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.
>
> Lei Áurea, May 13, 1888

Brazil was the last country to abolish slavery in the Western Hemisphere, in 1888. It did so under 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). With the benefit of hindsight, it is maybe easy to believe that slavery was doomed to disappear eventually.[1] 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, 'only' 472,382 disembarked in mainland North America, nearly twelve times fewer. In 1872, up to 53% of a district's population was enslaved (see Figure 1 for municipality-level shares). Maybe the real puzzle is not why Brazil abolished slavery so late, but how it came to abolish it at all.

Abolition was the result of a protracted legislative battle between members of the elite, who did not all profit from slavery equally. As the center of gravity of the economy moved from north to south, and from sugarcane to coffee,[2] so did a massive

[1] 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.

[2] In the second half of the century, coffee exports and export prices respectively increased by 341% and 91%, while exports of sugar continued growing by a mere 33% and prices actually decreased by 11% (Viotti da Costa, 1989). Hence, whereas sugar represented 49% of the country's exports and coffee 19% in 1822, by 1913 sugar and cotton together accounted for less than 3% of Brazil's exports, while coffee had gone up to 60% (Leff, 1991).

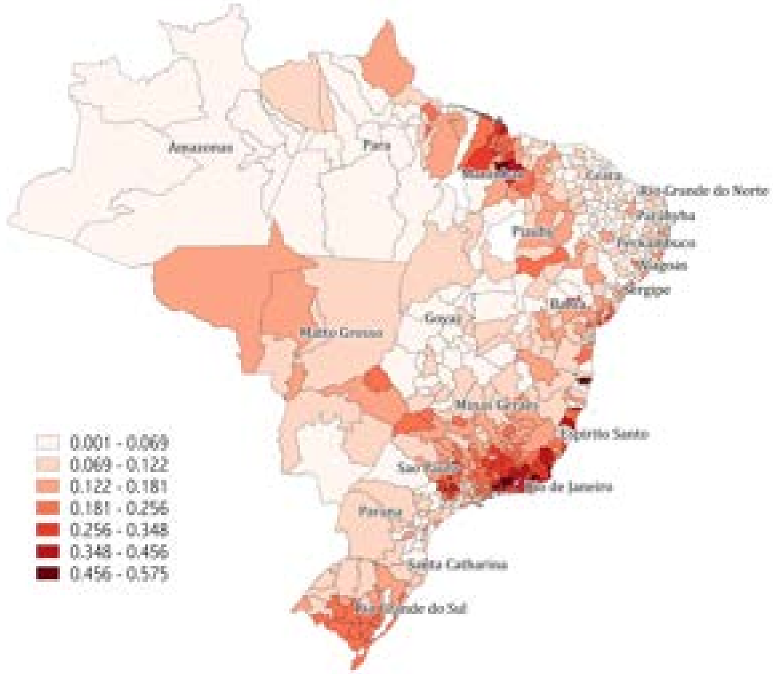

Figure 1: Enslaved population by municipalities in 1872 (Brazil, 1874). Municipality boundaries in 1872 come from IBGE (2010).

number of enslaved workers.[3] It is thus unsurprising that the northern elites were ready earlier for a transition to free labor, and that the elite as a whole was divided on the issue of slavery.[4] Even within the elite of the *Centro-Sul*, the Southern coffee-growing region, frontier planters and old *latifundiários* started adopting somewhat antagonistic stances after the 1871 law. Frontier planters struggled to attract an adequate supply of labor for abundant land. Meanwhile, landowners in older settlement regions were quickly exhausting their land (Reis and Reis, 1988).[5]

[3] The enslaved represented 23% of the northeastern population in 1823 and less than 10% in 1872, a 30% nominal decline. In 1874, more than 50% of the country's enslaved population was located in the Centro-Sul (Stein, 1957 and Klein, 1971). Carvalho de Mello (1977), Graham (2008) and Slenes (2008) describe the internal slave trade in some details.

[4] Conrad (1972, p. 67) quoted the northern representative Araújo Lima who, as early as 1854, established a parallel with the United States: "Be certain that you will have opposite interests, provinces with slaves, provinces without slaves (...). You will have the kind of struggles and antagonisms (...) which have placed the American Union in such imminent danger."

[5] The enslaved population decreased everywhere between 1873 and 1882, except in Rio de Janeiro's coffee *municípios* (+4.85%), in the Paulista part of the Paraíba Valley (+10.54%), and in the new coffee region of São Paulo (+45.51%). In 1882, the total enslaved population of Rio's coffee region comprised 156,009 individuals, 37,649 in the part of the Paraíba Valley belonging to São Paulo, and 38,242 in the new coffee region to the northwest of São Paulo (Conrad, 1972, p. 294-5).

This article establishes that differences in access to alternative sources of labor (immigrants) and in the resistance of the enslaved generated fault lines in the elite's support for slavery.[6] Our main results correspond to two sources of variation in local elites' support for slavery: labor demand, generally associated with the agency of landowners who coerce labor when it is scarce,[7] and workers' exit options, which emphasize the agency of coerced workers.[8] 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 that 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 support for both effects in Brazil.

Even if the Brazilian elite collectively profited from the institution of slavery,[9]

[6]While a growing body of evidence has demonstrated that the shadow of coercive systems is particularly long, the literature still largely lacks systematic evidence on the politics that ended such systems. 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, 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, Fujiwara et al. (2019) showed that slavery had a persistent influence on contemporary income inequality, and Seyler (2021) that support for slavery influenced development, poverty, and inequality.

[7]The original prediction of Nieboer (1900) and Domar (1970) is 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.' Note that Ashraf et al. (2018) formulated a related mechanism to explain labor emancipation: they proposed that it is the rise of capital-skill complementarity that eventually made the employment of free skilled workers more profitable in Prussia.

[8]The institutional consequences of the mobility of scarce labor 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.

[9]Williams (1944) was among the first to argue that slavery had started to decline because it

and even if a large share of the elite still had a vested interest in slavery at the time of abolition,[10] it still faced a coordination problem. For Brazilian coffee producers, the competition was domestic.[11] Even in the slavery-intensive coffee-growing regions, some planters felt that slavery profited their domestic competitors comparatively more than would free labor. Shifting to free labor may have raised the costs of production for all, but if it raised the costs of their competitors enough relative to theirs, then it may even have been advantageous for these planters.[12]

i) 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.[13] Access to immigrant labor was heterogeneous across the land. Foreigners represented between 0% and 24% of a district's population in 1890. We estimate that an increase in the share of immigrants in a district by one standard deviation [SD] (4 percentage points) was associated with a 14 percentage points increase in the corresponding legislator's likelihood to vote in favor of emancipation bills. This result echoes the Brazilian historiography, which emphasizes the importance of the substitution between enslaved workers and European immigrants at the end of the abolition period (Conrad, 1972; Klein and Luna, 2009), and it is hard to reconcile with a simple story of cultural influence.

was simply no longer productive (his predecessors had promoted the role of humanistic sentiments). Although Fogel and Engerman (1974) defended that slavery was still productive in the antebellum South when it was abolished, other scholars have argued that its inefficiency condemned slavery to eventually disappear (see Sutch, 2018 for a related discussion). Recent contributions illustrated how coercive institutions respond to profitability considerations (Naidu and Yuchtman, 2013, Carvalho and Dippel, 2020, Dippel et al., 2020, and Masera and Rosenberg, 2022).

[10] Once a coercion system is established, Engerman (1973) suggested that strong incentives for its perpetuation arise to avoid capital loss for the slaveholders.

[11] The country became the world's largest coffee exporter in 1831 (Nützenadel and Trentmann, 2008 and Klein, 2010), and it supplied 80% of the world's coffee well into the 1920s.

[12] In the textbook duopoly model with inverse demand $P = a - q_1 - q_2$ and constant marginal costs c_1 and c_2, the equilibrium profit of the first producer is $\pi_1 = (a - 2c_1 + c_2)^2/9$. Even if c_1 increases, her profits π_1 may still increase if c_2 also increases twice as much. This asymmetry in costs was already at the heart of the argument that Aghion and Schankerman (2004) proposed to establish the constituency of firms in favor of competition-enhancing policies, and Kennard (2020) for climate change regulation. It is simpler than the frameworks that Lagerlöf (2009), Acemoglu and Wolitzky (2011), or Rogowski (2013) proposed to address slavery in particular: to test their precise mechanisms, we would need decomposed wage data that are, to the best of our knowledge, unavailable for nineteenth-century Brazil.

[13] Immigrant labor in 1890 was often tied by debt obligation to the land (Hall, 1969, Holloway, 1980, Rocha et al., 2017, and Witzel de Souza, 2019). In contrast, previous waves of immigration – very limited in scale compared to the stream starting in the 1880s and the events of the Age of Mass Migration – seemed to provide a substitute to free colored labor (Witzel de Souza, 2022).

ii) Enforcement of the coercive institution depended on how difficult it was for enslaved individuals to escape their condition. In line with an *outside option effect*, landowners for whom enforcement was more costly found it advantageous to switch to free labor. We capture 'distance to freedom' by constructing a collection of measures of the proximity to *quilombos*, communities of maroons that were able to escape and hide in the hinterland. The existence of quilombos allows us to identify and leverage variation in local features that were conducive to allowing runaways to successfully escape, thus raising the cost of enforcing coercion. We estimate that increasing the average area of land occupied by quilombos in a district by the interquartile range (20km^2) was associated with a 6 percentage points increase in a high-prevalence legislator's likelihood to vote in favor of emancipation bills (and decreasing the average distance to the nearest quilombo by one SD, i.e. 29km, with a 3 percentage points increase). The outside option effect establishes a degree of agency for coerced workers themselves in precipitating the collapse of the legal coercion system in Brazil.[14]

We draw from a wealth of archival records, census surveys, geo-referenced spatial sources and historical maps. We retrieve all roll-call votes on each emancipation-related bill in the Empire's three last legislatures, ie. between 1882 and 1889, from the annals of the *Câmara dos Deputados*, the lower chamber of the Brazilian Parliament. We match every legislator recorded in these votes to the electoral district in which they were elected, and we construct a database of relevant descriptors of each municipality (município – closer to counties in the US sense) within each electoral district, that we aggregate at the level of districts.[15] We conduct a wide array of exercises to assess and address endogeneity threats, which overwhelmingly point to causal estimates very much in line with our simpler OLS specifications.

To ensure that we are capturing exogenous variations in the location of immigrants, we construct a religion-based leave-out shift-share (or 'Bartik') instrument

[14]This agency, overlooked in the economics literature, is a recurrent theme of the historiography of abolition (e.g. Needell, 2001, Graden, 2006, Machado, 2011, and Roberts, 2015).

[15]Legislators do not necessarily have to be perfect preferences, or endowment aggregators, for this to make sense. If they do act as agents of their constituents, or if they are selected to reflect the preferences of their constituents, then we expect to find a statistical relationship between their vote and the economic interests of their constituencies. Before us, Jha (2015) and Aidt and Franck (2015, 2019) also considered how legislative coalitions formed to back institutional change – democratization in particular. Mian et al. (2010) considered the local determinants of US Representatives' votes on significant pieces of legislation.

– similar in spirit to Tabellini's (2020) approach – that predicts the inflows of immigrants between 1872 and 1890 based on preexisting migration networks in each district. In contrast with prior decades, the looming abolition of slavery and resulting labor demand drove a proactive and centralized immigration policy (de Carvalho Filho and Monasterio, 2012, Rocha et al., 2017, and Witzel de Souza, 2019).[16] This helps address two concerns raised by Jaeger et al. (2018): first, it implies that inflows of immigrants from each sending country are less likely to have been responding to municipality-specific conditions, and second, shift-share instruments are more likely to isolate the exogenous component of immigrant inflows when the latter vary significantly over time. A challenge to this identification strategy is that municipality-level conditions affecting the distribution of immigrants by religion before 1872 must be unrelated to abolitionism in the 1880s (Goldsmith-Pinkham et al., 2020 and Borusyak et al., 2022). To alleviate this concern, we systematically control for immigration levels in 1872, for a large set of district characteristics that may have attracted earlier migrants, and for unobserved characteristics of provinces and votes. We also address the concern that immigrants of specific religions or from specific countries may have selected their destination based on emancipation prospects by controlling for individual religion/country shares, and show that results are robust to various ways to build and scale the instrument.

A key difference with the United States is that escaping slavery did not 'simply' mean heading north and trying to reach Canada (Allen, 2015). Instead, escapees sought to reach the open frontier and often founded or joined quilombos, whose locations were driven by considerations of security and remoteness. We predict the location and size of quilombos using variation in the extent to which local features of the land facilitated successful escapes. In particular, we exploit the variation induced by the interaction between ruggedness and remoteness. In the proximity of large settlement areas, a rugged terrain facilitated the escape of fugitives. Remoteness could substitute for ruggedness: terrain ruggedness was less important at a safe distance from cities. This instrument is only valid if within-province variation in this

[16] This also implies that immigrant networks in 1880s Brazil certainly played a less central role than in many similar contexts. Still, Witzel de Souza (2019) suggested that such networks did play a role, in particular with early movers becoming credit suppliers to prospective migrants, and Lesser (2013) argued that the centralized allocation of migrants placed great emphasis on preexisting networks. In any case, we show that the association generated is sufficiently strong to dissipate concerns of weak identification.

interaction does not affect legislators' voting decisions other than by its influence on slaves' 'outside options' (e.g. via trade and transport costs). We show that this assumption is likely to hold. Importantly, the variation driving the instrument lies in the interaction, and results remain identical when we control for remoteness and ruggedness, as well as when we flexibly control for nonlinearities in the un-interacted terms. Moreover, a key feature of our empirical design is that quilombos do not influence legislators' voting decisions unless interacted with the prevalence of slavery. In a placebo test, we exploit this feature to show that the un-interacted instrument has, as expected, no effects in the reduced form.

To establish the robustness of our results, we conduct a number of additional tests in the appendices. We check that our results hold when restricting our sample to the three most significant bills related to emancipation over the period to account for a possible heterogeneity in the content of all bills. We do not find that absenteeism may have been used strategically. Our results are not driven by outliers, by our preferred measure of the proximity to freedom, or by the specifics of our preferred definitions of our instruments. As a placebo exercise, we use data from Lambais (2020) (collected from Guimarães, 1996 and Silva, 2003) on destroyed quilombos to verify that voting for emancipation was not driven by local conflicts. We find support for the two effects both in separate and in joint specifications. We systematically supplement our instruments with heteroskedasticity-based instruments built as functions of the model's data, following the procedure of Lewbel (2012) (see Appendix A.1.2 for a discussion). We instrument the prevalence of slavery, separately and jointly with the two main instruments. We also verify that the identification strategy proposed by Fujiwara et al. (2019), who argue that the Portuguese empire, East of the Tordesillas meridian, had a comparative advantage in the Atlantic slave trade over the Spanish empire, yields estimates consistent with our original instrument for the prevalence of slavery.

The remainder of the paper proceeds as follows. Section 1 provides additional elements of historical context. Section 2 describes our data sources. Section 3 presents our empirical strategy. Sections 4 and 5 lay out the results. Section 6 concludes.

1 Abolition laws in Brazil

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 oligarchical political system of the Empire of Brazil, founded in 1822, enfranchised a limited elite, and within the elite, historians argue that the interests of slaveholders dominated. Among many similar statements, we can quote Conrad (1972, p. 16): "[M]uch of the real power in the provinces was in the hands of the slaveholding landlord class," or Viotti da Costa (1989, p. 179): "[P]oliticians 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." 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* (that liberated children born of enslaved mothers) temporarily placated abolitionist sentiments stirred by the American Civil War (Conrad, 1972), with little immediate effect. 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.[17] 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

[17] We consider the 1880s, and more precisely, the three legislatures after the electoral reform in 1881 (*Lei Saraiva*, Jan 9, 1881), during which voting rights and the electoral map are homogeneous. While we would have liked to include the 1871 law in our sample, if only to control for pre-trends in the vote, the Câmara dos Deputados was at the time composed of two or three representatives from each of 42 districts, hard to match with the 122 districts of the three legislatures. This period ends with a military coup on Nov 15, 1889, which established the first Brazilian Republic.

