(1)	(2)	(3)	(4)
Dist. to closest quilombo	0.0293***	0.0331***	0.0318***	0.0298***
	(0.0108)	(0.0120)	(0.0118)	(0.0114)
Dist. to closest quilombo × Share of enslaved	-0.232***	-0.246***	-0.244***	-0.244***
	(0.0713)	(0.0708)	(0.0702)	(0.0684)
1890 share of foreigners	-8.529***	-8.444***	-7.154**	-6.210*
	(3.198)	(3.259)	(3.196)	(3.195)
1890 share of foreigners × Share of enslaved	48.17***	50.10***	48.00***	40.80**
	(16.00)	(16.42)	(16.10)	(17.28)
Share of enslaved population	-1.730***	-1.896***	-1.523**	-1.319**
	(0.530)	(0.545)	(0.595)	(0.607)
Soil suitability to coffee		-0.00469	-0.00675	-0.00636
		(0.00695)	(0.00705)	(0.00731)
Soil suitability to sugarcane		-0.000716	0.00191	0.00370
		(0.00797)	(0.00785)	(0.00845)
Soil suitability to cotton		0.00266	0.00215	0.000246
		(0.00635)	(0.00629)	(0.00658)
Average rainfall		0.000291	0.000354*	0.000340
		(0.000197)	(0.000213)	(0.000213)
Longitude		0.0386*	0.0508**	0.0424*
		(0.0215)	(0.0240)	(0.0229)
Latitude		0.00388	-0.00206	-0.00703
		(0.0166)	(0.0168)	(0.0192)
Log(Shortest dist to coast)		0.108*	0.0737	0.0704
		(0.0597)	(0.0615)	(0.0622)
Log(Dist to closest river)		-0.00951	-0.00532	-0.000560
		(0.0275)	(0.0297)	(0.0289)
average HMI		2.501	1.346	1.415
		(2.546)	(2.401)	(2.490)
1872 share of free foreigners			-0.789	-1.061
			(1.629)	(1.698)
Share of free colored			0.305	0.320
			(0.356)	(0.381)
Literacy rate			0.452	0.614
			(0.554)	(0.533)
log(Pop density)			-0.0572	-0.0532
			(0.0377)	(0.0358)
Previous gov. appointment				0.00812
				(0.0542)
Reelected				0.00447
				(0.0537)
Lawyer				0.0479
				(0.0964)
Doctor				0.163
				(0.131)
Scientist				-0.0183
				(0.182)
Civil servant				-0.0469
				(0.0890)
Priest				0.269*
				(0.163)
Owner/Businessman				-0.243*
				(0.141)
Military				-0.622**
				(0.256)
N	1284	1284	1284	1284
Mean dep. var	0.56	0.56	0.56	0.56
Province × Vote FE	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses. Spatial HAC standard errors not reported for the sake of brevity. Column one include no controls other than slavery prevalence. Column 2 adds geographic controls. Column 3 adds demographic controls. Column 4 adds legislator controls. Province fixed effects are interacted with vote FE.

Table A.8: Outside option and labor demand effects (baseline OLS with un-interacted controls) — Outside option is area occupied by quilombos

	𝟙(Abolition vote)			
	(1)	(2)	(3)	(4)
Quilombos occupied area	-0.00152	-0.00170*	-0.00242**	-0.00224*
	(0.000998)	(0.000947)	(0.00109)	(0.00115)
Quilombos occupied area × Share of enslaved	0.0183***	0.0186***	0.0214***	0.0214***
	(0.00563)	(0.00610)	(0.00712)	(0.00788)
1890 share of foreigners	-6.325**	-6.908**	-5.477*	-4.443
	(3.185)	(3.239)	(3.267)	(3.234)
1890 share of foreigners × Share of enslaved	40.01**	44.50***	41.63**	35.25*
	(16.51)	(17.01)	(17.42)	(18.28)
Share of enslaved population	-2.329***	-2.672***	-2.000***	-1.886***
	(0.649)	(0.679)	(0.624)	(0.662)
Soil suitability to coffee		-0.00384	-0.00571	-0.00467
		(0.00673)	(0.00679)	(0.00670)
Soil suitability to sugarcane		-0.00226	-0.000848	0.000593
		(0.00695)	(0.00684)	(0.00701)
Soil suitability to cotton		0.00471	0.00515	0.00225
		(0.00607)	(0.00614)	(0.00650)
Average rainfall		0.000267	0.000293	0.000237
		(0.000198)	(0.000218)	(0.000224)
Longitude		0.0388*	0.0426	0.0354
		(0.0220)	(0.0259)	(0.0229)
Latitude		-0.00552	-0.0112	-0.0128
		(0.0195)	(0.0192)	(0.0202)
Log(Shortest dist to coast)		0.0756	0.0352	0.0333
		(0.0608)	(0.0630)	(0.0632)
Log(Dist to closest river)		-0.0167	-0.0146	-0.0116
		(0.0290)	(0.0300)	(0.0305)
average HMI		2.120	1.361	1.700
		(2.582)	(2.470)	(2.378)
1872 share of free foreigners			-0.834	-1.130
			(1.541)	(1.614)
Share of free colored			0.829*	0.798
			(0.440)	(0.487)
Literacy rate			0.597	0.688
			(0.536)	(0.499)
log(Pop density)			-0.0452	-0.0373
			(0.0415)	(0.0389)
Previous gov. appointment				-0.00167
				(0.0522)
Reelected				-0.0166
				(0.0522)
Lawyer				0.126
				(0.108)
Doctor				0.208
				(0.143)
Scientist				-0.00173
				(0.188)
Civil servant				0.0162
				(0.0905)
Priest				0.368**
				(0.165)
Owner/Businessman				-0.200
				(0.149)
Military				-0.613**
				(0.255)
N	1263	1263	1263	1263
Mean dep. var	0.56	0.56	0.56	0.56
Province × Vote FE	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses. Spatial HAC standard errors not reported for the sake of brevity. Column one include no controls other than slavery prevalence. Column 2 adds geographic controls. Column 3 adds demographic controls. Column 4 adds legislator controls. Province fixed effects are interacted with vote FE.

Table A.9: Outside option and labor demand effects – Probit

	𝟙(Abolition vote)			
	(1)	(2)	(3)	(4)
Panel A: Outside option is distance to closest quilombo				
Dist. to closest quilombo	0.0413	0.0416	0.0409	0.0376
	(0.0121)***	(0.0129)***	(0.0122)***	(0.0130)***
Dist. to closest quilombo × Share of enslaved	-0.319	-0.299	-0.310	-0.301
	(0.0749)***	(0.0736)***	(0.0697)***	(0.0739)***
1890 share of foreigners	-10.75	-10.46	-9.413	-7.438
	(3.432)***	(3.886)**	(3.771)**	(3.508)**
1890 share of foreigners × Share of enslaved	59.95	61.15	66.35	53.12
	(17.14)***	(19.43)***	(19.31)***	(19.28)***
N	1284	1284	1284	1284
Panel B: Outside option is area occupied by quilombos				
Quilombos occupied area	-0.00217	-0.00219	-0.00295	-0.00259
	(0.00142)	(0.00137)	(0.00136)**	(0.00144)*
Quilombos occupied area × Slavery prevalence	0.0238	0.0230	0.0260	0.02417
	(0.00656)***	(0.00665)***	(0.00701)***	(0.00719)***
1890 share of foreigners	-7.407	-8.307	-6.949	-5.0129
	(3.326)**	(3.679)**	(3.747)*	(3.457)
1890 share of foreigners × Slavery prevalence	46.24	52.57	54.75	43.30
	(16.93)***	(18.98)***	(20.21)***	(19.90)**
N	1263	1263	1263	1263
Mean dep. var	0.56	0.56	0.56	0.56
Share of enslaved × Vote FE	✓	✓	✓	✓
Province × Vote FE	✓	✓	✓	✓
Geographic controls	✗	✓	✓	✓
Demographic controls	✗	✗	✓	✓
Legislator controls	✗	✗	✗	✓

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered by district reported in parentheses. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE.

Table A.10: Distances to freedom

	𝟙(Abolition vote)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Distance to closest K quilombos	0.0310	0.00888	0.00660	0.0110	0.0201	0.0444	0.0474	0.0473	0.0866
	(0.0117)	(0.0125)	(0.0112)	(0.0119)	(0.0153)	(0.0163)***	(0.0216)**	(0.0216)**	(0.0334)***
	{0.0101}***	{0.00832}	{0.00836}	{0.00910}	{0.00825}**	{0.00650}***	{0.0153}***	{0.0151}***	{0.0259}***
Dist. to closest K quilombos × Share of enslaved	-0.251	-0.170	-0.168	-0.182	-0.234	-0.341	-0.349	-0.346	-0.473
	(0.0701)***	(0.0776)**	(0.0733)**	(0.0747)**	(0.0750)***	(0.0851)***	(0.0932)***	(0.0928)***	(0.119)***
	{0.0523}***	{0.0614}***	{0.0593}***	{0.0544}***	{0.0561}***	{0.0578}***	{0.0724}***	{0.0717}***	{0.0877}***
1890 share of foreigners	-5.097	-5.518	-5.920	-5.458	-4.750	-3.534	-3.580	-3.478	-3.677
	(3.495)	(3.487)	(3.533)*	(3.533)	(3.559)	(3.291)	(3.265)	(3.250)	(3.220)
	{2.647}*	{3.043}*	{3.094}*	{2.910}*	{2.759}*	{2.878}	{2.848}	{2.847}	{2.945}
1890 share of foreigners × Share of enslaved	35.85	40.57	41.82	40.25	36.21	33.96	34.98	34.44	37.04
	(18.45)*	(19.06)**	(19.43)**	(19.24)**	(18.88)*	(18.19)*	(18.18)*	(18.11)*	(18.16)**
	{15.14}**	{18.03}**	{17.80}**	{16.87}**	{15.69}**	{16.28}**	{16.35}**	{16.33}**	{16.19}**
K	1	2	3	4	5	6	7	8	9
N	1284	1284	1284	1284	1284	1284	1284	1284	1284
Mean dep. var	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56
Share of enslaved × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses, and Conley standard errors (with a 250km window) are reported in curly brackets. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE.