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.4 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.

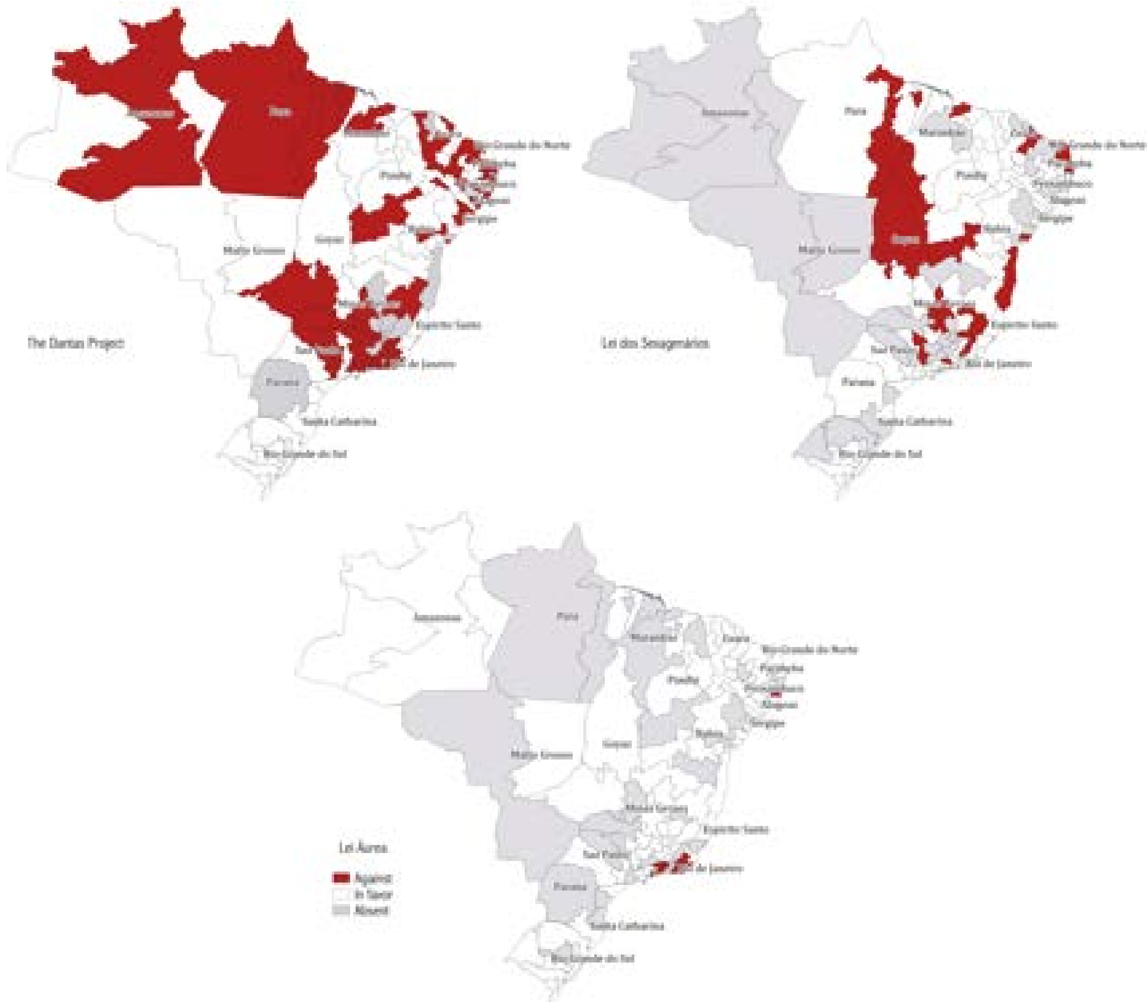

Figure 2: Vote by district on the 1884 Dantas Project, the 1885 Lei dos Sexagenários, and the 1888 *Lei Áurea.*

2 Data

We used archival records, census surveys, historical maps, and geo-referenced data sources to conduct our empirical analysis. We extracted information from historical sources using a combination of geo-referencing, optical character recognition, text mining, and manual coding when document quality left no other option. Because votes correspond to districts, we aggregated most of our variables from the level of the municipality to the district (642 municipalities in 1872 to match with the 122 districts after the 1881 electoral reform). Table 1 provides summary statistics of our main variables. Appendix A.3 gives more details about the construction of our variables.

Table 1: Summary statistics

Statistic	N*	Mean	St. Dev.	Min	Max
Political variables (by district × vote)					
Abolitionist vote	1,284	0.56	0.50	0	1
Absence on roll call day	1,586	0.19	0.39	0	1
Reelected	1,586	0.32	0.47	0	1
Other gov. appointment	1,586	0.54	0.50	0	1
Occupation/Education:					
Law	1,586	0.66	0.47	0	1
Medicine	1,586	0.15	0.36	0	1
Science	1,586	0.047	0.21	0	1
Priesthood	1,586	0.010	0.10	0	1
Owner/Businessman	1,586	0.033	0.18	0	1
Military	1,586	0.003	0.05	0	1
Civil service	1,586	0.057	0.23	0	1
Political affiliation: Liberal	1,586	0.47	0.50	0	1
Demographic variables (by district)					
Share of enslaved in 1872	122	0.148	0.094	0.018	0.537
Share of free foreigners in 1872	122	0.025	0.047	0.000	0.267
Share of foreigners in 1890	122	0.019	0.040	0.000	0.237
Share of free colored in 1872	122	0.476	0.163	0.104	0.829
Share of literates in 1872	122	0.161	0.065	0.080	0.362
Colonial settlements during 1748-1800**	122	0.11	0.59	0	4
Colonial settlements during 1800-1870**	122	0.16	0.66	0	4
Colonial settlements during 1870-1930**	122	0.94	3.64	0	36
Colonial settlements during 1930-1970**	122	0.76	3.47	0	28
Coercion variables (by district)					
Av. number of quilombos	122	4.29	6.67	0.0	52.0
Av. dist. to closest 1-quilombo municipality	122	21.93	29.00	0.0	196.6
Av. dist. to closest 2-quilombos municipality	122	48.96	85.81	0.0	737.1
Av. dist. to closest 3-quilombos municipality	122	67.67	92.42	0.0	737.1
Av. dist. to closest 4-quilombos municipality	122	76.82	92.48	0.0	737.1
Av. dist. to closest 5-quilombos municipality	122	86.58	92.94	0.0	737.1
Av. dist. to closest 6-quilombos municipality	122	111.1	111.2	0.0	861.4
Av. dist. to closest 7-quilombos municipality	122	117.8	112.8	0.0	861.4
Av. dist. to closest 8-quilombos municipality	122	118.8	112.7	0.0	861.4
Av. dist. to closest 9-quilombos municipality	122	138.3	121.5	0.0	861.4
Av. area quilombola	122	112.7	729.3	0.004	7,198
Geographical variables (by district)					
Average coffee suitability	122	26.35	12.24	0.00	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	0.45	0.37	0.03	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	0.292	0.027	0.235	0.354
Av. travel time to nearest prov. capital	122	51.61	41.68	0.0	192.3
Av. pop. density	122	19.53	41.41	0.024	232.3
*Miscellaneous variables (by district)****					
Av. distance to nearest gold supply road	122	229.2	276.1	2.067	1,380
Distance to nearest diamond mine	122	657.8	369.8	64.74	1,594
Gold mining indicator	122	0.172	0.379	0	1
16th cent. Indigenous enslavement indicator	122	0.094	0.246	0	1
17th-18th cent. Indigenous enslavement indicator	122	0.411	0.464	0	1

* 122 districts × 13 laws = 1586 obs. Dist. in km, surf. in km^2, time in hr, dens. in km^{-2}.
** Used in Appendix A.2.2; *** Used in Appendix A.2.1.

Political variables. We collected 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,[18] starting with the first no-confidence vote against the Dantas Cabinet in 1884 and ending with the vote for the Lei Áurea in 1888. Jobim and Porto (1996) reported the post-1881 district-level electoral division, which we geo-reference using IBGE (2010). Nogueira and Firmo's (1973) encyclopedia of parliamentarians allows us to cross-reference the matching of each legislator to a unique district, and provides a number of controls for individual legislators' characteristics, such as their party affiliation, reelection status, other government appointments, and occupation/education. Figure A.13 in Appendix A.5 provides an example of the biographical records compiled in Nogueira and Firmo (1973).

Demographic variables. One essential variable in our study is the share of enslaved persons in a district's total population in 1872, which we compute from the first nation-wide demographic census in the country's history (Brazil, 1874). From the 1872 census, we also use two variables as controls: *i)* the share of the non-captive colored population (defined as the sum of blacks, brown-skinned and mixed-race), and *ii)* the literacy rate. Finally, we use data on the nationality and religion of foreigners from the 1872 and 1890 censuses (Brazil, 1895, is the second census). All these variables are aggregated from the municipality to the district level.

Quilombos. Quilombos were communities founded by maroons as early as the sixteenth century (most of them before the nineteenth), that offered refuge to other runaways (Anderson, 1996). For each district, we compute the number of quilombos and the total area occupied by quilombos in the district with 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 slaves.[19]

[18] Other bills for which the vote of individual representatives is recorded were hard to connect to the issue of abolition. 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 hardcore slaveholders. In Appendix A.2.4 we verify that our results are robust to both selection on bills and legislators.

[19] The distribution of quilombos across districts is skewed. The largest quilombo in the Fundação

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. 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), and the Human Mobility Index (HMI) developed by Özak (2010, 2018) to compute 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.

Miscellaneous. We geo-reference several maps from the Atlas Histórico do Brasil (CPDOC, 2016) in order to compute a number of variables: *i)* the distance to the closest supply line to eighteenth-century mining areas, averaged across municipalities, *ii)* the distance from each district's centroid to the closest eighteenth-century diamond mine, *iii)* an indicator variable capturing zones of 18th century gold mining, and *iv)* indicator variables and surfaces of areas where Indigenous peoples were repressed and enslaved between the sixteenth and the eighteenth centuries.

3 Empirical approach

To examine the labor demand and outside option effects, we estimate a simple reduced-form model of the determinants of legislators' voting behavior on emancipation-related bills. These determinants include ideological preferences, reelection incentives, perhaps even their own material interest, and the representation of the preferences of their constituents.

During each roll call v, a legislator representing district i from province j faces a binary choice: adopting a pro-emancipation stance or not. Let $P_{ijv} = 1$ if the

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 Goyaz, is significantly smaller, still measuring 2618 km^2. The same consideration for the number of quilombos in a municipality explains the pattern of maximum average distances as n increases. A number of other works use quilombos, albeit in a different context. Fujiwara et al. (2019) and Papadia (2019) used quilombos to measure slavery. Closer to our interpretation (and in line with Schwartz, 1992), Lambais (2020) used quilombos as a measure of the resistance of enslaved persons, whose long-run effects on economic development are the main object of his study.

legislator representing district i of province j adopts a pro-emancipation stance on the v^{th} roll call (and $P_{ijv} = 0$ otherwise). Also denote L_{ijv} and O_{ijv} the labor demand and outside option effects respectively, and $\tilde{\mathbf{x}}_{ijv}$ the complete vector of covariates and fixed effects [FE] relating to a legislator's decision. Ideally, we would estimate the following general specification

$$P_{ijv} = \alpha_1 L_{ijv} + \alpha_2 O_{ijv} + \tilde{\mathbf{x}}'_{ijv}\boldsymbol{\theta} + \varepsilon_{ijv}. \tag{1}$$

Unfortunately, as L_{ijv} and O_{ijv} are not directly observable, we have to devise appropriate proxies. For clarity, in the main text, we proceed with a simple linear probability model, and we consider each effect separately. We verify that each result holds in a generalized linear model (GLM) in Appendix A.2, and that they hold jointly in Appendix A.2.5.

Labor-demand effect: In the spirit of the Nieboer-Domar hypothesis, labor scarcity should encourage the elite to want to coerce workers. The arrival of a large number of immigrants in the late 1880s, many of which were destined to provide labor in Southeastern coffee plantations, should have alleviated the reliance of plantation owners on coercive institutions. Focusing on this effect, we estimate a nested version of equation 1 (implicitly assuming $O_{ijv} = 0$),

$$P_{ijv} = \zeta_j + \delta_v + S_{ij}^{1872}\beta + F_{ij}^{1872}\lambda + F_{ij}^{1890}\mu + \mathbf{x}'_{ijv}\boldsymbol{\gamma} + \varepsilon_{ijv}, \tag{2}$$

where S_{ij}^{1872} measures the share of enslaved individuals in the population of district i, whereas F_{ij}^{1872} and F_{ij}^{1890} respectively measure the share of (free) foreigners in district i in 1872 and 1890. ζ_j and δ_v are resp. province and vote FE, and $\mathbf{x}_{ijv}$ is a vector of both legislator-vote-level and district-level covariates.

While efforts to attract immigrant agricultural workers had been ongoing since the 1830s, they only started paying off in the second half of the 1880 decade. Our hypothesis therefore corresponds to $\mu > 0$. We systematically control for the presence of free immigrants in 1872, or alternatively consider the variation in the number of foreigners between 1872 and 1890 normalized by 1890 population (hereafter $\Delta_{1890-1872}$). We expect this effect to be primarily driven by slavery-intensive districts (intuitively, substitution possibilities away from slavery should matter more as a predictor of voting decisions where slavery is most prevalent). To assess this, we introduce interaction terms between immigration and the prevalence of slavery in some specifications (in which case we expect the effect of immigration to increase with the prevalence of slavery), or alternatively restrict our sample to the coffee-growing, slavery-intensive Sudeste (Southeast) region.[20]

This equation highlights the importance of the prevalence of slavery in 1872 as a factor in the abolition votes. There is little doubt that the observed prevalence reflects the profitability of the institution, and that a negative β in equation 2 (and 3 below) reflects the reluctance of slaveholding elites to let go of a profitable investment (in the spirit of Engerman, 1973). In the main text specifications, we always show the regression coefficient for prevalence, although we postpone a detailed discussion to Appendix A.1.1.

Outside option effect: Support for coercive institutions should also be weaker when it is less hard for coerced workers to escape. Again, we estimate a nested version of equation 1 (implicitly assuming $L_{ijv} = 0$),

$$P_{ijv} = \zeta_j + \delta_v + S_{ij}^{1872}\beta + Q_{ij}\phi + \mathbf{x}'_{ijv}\boldsymbol{\gamma} + \varepsilon_{ijv}, \tag{3}$$

where Q_{ij} captures the 'proximity to freedom' in district i. Our hypothesis corresponds to $\phi > 0$. Again, we expect this effect to be primarily driven by slavery-intensive districts (the cost of enforcing coercion should matter more as a predictor of voting decisions where coercive institutions are most prevalent). In our preferred specifications, we therefore introduce an interaction term between proximity to freedom and the prevalence of slavery (in which case we expect the effect of proximity to freedom to increase with the prevalence of slavery), or alternatively restrict our sample to the Sudeste. Again, we highlight the importance of the prevalence of slavery in 1872 as a factor in the abolition vote.