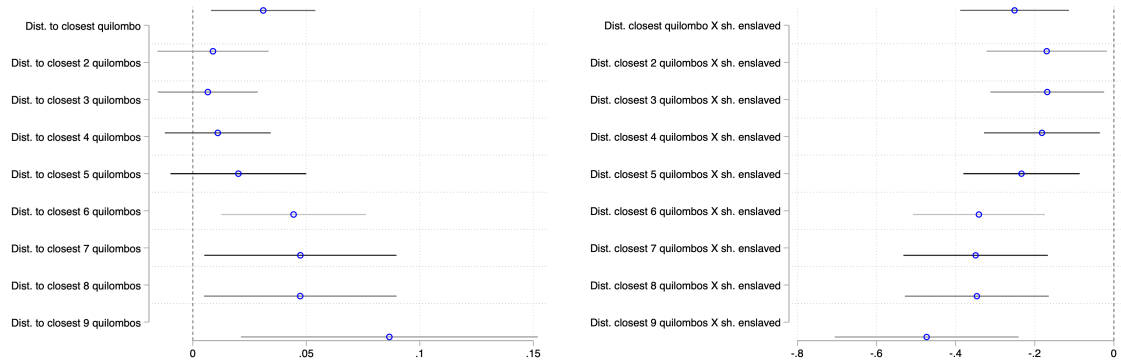


Figure A.9: Distances to freedom

Notes: This figure plots the coefficients estimated in Table A.10, with 95% confidence intervals. These regressions include all fixed effects and controls, and standard errors are two-way clustered by district and vote. As K increases, a decrease in proximity to freedom has a larger effect on the likelihood that a representative votes in favor of emancipation, holding slavery-intensity constant.

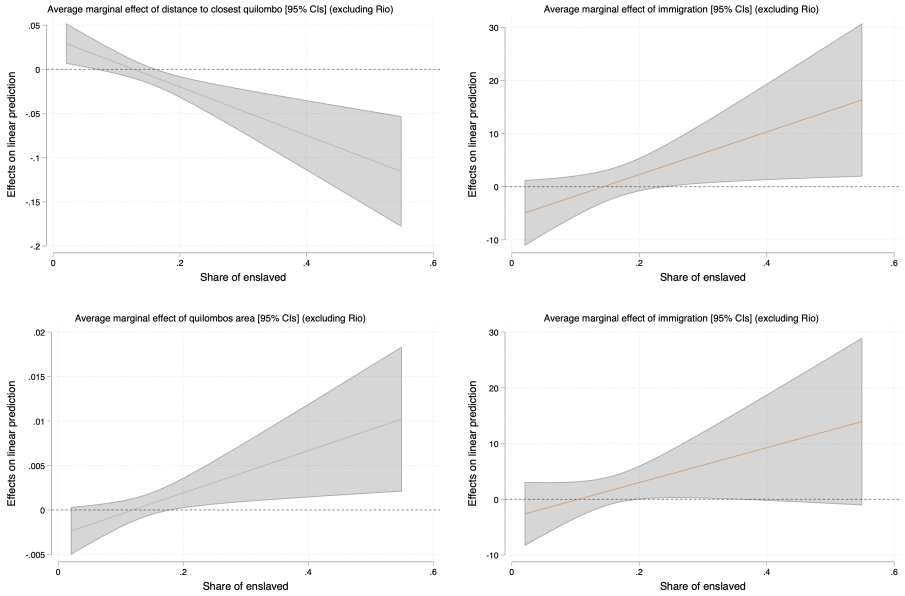


Figure A.10: Marginal effects of outside option and labor demand effects – Excluding Rio de Janeiro

Notes: marginal effects of regressions analogous to the fourth column of panels A and B of table 2, but excluding Rio de Janeiro city (first district of Rio de Janeiro Province). Top left uses distance to the closest quilombo, bottom left uses the quantity of land occupied by quilombos. Regressions always include province-by-vote fixed effects, legislator controls, and vote fixed effects interacted with both geographic and demographic controls (coffee, sugar, and cotton suitability, average rainfall, longitude and latitude, distance to coast and to closest river, district area, and average HMI, population density, share of foreigners in 1872, share of free colored and literacy). Standard errors are two-way clustered by district and vote.

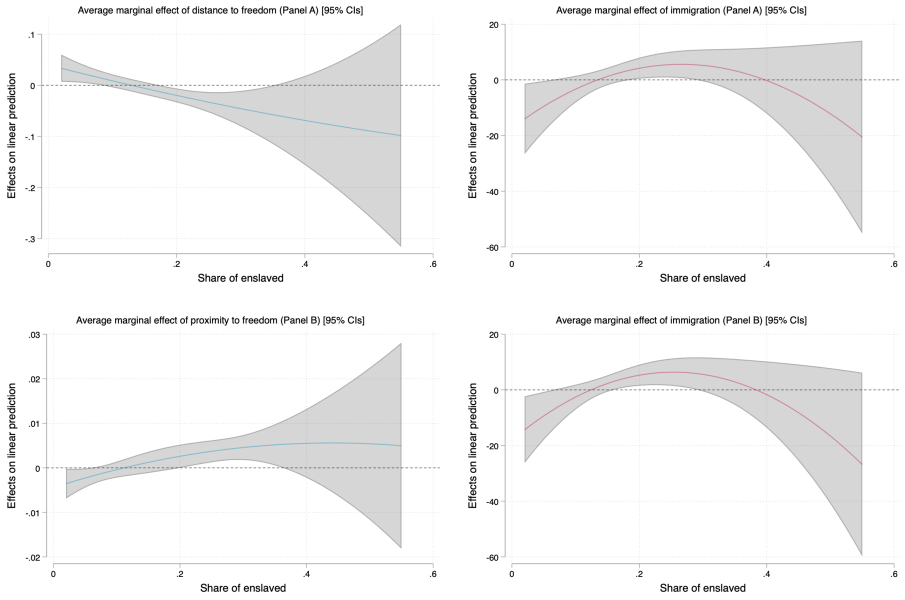


Figure A.11: Marginal effects of outside option and labor demand effects – Polynomial

Notes: marginal effects of regressions analogous to the fourth column of panels A and B of table 2, but flexibly controlling for a slavery polynomial and a quadratic interaction. Top left uses distance to the closest quilombo, bottom left uses the quantity of land occupied by quilombos. Regressions always include province-by-vote fixed effects, legislator controls, and vote fixed effects interacted with both geographic and demographic controls (coffee, sugar, and cotton suitability, average rainfall, longitude and latitude, distance to coast and to closest river, district area, and average HMI, population density, share of foreigners in 1872, share of free colored and literacy). Standard errors are two-way clustered by district and vote. The fact that the immigration effect appears more cleary nonmonotonic is unsuprising, and most likely due to the influence of Rio de Janeiro (province). In 1872, only five districts have more than 35% of their population enslaved, and they are all situated in the province of Rio de Janeiro, which notoriously struggled to attract immigrants and remained largely anti-abolition until the very end (Conrad, 1972; Klein and Luna, 2009)

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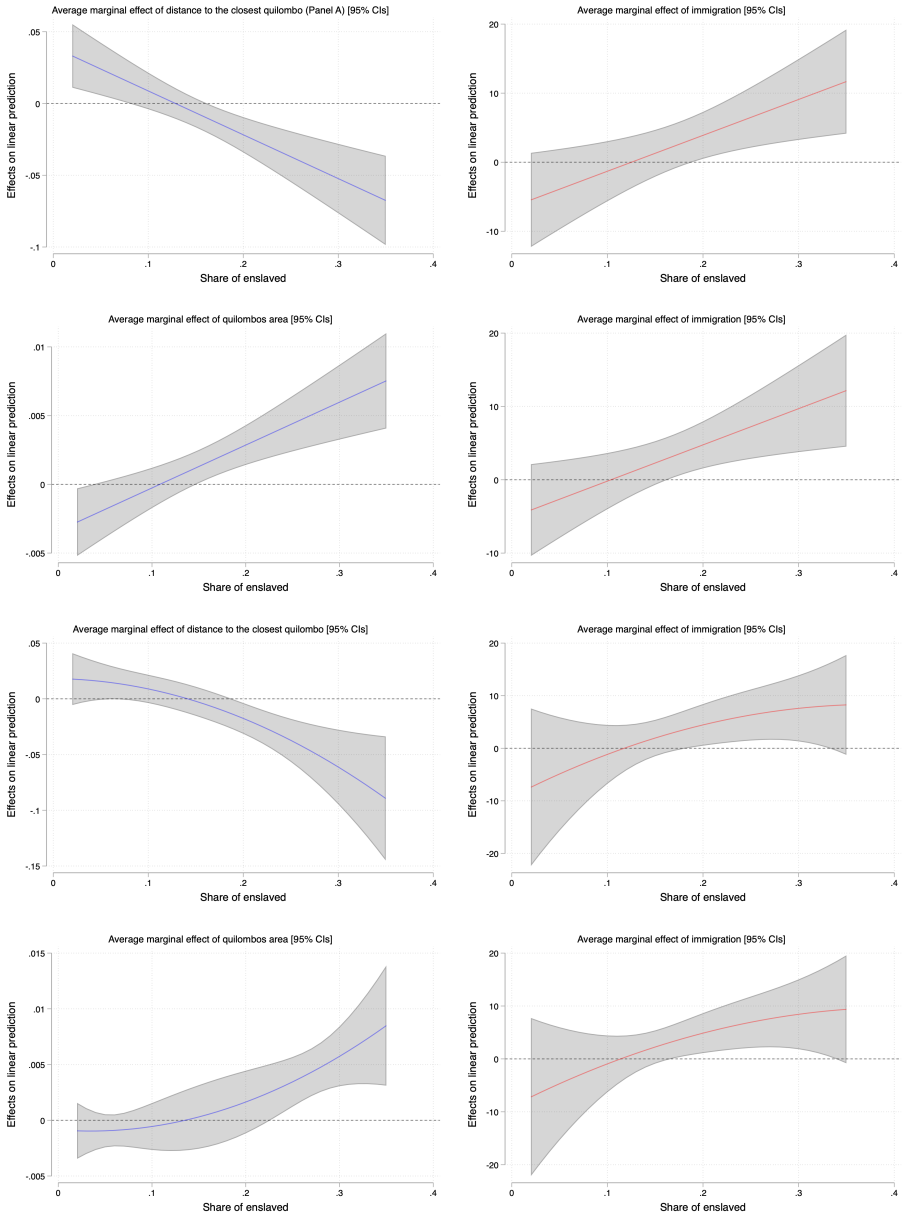