We consider a number of measures of proximity to freedom to capture captive workers' outside options. In general, we build Q_{ij} as a function of the location and size of quilombos. The existence of the latter allow us to identify local features that made it easier for captive workers to successfully escape, and our favorite measures

[20] It should be noted that $\mu > 0$ does not merely reflect support for unconditionally free labor, to the extent that immigrant workers at the end of the 19th century were themselves subject to some degree of coercion. In the province of São Paulo, immigration was part of an official program that sponsored European immigrants and dispatched them to plantations. According to Lanza et al. (2021), immigrants in the province had no influence in where they were allocated under that program (except, to a limited extent, in government-sponsored immigrant settlements, cf. Rocha et al. 2017). While their allocation may still have been driven by municipality-specific needs, at least this helps us control for the relative attractiveness of municipalities for the migrants themselves.

of proximity to freedom rely on either the size of lands captured and controlled by maroons (quilombolas) or the physical distance to the closest quilombos.[21]

3.1 A discussion of identification

Because of data scarcity, many of our variables are either cross-sectional or fall outside of the voting panel time-frame. Nonetheless, unobserved heterogeneity (such as variations in norms and culture) across provinces are absorbed by province FE. Additionally, we capture variation over bills (constant across districts) with vote FE. Hence, a causal interpretation of our coefficients of interests is equivalent to assuming that the allocation of immigrants and quilombos *within provinces and bills* is as good as random, conditional on a wide array of political, demographic and geographical controls. We control for the party of the representative of district i at the time of vote v, and an array of geographic and demographic descriptors of district i (prevalence of slavery, ethnicity and literacy rates, population density, soil suitability to the main export crops, rainfall, distance to the coast and to the closest river, geographic coordinates, and human mobility index).

We are attentive to the list of controls that we include in our regressions. It is likely that the stance of parties on abolition contributed to whom electors choose to be their representatives, as well as their occupations. It is also possible that demographic variables, such as the share of enslaved and of free colored individuals in the population, are endogenously determined by our explanatory variables and by the vote for or against abolition. In Appendix A.1.1, we consider in details what controls affect the regression coefficient of the vote P_{ijv} on prevalence S_{ij}^{1872} to determine which ones are probably 'bad' control variables. Since these controls are also possible confounders of the mechanisms, and since we find that their inclusion yields more conservative estimates, we choose to include them in all the specifications in the main text.

Identification is further complicated by the non-random allocation of immigrants

[21] According to Reis (1996), dos Santos Gomes (2005, 2015), and Lambais (2020), repression against quilombos (and violent actions by inhabitants of quilombos) increased considerably after the 1871 Lei do Ventre Livre. We may be worried that violent conflict with quilombolas influenced voting decisions in a way that competes with our measure of proximity to freedom. We address this concern in Table A.20 of Appendix A.2.3, and also discuss the concern that other (possibly endogenous) covariates capturing local emancipation-related cultural norms might falsify the influence of proximity to freedom.

and quilombos across districts: immigrants might have selected their destination based on emancipation status, and quilombos may tend to be located in abolition-friendly districts. We employ a number of procedures to assess and address such threats. First, for each of our two main explanatory variables (immigrants as a share of the population in 1890 and proximity to quilombos), we build a set of instrumental variable strategies, leveraging preexisting migration networks and topographic determinants of the location of quilombos. We describe these in details in sections 5.1 and 5.2. Second, because we interact our explanatory variables with the prevalence of slavery, we must also consider the possible endogeneity of the effect of slavery on emancipation. Some districts may have had a smaller enslaved population because of long-standing abolitionist beliefs, or other deeply rooted norms might have simultaneously determined abolitionism and the local prevalence of slavery. In Appendix A.2.1, we use long-predating historical determinants of the location of enslaved populations across Brazil to instrument the local prevalence of slavery in 1872, and conduct a number of additional validation exercises (in particular, we follow Fujiwara et al., 2019 in exploiting the colonial boundaries between the Portuguese and Spanish empires within Brazil to run a regression discontinuity design. Third, we supplement each set of instruments with a heteroskedasticity-based identification approach, following Lewbel's (2012) procedure. We discuss the advantages of instruments generated from the data in more details in Appendix A.1.2. Fourth, to preempt a worry that our instruments may be co-dependent, we also conduct pairwise multi-instrumentation exercises and consider all instruments together (section 5.3 and Appendix A.2.5). This variety of exercises overwhelmingly suggest estimates closely in line with the OLS.

Finally, a note on standard errors. We systematically address the possible spatial and serial correlation of observations with two-way clustered standard errors and HAC standard errors. Clustering at the level of the district allows all the votes of a district's representative to be correlated non-parametrically, under the parametric assumption that observations are uncorrelated across groups. Clustering at the level of the vote allows arbitrary dependence between districts for each bill.[22] We compute

[22]Because our time-dimension is 'shorter' than our spatial-dimension (i.e. there are 13 bills and 122 districts) and since the asymptotic theory underlying two-way clustering relies on clusters in the smallest dimension, we may be worried that two-way clustered standard errors are too demanding for nonlinear models (presented in the appendices). Therefore, we also report one-way clustered standard errors at the district level for these.

HAC standard errors using Conley's (1999; 2010) approach, allowing auto-correlation within a given radius from each district's centroid (we select a 250 km radius for the spatial kernel – in addition to allowing observations to be serially correlated across the 13 bills – but results remain qualitatively equivalent when we vary this threshold from 50 to 1000km).

4 OLS results

4.1 Substitution with immigrant labor

In Table 2, we report the results of estimating equation 2, investigating the influence of substitution possibilities away from slavery on voting decisions. Column 1 presents results including the share of foreigners in 1872 and 1890, and column 2 including instead the net immigration between the two dates (foreigners in 1890 − free foreigners in 1872, as a proportion of the 1890 population in each district). Column 3 presents the results of a specification including interaction terms with the prevalence of slavery, and column 4 limits our sample to the districts in the Sudeste region.

A higher presence of immigrants in 1890 (alternatively, immigration between 1872 and 1890) is associated with a higher probability that a district's representative votes in favor of abolition-related bills. Our preferred specification (column 1) considers separately the shares of foreigners in 1872 and 1890: it illustrates and corroborates historical accounts following which the massive arrival of immigrants to plantation areas in the late 1880s alleviated the reliance of slaveholders on captive workers. This is also consistent with the *labor demand effect* (and the Nieboer-Domar hypothesis): substitution possibilities away from enslaved workers became critical once the institution of slavery were threatened. Column 2 paints a similar picture. Columns 3 and 4 confirm that the bulk of the effect is driven by slavery-intensive areas, although the specification in column 3 appears underpowered and results in imprecisely estimated interactions.[23]

Taking the estimates of column 1, controlling for the presence of foreigners in 1872, a 1 percentage point increase in the share of foreigners in 1890 is associated with a 3.4

[23] We obtain similar results with the column 2 specification restricted to the Sudeste region – the subsample used in column 4. In this case, the coefficient associated with $\Delta_{1890\text{-}1872}$ is also positive and strongly significant.

Table 2: Immigration and voting decisions

	$\mathbb{1}$(Abolition vote)			
	(1)	(2)	(3)	(4)
1872 share of enslaved	-0.840	-0.754	-1.140	-1.163
	(0.182)***	(0.200)***	(0.347)***	(0.285)***
	{0.353}**	{0.355}**	{0.488}**	{0.433}***
1872 share of free foreigners	-2.894		-3.592	1.970
	(1.197)**		(2.322)	(1.483)
	{0.713}**		{1.943}*	{1.327}
1890 share of foreigners	3.432		2.380	3.924
	(0.907)***		(3.153)	(0.858)***
	{0.703}***		{3.310}	{1.124}***
$\Delta_{1890\text{-}1872}$		1.942		
		(0.523)***		
		{0.569}***		
1872 sh. foreigners × sh. enslaved			2.868	
			(10.850)	
			{9.511}	
1890 sh. foreigners × sh. enslaved			6.327	
			(16.844)	
			{18.674}	
Controls	All	All	All	All
Province & Vote FEs	Yes	Yes	Yes	Yes
Observations	1,284	1,284	1,284	453
R-squared	0.428	0.426	0.428	0.398
Mean dep. var.	0.56	0.56	0.56	0.43

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. Columns 1 and 2 present OLS results using immigrants in 1872 and 1890 and the differential between the two respectively. Column 3 introduces interaction terms between the prevalence of slavery and each immigration wave, and column 4 focuses on the Sudeste region. Controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored and literacy.

percentage points increase in the probability to vote in favor of an emancipation bill.[24] The share of foreigners ranges from 0.0% in the 4th district of the province of Parahyba to 23.7% in the districts 1 to 3 of Rio, with a SD of 4.0%: these results imply that a SD increase in the share of foreigners in 1890 increases a representative's probability

[24] Table A.11 in Appendix A.2.2 reports very similar results in probit specifications.

to vote in favor of emancipation by 13.6 percentage points, a 24.3% increase from the sample mean. Looking specifically at the Sudeste, a SD increase (5.9%) in the share of foreigners in 1890 is associated with a 23.2 percentage points increase in the likelihood to vote in favor of emancipation, a fairly large increase of 54.0% from the subsample mean.

The relationship between immigrants and abolitionist votes is consistent with a labor demand effect, in line with the historiography of the period (Conrad, 1972; Klein and Luna, 2009; Witzel de Souza, 2022), which stresses the importance of the substitution of enslaved labor with newly arrived immigrants. It is hard to simply reconcile these results with a cultural influence hypothesis, whereby the new ideas brought by immigrants may have positively influenced emancipation decisions. Immigrants in 1872 did not seem to predict the abolitionist vote (to the contrary), or to provide a substitute to enslaved labor. A large fraction of the immigrants present in 1890 had arrived in just the few preceding years.[25] They were mostly disenfranchised, and it is hard to imagine that they would have had such a large direct effect on the abolitionist vote in such a short time frame. Moreover, inasmuch as we have been able to measure the spread of ideas (through the establishment of clubs and pro-abolition newspapers), we have not found support for this alternative interpretation (results available upon request).

4.2 Exit and proximity to freedom

In Table 3, we report the results of estimating equation 3, investigating the influence of proximity to freedom on voting decisions. Columns 1 to 3 use the average area of land belonging to quilombolas in a district's municipalities as a measure of proximity to freedom. Columns 4 to 6 use the (log of the) distance to the nearest quilombo.[26] Columns 1 and 4 present the specifications with the full sample and vector of control

[25]The *Sociedade Promotora da Imigração* – the society for the promotion of immigration – was created in 1886 with the goal of attracting European immigrants to São Paulo (the province where the overwhelming majority of immigrants settled during the period). 6,500 immigrants entered São Paulo in 1885, 32,000 in 1887, and 90,000 in 1888 (Conrad, 1972).

[26]In Table A.16 in Appendix A.2.3, we report the results using the (log of the) distance to the nearest N quilombos, with N going from 1 to 9, as more measures of the distance to freedom. The magnitude and the significance of the estimates increases with N: a greater number of quilombos captures better prospects of freedom. Maintaining the distance constant, the promise of freedom is greater when multiple quilombos can be reached, indicating that it is harder to locate and retrieve maroons. Presenting $N = 1$ in Table 3 is a conservative choice.

variables. Columns 2 and 5 include the interaction of proximity/distance to freedom with the prevalence of slavery. Columns 3 and 6 present the results of the specification limited to the districts of the Sudeste region.

Table 3: Exit and voting decisions

	𝟙(Abolition vote)					
	(1)	(2)	(3)	(4)	(5)	(6)
Share of enslaved	-0.912 (0.232)*** {0.355}**	-0.990 (0.238)*** {0.375}***	-1.177 (0.068)*** {0.495}**	-0.859 (0.246)*** {0.361}**	-0.695 (0.260)*** {0.329}**	-0.366 (0.292) {0.659}
Av area quilombola	-9.8e-5 (4.0e-4) {4.2e-4}	-1.1e-3 (7.4e-4) {7.3e-4}	3.7e-3 (8.1e-4)*** {1.0e-3}***			
Sh. enslaved × Av. area quil.		8.5e-3 (4.7e-3)* {3.9e-3}**				
Ln av. dist. 1 quil.				-2.5e-3 (3.1e-3) {3.4e-3}	0.011 (6.9e-3) {7.4e-3}	-0.019 (5.4e-3)*** {3.1e-3}***
Sh. enslaved × Ln av. dist. 1 quil.					-0.093 (0.040)** {0.044}**	
Controls	All	All	All	All	All	All
Province & Vote FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,263	1,263	453	1,284	1,284	453
R-squared	0.418	0.420	0.408	0.421	0.424	0.408
Mean dep. var.	0.56	0.56	0.43	0.56	0.56	0.43

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. Columns 1-3 use the average area of land belonging to quilombolas as a measure of proximity to freedom, introduced alone, interacted with the prevalence of slavery, and focusing on the Sudeste region. Columns 4-6 proceed similarly using the average distance to the closest municipality with at least one quilombo. Controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored and literacy.

As the prevalence of slavery increases, proximity (distance) to freedom is associated with a higher (lower) probability that a district's representative votes in favor of abolitionist bills. Heterogeneity is particularly important here, and this effect is entirely driven by slavery-intensive districts. The un-interacted effect is thus not significant in columns 1 and 4, but it becomes so when the interaction with the prevalence of slavery is introduced (columns 2 and 5), or when we limit our sample to the Sudeste region (columns 3 and 6).[27] Quilombos were dispersed across the entire territory but they only matter in explaining voting behavior on emancipation-related bills in districts where slavery was indeed prevalent at the end of the nineteenth century.

Our preferred measure is the area of quilombola land (columns 1 to 3). According to column 2, a 1 km^2 (247 acres) increase in the average area of quilombola land is associated with a $-.0011 + .009 \times 50\% = .32$ percentage point increase in the probability to vote in favor of an emancipation bill in a district with a 50% prevalence of slavery. The average area of quilombos per municipality ranges between districts from 0.004 km^2 (1 acre) to roughly 3600 km^2 (.9 million acres). As the distribution is skewed, instead of the SD, we consider the interquartile range (19.8 km^2) as the relevant unit of comparison. An increase in the area of quilombos by the interquartile range (IQR) in a district with a 50% prevalence is associated with an increased probability to vote in favor of abolition by 6.2 percentage points (an 11.1% increase from the sample mean). According to column 3, an IQR (16.9 km^2) increase in the area occupied by quilombos is associated with a 6.3 percentage points increase in the probability to vote in favor of emancipation in the Sudeste (this corresponds to a 14.9% increase from the subsample mean). Alternatively, according to column 5, decreasing the distance to the nearest quilombo by one SD (29 km) away from the mean is associated with a 3.0 percentage points increase in the probability to vote in favor of an emancipation bill in a district with a 50% prevalence of slavery (a 5.4% increase from the sample mean).

5 Addressing endogeneity concerns

To mitigate remaining concerns about the endogeneity of our main variables of interest – immigration in 1890 and proximity to freedom – we construct an array of independent instruments leveraging variation in *i)* preexisting migration networks, and *ii)* topographic determinants of the location of quilombos. We then supplement these with a heteroskedasticity-based identification approach (see Appendix A.1.2 for a discussion). In each case, our results closely support the findings of the correspond-

[27] Table A.15 in Appendix A.2.3 reports comparable results in probit specifications, and Figure A.12 of Appendix A.5 reports graphical representations of marginal effects and predictive margins. In Table A.17, we also report the results of a specification that uses average area of quilombola land as the measure of the proximity to freedom including the outlier districts, both in logs and in levels.

ing OLS specification, which we find reassuring (Young, 2022). We then consider jointly our two hypotheses, in the spirit of the more general specification in equation 1. This leads us to also construct an array of independent instruments of the geographical distribution of slavery, and to instrument jointly prevalence, immigration, and the location of quilombos.