Figure A.12: Marginal effects of outside option and labor demand effects – Excluding top 5 slavery districts

Notes: marginal effects of regressions analogous to the fourth column of panels A and B of table 2, but excluding the 5 largest districts in terms of the relative share of the enslaved population (RJ5, RJ7, RJ9, RJ10, RJ11). Top four panels are linear, bottom four control for slavery polynomial and quadratic interactions. Regressions always include province-by-vote fixed effects, legislator controls, and vote fixed effects interacted with both geographic and demographic controls (coffee, sugar, and cotton suitability, average rainfall, longitude and latitude, distance to coast and to closest river, district area, and average HMI, population density, share of foreigners in 1872, share of free colored and literacy). Standard errors are two-way clustered by district and vote. This corroborates the role of Rio de Janeiro (and the Paraíba Valley) in the Brazilian historiography.

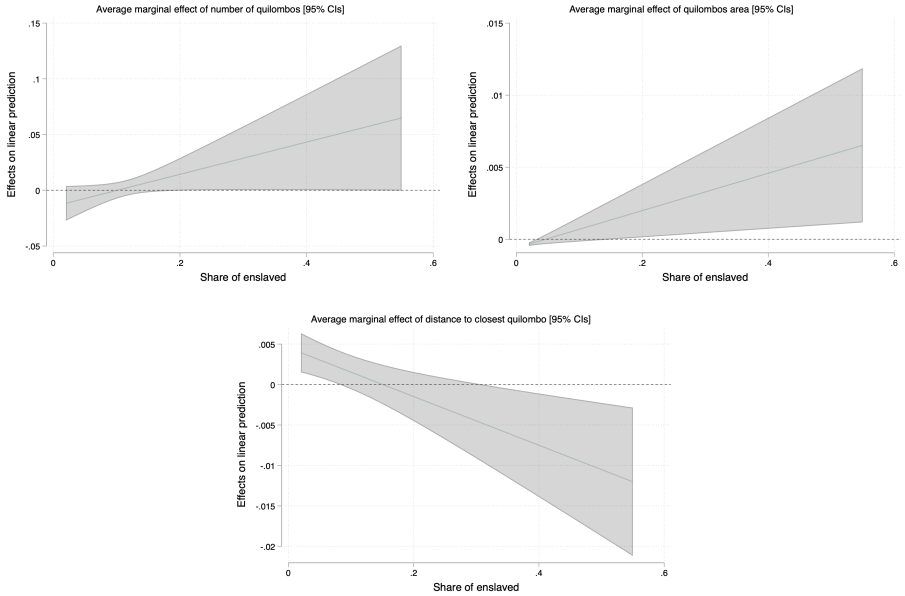


Figure A.13: Marginal effects of alternative proximity to freedom measures

Notes: top left uses total number of quilombos by district, top right uses land area occupied by quilombos (without excluding outliers), bottom center uses distance to closest quilombo (without taking the log). Regressions always include province-by-vote fixed effects, legislator controls, and vote fixed effects interacted with both geographic and demographic controls (coffee, sugar, and cotton suitability, average rainfall, longitude and latitude, distance to coast and to closest river, district area, and average HMI, population density, share of foreigners in 1872, share of free colored and literacy). Standard errors are two-way clustered by district and vote.

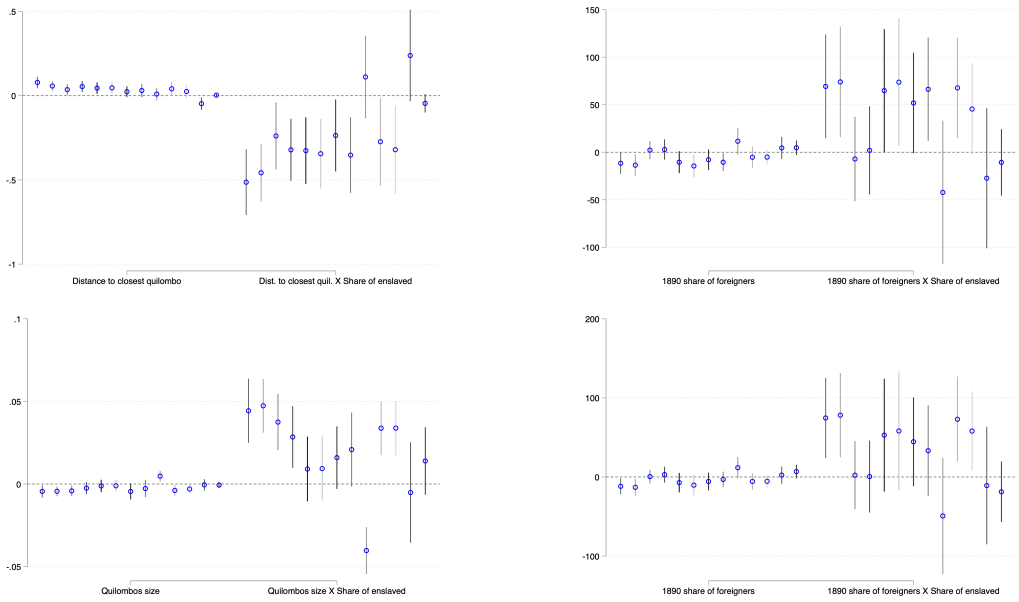


Figure A.14: Dis-aggregation by vote

Notes: This figure plots the coefficients from regressions analogous to equation 1 estimated separately for each of the thirteen votes. Regressions include all controls (un-interacted) and province fixed effects. Table A.11 investigates robustness to alternative interprations of voting decisions and shows that results remain identical.

Table A.11: Outside option and labor demand effects – Selection and sensitivity

	Selection on			Sensitivity to	
	Legislators		Bills	Area poly.	Feigenberg
	𝟙(Absence)	𝟙(Abolition vote)			
	(1)	(2)	(3)	(4)	(5)
Panel A: Outside option is distance to closest quilombo					
Dist. to closest quilombo	0.00147	0.0233	0.0373	0.0317	0.0298
	(0.00728)	(0.0112)**	(0.0120)***	(0.0139)**	(0.0124)**
	{.0060364}	{.0082691}***	{.0087145}***	{.0106136}***	{.0114679}*
Dist. to closest quilombo × Share of enslaved	0.00357	-0.196	-0.306	-0.254	-0.251
	(0.0407)	(0.0684)***	(0.0676)***	(0.0825)***	(0.0773)***
	{.033959}	{.0538287}***	{.0498802}***	{.0632228}***	{.0740705}**
1890 share of foreigners	2.210	-5.585	-7.875	-5.372	-7.442
	(2.480)	(2.600)**	(3.096)**	(3.666)	(4.534)
	{2.709461}	{2.571349}**	{2.521721}***	{2.299916}**	{4.25809}*
1890 share of foreigners × Share of enslaved	-0.339	29.64	46.06	38.25	34.68
	(12.07)	(14.67)**	(16.44)***	(19.47)**	(25.12)
	{13.21556}	{14.65849}**	{13.71498}***	{14.41872}***	{25.63306}
N	1586	1586	1284	1284	1284
Panel B: Outside option is area occupied by quilombos					
Quilombos occupied area	-0.000755	-0.00199	-0.00380	-0.00300	-0.00181
	(0.000567)	(0.00102)*	(0.00109)***	(0.00145)**	(0.00165)
	{.0006265}	{.000833}**	{.0011329}***	{.0013148}**	{.0010945}*
Quilombos occupied area × Slavery prevalence	0.00519	0.0176	0.0316	0.0241	0.0188
	(0.00466)	(0.00702)**	(0.00661)***	(0.00922)***	(0.0103)*
	{.0042294}	{.0056825}***	{.0063468}***	{.0077995}***	{.0078298}**
1890 share of foreigners	1.469	-4.460	-5.210	-3.161	-7.377
	(2.355)	(2.826)	(2.864)*	(3.490)	(4.310)*
	{2.432666}	{2.699087}*	{2.837854}*	{2.618277}	{3.931663}*
1890 share of foreigners × Slavery prevalence	2.399	27.37	37.75	31.02	37.61
	(11.61)	(16.07)*	(15.95)**	(19.47)	(24.20)
	{12.26733}	{16.07921}*	{16.45625}**	{16.69205}*	{23.31553}*
N	1560	1560	1263	1263	1263
Mean dep. var	0.190	0.453	0.589	0.560	0.560
Share of enslaved × Vote FE	✓	✓	✓	✓	✓
Province × Vote FE	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors reported in parentheses. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE. Columns 1 and 2 investigate selection on legislators (respectively looking at absences as the dependent variable and coding absences as zeros in pro-emancipation votes). Column 3 investigates robustness to an alternative interpretation of voting decisions. Column 4 investigates sensitivity to controlling for district area interacted with votes fixed effects as well as nonlinearities in area. Column 5 investigates sensitivity to Feigenberg et al.'s 2023 approach to account for omitted variable bias with heterogeneous treatment.

Table A.12: Other mechanisms – Part 1: Outside option is distance to closest quilombo

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Aggregate effects								
Dist. to closest quilombo	0.0289**	0.0393***	0.0307***	0.0302**	0.0316**	0.0343***	0.0282	0.0403
	(0.0116)	(0.0140)	(0.0115)	(0.0121)	(0.0124)	(0.0127)	(0.0258)	(0.0257)
Dist. to closest quilombo × Share of enslaved	-0.237***	-0.293***	-0.252***	-0.243***	-0.254***	-0.276***	-0.368**	-0.442***
	(0.0693)	(0.0812)	(0.0686)	(0.0728)	(0.0746)	(0.0768)	(0.161)	(0.158)
1890 share of foreigners	-4.220	-6.111*	-4.755	-5.300	-4.841	-5.595	-16.82***	-17.67***
	(3.565)	(3.500)	(3.578)	(3.523)	(3.460)	(3.441)	(3.300)	(3.208)
1890 share of foreigners × Slavery prevalence	31.94*	42.05**	34.61*	37.01**	34.42*	35.09*	128.9***	130.3***
	(18.66)	(18.47)	(19.06)	(18.54)	(18.59)	(18.35)	(17.93)	(17.33)
Share of capitalists	-13.06**							
	(5.752)							
Share of industrialists		24.70						
		(17.63)						
Dist. closest insurrection			0.221					
			(0.304)					
Dist. closest Lavoura club				0.137				
				(0.206)				
Dist. closest abolitionist club					-0.0934			
					(0.206)			
Dist. closest abolitionist news						-0.331		
						(0.204)		
Number of destroyed quilombos								-0.0376
								(0.0230)
Panel B: Heterogeneity by slavery intensity								
Dist. to closest quilombo	0.0288**	0.0322**	0.0311***	0.0304**	0.0323***	0.0344***	0.0282	0.0813***
	(0.0115)	(0.0149)	(0.0114)	(0.0121)	(0.0121)	(0.0130)	(0.0258)	(0.0305)
Dist. to closest quilombo × Slavery prevalence	-0.235***	-0.243***	-0.254***	-0.244***	-0.262***	-0.277***	-0.368**	-0.677***
	(0.0686)	(0.0918)	(0.0683)	(0.0740)	(0.0740)	(0.0814)	(0.161)	(0.190)
1890 share of foreigners	-4.128	-6.672*	-4.390	-5.250	-4.181	-5.523	-16.82***	-17.78***
	(3.549)	(3.503)	(3.526)	(3.679)	(3.864)	(3.707)	(3.300)	(3.096)
1890 share of foreigners × Slavery prevalence	31.79*	47.70***	33.53*	36.79*	31.07	34.71*	128.9***	131.0***
	(18.65)	(18.34)	(18.76)	(19.51)	(21.68)	(20.55)	(17.93)	(16.74)
Share of capitalists	-9.008							
	(19.66)							
Share of capitalists × Slavery prevalence	-28.94							
	(125.9)							
Share of industrialists		-31.40						
		(36.22)						
Share of industrialists × Slavery prevalence		433.2*						
		(226.7)						
Dist. closest insurrection			0.472					
			(0.402)					
Dist. closest insurrection × Slavery prevalence			-2.590					
			(2.580)					
Dist. closest Lavoura club				0.149				
				(0.259)				
Dist. closest Lavoura club × Slavery prevalence				-0.0971				
				(1.280)				
Dist. closest abolitionist club					0.0126			
					(0.282)			
Dist. closest abolitionist club × Slavery prevalence					-0.726			
					(1.550)			
Dist. closest abolitionist news						-0.322		
						(0.261)		
Dist. closest abolitionist news × Slavery prevalence						-0.0726		
						(1.160)		
Number of destroyed quilombos								-0.572***
								(0.147)
Number of destroyed quilombos × Slavery prevalence								2.859***
								(0.779)
N	1284	1284	1284	1284	1284	1284	335	335
Mean dep. var	0.560	0.560	0.560	0.560	0.560	0.560	0.531	0.531
Share of enslaved × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Province × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors reported in parentheses. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average Human Mobility Index. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE. Covariate distances expressed in thousands of km. Columns 7 and 8 restrict the sample to provinces with destroyed quilombos observations: Goías, Mato Grosso, Minas Gerais, and São Paulo. Column 7 is baseline and column 8 includes destroyed quilombos.

Table A.13: Other mechanisms – Part 2: Outside option is quilombos occupied area

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Aggregate effects								
Quilombos occupied area	-0.00171	-0.00245*	-0.00226*	-0.00251*	-0.00204*	-0.00169	-0.0106***	-0.0122***
	(0.00129)	(0.00132)	(0.00135)	(0.00137)	(0.00123)	(0.00127)	(0.00318)	(0.00315)
Quilombos occupied area × Share of enslaved	0.0181**	0.0220**	0.0208**	0.0210**	0.0217**	0.0198**	0.0164	0.0215*
	(0.00886)	(0.00890)	(0.00875)	(0.00854)	(0.00874)	(0.00837)	(0.0139)	(0.0124)
1890 share of foreigners	-2.249	-4.002	-3.099	-4.085	-2.379	-3.191	-20.97***	-21.71***
	(3.636)	(3.560)	(3.516)	(3.732)	(3.435)	(3.497)	(3.556)	(3.195)
1890 share of foreigners × Share of enslaved	25.04	34.13*	29.22	33.80*	24.76	27.08	170.2***	172.4***
	(20.05)	(19.57)	(19.71)	(20.47)	(19.64)	(20.14)	(21.80)	(19.64)
Share of capitalists	-12.55*							
	(6.522)							
Share of industrialists		18.74						
		(16.41)						
Dist. closest insurrection			0.0930					
			(0.333)					
Dist. closest Lavoura club				0.280				
				(0.259)				
Dist. closest abolitionist club					-0.289			
					(0.206)			
Dist. closest abolitionist news						-0.262		
						(0.199)		
Number of destroyed quilombos								-0.0444**
								(0.0180)
Panel B: Heterogeneity by slavery intensity								
Quilombos occupied area	-0.00172	-0.00243*	-0.00407**	-0.00223	-0.00218	-0.00150	-0.0106***	-0.0117***
	(0.00131)	(0.00127)	(0.00175)	(0.00158)	(0.00140)	(0.00152)	(0.00318)	(0.00330)
Quilombos occupied area × Share of enslaved	0.0178**	0.0232***	0.0317***	0.0199**	0.0227**	0.0187*	0.0164	0.0205
	(0.00847)	(0.00804)	(0.0111)	(0.00915)	(0.0106)	(0.0101)	(0.0139)	(0.0125)
1890 share of foreigners	-2.195	-5.941*	-2.702	-4.519	-1.890	-3.740	-20.97***	-20.07***
	(3.574)	(3.480)	(3.335)	(3.861)	(3.837)	(3.722)	(3.556)	(3.474)
1890 share of foreigners × Share of enslaved	24.97	47.28**	30.62	35.35*	22.43	29.59	170.2***	162.2***
	(20.00)	(19.01)	(18.62)	(21.08)	(22.26)	(21.60)	(21.80)	(22.84)
Share of capitalists	-9.128							
	(21.52)							
Share of capitalists × Slavery prevalence	-24.29							
	(137.3)							
Share of industrialists		-55.21						
		(35.60)						
Share of industrialists × Slavery prevalence		585.8**						
		(241.6)						
Dist. closest insurrection			1.190*					
			(0.617)					
Dist. closest insurrection × Slavery prevalence			-8.630**					
			(0.00440)					
Dist. closest Lavoura club				0.138				
				(0.395)				
Dist. closest Lavoura club × Slavery prevalence				0.867				
				(1.470)				
Dist. closest abolitionist club					-0.204			
					(0.295)			
Dist. closest abolitionist club × Slavery prevalence					-0.565			
					(1.870)			
Dist. closest abolitionist news						-0.348		
						(0.301)		
Dist. closest abolitionist news × Slavery prevalence						0.508		
						(1.360)		
Number of destroyed quilombos								-0.336**
								(0.162)
Number of destroyed quilombos × Slavery prevalence								1.581*
								(0.879)
N	1263	1263	1263	1263	1263	1263	335	335
Mean dep. var	0.560	0.560	0.560	0.560	0.560	0.560	0.531	0.531
Share of enslaved × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Province × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors reported in parentheses. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE. Covariate distances expressed in thousands of km. Columns 7 and 8 restrict the sample to provinces with destroyed quilombos observations: Goías, Mato Grosso, Minas Gerais, and São Paulo. Column 7 is baseline and column 8 includes destroyed quilombos.