5.1 The location of immigrants

Shift-share instruments, which exploit heterogeneous exposure to common shocks, have become common to address the endogeneity of immigrants' location (Felbermayr et al., 2010, Andersen and Dalgaard, 2011, Ortega and Peri, 2014, Alesina et al., 2016, Docquier et al., 2016, Beine and Parsons, 2017, Bahar and Rapoport, 2018, Burchardi et al., 2019, Docquier et al., 2020, and Tabellini, 2020). Such instruments are typically constructed by interacting past immigration disaggregated by nationality shares with inflows of immigrants from each sending country.

A particular difficulty in our case is that the 1890 census does not decompose immigration by nationality. We circumvent this difficulty thanks to the rich information on *religion* shares in each municipality, and based on the empirical regularity that the native population in 1890 was largely Roman Catholic.[28] We build a leave-out religion-based Bartik-like instrument, based on a matching of 1872-1890 predicted nationality-by-religion shares. We approximate the number of immigrants $\widehat{I}_{ir1890}$ of religion r in district i by the number of individuals of religion r in district i for every religion except Catholicism. We get the number of Catholic foreigners by the difference between the number of Catholics and the native population in each district. The 1872 data set provides a decomposition of the immigrant population by nationality, but only distinguishes Catholics and non-Catholics. To circumvent this, we match nationalities with the dominant religion in each country of origin (e.g. non-Catholic German immigrants are counted as Protestant, etc. See Table A.27 in Appendix A.5 for details). We can then compute the share $\widehat{\alpha}_{ir1872}$ of foreigners of religion r in dis-

[28] 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.

trict i as the sum for each origin country of the shares of such individuals. Finally, we write our religion-based instrument as:

$$Z_{i1890} = \frac{1}{P_{i1890}} \sum_r \widehat{\alpha}_{ir1872} \widehat{I}_{r1890}^{-i},$$

where $\widehat{I}_{r1890}^{-i}$ is the predicted number of immigrants of religion r in 1890, net of those that settled in district i, and P_{i1890} is the 1890 population of district i.

In Table 4, we report the 2SLS results of our specifications investigating the influence of substitution possibilities away from slavery on voting decisions, mirroring the specification in Table 2. We instrument the share of immigrants in 1890 by: *i)* the shift-share instrument, *ii)* the heteroskedasticity-based instruments described in Appendix A.1.2, and *iii)* the combination of both. Columns 1 to 3 of Table 4 present the first stage of the 2SLS specification with all controls and FE, and columns 4 to 6 the corresponding second stage estimation. In all cases, the first stage regression shows a comfortably large association between the share of foreigners in 1890 and our set of instruments.[29] We easily reject homoskedasticity with respect to exogenous regressors, which confirms the relevance of generated instruments to supplement our shift-share. 2SLS estimates are almost identical under two completely different identification assumptions, which is once again reassuring. They are also virtually identical to their OLS counterparts in Table 2. We cannot reject the hypothesis that the share of foreigners in 1890 can be treated as exogenous, making the OLS specification consistent and more efficient than the 2SLS.

For this instrument to be valid, municipality-level conditions that may have affected the distribution of immigrants by religion before 1872 must be unrelated to abolition patterns in the 1880s (Goldsmith-Pinkham et al., 2020 and Jaeger et al., 2018). Witzel de Souza (2022) finds that the location of immigrants in 1872 was uncorrelated with the distribution of the enslaved population, but correlated with, inter alia, the distribution of the free colored population. To deal with this concern, in addition to our full vector of controls (which includes several baseline year characteristics that may have contributed to attracting immigrants, such as population density and other demographic covariates), we systematically control for 1872 immigration in

[29] Some first-stage F-statistics are rather low compared to what recent advances in the literature suggest (see, e.g. Lee et al., 2022), but they are in general large enough to dissipate weak identification concerns, in particular for such demanding statistics (Andrews et al., 2019).

Table 4: Immigration and voting decisions – 2SLS

	First stage			*Second stage*		
	1890 Share of foreigners			𝟙(Abolition vote)		
	(1)	(2)	(3)	(4)	(5)	(6)
Pred. 1890 sh. of foreigners				3.593	3.499	3.568
				(1.294)***	(1.132)***	(1.039)***
				{1.488}**	{0.988}***	{0.799}***
1872 share of enslaved	0.050	0.0011	0.028	-0.838	-0.839	-0.838
	(0.037)	(0.018)	(0.022)	(0.181)***	(0.178)***	(0.179)***
	{0.013}***	{0.017}	{0.017}*	{0.349}**	{0.351}**	{0.351}**
1872 share of free foreigners	1.020	0.920	0.969	-3.045	-2.958	-3.022
	(0.049)***	(0.038)***	(0.041)***	(1.394)**	(1.507)**	(1.378)**
	{0.042}***	{0.037}***	{0.033}***	{1.548}**	{1.088}***	{0.959}***
Z_{1890}	-0.113		-0.058			
	(0.020)***		(0.014)***			
	{0.021}***		{0.014}***			
Het. instr. (latitude)		-0.081	-0.074			
		(0.010)***	(0.011)***			
		{0.016}***	{0.018}***			
Het. instr. (dist. coast)		-0.311	-0.213			
		(0.111)***	(0.109)*			
		{0.127}**	{0.103}**			
Controls & FEs	All	All	All	All	All	All
Observations	1,284	1,284	1,284	1,284	1,284	1,284
Mean dep. var.	0.019	0.019	0.019	0.560	0.560	0.560
F-stat	32.54/29.95	34.60/141.5	30.04/32.98			
B-P p-value		0.000	0.000			
P-H p-value					0.736	0.735
Hansen J p-value					0.650	0.892
Endog. test p-value				0.846	0.984	0.869

Note: *p<0.1; **p<0.05; ***p<0.01. 2SLS results with district-vote two-way clustered standard errors reported in parentheses, and Conley standard errors (with a 250km window) reported in curly brackets. Columns 1 and 4 only use our standard instrument, columns 2 and 5 only use heteroskedasticity-based instruments, and columns 3 and 6 use both. Controls: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored and literacy. When there is a unique endogenous variable, the first F-stat is the Montiel Olea and Pflueger (2013) F-stat and the second is a spatial HAC-corrected Kleibergen-Paap rank test (computed by adapting code from König et al., 2017). Only the latter is presented when there are multiple endogenous variables. The Breusch-Pagan test checks for heteroskedasticity in the first-stage, and Pagan-Hall in the second. The Hansen J statistic checks the validity of overidentification restrictions. The endogeneity test (Durbin-Wu-Hausman-like but robust to violations of conditional homoskedasticity) assesses whether instrumented regressors can be treated as exogenous.

our IV specifications. This not only mechanically predicts higher future immigration via the instrument, it may also have a distinct effect on emancipation-related voting (Tabellini, 2020). The variation used to identify the influence of 1890 immigrants is therefore in the religious composition of municipalities' foreign populations, not in the actual size of the immigrant population. In Appendix A.2.2, we also address the concern that immigrants from specific religions or nationalities selected their destinations based on the possibility that these would be more inclined to vote for emancipation by controlling for individual shares and for *núcleos coloniais* (state-sponsored settlements). We also consider alternative ways to construct and scale the instrument, and show that results are stable across a wide range of specifications. Finally, both the conflation of responses to immigrants' arrival put forward by Jaeger et al. (2018) and the possibility that inflows of immigrants from each sending country maybe responding to municipality-specific conditions appear unlikely in our context. Immigration increased vastly in the late 1880s, and responded to altogether different incentives, notably to a proactive immigration policy that was largely decided in Parliament as a manner to provide an alternative source of manual labor when abolition had become a probable outcome. Even the location of migrants was partly determined in Parliament: immigrants in Brazil were more often than not debt-bonded laborers, credit constrained and tied to the land (Rocha et al., 2017 and Witzel de Souza, 2019).

Appendix A.5 presents a number of robustness checks. In Table A.12, we report the results of instrumenting net immigration between 1872 and 1890, which again yields results almost numerically identical to their OLS analogues in Table 2. In Table A.13, we consider alternative definitions of our shift-share instrument. These alternative definitions are overall comfortably strong and all yield comparable estimates. Table A.14 also provides the results of an OLS specification focusing on the province of São Paulo only. While this relies on a very small sample, the case of São Paulo is particularly interesting because immigrants had no agency on their assignment across the province. These results confirm the role of immigration in driving abolition voting in the province.

5.2 The location of quilombos

There is little doubt that the decision of where to establish 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 escape. In Brazil, Klein and Luna (2009, p. 195) point out that a permanent escape depended on "the existence of dense forests or inaccessible mountains within a short distance from their homes." Remoteness – which we measure as travel time from the nearest provincial capital computed using Özak's (2010, 2018) HMI and a least cost path approach – in turn made repression harder, and the establishment of successful (and surviving) quilombos more likely.

Our main instrument for the location of quilombos in Eq. 3 interacts the average topographic ruggedness index (TRI) with the average travel time to the provincial capital. Ruggedness and remoteness act as substitutes for the successful establishment of durable quilombos: in the proximity of large settlement areas, rugged terrain was critical in providing a defensive advantage and allowing to remain hidden, whereas it played a much less deciding role at a safe distance.[30] In other words, the likelihood of finding surviving quilombos in the vicinity of settlement areas increases with ruggedness, but not in particularly remote areas. In Table 5, we report the 2SLS results of our specifications investigating the influence of proximity to freedom on voting decisions, mirroring the specification in Table 3. In columns 1 and 2, we respectively present the first and second stages of a specification with only one endogenous variable: the average area occupied by quilombos, which we instrument using the instrument discussed above only. In columns 2, 3 and 7, we present the first (columns 2 and 3) and second (column 7) stages of a specification including two endogenous variables: the average area occupied by quilombos, and its interaction with the prevalence of slavery. We instrument the former using only the instrument discussed above, and the latter with the interaction between this instrument and the prevalence of slavery (we factor in the possible endogeneity of the prevalence of slavery in the next section). Finally, we proceed similarly in columns 4, 5 and 8 but now complement our standard instrument and its interaction with the heteroskedasticity-based instruments described in section A.1.2 (columns 5 and 6 for the first stages, and column 8 for the second stage).

Column 1 shows that our standard instrument yields a comfortably large association with the instrumented variable, while column 2 provides results very similar to their OLS analogues in column 1 of Table 3. Without taking heterogeneity into account, the effect of proximity to freedom is again close to zero and insignificant. Second stage estimates of our specifications that include an interaction term in columns 7 and 8 are similarly close to their OLS counterparts in column 2 of

[30] We provide an unconditional plot of the relationship between the location of quilombos and the instrument in Figure A.11c in the Appendix A.5. A contour plot (not provided but available on demand) showing how ruggedness and remoteness predict quilombos offers a very clear image of this substitution effect: the area belonging to quilombolas tends to be large when either ruggedness or remoteness is high, but not when either both or none is high. This substitution effect is found again in the reduced form within slavery-intensive areas: ruggedness and remoteness positively predict pro-abolition voting decisions when either one is high, but not when they are simultaneously low or high. That we find this particular pattern in both instances is reassuring, as it is hard to reconcile with any interpretation other than the influence of coercion costs/proximity to freedom.

Table 5: Exit and voting decisions – 2SLS

	No interaction		*With interaction*					
	1st stage	*2nd stage*	*1st stages*				*2nd stages*	
	Av. area quil.	𝟙(Ab. vote)	*Av. area quil.*	*Av. area quil.×ensl.*	*Av. area quil.*	*Av. area quil.×ensl.*	𝟙(Abolition vote)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pred. av. area quilombola		−0.00096 (0.00094) {0.00091}					−0.0032 (0.0012)*** {0.0015}**	−0.0019 (0.00093)** {0.0011}*
Pred. av. area quil. × Sh. slaves							0.019 (0.0059)*** {0.0055}***	0.014 (0.0054)** {0.0055}**
1872 sh. ensl.	66.48 (54.98) {55.69}	−0.863 (0.230)*** {0.357}**	204.2 (130.4) {121.0}	−18.01 (23.98) {23.99}	129.0 (71.33)* {63.39}*	−1.687 (12.12) {11.53}	−1.041 (0.264)*** {0.398}***	−1.028 (0.242)*** {0.383}***
Av. TRI	49.14 (15.13)*** {13.67}***		77.29 (30.00)*** {28.42}***	5.591 (4.554) {3.915}	39.18 (16.03)** {13.41}***	4.017 (2.801) {2.454}		
Travel time	1.333 (0.237)*** {0.174}***		1.492 (0.303)*** {0.307}***	0.029 (0.046) {0.047}	0.648 (0.162)*** {0.169}***	−0.0063 (0.024) {0.023}		
Av. TRI × travel time	−1.421 (0.384)*** {0.334}***		−1.531 (0.693)** {0.639}**	−0.085 (0.093) {0.087}	−0.872 (0.368)** {0.286}***	−0.090 (0.067) {0.050}		
Av. TRI × Sh. ensl.			−129.4 (126.4) {116.2}	8.213 (23.85) {23.72}	−188.0 (79.34)** {59.91}***	−25.00 (15.32) {10.31}**		
Travel time × Sh. ensl.			−1.645 (2.689) {2.865}	1.207 (0.581)** {0.588}**	−0.616 (1.219) {1.257}	0.650 (0.208)*** {0.217}***		
Av. TRI × travel time × Sh. slaves			0.841 (3.416) {3.428}	−0.725 (0.664) {0.634}	2.231 (1.969) {1.507}	0.165 (0.367) {0.226}		
Het. instr. (latitude)					0.265 (0.028)*** {0.026}***	0.068 (0.0072)*** {0.0080}***		
Het. instr. (dist. coast)					0.624 (0.037)*** {0.027}***	0.048 (0.0080)*** {0.0078}***		
Controls and FE	All	All	All	All	All	All	All	All
Observations	1263†	1263†	1263†	1263†	1263†	1263†	1263†	1263†
Mean dep. var.	23.70	0.556	23.70	2.901	23.70	2.901	0.556	0.556
F-stat		16.80/20.60					13.58	39.06
B-P p-value					0.000	0.000		
P-H p-value								0.843
Hansen J p-value		0.779					0.962	0.732
Endog. test p-value		0.241					0.165	0.155

Note: *p<0.1; **p<0.05; ***p<0.01; † two outlier districts dropped, i.e. 26 obs. out of 1586, and 21 out of the 1284 for which a vote is registered. District-vote two-way clustered SE in parentheses. Conley SE (with a 250km window) in curly brackets. Columns 1-2 present 1st and 2nd stages of a specification with no interaction term. Columns 3-4 present 1st stages estimates of a specification with interaction, and column 7 the corresponding 2nd stage. Columns 5-6 present 1st stages estimates of a specification with interaction and including heteroskedasticity-based instruments, and column 8 presents the corresponding 2nd stage. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE. See Table 4 for details on the tests.

Table 3, albeit slightly larger (not statistically significantly so). Again, we cannot reject the null that endogenous variables can be treated as exogenous, so that we may prefer OLS estimates for efficiency reasons. While we might be slightly concerned about weak identification in the first stages presented in columns 3 and 4, adding heteroskedasticity-based instruments in columns 5 and 6 largely dissipates these concerns (and see footnote 29). here as well, we easily reject homoskedasticity with respect to exogenous regressors in the first stage, which confirms the relevance of using generated instruments to supplement our main instrument. It is reassuring that 2SLS estimates are comparable under two completely different sets of identification assumptions.