Table A.14: Cultural influence – Part 1: Outside option is distance to closest quilombo

	Baseline	1872 nationality shares		Sponsored settlements		1890 Religion shares	
				𝟙(Abolition vote)			
	(1)		(2)		(3)		(4)
Dist. to closest quilombo	0.0310***		0.0341***		0.0316***		0.0308**
	(0.0117)		(0.0120)		(0.0117)		(0.0120)
Dist. to closest quilombo × Slavery prevalence	-0.251***		-0.263***		-0.264***		-0.251***
	(0.0701)		(0.0667)		(0.0655)		(0.0730)
1890 share of foreigners	-5.097		-4.735		-6.946*		-8.001
	(3.495)		(3.499)		(3.966)		(5.062)
1890 share of foreigners × Share of enslaved	35.85*		35.91**		48.91***		46.59**
	(18.45)		(17.80)		(18.89)		(21.12)
		Germany	-0.324	Azores 1750s	0.0754	Orth. Catholic	-56.25
			(0.611)		(0.0499)		(117.5)
		Austria	-1.518	Spain 1870s	0.405*	Evangelical	-0.608
			(4.460)		(0.238)		(7.597)
		Argentina	-0.380	Germany 1800s	0.110**	Presbyterian	-163.0
			(1.154)		(0.0553)		(218.2)
		Belgium	13.34	Germany 1870s	-0.0687	Other protestants	1.235
			(8.880)		(0.0513)		(2.139)
		France	0.496	Netherlands 1870s	-0.163	Islam	564.6
			(1.041)		(0.178)		(1188.7)
		Greece	-6.472	Italy 1870s	0.0550	Positivist	120.8
			(6.271)		(0.0490)		(209.3)
		Spain	-2.929*	Japan 1870s	0.00697	None	25.56
			(1.677)		(0.102)		(25.30)
		Netherlands	5.664**	Others 1800s	1.414***		
			(2.792)		(0.532)		
		UK	-0.726	Others 1870s	0.0268		
			(0.702)		(0.0509)		
		Italy	-0.967	Switzerland 1800s	-0.255***		
			(0.678)		(0.0960)		
		Japan	382.2				
			(390.2)				
		Mexico	-159.1				
			(119.2)				
		USA	-2.035***				
			(0.682)				
		Paraguay	-0.304				
			(1.017)				
		Portugal	-0.386				
			(0.560)				
		Russia	13.03**				
			(6.045)				
N	1284		1284		1284		1284
Mean dep. var	0.560		0.560		0.560		0.560
Share of enslaved × Vote FE	✓		✓		✓		✓
Province × Vote FE	✓		✓		✓		✓
Geographic controls × Vote FE	✓		✓		✓		✓
Demographic controls × Vote FE	✓		✓		✓		✓
Legislator controls	✓		✓		✓		✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors reported in parentheses. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE. Column 1 provides baseline estimates. Column 2 controls for the shares of immigrants from major sending countries within a district's population in 1872. Column 3 controls for state-sponsored settlements (only observed by waves). Column 4 controls for religion shares in 1890 (excluding roman catholics).

Table A.15: Cultural influence – Part 2: Outside option is quilombos occupied area

	Baseline	1872 nationality shares		Sponsored settlements		1890 Religion shares	
				𝟙(Abolition vote)			
	(1)		(2)		(3)		(4)
Quilombos occupied area	-0.00213*		-0.00181*		-0.00222*		-0.00194
	(0.00123)		(0.00107)		(0.00123)		(0.00123)
Quilombos occupied area × Slavery prevalence	0.0205**		0.0163***		0.0217***		0.0167
	(0.00837)		(0.00610)		(0.00764)		(0.0109)
1890 share of foreigners	-3.214		-3.179		-2.277		-6.377
	(3.489)		(2.883)		(3.759)		(4.802)
1890 share of foreigners × Share of enslaved	29.51		30.29*		33.29		41.01*
	(19.48)		(16.20)		(20.41)		(21.53)
		Germany	-0.123	Azores 1750s	0.118**	Orth. Catholic	-96.84
			(0.701)		(0.0530)		(115.2)
		Austria	-3.481	Spain 1870s	0.405*	Evangelical	-2.383
			(4.116)		(0.256)		(7.371)
		Argentina	-0.450	Germany 1800s	0.141**	Presbyterian	-50.05
			(1.116)		(0.0553)		(190.5)
		Belgium	13.02	Germany 1870s	-0.0687	Other protestants	0.851
			(9.042)		(0.0567)		(2.366)
		France	0.792	Netherlands 1870s	-0.170	Islam	959.4
			(0.956)		(0.168)		(1104.3)
		Greece	-3.916	Italy 1870s	0.0129	Positivist	33.72
			(5.467)		(0.0461)		(242.6)
		Spain	-1.836	Japan 1870s	0.191*	None	28.74
			(1.847)		(0.101)		(37.67)
		Netherlands	5.956**	Others 1800s	1.714***		
			(2.677)		(0.543)		
		UK	-0.275	Others 1870s	0.0168		
			(0.808)		(0.0432)		
		Italy	-0.509	Switzerland 1800s	-0.229**		
			(261.7)		(0.0910)		
		Japan	502.1*				
			(390.2)				
		Mexico	-145.4				
			(123.1)				
		USA	-1.453*				
			(0.871)				
		Paraguay	-0.193				
			(0.918)				
		Portugal	-0.0982				
			(0.708)				
		Russia	14.80**				
			(6.157)				
N	1263		1263		1263		1263
Mean dep. var	0.560		0.560		0.560		0.560
Share of enslaved × Vote FE	✓		✓		✓		✓
Province × Vote FE	✓		✓		✓		✓
Geographic controls × Vote FE	✓		✓		✓		✓
Demographic controls × Vote FE	✓		✓		✓		✓
Legislator controls	✓		✓		✓		✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors reported in parentheses. Geographic controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE. Column 1 provides baseline estimates. Column 2 controls for the shares of immigrants from major sending countries within a district's population in 1872. Column 3 controls for state-sponsored settlements (only observed by waves). Column 4 controls for religion shares in 1890 (excluding roman catholics).

Table A.16: No interaction regressions

	OLS	TSLS		Reduced form
		1st stage	2nd stage	
	𝟙(Abolition vote)	Endogenous var.	𝟙(Abolition vote)	𝟙(Abolition vote)
	(1)	(2)	(3)	(4)
Panel A: Endogenous variable is area occupied by quilombos, instrumented with substitution instrument				
Quilombos occupied area	0.000488		0.00118	
	(0.000786)		(0.00257)	
1890 share of foreigners	1.813	-93.40	1.894	1.784
	(1.437)	(143.0)	(1.503)	(1.461)
SubIV		646.8***		0.763
		(227.6)		(1.671)
N	1263	1263	1263	1263
Mean dep. var	0.56	23.80	0.56	0.56
Endog test p-value			0.759	
MP F-stat			8.075	
Under-id test p-value			0.022	
Weak IV-robust AR interval			[-0.0045, 0.0089]	
Panel B: Endogenous variable is 1890 share of foreigners, instrumented with shift-share instrument				
Quilombos occupied area	0.000488	0.00002	0.00062	0.00048
	(0.000786)	(0.00003)	(0.00074)	(0.00079)
1890 share of foreigners	1.813		1.913	
	(1.437)		(2.632)	
SSIV (/1000)		0.00134***		0.0035
		(0.00028)		(0.0036)
N	1263	1263	1263	1263
Mean dep. var	0.56	0.019	0.56	0.56
Endog test p-value			0.870	
MP F-stat			22.24	
Under-id test p-value			0.006	
Weak IV-robust AR interval			[-0.928, 4.474]	
Province × Vote FE	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors are two-way clustered by district and vote. SubIV is the baseline substitution instrument: Ruggedness × [1 − Remoteness]. SSIV is the baseline unscaled (not divided by district population) origin country-based shift-share instrument. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE. For the endogeneity test (heteroskedasticity-robust), the null hypothesis is that there is no difference between the OLS and the IV regression. The MP F-stat is the cluster- and heteroskedasticity-robust Montiel Olea & Pflueger F-statistic. The under-identification test is the Kleibergen-Paap rk LM statistic. Under the null hypothesis, the equation is underidentified. Weak IV-robust AR interval stands for weak instrumental variable-robust Anderson-Rubin interval and is the (cluster-, heteroskedasticity- and weak identification-robust) set of all values for the parameter of interest such that the null hypothesis cannot be rejected at the 95% significance level.

Table A.17: Alternative proximity to freedom measures (OLS and TSLS)

	Panel A: Proximity to freedom is number of quilombos				*Panel B: Proximity to freedom is distance to closest quilombo (not logged)*			
	OLS	1st stage		2nd stage	OLS	1st stage		2nd stage
	𝟙(Abolition vote)	# Quilombos	# Quilombos × Sh. of enslaved	𝟙(Abolition vote)	𝟙(Abolition vote)	Dist. closest quil.	Dist. closest quil. × Sh. of enslaved	𝟙(Abolition vote)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Proximity to freedom	-0.0155*			-0.0287	0.00400***			0.0000369
	(0.00883)			(0.0279)	(0.00105)			(0.00621)
Proximity to freedom × Share of enslaved	0.153**			0.263*	-0.0280***			-0.0310
	(0.0755)			(0.144)	(0.00905)			(0.0278)
1890 share of foreigners	-7.406**	116.3*	31.53***	-9.431*	-5.924*	-466.8*	-133.2**	-8.586*
	(3.675)	(66.33)	(9.894)	(4.966)	(3.390)	(264.3)	(54.39)	(4.428)
1890 share of foreigners × Share of enslaved	49.99**	-752.3*	-177.5***	59.96**	41.45**	2963.8**	737.8***	57.63**
	(21.40)	(429.2)	(58.02)	(29.33)	(20.06)	(1350.6)	(284.0)	(27.81)
SubIV		-146.1**	-20.78**			537.7**	41.74	
		(66.85)	(9.167)			(258.2)	(38.10)	
SubIV × Share of enslaved		573.6**	123.1***			-3018.5***	-517.2**	
		(265.1)	(38.34)			(1090.7)	(219.5)	
N	1284	1284	1284	1284	1284	1284	1284	1284
Endog test p-value				0.693				0.505
SW F-stat		16.55	7.72			4.21	8.82	
Under-id test p-value				0.021				0.045
Weak IV-robust AR interval				[0.0384, 0.4881]				[-0.0722, 0.0115]
Province × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average HMI, average TRI (ruggedness), and ATT (remoteness). Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE, and TRI and ATT are also interacted with slavery intensity.