To be valid, our instrument should not be correlated with the decision to vote in favor or against abolition in the 1880s, except through its influence on the location of quilombos. A concern is that the TRI tends to be negatively associated with economic outcomes at the country level, because it increases transport costs and makes trading more difficult (Nunn and Puga, 2012). This is however unlikely to be an issue within provinces, and we systematically control for determinants of economic activity (in particular population density and soil suitability) and other shifters of remoteness (in particular distance to the coast and average human mobility index). Most importantly, in Table A.18 in Appendix A.2.3, we show that results remain qualitatively identical whether un-interacted terms are used as instruments or controls. This implies that the variation driving the instrument lies in the different effect that ruggedness has on the likelihood to find quilombos in remote areas compared to non-remote areas, and not in ruggedness or remoteness themselves. We can thus control for those (which results in slightly weaker identification but almost numerically identical estimates), and the resulting variation is much less likely to violate the exclusion restriction than the un-interacted terms. We also make sure that the instrument is not picking up nonlinearities by flexibly controlling for nonlinearities in un-interacted terms.[31]

Table A.19 in Appendix A.2.3 also provides a placebo test of the instrument's exclusion restriction. A key feature of the results presented in Table 3 is that quilombos have no effect on legislators' voting decisions in the full sample unless interacted with

[31] One might expect ruggedness and remoteness to be strongly positively correlated, but this turns out not to be the case. Ruggedness and remoteness rather tend to be weakly negatively correlated, and have a non-significant relationship within provinces.

the prevalence of slavery. If the exclusion restriction does not hold and the instrument affects voting decisions through other channels than by determining the location and size of quilombos, we may expect direct effects of ruggedness, remoteness, and their interaction on abolition voting in the specification with no interaction with slavery. In Table A.19, we present reduced form results showing that this is not the case, lending further support to the instrument's validity.

Finally, a remaining possible concern is that quilombos may be viewed as a bad control, to the extent that the number of quilombos and their size can conceivably constitute an outcome of slavery. Because we are not overly concerned with a possible reverse causation effect of abolitionism on quilombos (most quilombos predate by far the nineteenth century), we believe that this is unlikely to be an issue. Nonetheless, in Figure A.4, we show that slavery and quilombos are at best marginally correlated once controls and FE are netted out. Quilombos were widespread and relatively common across the country, and largely independent from the distribution of slavery in 1872.

5.3 Joint instrumentation

As a preliminary exercise, we examine the possibility that some districts had a smaller enslaved population because of long-standing abolitionist beliefs, or that another omitted variable (for instance deeply rooted norms) determined abolitionism and the local prevalence of slavery simultaneously. This would be a concern if it led to biased estimates of the effect of prevalence or of our main explanatory variables interacted with prevalence. We use long-predating historical determinants of the location of enslaved populations across Brazil to instrument the local prevalence of slavery in 1872 in Appendix A.2.1. We also replicate Fujiwara et al.'s (2019) regression discontinuity design, and conduct a number of validation exercises with alternative instruments and samples. All yield estimates similar to the OLS in Table A.6 in Appendix A.1.1.

Then, we instrument jointly each of our two explanatory variables and the prevalence of slavery. In Table A.22 in Appendix A.2.5, we instrument both the prevalence of slavery in 1872 and the share of immigrants in 1890 with: *i)* their respective standard instruments and *ii)*, both their standard instruments and heteroskedasticity-based instruments. This specification is quite demanding for standard instruments, but it yields estimates consistent with our simpler instrumentation exercises when we include heteroskedasticity-based instruments. We cannot reject that both variables are jointly exogenous. In Table A.23, we factor in the possible endogeneity of the prevalence of slavery in our estimation of the outside option effect. We instrument the prevalence of slavery, the area of quilombola land, and their interaction. We supplement these instruments with heteroskedasticity-based instruments. We cannot reject that both variables and their interaction are jointly exogenous. This is a particularly demanding specification, that results in somewhat weak first stages. It still yields comparable estimates in magnitude, and incorporating heteroskedasticity-based instruments dissipates weak identification concerns.

Finally, we evaluate the labor demand and the outside option effects jointly. Table A.24 mirrors closely the corresponding OLS estimates in Tables 2 and 3. We then instrument each explanatory variable with: *i)* their respective standard instruments and *ii)*, both their standard instruments and heteroskedasticity-based instruments in Tables A.25 (second stage) and A.26 (first stage). This is our most demanding specification. Again, it results in weaker first stages, but it yields comparable estimates in magnitude, and incorporating heteroskedasticity-based instruments dissipates weak identification concerns. Point estimates are comparable in magnitude and significance with simpler specifications, and we cannot reject the joint exogeneity of the three variables and of their interactions. This justifies ex post the focus on the more readable specifications above.

6 Conclusions

In this paper, we investigated the determinants of the persistence and change of labor coercion institutions. We considered 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 paper also makes an original contribution to the historiography of nineteenth-century Brazil. From the archival records of the Imperial parliament, we built a district-vote-level data set documenting political decision-making on emancipation-related bills during the last decade of the Empire. We relied on census surveys, historical maps, and geo-referenced data sources to identify local variations in slaveholders' interests. We proposed a two-pronged instrumental variables strategy, leveraging historical and topographic determinants of the location of enslaved populations and maroons across space as well as heteroskedasticity with respect to the regressors.

Our first result established that 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*. Our second result established that slaveholders' interests differed between districts depending on the local cost of enforcing the institutions of coercion. Representatives from high-prevalence 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, favored emancipation because restricting workers' mobility proved costly under the prevailing arrangement. This second 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 paper revealed the importance of the *mobility* of labor (the ability for enslaved individuals to withdraw themselves 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 Additional empirical considerations

A.1.1 Good and bad controls

Table A.6 looks at the association between a number of important covariates and voting decisions on emancipation-related roll calls, including FE and clustering standard errors by both district and vote. In column 1, we report the raw association between the prevalence of slavery and our binary abolitionist voting outcome. In column 2 to 5, we consider how this association is affected by the progressive introduction of geographic, political, and demographic controls. In column 6, we include the results of the probit specification with all FE and controls, and district-level clustered standard errors. Probit results in column 6 closely resemble their counterparts in column 5. Overall, this table shows that the prevalence of slavery is the main driver of voting behavior. This influence is mostly unaffected by geographical covariates, and only decreases when potentially endogenous – 'bad' – controls are included.

As expected, in all specifications a higher prevalence of slavery in a district is associated with a lower probability that the district's representative votes if favor of abolitionist bills. According to our estimate in column 5 of Table A.6 a 1 percentage point increase in the prevalence of slavery in the 1872 population is associated with a .90 percentage point decrease in the probability to vote in favor of an emancipation-related bill.[32] The magnitude of the effect decreases when we introduce political or demographic controls. In our sample, 46% of members of the Câmara dos Deputados are Conservatives, almost 47% are Liberals (the party of abolition), and around 2%

[32]Slavery prevalence ranges from 0.0% in the first district of the Amazonas province to 53.4% in the tenth district of Rio de Janeiro, with a SD of 9.1%: these results imply that a SD increase in the prevalence of slavery decreases a representative's probability to vote in favor of emancipation by 8.2 percentage points, a 14.6% decrease from the sample mean. The probit in column 6 yields a comparable marginal effect of the prevalence of slavery on the abolitionist vote, ie. .87 instead of .90 percentage points. In Table A.7, we propose an instrumentation of the prevalence of slavery that suggests a causal interpretation of these coefficients.

Table A.6: Main covariates and voting decisions – OLS and GLM

	𝟙(Abolition vote)					
	(1)	(2)	(3)	(4)	(5)	(6)
Share of enslaved	-1.210	-1.250	-0.611	-0.642	-0.896	-0.868
	(0.383***)	(0.433)***	(0.213)***	(0.430)	(0.219)***	(0.332)***
Coffee suitability		-0.008			-0.001	-0.002
		(0.007)			(0.005)	(0.005)
Sugar suitability		0.003			0.003	0.005
		(0.007)			(0.005)	(0.006)
Cotton suitability		0.004			0.002	0.001
		(0.006)			(0.004)	(0.004)
Rainfall		0.0003			0.00005	0.00005
		(0.0002)			(0.0001)	(0.0001)
Longitude		0.039			0.0004	-0.0003
		(0.024)*			(0.018)	(0.019)
Latitude		-0.008			0.008	0.002
		(0.018)			(0.015)	(0.017)
Ln(Pop. den.)		-0.013			-0.036	-0.027
		(0.038)			(0.028)	(0.029)
Ln(Dist. coast)		0.085			-0.040	-0.028
		(0.063)			(0.047)	(0.049)
Ln(Dist river)		0.006			-0.031	-0.032
		(0.027)			(0.022)	(0.017)*
HMI		2.691			1.045	1.412
		(2.553)			(2.031)	(2.116)
Liberal			0.451		0.472	0.384
			(0.083)***		(0.078)***	(0.029)***
Other gov. app.			0.047		0.046	0.040
			(0.036)		(0.032)	(0.033)
Reelected			0.013		0.016	0.020
			(0.042)		(0.038)	(0.034)
Law			0.063		-0.076	-0.058
			(0.054)		(0.060)	(0.055)
Medicine			-0.048		-0.054	-0.018
			(0.081)		(0.084)	(0.069)
Science			-0.182		-0.209	-0.190
			(0.121)		(0.134)	(0.099)*
Civil serv.			-0.054		-0.032	-0.007
			(0.044)		(0.051)	(0.062)
Priest.			0.149		0.147	0.124
			(0.123)		(0.134)	(0.123)
Owner/Businessman			-0.204		-0.207	-0.187
			(0.076)**		(0.081)***	(0.083)**
Military			-0.260		-0.316	-0.314
			(0.131)*		(0.067)***	(0.166)*
Share of free colored				0.727	-0.248	-0.227
				(0.316)**	(0.256)	(0.300)
Literacy rate				0.533	-0.039	-0.153
				(0.532)	(0.241)	(0.358)
Controls	None	Geo.	Pol.	Dem.	All	All
Province & Vote FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,284	1,284	1,284	1,284	1,284	1,269†
R-squared	0.244	0.255	0.410	0.249	0.421	0.367
Mean dep. var.	0.56	0.56	0.56	0.56	0.56	0.56

Note: *p<0.1; **p<0.05; ***p<0.01; †15 observations automatically dropped for predicting success perfectly. For OLS results, district-vote two-way clustered SE are reported in parentheses. Conley SE available upon request. Probit results are reported as average marginal effects, with district-level clustered SE reported in parentheses.

are Republicans (the remainder's affiliation – about 5% – is unknown, sometimes because legislators simultaneously occupy Ministerial positions and are thus unaffiliated). Since liberal representatives tended to be elected in districts with a lower prevalence of slavery (and conservatives in high-prevalence districts), it is unsurprising for part of the effect observed in column 1 to be captured by political affiliation. A legislator's occupation seems to play a role in his voting behavior only when this legislator is reported as an "Owner/Businessman": these legislators tend to vote more against abolition. This is not a surprising result, but including a whole set of occupations barely affects the regression, and owners only represent 3.3% of legislators. The magnitude of the relationship also decreases when we introduce demographic controls, which may reflect the fact that coerced workers are better represented in districts with a higher share of free colored individuals, but also that the latter may as well constitute an outcome of slavery prevalence.

In fact, the political affiliation of a representative, his occupation, and demographic covariates are possibly endogenously determined by both the prevalence of slavery and the vote for or against abolition (in particular, the share of free colored individuals in the population appears to 'collide' with the relationship between the prevalence of slavery and voting behavior in column 4). In the end, demographic and political variables have features of confounders that we need to control for, and also of colliders that bias conservatively our estimates. Including these controls downplays the magnitude of our estimates, although not enough to remove their significance.

A.1.2 Heteroskedasticity-based identification

Instruments generated from the data are generally not a silver bullet to endogeneity issues, but they provide a valuable tool for identification and testing. This approach is especially appropriate with triangular systems, such as what arises in classical measurement error frameworks (a common issue when dealing with nineteenth century census data and with imperfect proxies) and omitted variables (we may e.g. be concerned that within-province variations in cultural norms influence both the successful establishment of quilombos and subsequent voting decisions). Dietrich and Wright (2015) and Fails (2019) also used a similar two-pronged identification strategy to analyze economic determinants of institutional change. On the issue of slavery, Bezemer et al. (2014) used a similar identification strategy to examine the long run development outcomes associated with indigenous slavery in Sub-Saharan Africa. Also related, Depetris-Chauvin and Özak (2020) used a similar strategy to analyze the early determinants of economic specialization.

In our context, the use of heteroskedasticity-based instruments provides three main advantages. First and foremost, they provide a uniform, albeit informal, test for the validity of our main instruments. In a context with multiple instrumental variable strategies and, given the limitations of historical data, limited possibilities to run placebos, it is helpful to compare estimates obtained from completely different identification assumptions, and reassuring that they yield numerically similar estimates (Baum and Lewbel, 2019). Second, they allow us to run overidentification tests whenever our main instruments would otherwise leave regression models just-identified. Third, they improve the precision of our estimates in specific cases where standard instruments are weaker and we may be concerned about weak identification. For more details on identification using moment restrictions, see Magnusson and Mavroeidis (2014) and Lewbel (2019).

We exploit heteroskedasticity with respect to strictly exogenous (geographic) regressors. To build the instruments, we run the following baseline auxiliary regressions:

$$\begin{aligned} F_{ij}^{1890} &= S_{ij}^{1872}\beta^{F} + \mathbf{x}_{ijv}'\boldsymbol{\sigma}^{F} + \delta_{v}^{F} + \zeta_{j}^{F} + \xi_{ijv}^{F} \\ Q_{ij} &= S_{ij}^{1872}\beta^{O} + \mathbf{x}_{ijv}'\boldsymbol{\sigma}^{O} + \delta_{v}^{O} + \zeta_{j}^{O} + \xi_{ijv}^{O}, \end{aligned} \tag{A.4}$$

from which the estimated residuals are used to create instruments given by $(\mathbf{x}_{ijv} - \overline{\mathbf{x}}_{ijv})'\hat{\xi}_{ijv}$, where $\overline{\mathbf{x}}_{ijv}$ is a mean-centering vector. Note that whenever a second-stage regression includes an interacted term (ie. a second endogenous variable) we run additional regressions with interactions as dependent variables. The inclusion of vote FE and time-varying controls in the second-stage regression requires the inclusion of the same FE and controls in the zero-stage regression. The heteroskedasticity-based instruments for each of our vote-invariant explanatory variable are therefore, maybe counterintuitively, time-varying.

Identification requires that the error terms of the first-stage regressions be heteroskedastic, which we verify using Breusch-Pagan tests. It also requires that the error terms of the second-stage regressions be homoskedastic. While this is hard to justify theoretically, we can at least verify empirically that their homoskedasticity cannot be rejected using Pagan-Hall tests. Hence, out of an initially large array of generated instruments, we select the subset that yields a strong association with the suspected endogenous variable subject to passing the first stage heteroskedasticity and the second stage homoskedasticity restrictions. In practice, only a handful of instruments satisfy these criteria, and this choice is atheoretical: it is driven by ex ante statistical reasons with no regard for ad hoc coefficient estimates.

In our preferred 2SLS specifications, we use latitude and distance to the coast to build instruments for both the share of immigrants in 1890 and the area occupied by quilombola land. Note that these instruments are not the same, seeing as they rely on residuals obtained from regressions with different variables as outcomes. In Appendix A.2.1, we use soil suitability to sugarcane, distance to the coast, and the human mobility index to build heteroskedasticity-based instruments for the prevalence of slavery.

A.2 Robustness checks

This section provides a number of additional robustness checks. Subsection A.2.1 *i)* proposes an original instrument for the prevalence of slavery, *ii)* tests the stability of our 2SLS results to alternative formulations of our instrument and to different instruments altogether, *iii)* presents a test of the validity of our preferred instrument, and *iv)* reports the results of replicating the RDD from Fujiwara et al. (2019). Subsection A.2.2 focuses on immigration and *i)* extends our results to a generalized linear model, *ii)* examines the stability of our results when instrumenting immigration as a flow rather than a stock, *iii)* tests alternative ways to scale the main instrument, and *iv)* considers alternative results limited to the province of São Paulo. Subsection A.2.3 focuses on proximity to freedom and *i)* extends our results to a generalized linear model, *ii)* explores alternative measures of proximity/distance to freedom, *iii)* shows that results are robust to alternative ways of dealing with outliers, *iv)* examines the stability of our preferred instrument, *v)* proposes a placebo test of our preferred instrument's exclusion restriction, *vi)* explores the possible influence of conflict with maroons (quilombolas), and *vii)* examines the relationship between slavery and quilombos. Subsection A.2.4 focuses on possible selection issues, and examines the stability of our results to *i)* selection on bills and *ii)* selection on legislators. Subsection A.2.5 focuses on multi-instrumentation and *i)* provides results instrumenting simultaneously the prevalence of slavery and 1890 immigration, *ii)* the prevalence of slavery and proximity to freedom, and *iii)* the prevalence of slavery, 1890 immigration, and proximity to freedom together.

A.2.1 Abolition voting and the prevalence of slavery

Our instrument for the prevalence of slavery interacts the average distance from municipalities' head towns to the nearest eighteenth-century caminho do ouro with a scaling variable. The discovery of large deposits of gold towards the end of the seventeenth century (soon followed by diamonds) in the province of Minas Gerais (literally, 'General Mines') justified, in the eighteenth century, the construction of these trade routes between the province and coastal areas (Zemella, 1951). These routes also helped ensure the continuous supply of mining areas in enslaved labor (from the north) and cattle (from the south). They were built by slaves, making the distance to these roads an interesting historical shifter of the prevalence of slavery into the nineteenth century (Borges Martins, 1980 and Klein and Luna, 2009). We may think of the distance to these roads as capturing historical slave-related activity. In general, proximity to caminhos do ouro is thus an indicator of higher slavery prevalence.[33]

In addition, we scale the distance to caminhos do ouro with a variable that captures, within provinces possibly distant from the roads, which municipalities were more likely to have already been settled in the seventeenth century. Our preferred scaling variable measures the repression and enslavement of Indigenous peoples in the sixteenth century. When sixteenth-century Portuguese settlers were laying the foundations of the plantation system, they first started experimenting with an enslaved Indigenous labor force. At its peak in the 1560s, Indigenous slavery counted tens of thousands of individuals. Because of a combination of widespread epidemics, continuous conflict with free Indigenous peoples, and increasing discomfort from the Crown with Indigenous enslavement after the Valladolid debate, the Portuguese turned away from Indigenous to African enslaved labor (Klein and Luna, 2009). In other words, areas where Indigenous peoples where repressed and driven out during the early days of the colonization of Brazil are more likely to have received settlers and slaves. Therefore, our instrument predicts a lower prevalence of slavery as distance to caminhos do ouro increases, except in places where Indigenous repression and enslavement were historically intensive, which were more likely to receive slaves, compared with other municipalities within the same province.[34]

In Table A.7, we report the results of instrumenting the prevalence of slavery in our baseline specification with: *i)* the instrument discussed above, *ii)* the heteroske-

[33] Figure A.11a in the Appendix A.5 provides an unconditional plot of this relationship.

[34] See Map A.9 in Appendix A.4 for a visual representation of the instrument.

dasticity-based instrument described in section A.1.2 (in this case, we exploit soil suitability to sugarcane, distance to the coast, and the human mobility index), and *iii)* the combination of both. Columns 1 to 3 present the first stages of the respective 2SLS specifications with all controls and FE, and columns 4 to 6 the corresponding second stages. Overall, the first stage regression shows a large enough association between the prevalence of slavery and our set of instruments. 2SLS estimates are slightly larger than their OLS counterparts in Table A.6. An endogeneity test is however never able to reject the null hypothesis that the endogenous variable can be treated as exogenous, so that we hereafter tend to prefer the OLS for efficiency reasons.

To be valid, this instrument should not be correlated with the decision to vote in favor or against abolition in the 1880s, except through its influence on the local prevalence of slavery. We make several observations.

First, we might worry that a 'preference for slavery' or a 'culture of abolitionism' predated the enslavement of Indigenous peoples and the establishment of the caminhos do ouro, and persisted into the nineteenth century. This seems implausible: it was northern districts that had the highest prevalence of slavery during the sugar boom, well into the nineteenth century. Unlike in the United States, slavery was never defended in Brazil on the grounds that it was a positive institution. Even the slaveholding elite appeared to defend slavery mostly as a necessary evil (Klein and Luna, 2009).

Second, the coffee planter elite's interests that played a fundamental role in perpetuating the coercion system in the second half of the nineteenth century are plausibly orthogonal to both Indigenous repression in the sixteenth century and mining interests in the eighteenth century. The emergence of the mining interests had large consequences on the configuration of economic activity, even leading to the relocation of the colony's capital from Salvador to Rio de Janeiro in 1763. We may thus be concerned that Indigenous repression and/or the location of the caminhos do ouro affected voting decisions in the 1880s through other channels than the prevalence of slavery. In particular, our main concern is that caminhos do ouro may have had a persisting impact on economic activity, other than trough slavery. If this affected voting decisions (up to centuries later), our identification assumption would be violated. However, caminhos do ouro lost their importance with the decline of the gold rush at the end of the eighteenth century. They were no longer maintained and became

Table A.7: Prevalence of slavery and voting decisions – 2SLS

	First stage			*Second stage*		
	Share of enslaved in 1872			1(Abolition vote)		
	(1)	(2)	(3)	(4)	(5)	(6)
Pred. share of enslaved				−2.776	−1.401	−1.704
				(1.108)**	(0.747)*	(0.637)***
				{1.597}*	{0.479}***	{0.477}***
Ln 16th rep. area × Dist. Gold Paths	1.4e-5		1.5e-5			
	(3.6e-6)***		(3.6e-6)***			
	{3.0e-6}***		{2.8e-6}***			
Het. instr. (Sugar)		−0.029	−0.022			
		(0.015)*	(0.014)			
		{0.019}	{0.017}			
Het. instr. (Dist. coast)		−0.279	−0.346			
		(0.129)**	(0.112)***			
		{0.193}	{0.174}**			
Het. instr. (HMI)		−17.87	−18.27			
		(3.898)***	(3.680)***			
		{5.107}***	{4.786}***			
Controls and FE	All	All	All	All	All	All
Observations	1,284	1,284	1,284	1,284	1,284	1,284
Mean dep. var.	0.148	0.148	0.148	0.560	0.560	0.560
F-stat	10.99/16.89	9.69/5.78	11.88/12.46			
B-P p-value		0.000	0.000			
P-H p-value					0.476	0.444
Hansen J p-value					0.322	0.356
Endog. test p-value				0.111	0.756	0.219

Note: *p<0.1; **p<0.05; ***p<0.01. 2SLS results with district-vote two-way clustered standard errors are reported in parentheses, and Conley standard errors (with a 250km window) are reported in curly brackets. Columns 1 and 4 use only our standard instrument, columns 2 and 5 use only heteroskedasticity-based instruments, and columns 3 and 6 use both. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE. See Table 4 for details on the tests.

free after Brazil became independent in 1822.[35] In addition, Indigenous peoples were almost entirely driven out from early settlement municipalities, so that our scaling variable is unlikely to affect voting decisions other than through settlement patterns.

Importantly, these concerns are further alleviated as we systematically control for

[35] In particular, the internal slave trade of the second half of the nineteenth century occurred mostly by sea and, increasingly, through the fast-expanding railway system (Graham, 2008 and Slenes, 2008). Caminhos do ouro were largely unpaved. They had become impractical and were left largely untended, at least until they became a cultural attraction in the early twenty-first century (Castriota, 2008 and do Carmo Pires, 2017).

province and vote intercepts, in addition to a wide range of controls capturing baseline year economic activity (notably land suitability controls for the main exports crops, population density, human mobility, and distance to rivers and to the coast).

We view the (non statistically significant) difference between OLS and 2SLS estimates as most likely stemming from heterogeneous treatment effects (Bisin and Moro, 2021). To understand 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. The effect of the 1872 prevalence of slavery on voting decisions is thus bound to be heterogeneous. 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. Now, because our instrument predicts the prevalence of slavery in the distant past, 'take-up of the treatment' – i.e. acquisitions and transfers of enslaved individuals – continued long after the gold paths determined the allocation of captive workers, including after we actually observe the treatment. 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 negative effect of the prevalence of slavery on voting decisions 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 of slavery on voting decisions. Importantly, this does not invalidate the instrument ; it simply means that it captures a local average treatment effect, i.e. the effect of the prevalence of slavery in locations where slaveholders had more to lose from abolition.

Table A.8 tests the stability of our results to different controls, formulations of the main slavery instrument, and an altogether different instrument. An important benefit of using an interaction instrument is that we can flexibly evaluate in what way it predicts slavery prevalence. In table A.8, we show that the un-interacted terms themselves do not matter in explaining slavery, i.e. distance to the nearest caminho do ouro and areas of Indigenous enslavement in the sixteenth century are never significant in the first stage. The interaction term remains almost numerically identical when un-interacted terms are dropped (in which case the instrument cannot be interpreted other than as the distance to gold paths weighted by Indigenous repression), which results in a stronger instrument, as un-interacted terms have no explanatory power. This implies that the variation driving the instrument comes from the differential effect of proximity to gold paths in zones of 16th century Indigenous repression compared to other areas (which is much less likely to violate the exclusion restriction), not distance to gold paths or Indigenous repression themselves. We can thus also flexibly control for the latter.

In Table A.8, we also consider the approach traditionally adopted in the literature (see e.g. Acharya et al., 2016) and instrument the prevalence of slavery with the average soil suitability to coffee, controlling for suitability to other crops. An issue with this approach in our setting is the level of aggregation: at the district level, many geographical covariates likely to correlate with the prevalence of slavery at a lower level are not significant (as illustrated in Table A.6). The resulting instrument is thus rather weak, but still yields results in line with our preferred specification.

Column 1 is similar to the baseline specification in Table A.7, but additionally controls for a dummy indicating mining areas and distance to the closest diamond mine. Column 2 includes the un-interacted terms as additional instruments, and shows that they have no explanatory power (results remain comparable, although significance falls just short of conventional levels and the F-stat drops below the usual weak identification threshold). Column 3 instead includes un-interacted terms as controls, which results in a larger impact of slavery, but is consistent with previous results in showing that heterogeneous treatment effects likely drive LATE estimates to be higher in magnitude (compliers are most likely to benefit from slavery). Column 4 is similar to our baseline specification but uses 16th to 18th century repression instead of 16th century only (this results in a slightly weaker instrument but similar results), and column 5-6 are analogous to column 2-3 (and provide similar results) with this new version of the instrument. Column 7 uses 17th-18th century repression while controlling for both Distance to gold paths and 16th century repression (which is consistent but results in a weaker instrument and falls slightly short of usual significance thresholds). Column 8 uses soil suitability to coffee to instrument slavery. This is a quite weak instrument, but still provides consistent results in terms of magnitude.

In addition, in Table A.9 we implement a falsification test and show that neither caminhos do ouro, nor Indigenous repression, nor their interaction seem to affect economic activity in the early twentieth century other than via slavery. The validity

Table A.8: Slavery and voting decisions – Sensitivity and alternative instr.

	𝟙(Abolition vote)							
Panel A: 2nd stage	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pred. share of enslaved	-4.043***	-1.838*	-8.608**	-3.137***	-1.394*	-5.980**	-2.828	-2.840
	(1.344)	(0.942)	(3.975)	(1.219)	(0.732)	(2.677)	(2.129)	(7.596)
Gold mining	-0.134							
	(0.127)							
Ln(Dist. diamond)	0.244**							
	(0.094)							
Ln 17-18th rep. area	-0.0028					-0.0032		
	(0.011)					(0.013)		
Dist. Gold Paths			6.0e-4			3.8e-4	2.2e-4	
			(4.5e-4)			(3.4e-4)	(2.8e-4)	
Ln 16th rep. area			0.036*			0.034**	0.015	
			(0.020)			(0.014)	(0.012)	
Ln 16th rep. area × Ln 17-18th rep. area						-0.0018		
						(0.0023)		
Panel B: 1st stage	*Share of enslaved*							
Ln 16th rep. area × Dist. Gold Paths	1.1e-5***	1.1e-5**	1.1e-5**	1.4e-5***	6.7e-6	6.7e-6		
	(3.0e-6)	(4.8e-6)	(4.8e-6)	(3.6e-6)	(4.8e-6)	(4.8e-6)		
Gold mining	-0.011							
	(0.028)							
Ln(Dist. diamond)	0.039**							
	(0.016)							
Ln 17-18th rep. area	-0.0019				-5.4e-4	-5.4e-4		
	(0.0020)				(0.0020)	(0.0020)		
Dist. Gold Paths		1.3e-5	1.3e-5		7.7e-5	7.7e-5	1.4e-4***	
		(4.4e-5)	(4.4e-5)		(5.4e-5)	(5.4e-5)	(3.8e-5)	
Ln 16th rep. area		0.0016	0.0016		0.0031**	0.0031**	0.0037**	
		(0.0017)	(0.0017)		(0.0012)	(0.0012)	(0.0015)	
Dist. Gold Paths × Ln 17-18th rep. area				-6.8e-6	-1.4e-5	-1.4e-5	-2.1e-5***	
				(6.8e-6)	(9.0e-6)	(9.0e-6)	(8.1e-6)	
Ln 16th rep. area × Ln 17-18th rep. area					-1.5e-4	-1.5e-4		
					(2.4e-4)	(2.4e-4)		
Coffee suit.	0.0016*	0.0012	0.0012	0.0012	0.0012	0.0012	0.0012	7.2e-4
	(9.2e-4)	(9.8e-4)	(9.8e-4)	(9.6e-4)	(9.8e-4)	(9.8e-4)	(9.5e-4)	(9.9e-4)
Controls & FE	All	All	All	All	All	All		
Observations	1,284	1,284	1,284	1,284	1,284	1,284	1,284	1,284
MO-P F-stat	13.02	5.471	5.454	7.505	3.619	3.755	6.592	0.520

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors in parentheses. This table presents alternative 2SLS specifications, with 2nd stages in Panel A and 1st stages in panel B. Column 1 only uses our preferred standard instrument, but additionally controls for mining activity (mining area dummy and distance to closest diamond mine) and 17th century Indigenous repression. Column 2 uses our preferred standard instrument and includes the non-interacted terms as instruments, while column 3 includes these non-interacted terms as flexible controls (results are qualitatively similar with higher polynomials). Column 4 uses 16th to 18th century repression (omitting interactions), while column 5 and 6 proceed similarly but use interacted terms as instruments and controls respectively. Column 7 uses 17th-18th repression only, controlling for 16th century repression. Column 7 uses the district-level average soil suitability to coffee cultivation as an instrument. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

of our preferred instrument crucially rests on the assumption that the distance to gold paths, scaled using 16th century Indigenous repression, does not affect emancipation voting other than through its influence on the geographical distribution of enslaved populations across districts, within provinces. An important concern is that gold paths and the repression of Indigenous peoples may have influenced economic activity through channels distinct from slavery alone (e.g. urbanization). Economic activity might have in turn affected abolition voting at the end of the 19th century. This is unlikely to be the case within provinces, in particular because gold paths appear to have ceased to matter significantly passed the decline of the Gold Rush at the end of the 18th century. In addition, Indigenous peoples were driven out from settlement areas passed the 16th century. We only use Indigenous repression to predict more accurately which areas were more likely to receive enslaved Africans after the 16th century, and results are robust to using alternative scaling variables. In order to formally test this assumption, we examine the influence of our instrument and its main components on economic outcomes in Table A.9. Ideally, we would examine this relationship using pre-treatment outcomes. There are however no available data that would allow us to do so, and we therefore resort to using economic outcomes closest to the treatment. Specifically, we evaluate the association between our instrument and the value of industrial production in 1907 (columns 1-4) and the GDP per capita in 1920 (both from the IBGE and digitized by the IPEA), at the cross-section and, importantly, netting out the prevalence of slavery in addition to our usual vector of controls and province FE. If, having netted out slavery, the distance to gold paths and Indigenous repression did influence economic activity in their own right, we would expect to find placebo effects in Table A.9. The coefficients associated with the instrument and its components are reassuringly never significant, lending further support to the validity of the instrument.