Table A.18: Alternative SSIV versions

	Panel A: SSIV is unscaled origin country-based			*Panel B: SSIV is unscaled origin religion-based*			*Panel C: SSIV is scaled origin country-based*		
	1st stage		2nd stage	1st stage		2nd stage	1st stage		2nd stage
	Sh. foreigners	Sh. foreigners × Sh. enslaved	𝟙(Abolition vote)	Sh. foreigners	Sh. foreigners × Sh. enslaved	𝟙(Abolition vote)	Sh. foreigners	Sh. foreigners × Sh. enslaved	𝟙(Abolition vote)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1890 share of foreigners			-4.220 (5.530) {3.206}			-2.589 (5.870) {3.556}			-12.29 (8.147) {5.371}**
1890 share of foreigners × Share of enslaved			46.81 (24.44)* {19.22}**			28.69 (27.50) {15.80}*			65.44 (36.65)* {26.77}**
Quilombos occupied area	0.00925 (0.0397) {0.0305}	-0.000961 (0.00856) {0.000961}	-0.00249 (0.00145)* {0.001042}**	0.0743 (0.0447)* {0.0296}**	0.0186 (0.00930)** {0.00499}***	-0.00239 (0.00142)* {0.0010}**	0.0799 (0.0605) {0.0400}**	0.0120 (0.0108) {0.000765}	-0.00246 (0.00150) {0.0010}**
Quilombos occupied area × Share of enslaved	0.000165 (0.000225) {0.00019}	0.0000490 (0.0000640) {0.000049}	0.0234 (0.00895)*** {0.00636}***	-0.000207 (0.000223) {0.000134}	-0.0000751 (0.0000519) {0.0000232}***	0.0229 (0.00877)*** {0.00635}***	-0.0000794 (0.000370) {0.000183}	0.0000848 (0.0000652) {0.000042}	0.0249 (0.00948)*** {0.00623}***
SSIV	0.00146 (0.000421)*** {0.000358}***	0.0000243 (0.0000918) {0.0000806}		0.0000261 (0.0000801) {0.0000911}	-0.0000496 (0.0000228)** {0.0000225}**		-0.465 (0.157)*** {0.112}***	-0.1000 (0.0272)*** {0.0202}***	
SSIV × Share of enslaved	0.000448 (0.00154) {0.00144}	0.00164 (0.000390)*** {0.000419}***		0.000604 (0.000445) {0.000476}	0.000428 (0.000119)*** {0.000113}***		1.108 (0.371)*** {0.282}***	0.300 (0.0653)*** {0.0575}***	
N	1263	1263	1263	1263	1263	1263	1263	1263	1263
Mean dep. var	0.0191	0.0036	0.56	0.0191	0.0036	0.56	0.0191	0.0036	0.56
Endog test p-value			0.143			0.137			0.198
SW F-stat	29.59	11.65		24.80	11.29		10.23	11.90	
Under-id test p-value			0.1047			0.0835			0.0219
Weak IV-robust AR interval			[24.40, 123.5]			[24.16, 122.3]			[17.99, 114.4]
Province × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Geographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls × Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Legislator controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses, and Conley standard errors (with a 250km window) are reported in curly brackets. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average HMI, average TRI (ruggedness), and ATT (remoteness). Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE, and TRI and ATT are also interacted with slavery intensity.

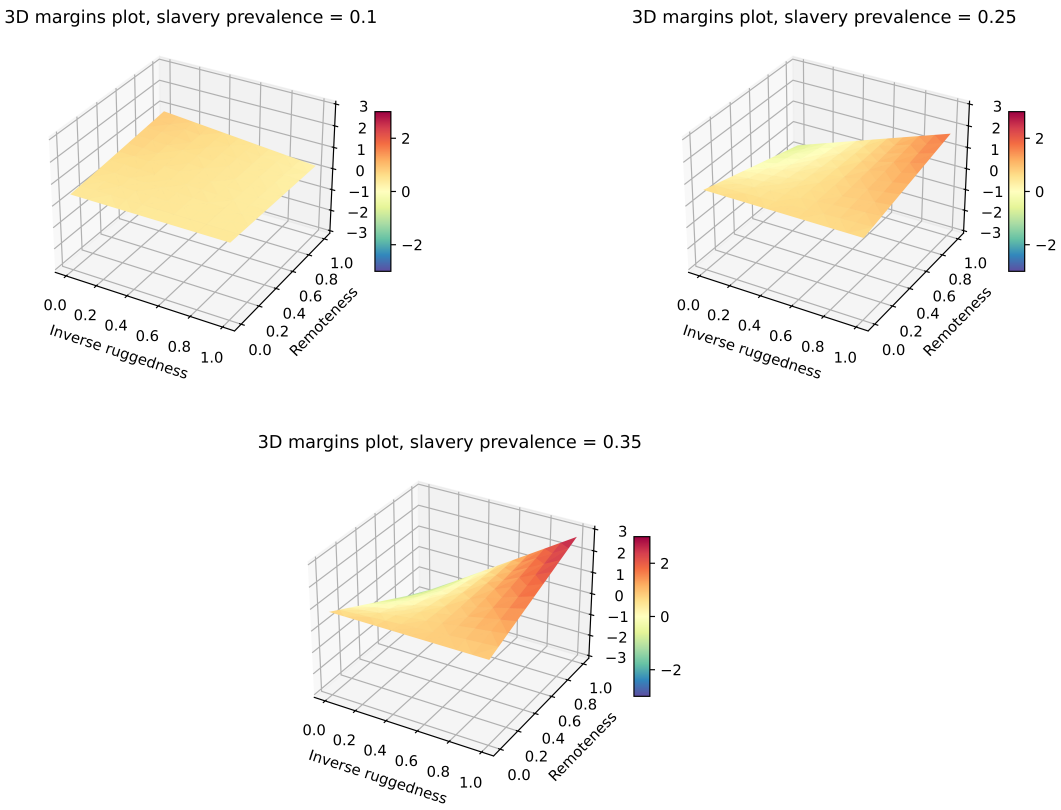


Figure A.15: 3D Margins – Reduced form

Notes: Reduced form margins of SubIV on voting decisions for different levels of slavery intensity: $S_{ij} = 0.1$ (top left), $S_{ij} = 0.25$ (top right), and $S_{ij} = 0.35$ (bottom). As slavery intensity increases, the substitution effect between ruggedness and remoteness becomes a significant predictor of legislators voting decisions. With high slavery intensive, the combination of low remoteness and high ruggedness is a strong predictor of pro-emancipation voting decisions. However, ruggedness and remoteness do not predict voting decisions within low slavery-intensity districts.

A.5 Additional maps and figures

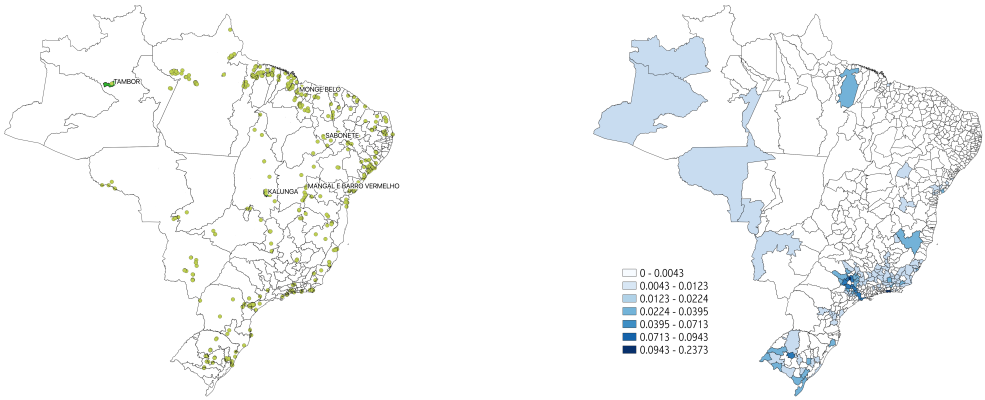


Figure A.16: Quilombos and share of foreigners in 1890 with district- and municipality-level boundaries respectively

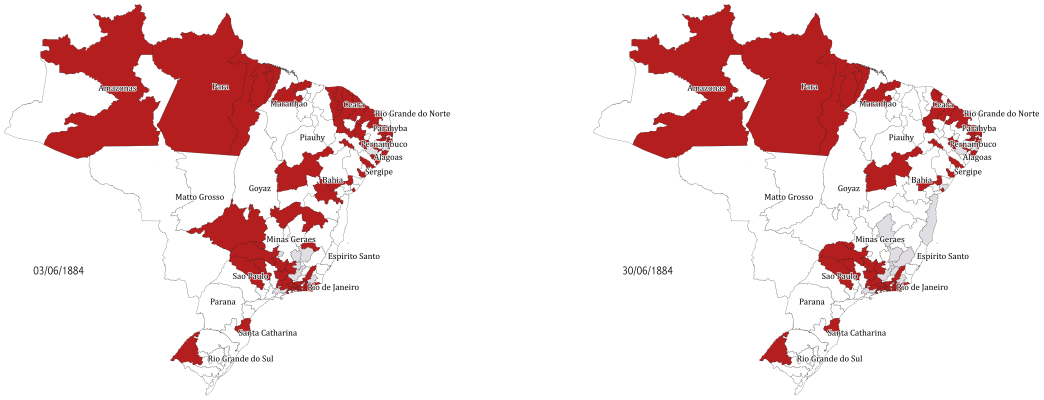


Figure A.17: Abolition maps – other bills (1)