Table A.9: Slavery instrument and further economic outcomes

	Prod. value 1907				GDP per cap. 1920			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dist. Gold Paths	-0.0044		-0.0097		0.000038		0.00011	
	(0.022)		(0.066)		(0.000055)		(.00010)	
Ln 16th rep. area		-1.522	-1.475			0.0043	0.0049	
		(1.115)	(1.122)			(0.0041)	(0.0041)	
Ln 16th rep. area × Dist. Gold Paths			-0.0027	-0.0014			-0.000013	-3.5e-6
			(0.0023)	(0.0058)			(9.2e-6)	(4.2e-6)
Observations	334	237	237	237	1,277	956	956	956
R-squared	0.521	0.568	0.569	0.517	0.516	0.493	0.492	0.495
Controls and Prov. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors clustered at the 1872 municipality level in parentheses. Columns 1-4 and 5-8 examine the influence of our instrument and its components on the value of industrial production in 1907 and the GDP per capita in 1920. Controls and FE: 1872 share of slaves, population density, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, human mobility index, share of free colored and of literacy, and province FE.

As an alternative approach and verification exercise, we also replicate the RDD in Fujiwara et al. (2019). This exploits the meridian of the Treaty of Tordesillas, used to divide the Spanish and Portuguese colonial empires in 1494. The land East of the meridian was assigned to the Portuguese, who Fujiwara et al. (2019) argue had a comparative advantage in the Atlantic slave trade. We report the RDD results in Table A.10, and provide discontinuity plots in Figure A.3. Similarly to Fujiwara et al. (2019), our preferred specifications use a 73 km interval around the discontinuity to account for the uncertainty surrounding its exact placement. Columns 1 to 3 use local linear regressions respectively with the full sample, correcting for the 73 km interval, and both correcting for the interval and limiting the sample to districts within a 1000 km distance from the discontinuity (again, following Fujiwara et al., 2019). Columns 4 to 6 proceed similarly with quadratic polynomials. Optimal bandwidth choices are computed using Calonico et al.'s (2014) procedure. Our favorite specification (column 2) uses local linear regressions and yields LATE estimates remarkably close to those provided by our standard instrument in Table A.7.

Table A.10: Prevalence of slavery and voting decisions – RDD

	$\mathbb{1}$(Abolition vote)					
	Linear			*Quadratic*		
	All	$d > 73$	$73 < d < 1000$	*All*	$d > 73$	$73 < d < 1000$
	(1)	(2)	(3)	(4)	(5)	(6)
Running variable: Distance to Tordesillas line						
Conventional	0.221	−2.846	−2.346	0.334	−6.452	−4.462
	(0.138)	(0.600)***	(0.587)***	(0.196)*	(1.853)***	(0.951)***
Robust bias-corrected	0.210	−3.197	−2.575	0.369	−6.814	−4.796
	(0.168)	(0.680)***	(0.632)***	(0.218)*	(1.928)***	(1.004)***
Bandwidth	322.1	145.9	164.8	357.3	218.9	262.4
Bias BW	453.5	392.8	441.3	485.2	446.7	548.4
Observations	1284	1237	809	1284	1237	809
Effective obs.	245	62	62	278	118	136

Note: *p<0.1; **p<0.05; ***p<0.01. This table reproduces the regression discontinuity approach from Fujiwara et al. (2019). Optimal bandwidths are selected using the procedure from Calonico et al. (2014).

Figure A.3 illustrates the discontinuity in the prevalence of slavery and votes on emancipation around the boundary, and the absence of a discontinuity in controls, such as rainfall, distance to the coast, the share of free colored, and the literacy rate.

A.2.2 Abolition voting and immigration

Table A.11 replicates Table 2 and includes probit specifications. The latter again closely mirror their OLS analogues.

Table A.12 proposes an alternative formulation of our religion-based immigration instrument, whereby variables are expressed in differences rather than in levels. This yields results consistent with our main 2SLS specification, and estimates similar to the corresponding OLS specification in Table 2.

Table A.13 assesses the stability of our instrument across various specifications. Column 1 corresponds to our baseline specification, where the instrument uses pre-

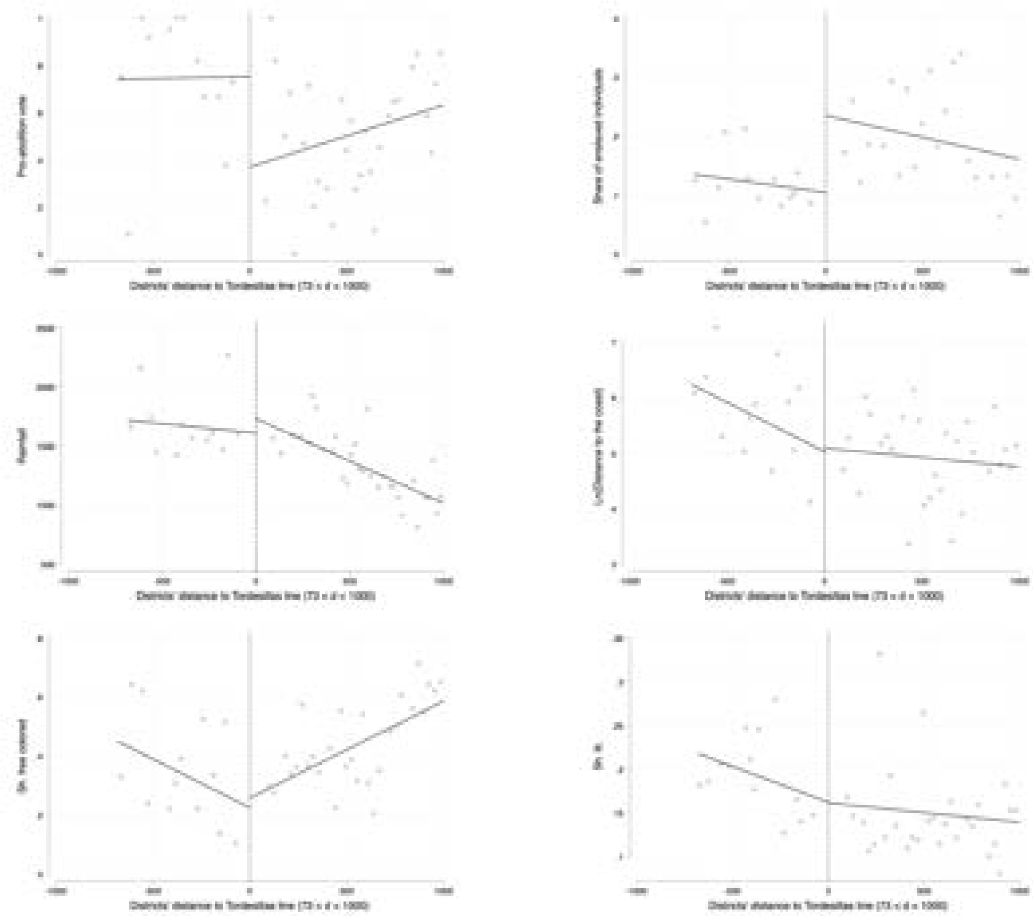


Figure A.3: Discontinuity plots

Notes: Regression discontinuity plots with distance to the Tordesillas line as the running variable. A first order polynomial is fitted on each side of the discontinuity and each point corresponds to a within-bin sample average. The number of bins is optimally chosen via the mimicking-variance evenly spaced method (Calonico et al., 2015).

dicted Roman Catholic population and is scaled using actual district population. In columns 2 and 3, we address the possible concern that district population may itself be an outcome of immigration, and use instead total population and predicted district population respectively as scaling variables. Results remain stable, although the instrument becomes weaker in the latter case. Columns 4 and 5 evaluate the robustness of the instrument to relaxing one of the main assumptions underlying its construction, namely that the number of foreign Roman Catholics can be approximated using overall Roman Catholics and native population. Coefficients remain again reassuringly stable when we use instead the actual Roman Catholic population, whether the instrument is scaled using district population (column 4) or total population (column

Table A.11: Immigration and voting decisions – OLS and GLM

	𝟙(Abolition vote)							
	OLS				*GLM*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1872 share of enslaved	-0.840	-0.754	-1.140	-1.163	-0.818	-0.719	-1.046	-1.215
	(0.182)***	(0.200)***	(0.347)***	(0.285)***	(0.294)***	(0.311)**	(0.501)**	(0.396)***
	{0.353}**	{0.355}**	{0.488}**	{0.433}***				
1872 share of free foreigners	-2.894		-3.592	1.970	-2.969		-3.456	2.379
	(1.197)**		(2.322)	(1.483)	(1.216)**		(2.041)*	(1.861)
	{0.713}**		{1.943}*	{1.327}				
1890 share of foreigners	3.432		2.380	3.924	3.383***		2.621	3.643
	(0.907)***		(3.153)	(0.858)***	(0.947)		(2.494)	(1.117)***
	{0.703}***		{3.310}	{1.124}***				
$\Delta_{1890\text{-}1872}$		1.942				1.974		
		(0.523)***				(0.565)***		
		{0.569}***						
1872 sh. foreigners × sh. enslaved			2.868				1.769	
			(10.850)				(10.60)	
			{9.511}					
1890 sh. foreigners × sh. enslaved			6.327				4.711	
			(16.844)				(13.70)	
			{18.674}					
Controls	All	All	All	All	All	All	All	All
Province & Vote FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,284	1,284	1,284	453	1,269	1,269	1,269	446
R-squared	0.428	0.426	0.428	0.398	0.376	0.374	0.376	0.390
Mean dep. var.	0.56	0.56	0.56	0.43	0.56	0.56	0.56	0.43

Note: *p<0.1; **p<0.05; ***p<0.01. For OLS results, district-vote two-way clustered standard errors are reported in parentheses, and Conley standard errors (with a 250km window) are reported in curly brackets. Probit results are reported as average marginal effects, with district-level clustered standard errors reported in parentheses. Columns 1 and 2 present OLS results using immigrants in 1872 and 1890 and the differential between the two respectively. Column 3 introduces interaction terms between the prevalence of slavery and each immigration wave, and column 4 focuses on the Sudeste region. Columns 5-8 reproduce the analysis with a probit. Controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored and literacy.

5).

The main identification assumption underlying the validity of our instrument is that municipality level conditions that may have influenced the inflow of immigrants of any given religion must not affect abolition patterns in the 1880s (within provinces). This leads us to systematically controlling for immigration in 1872, which (in addition to being mechanically correlated with immigration in 1890) possibly has a distinct effect on emancipation-related voting (so that the variation we use to identify the influence of 1890 immigrants is in the religious composition of municipalities' foreign populations, not in the actual size of the immigrant population). One might still be concerned that immigrants from specific religions or nationalities selected their destinations based on the possibility that these would be more inclined to vote for

Table A.12: Immig. and voting decisions – Instrumenting 1872-1890 net immigration

	First stage			*Second stage*		
		$\Delta_{1890\text{-}1872}$			$\mathbb{1}$(Abolition vote)	
	(1)	(2)	(3)	(4)	(5)	(6)
Pred. $\Delta_{1890\text{-}1872}$				2.260**	1.837***	1.892***
				(0.888)	(0.447)	(0.453)
$Z_{1872\text{-}1890}$	-0.514***		-0.151***			
	(0.120)		(0.055)			
Het. instr. (latitude)		-0.072***	-0.066***			
		(0.0098)	(0.010)			
Het. instr. (dist. coast)		-0.366***	-0.328***			
		(0.086)	(0.081)			
Controls & FE	All	All	All	All	All	All
Observations	1,284	1,284	1,284	1,284	1,284	1,284
MO-P F-stat	18.395	41.784	40.718			
B-P p-value		0.000	0.000			
P-H p-value					0.759	0.510
Hansen J					0.260	0.377

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors in parentheses. This table reproduces the results of Table 4 using the inflow of immigrants instead of the stock. Accordingly, we adapt the instrument of section 5.1 as $Z_{i1872\text{-}1890} = \frac{1}{\widehat{P}_{i1890}} \sum_r \widehat{\alpha}_{ir1872} \widehat{\Delta}^{-i}_{r1890\text{-}1872}$. Columns 1-3 present alternative 2SLS first stages, and columns 4-6 the corresponding second stages. Columns 1 and 4 only use our standard instrument, columns 2 and 5 only use het.-based instruments, and columns 3 and 6 use both. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE. See Table A.7 for details on the tests.

emancipation. We control for this possibility in column 6 of Table A.13, and also add controls for colonial enclaves (núcleos coloniais) in column 7. Results remain qualitatively similar in both cases.

Table A.14 reduces our sample to the province of São Paulo only. This presents the drawback of drastically reducing the variation we can muster, but is interesting because of the quasi-random nature of immigrants' assignment across districts in 1890. From the second half of the 1880s, immigration in São Paulo was driven by an official immigration program. European immigrants were sponsored and hosted in São Paulo before being dispatched to locations in need of agricultural labor. They had no agency as to their final destination, and could not find work outside the official program for the first few years after their arrival. These results suggest that immigration was indeed a key factor driving abolition voting in the province, corroborating the historical narrative.

A.2.3 Abolition voting and the cost of coercion

Table A.15 replicates Table 3 with a probit model. All estimates are comparable in magnitude and significance with OLS estimates.

Table A.13: Immig. and voting decisions – Alternative instruments and instruments validity

Panel A: 1st stage	1890 share of foreigners (1)	(2)	(3)	(4)	(5)	(6)	(7)
Z_{1890}	-0.113	3.895	-0.080	-0.0035	0.794	-0.137	-0.094
	(0.020)***	(16.580)	(0.025)***	(0.0007)***	(0.204)***	(0.026)***	(0.021)***
Share of enslaved	0.050	-0.015	0.030	0.045	-0.017	0.055	0.053
	(0.037)	(0.038)	(0.038)	(0.037)	(0.034)	(0.024)**	(0.034)
1872 share of free foreigners	1.020	0.847	1.039	1.029	0.265	0.963	0.991
	(0.049)***	(0.408)**	(0.071)***	(0.055)***	(0.194)	(0.041)***	(0.043)***
Panel B: 2nd stage	$\mathbb{1}$(Abolition vote)						
Pred. 1890 share of foreigners	3.593	33.237	4.803	3.442	6.356	2.444	2.810
	(1.294)***	(121.080)	(2.275)**	(1.340)**	(2.125)***	(1.225)**	(1.522)*
Share of enslaved	-0.838	-0.513	-0.825	-0.840	-0.808	-1.089	-0.812
	(0.181)***	(1.542)	(0.183)***	(0.184)***	(0.155)***	(0.242)***	(0.184)***
1872 share of free foreigners	-3.045	-30.787	-4.178	-2.904	-5.631	-2.509	-2.246
	(1.394)**	(114.095)	(2.017)**	(1.412)**	(2.328)**	(1.305)*	(1.636)
Controls & FE	All	All	All	All	All	All	All
Observations	1,284	1,284	1,284	1,284	1,284	1,284	1,284
F-stat	32.54	0.056	10.87	22.97	15.75	27.30	20.39

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors in parentheses. In columns 1 to 3, the instrument uses the predicted foreign Roman Catholic population and is weighted using district, total and predicted district population respectively (predicted district population is given by the formula $\widehat{P}_{i1890} = P_{ij1872}(1+(P^{-i}_{1890} - P^{-i}_{1872})/P^{-i}_{1872})$). In columns 4 and 5, the instrument uses the actual Roman Catholic population and is weighted using district and total population respectively. Column 6 controls for individual country shares and Column 7 controls for núcleos coloniais. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

In Table A.16 we extend our measures of distance to freedom by considering the distance to the N closest quilombos, $N \in \{1;9\}$. As expected, the magnitude and the significance of the estimates increases with N: a greater number of quilombos captures better prospects of freedom. Maintaining the distance constant, the promise of freedom is greater when local features of the land are more conducive to allowing successful escapes, as captured by the number of quilombos. For distances relative to a high number of quilombos, the support for coercive institutions is entirely driven by municipalities for which distance to freedom is large, i.e. which faced lower coercion costs. Again, this suggests that heterogeneity with respect to the cost of enforcing coercion plays an important role in determining voting behavior.