Table A.19: Shares used as instruments – origin countries SSIV version

	Panel A: Un-interacted shares			*Panel B: Interacted shares*		
	1st stage		2nd stage	1st stage		2nd stage
	Share of foreigners	Share of foreigners × Sh. of enslaved	𝟙(Abolition vote)	Share of foreigners	Share of foreigners × Sh. of enslaved	𝟙(Abolition vote)
	(1)	(2)	(3)	(4)	(5)	(6)
1890 sh. foreigners			-5.005			-4.389
			(3.677)			(3.233)
1890 sh. foreigners × Sh. enslaved			51.77**			32.46*
			(21.23)			(19.22)
Quilombos area	0.0000668*	0.00000913	-0.00183	0.0000851***	0.0000122**	-0.00182
	(0.0000400)	(0.00000810)	(0.00156)	(0.0000326)	(0.00000525)	(0.00154)
Quilombos area × Sh. enslaved	-0.000581**	-0.0000889	0.0210*	-0.000281	-0.0000402	0.0201*
	(0.000277)	(0.0000629)	(0.0109)	(0.000245)	(0.0000406)	(0.0108)
Share Germany	-0.213	-0.0709**		0.133	-0.281	
	(0.204)	(0.0341)		(1.584)	(0.298)	
Share Austria	-0.111	0.00762		6.098***	1.009***	
	(0.133)	(0.0253)		(0.951)	(0.175)	
Share Argentina	0.222***	0.0333***		-0.0524	0.114	
	(0.0484)	(0.00826)		(2.151)	(0.386)	
Share Belgium	-0.254	0.00216		-1.604*	-0.133	
	(0.208)	(0.0459)		(0.958)	(0.164)	
Share Spain	-0.351	-0.212**		-0.601	-0.0640	
	(0.406)	(0.0829)		(1.600)	(0.277)	
Share Holland	0.341**	0.0892***		-0.644	-0.105	
	(0.146)	(0.0265)		(0.414)	(0.0825)	
Share Hungary	-0.0297	-0.0235		-0.504	-0.123	
	(0.0993)	(0.0193)		(0.436)	(0.0827)	
Share England	0.360	0.0602		2.359***	0.461***	
	(0.444)	(0.0851)		(0.751)	(0.133)	
Share Italy	1.576***	0.475***		-3.442***	-0.564***	
	(0.261)	(0.0546)		(1.157)	(0.197)	
Share Japan	0.729***	0.202***		-4.711	-0.630	
	(0.243)	(0.0476)		(4.217)	(0.714)	
Share U.S.A.	0.577***	0.105***		-1.696***	-0.273***	
	(0.124)	(0.0251)		(0.392)	(0.0752)	
Share Middle East	-0.00431	0.0440**		7.968***	1.699***	
	(0.121)	(0.0224)		(2.171)	(0.382)	
Share Paraguay	-0.868***	-0.157***		-1.623***	-0.252**	
	(0.212)	(0.0360)		(0.614)	(0.110)	
Share Portugal	-2.821***	-0.386***		4.049*	0.361	
	(0.713)	(0.128)		(2.250)	(0.377)	
Share Russia	0.261	0.0343		0.0374	-0.0499	
	(0.169)	(0.0310)		(0.394)	(0.0708)	
Share Switzerland	0.109***	0.0511***		-2.723***	-0.627***	
	(0.0324)	(0.00677)		(0.627)	(0.112)	
Share Sweden	-0.0824	-0.0345**		-5.458***	-0.792***	
	(0.0961)	(0.0168)		(1.240)	(0.250)	
Share Turkey	-0.714***	-0.144***		-8.963	-1.961	
	(0.154)	(0.0311)		(9.751)	(1.622)	
Share Others	2.998***	0.296*		5.195**	0.796**	
	(0.913)	(0.168)		(2.128)	(0.400)	

Cont.

	Continued: Shares used as instrument – origin countries SSIV version					
	Panel A: Un-interacted shares			*Panel B: Interacted shares*		
	1st stage		2nd stage	1st stage		2nd stage
	Share of foreigners	Share of foreigners × Sh. of enslaved	𝟙(Abolition vote)	Share of foreigners	Share of foreigners × Sh. of enslaved	𝟙(Abolition vote)
Share of enslaved × Share Germany				4.257	3.309	
				(12.31)	(2.374)	
Share of enslaved × Share Austria				-46.45***	-7.191***	
				(6.751)	(1.295)	
Share of enslaved × Share Argentina				-6.823	-2.508	
				(16.07)	(2.886)	
Share of enslaved × Share Belgium				10.17**	1.400*	
				(4.373)	(0.801)	
Share of enslaved × Share Spain				-8.161	-1.484	
				(10.64)	(2.038)	
Share of enslaved × Share Holland				2.741	0.176	
				(3.091)	(0.573)	
Share of enslaved × Share Hungary				5.042	0.996	
				(3.372)	(0.652)	
Share of enslaved × Share England				-11.14**	-2.287***	
				(4.476)	(0.805)	
Share of enslaved × Share Italy				20.33***	3.401***	
				(5.863)	(1.002)	
Share of enslaved × Share Japan				18.57	1.527	
				(13.63)	(2.504)	
Share of enslaved × Share U.S.A.				15.48***	2.798***	
				(3.338)	(0.649)	
Share of enslaved × Share Middle East				-32.67***	-6.828***	
				(10.38)	(1.829)	
Share of enslaved × Share Paraguay				2.894	-0.115	
				(5.148)	(0.913)	
Share of enslaved × Share Portugal				-24.23*	-1.085	
				(14.15)	(2.265)	
Share of enslaved × Share Russia				7.501***	1.866***	
				(2.207)	(0.400)	
Share of enslaved × Share Switzerland				6.811***	1.414***	
				(1.818)	(0.338)	
Share of enslaved × Share Sweden				32.65***	4.693***	
				(6.922)	(1.385)	
Share of enslaved × Share Turkey				66.61	14.37	
				(73.63)	(12.24)	
Share of enslaved × Share Others				-18.60	-4.146	
				(17.46)	(3.349)	
N	1263	1263	1263	1263	1263	1263
Mean dep. var	0.0191	0.0036	0.56	0.0191	0.0036	0.56
SW F-stat	79.11	50.58		1117	1547	
Hansen J p-val.			0.369			0.114
Hansen J p-val. (LIML)			0.481			0.115
Hansen J p-val. (BTSLS)			0.385			0.114
Prov. × Vote FE	✓	✓	✓	✓	✓	✓
Geo. × Vote FE	✓	✓	✓	✓	✓	✓
Demo. × Vote FE	✓	✓	✓	✓	✓	✓
Legi. cont.	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average HMI, average TRI (ruggedness), and ATT (remoteness). Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE, and TRI and ATT are also interacted with slavery intensity. Regressions are re-estimated using LIML and BTSLS, which yields nearly identical results. Coefficients are not reported (available upon request) but Hansen-J p-values from these additional regressions are reported.

Table A.20: Shares used as instruments – origin religions SSIV version

	Panel A: Un-interacted shares			*Panel B: Interacted shares*		
	1st stage		2nd stage	1st stage		2nd stage
	Share of foreigners	Share of foreigners × Sh. of enslaved	𝟙(Abolition vote)	Share of foreigners	Share of foreigners × Sh. of enslaved	𝟙(Abolition vote)
	(1)	(2)	(3)	(4)	(5)	(6)
1890 sh. foreigners			-11.30*			-8.948**
			(6.396)			(4.315)
1890 sh. foreigners × Sh. enslaved			72.96**			71.96***
			(37.15)			(23.69)
Quilombos area	0.0000966**	0.0000139	-0.00196	0.0000869**	0.0000192*	-0.00191
	(0.0000472)	(0.0000120)	(0.00153)	(0.0000428)	(0.00000990)	(0.00155)
Quilombos area × Sh. enslaved	-0.000356	-0.0000581	0.0213**	-0.000457	-0.000130	0.0215**
	(0.000385)	(0.0000992)	(0.0108)	(0.000394)	(0.0000918)	(0.0109)
Sh. Roman Catholicism	0.573***	0.0924**		0.401	-0.359**	
	(0.207)	(0.0421)		(0.777)	(0.149)	
Sh. Orthodox Catholicism	0.0971	-0.0518		1.457**	0.289***	
	(0.268)	(0.0501)		(0.699)	(0.104)	
Sh. Evangelicalism	0.621***	0.131***		0.987	0.109	
	(0.232)	(0.0467)		(1.138)	(0.143)	
Sh. Presbyterianism	0.120***	0.00605		-0.129	-0.154*	
	(0.0431)	(0.00902)		(0.652)	(0.0921)	
Sh. Protestantism (others)	-0.465***	-0.0741***		-1.846	-0.282*	
	(0.158)	(0.0283)		(1.216)	(0.149)	
Sh. Islam	0.286	0.0398		-0.669	-0.244	
	(0.180)	(0.0375)		(1.076)	(0.224)	
Sh. Positivism	-0.579***	-0.0911**		-0.178	0.0209	
	(0.167)	(0.0384)		(0.176)	(0.0358)	
Sh. None/Others	-0.0417	0.00355		-0.243*	-0.0415	
	(0.0414)	(0.0119)		(0.126)	(0.0323)	

Cont.

Continued: Shares used as instrument – origin religions SSIV version

	Panel A: Un-interacted shares			*Panel B: Interacted shares*		
	1st stage		2nd stage	1st stage		2nd stage
	Share of foreigners	Share of foreigners × Sh. of enslaved	𝟙(Abolition vote)	Share of foreigners	Share of foreigners × Sh. of enslaved	𝟙(Abolition vote)
Share of enslaved × Share Roman Catholicism				0.241	2.169***	
				(3.548)	(0.631)	
Sh. enslaved × Sh. Orthodox Catholicism				-6.027**	-1.445***	
				(2.815)	(0.527)	
Sh. enslaved × Sh. Evangelicalism				-1.927	0.127	
				(5.718)	(0.722)	
Sh. enslaved × Sh. Presbyterianism				1.571	1.213*	
				(5.043)	(0.712)	
Sh. enslaved × Sh. Protestantism (others)				11.08	1.743*	
				(8.332)	(1.014)	
Sh. enslaved × Sh. Islam				6.179	2.023	
				(7.994)	(1.646)	
Sh. enslaved × Sh. Positivism				-3.162	-0.723*	
				(1.990)	(0.369)	
Sh. of enslaved × Sh. None/Others				2.061*	0.358	
				(1.122)	(0.282)	
N	1263	1263	1263	1263	1263	1263
Mean dep. var	0.0191	0.0036	0.56	0.0191	0.0036	0.56
SW F-stat	3.46	3.53		16.80	13.08	
Hansen J p-val.			0.603			0.936
Hansen J p-val. (LIML)			0.606			0.937
Hansen J p-val. (BTSLS)			0.604			0.936
Prov. × Vote FE	✓	✓	✓	✓	✓	✓
Geo. × Vote FE	✓	✓	✓	✓	✓	✓
Demo. × Vote FE	✓	✓	✓	✓	✓	✓
Legi. cont.	✓	✓	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average HMI, average TRI (ruggedness), and ATT (remoteness). Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE, and TRI and ATT are also interacted with slavery intensity. Regressions are re-estimated using LIML and BTSLS, which yields nearly identical results. Coefficients are not reported (available upon request) but Hansen-J p-values from these additional regressions are reported.

Table A.21: SSIV excluding influential origin countries/religions (2nd stages)

	Origin country-based SSIV			*Origin religion-based SSIV*
	No Italians	No Portuguese	No Spanish	No Roman Catholics
	𝟙(Abolition vote)	𝟙(Abolition vote)	𝟙(Abolition vote)	𝟙(Abolition vote)
	(1)	(2)	(3)	(4)
1890 sh. foreigners	-6.334	-7.534*	-6.852	-14.19*
	(4.636)	(4.565)	(4.750)	(8.177)
1890 sh. foreigners × Sh. enslaved	50.06**	55.47**	52.35**	94.42**
	(24.10)	(23.40)	(23.04)	(38.35)
Quilombos area	-0.00186	-0.00188	-0.00187	-0.00202
	(0.00154)	(0.00156)	(0.00156)	(0.00158)
Quilombos area × Sh. enslaved	0.0207**	0.0208*	0.0208*	0.0220**
	(0.0105)	(0.0109)	(0.0109)	(0.0110)
N	1263	1263	1263	1263
Mean dep. var	0.56	0.56	0.56	0.56
Endog test p-value	0.6839	0.5238	0.6070	0.2476
SW F-stats	14.66/11.50	39.00/66.05	46.14/82.56	22.56/12.16
Under-id test p-value	0.0093	0.0050	0.0058	0.0241
Prov. × Vote FE	✓	✓	✓	✓
Geo. × Vote FE	✓	✓	✓	✓
Demo. × Vote FE	✓	✓	✓	✓
Legi. cont.	✓	✓	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average HMI, average TRI (ruggedness), and ATT (remoteness). Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE, and TRI and ATT are also interacted with slavery intensity. Regressions iteratively exclude the origins with the largest Rotemberg weight.

Table A.22: Reduced form with interactions

	𝟙(Abolition vote) (1)	𝟙(Abolition vote) (2)
1890 share of foreigners	-4.010	
	(3.114)	
1890 share of foreigners × Share of enslaved	31.71*	
	(18.76)	
SubIV	-2.741	
	(2.826)	
SubIV × Share of enslaved	21.06*	
	(12.07)	
SSIV		-0.0147*
		(0.00827)
SSIV × Share of enslaved		0.0915**
		(0.0394)
Quilombos area		-0.00183
		(0.00150)
Quilombos area × Share of enslaved		0.0193*
		(0.0103)
N	1263	1263
Mean dep. var	0.56	0.56
Province × Vote FE	✓	✓
Geographic controls × Vote FE	✓	✓
Demographic controls × Vote FE	✓	✓
Legislator controls	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average HMI, average TRI (ruggedness), and ATT (remoteness). Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE, and TRI and ATT are also interacted with slavery intensity.

Table A.23: TSLS – Slavery intensity instrumented (2nd stages)

	𝟙(Abolition vote) (1)	𝟙(Abolition vote) (2)
Quilombos area	-0.00562***	-0.00554***
	(0.00211)	(0.00208)
Quilombos area × Share of enslaved	0.0415***	0.0404***
	(0.0116)	(0.0110)
1890 share of foreigners	-10.42	-8.924
	(7.200)	(7.092)
1890 share of foreigners × Share of enslaved	72.957*	62.703
	(43.29)	(42.804)
SubIV & SSIV	✓	✗
Het.-based instruments	✓	✓
N	1263	1263
Mean dep. var	0.56	0.56
F-stats	36.44	9.807
Hansen J p-val. p-value	0.358	0.324
Endog test p-value	0.299	0.149
Province × Vote FE	✓	✓
Geographic controls × Vote FE	✓	✓
Demographic controls × Vote FE	✓	✓
Legislator controls	✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors are reported in parentheses. Geographic controls include: share of enslaved population, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, average HMI, average TRI (ruggedness), and ATT (remoteness). Demographic controls include: population density, share of foreigners in 1872, share of free colored and literacy. Legislator controls (time varying) include: reelection status, other government appointment, and occupation and education fixed effects. Province fixed effects and non time-varying controls are interacted with vote FE, and TRI and ATT are also interacted with slavery intensity.

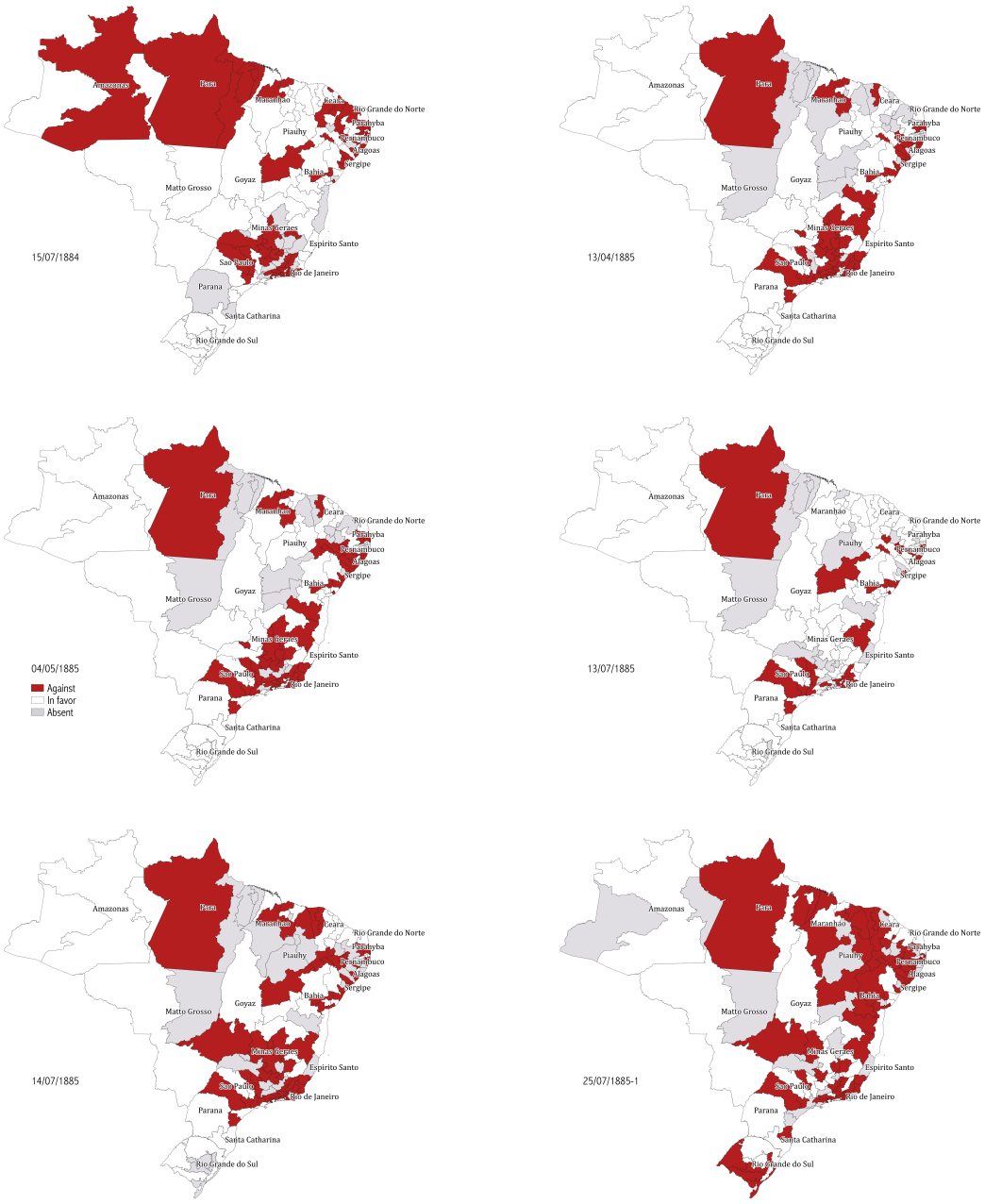


Figure A.18: Abolition maps – other bills (2)

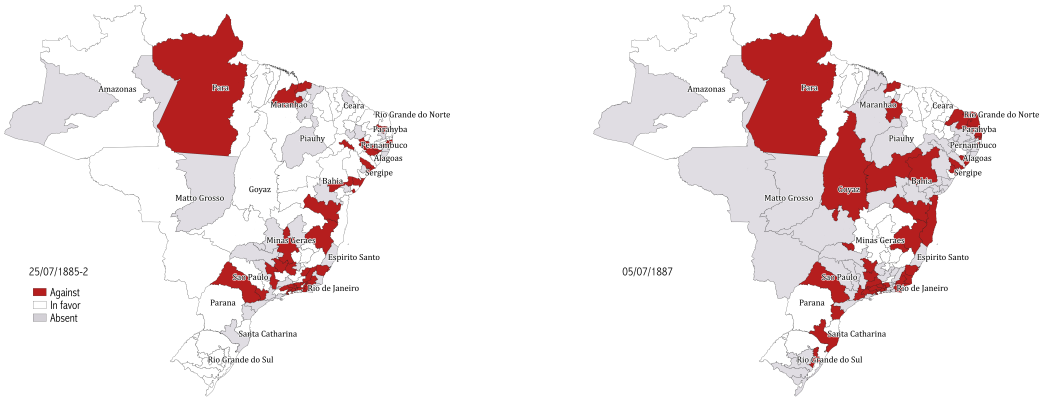


Figure A.19: Abolition maps – other bills (3)

ARAÚJO, Joaquim Aurélio Barreto Nabuco de

Pernambuco, 19-08-1849 — Washington, 17-01-1910
Bacharel em Direito, Diplomata e Historiador

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Figure A.20: Example biographic record from Nogueira and Firmo (1973)

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