In Table A.17, we consider alternative ways to deal with outliers. Column 1 keeps the explanatory variable as originally measured by the INCRA (2020), column 2 uses this variable in logs to penalize extreme values, column 3 drops the two outlier districts, and column 4 does both.

Table A.18 examines the validity and stability of our preferred instrument under an alternative specification, in which the un-interacted terms are used as controls rather than instruments, so that the influence of distance to freedom is identified using only the differential effect of ruggedness by the degree of remoteness, not ruggedness

Table A.14: Immigration and voting decisions – São Paulo only

𝟙(Abolition vote)	
	(1)
Pred. 1890 sh. of foreigners	5.951***
	(0.318)
1872 sh. of enslaved	0.317***
	(0.088)***
1872 sh. of free foreigners	-12.41***
	(1.580)
Observations	98
Mean dep. var.	0.480
R-squared	0.524

Note: *p<0.1; **p<0.05; ***p<0.01. District-level clustered standard errors in parentheses. This table provides OLS results focusing on the province of São Paulo only. Controls include all but legislator-level covariates: population density, coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, human mobility index, and share of free colored and of literacy.

or remoteness themselves. Note that results also remain very similar when we flexibly control for nonlinearities in un-interacted terms (not presented). Columns 1-2 provide the results of a specification with no interaction term, with average quilombola land the only endogenous variable instrumented by our standard instrument, but with un-interacted terms as controls. Again, quilombos have no influence in and of themselves, despite the instrument being strong enough following the usual standards. Columns 3 and 4 provide the first stages of a specification in which the average area of quilombola land and its interaction with slavery are instrumented by our standard instrument and its interaction with slavery, with the second stage being provided in column 7. Columns 5 and 6 proceed similarly but also include heteroskedasticity-based instruments, with the second stage being provided in column 8. Identification is slightly weaker for these regressions, but results remain overall qualitatively very similar to those presented in Table 5.

In Table A.19, we run a placebo test of our main instrument's validity. A key feature of our proximity to freedom measure is that by themselves, quilombos have

Table A.15: Exit and voting decisions - GLM

	$\mathbb{1}$(Abolition vote)					
	(1)	(2)	(3)	(4)	(5)	(7)
Share of enslaved	−0.880	−0.917	−1.181	−0.826	−0.657	−0.128
	(0.335)***	(0.338)***	(0.378)***	(0.342)**	(0.357)*	(0.455)
Av area quilombola	0.000013	−.00093	0.0046			
	(0.00044)	(0.00060)	(0.0015)***			
Sh. enslaved × Av. area quil.		0.0081				
		(0.0037)**				
Ln av. dist. 1 quil.				−0.0028	0.0076	−0.021
				(0.0034)	(0.0073)	(0.0083)**
Sh. enslaved × Ln av. dist. 1 quil.					−0.073	
					(0.043)*	
Controls	All	All	All	All	All	All
Province & Vote FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,248	1,248	453	1,269	1,269	453
R-squared	0.364	0.366	0.374	0.368	0.369	0.370
Mean dep. var.	0.56	0.56	0.43	0.56	0.56	0.43

Note: *p<0.1; **p<0.05; ***p<0.01. Probit results are reported as average marginal effects, with district-level clustered standard errors reported in parentheses. Columns 1-3 use the average area occupied by land belonging to quilombolas as a measure of distance to freedom, introduced alone (column 1), interacted with the prevalence of slavery (column 2), and focusing on the Sudeste region (column 3). Columns 4-6 proceed similarly with the average distance to the closest municipality with at least one quilombo. Strictly exogenous controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI. Controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored and literacy.

no influence on legislators' voting decisions. If our instrument is valid and indeed only impacts voting decisions through its influence on the location and size of quilombos, we should therefore observe no effect of the instrument in the reduced form regression with no interaction term, similarly to the equivalent OLS and 2SLS regressions. If, on the contrary, ruggedness, remoteness, and their interaction influence voting decisions on abolition-related roll calls through different channels than that of proximity to freedom (e.g. trade), than we would expect a statistically significant effect of the instrument in the reduced form with no interaction. Columns 1 and 2 of Table A.19 respectively provide results for the equivalent OLS and 2SLS specifications (for the sake of comparison), whereas column 3 presents results of the reduced form. Reassuringly, none of the coefficient associated with the instrument's component are statistically significant, which supports the validity of the exclusion restriction. (And they are also non-significant if added individually.)

In Table A.20, we examine the influence of conflict with quilombolas. Indeed, a

Table A.16: Exit and voting decisions – Distances to freedom

	𝟙(Abolition vote)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Strictly exogenous controls, no interaction								
1872 Share of enslaved	-1.175	-1.093	-1.081	-1.123	-1.177	-1.224	-1.235	-1.234	-1.291
	(0.448)***	(0.428)**	(0.418)***	(0.423)***	(0.429)***	(0.428)***	(0.424)***	(0.424)***	(0.410)***
	{0.267}***	{0.247}***	{0.237}***	{0.238}***	{0.239}***	{0.278}***	{0.287}***	{0.287}***	{0.298}***
Ln av. dist. *N* quil.	-0.003	-0.008	-0.009	-0.007	-0.005	0.004	0.001	0.001	-0.014
	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.013)	(0.015)	(0.015)	(0.015)
	{0.006}	{0.004}*	{0.005}	{0.008}	{0.009}	{0.012}	{0.014}	{0.014}	{0.016}
R-squared	0.256	0.258	0.259	0.257	0.256	0.255	0.255	0.255	0.256
	Panel B: Strictly exogenous controls, with interaction								
1872 Share of enslaved	-0.774	-0.617	-0.622	-0.603	-0.360	-0.267	-0.149	-0.151	0.120
	(0.394)**	(0.463)	(0.466)	(0.465)	(0.439)	(0.438)	(0.495)	(0.496)	(0.569)
	{0.252}***	{0.295}**	{0.284}**	{0.274}**	{0.270}	{0.252}	{0.325}	{0.324}	{0.495}
Ln av. dist. *N* quil.	0.027	0.014	0.011	0.014	0.027	0.050	0.054	0.054	0.072
	(0.012)**	(0.014)	(0.013)	(0.013)	(0.014)*	(0.018)***	(0.025)**	(0.025)**	(0.025)***
	{0.012}**	{0.010}	{0.012}	{0.013}	{0.012}**	{0.010}***	{0.014}***	{0.014}***	{0.029}**
Sh. enslaved × Ln av. dist. *N* quil.	-0.207	-0.146	-0.136	-0.149	-0.223	-0.300	-0.311	-0.310	-0.375
	(0.070)***	(0.083)*	(0.080)*	(0.080)*	(0.081)***	(0.084)***	(0.102)***	(0.102)***	(0.105)***
	{0.063}***	{0.061}**	{0.061}**	{0.058}**	{0.059}***	{0.060}***	{0.066}***	{0.066}***	{0.104}***
R-squared	0.270	0.263	0.263	0.262	0.265	0.266	0.266	0.265	0.266
N	1	2	3	4	5	6	7	8	9
Observations	1,284	1,284	1,284	1,284	1,284	1,284	1,284	1,284	1,284
Province & Vote FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.56

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered standard errors in parentheses. Conley standard errors (with a 250km window) in curly brackets. In columns 1-9, we measure distance to freedom by the distance to the closest N quilombos, with $N \in \{1;9\}$. Strictly exogenous controls include coffee, sugar, and cotton suitability, rainfall, longitude and latitude, distance to coast and to closest river, and average HMI.

Table A.17: Exit and voting decisions – outliers

	𝟙(Abolition vote)			
	(1)	(2)	(3)	(4)
1872 Share of enslaved	-0.971	-1.083	-0.990	-1.094
	(0.236)***	(0.226)***	(0.238)***	(0.233)***
	{0.368}**	{0.393}***	{0.375}***	{0.388}***
Av. quilombo area	-0.00017	-0.046	-0.0011	-0.047
	(0.000054)***	(0.023)**	(0.00074)	(0.022)**
	{0.000072}**	{0.030}	{0.00073}	{0.030}
Sh. enslaved × Av. quil. area	0.0038	0.149	0.0085	0.143
	(0.0027)	(0.080)*	(0.0047)*	(0.079)*
	{0.0024}	{0.085}*	{0.0039}**	{0.085}*
Outliers excluded	No	No	Yes	Yes
In logs	No	Yes	No	Yes
Controls and FE	All	All	All	All
Observations	1,284	1,284	1,263†	1,263†
R^2	0.423	0.423	0.420	0.420

Note: *p<0.1; **p<0.05; ***p<0.01; † two outlier districts dropped, i.e. 26 obs. out of 1586, and 21 out of the 1284 for which a vote is registered. District-vote two-way clustered standard errors are reported in parentheses, and Conley standard errors (with a 250km window) are reported in curly brackets. This table compares different ways to deal with outlier districts. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

possible concern is that escapes from plantations were frequently violent, in particular towards the second half of the nineteenth century. Intuitively, one might therefore think that the possibility of violent conflict with quilombolas may influence voting

Table A.18: Exit and voting decisions – 2SLS sensitivity

	No interaction		*With interaction*					
	1st stage	*2nd stage*	*1st stages*				*2nd stages*	
	Av. area quil.	𝟙(Ab. vote)	*Av. area quil.*	*Av. area quil.×ensl.*	*Av. area quil.*	*Av. area quil.×ensl.*	𝟙(Abolition vote)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pred. av. area quilombola		-0.00043					-0.0025	-0.0016
		(0.0015)					(0.0016)	(0.0010)
Pred. av. area quil. × Sh. slaves							0.020***	0.014**
							(0.0064)	(0.0057)
1872 sh. ensl.	66.48	-0.889***	204.2	-18.01	129.0*	-1.687	-1.072***	-1.032***
	(54.98)	(0.220)	(130.4)	(23.98)	(71.32)	(12.12)	(0.250)	(0.234)
Av. TRI	49.14***	-0.026	77.29***	5.591	39.18**	4.017	0.0085	0.0019
	(15.13)	(0.060)	(29.99)	(4.554)	(16.03)	(2.801)	(0.062)	(0.058)
Travel time	1.333***	-0.00060	1.492***	0.029	0.648***	-0.0063	-0.00084	-0.0010
	(0.237)	(0.0012)	(0.303)	(0.046)	(0.162)	(0.024)	(0.0012)	(0.00081)
Av. TRI × travel time	-1.421***		-1.531**	-0.085	-0.872**	-0.090		
	(0.384)		(0.693)	(0.093)	(0.368)	(0.067)		
Av. TRI × Sh. ensl.			-129.4	8.213	-188.0**	-25.00		
			(126.4)	(23.85)	(79.34)	(15.32)		
Travel time × Sh. ensl.			-1.645	1.207**	-0.612	0.650***		
			(2.689)	(0.581)	(1.219)	(0.208)		
Av. TRI × travel time × Sh. slaves			0.841	-0.725	2.231	0.165		
			(3.416)	(0.664)	(1.969)	(0.367)		
Het. instr. (latitude)					0.265***	0.068***		
					(0.028)	(0.0072)		
Het. instr. (dist. coast)					0.624***	0.048***		
					(0.037)	(0.0080)		
Controls and FE	All	All	All	All	All	All	All	All
Observations	1263†	1263†	1263†	1263†	1263†	1263†	1263†	1263†
Mean dep. var.	23.70	0.556	23.70	2.901	23.70	2.901	0.556	0.556
MO-P F-stat		13.52					3.512‡	38.12‡
B-P p-value					0.000	0.000		
P-H p-value								0.7970
Hansen J p-value							0.864	0.499
Endog. test p-value		0.607					0.170	

Note: *p<0.1; **p<0.05; ***p<0.01; † two outlier districts dropped, i.e. 26 obs. out of 1586, and 21 out of the 1284 for which a vote is registered; District-vote two-way clustered standard errors in parentheses. ‡Kleibergen-Paap F-Statistic, as Montiel Olea-Pflueger does not apply to multiple endogenous variables. Columns 1-2 present 1st and 2nd stages of a specification with no interaction term. Columns 3-4 present 1st stages estimates of a specification with interaction, and column 7 the corresponding 2nd stage. Columns 5-6 present 1st stages estimates of a specification with interaction and including heteroskedasticity-based instruments, and column 8 presents the corresponding 2nd stage. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE. See Table A.7 for details on the tests.

decisions in a way that competes with our measure of proximity to freedom. We believe that this is unlikely, notably because our measure of proximity to freedom relies on *surviving* quilombos, as measured by the INCRA in 2020. We only use the existence of surviving quilombos as an indication that some features of the land were favorable to allowing enslaved individuals to make it in the wild when they managed

Table A.19: Exit and voting decisions – Placebo test

	𝟙(Abolition vote)		
	(1)	(2)	(3)
1872 Share of enslaved	-0.912***	-0.863***	-0.896***
	(0.232)	(0.230)	(0.221)
Av. quilombo area	-9.8e-5	-9.6e-4	
	(4.0e-4)	(9.4e-4)	
Av. terrain ruggedness			-0.048
			(0.103)
Travel time to nearest cap.			-0.0011
			(0.0012)
Ruggedness × Travel time			5.1e-4
			(0.0020)
Observations	1,263†	1,263†	1,284
Controls and FE	All	All	All

Note: *p<0.1; **p<0.05; ***p<0.01; † two outlier districts dropped, i.e. 26 obs. out of 1586, and 21 out of the 1284 for which a vote is registered. District-vote two-way clustered standard errors in parentheses. This table presents a placebo test of Hypothesis 3. Column 1 is the OLS, Column 2 is the second stage of the 2SLS (first stage is the same as in Table A.18 col. 1), and column 3 is the reduced form. For the instrument to be valid, we should not observe any placebo effect of the instrument's components in column 3. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

to escape from plantations, which locally raised to costs of enforcing coercion. We therefore view this as mostly orthogonal to the possibility of conflict.

Nonetheless, to investigate this possibility, we make use of data digitized by Lambais (2020) on destroyed quilombos in Minas Gerais and Central Brazil (originally from Guimarães, 1996, and Silva, 2003). In columns 1 to 4 we investigate the influence of the district-level number of destroyed quilombos on voting decisions, in *i)* the full sample (columns 1 and 2, the latter including an interaction with the prevalence of slavery), *ii)* a subsample comprised only of provinces for which the number of destroyed quilombos is nonzero (column 3), and *iii)* the Sudeste. Columns 5 to 8 proceed similarly but including our preferred measure of proximity to freedom, in an attempt to test its sensitivity to taking into account conflict with maroons in the same regression. The number of destroyed quilombos is never significant across these regressions, whether introduced alone, interacted with the prevalence of slavery, or alongside proximity to freedom. Similarly, the effect of proximity to freedom remains unchanged compared to the equivalent regressions in Table 3. Proximity to freedom is unsurprisingly not significant in the subsample comprised only of provinces for which Lambais (2020) has data, as this includes mostly districts in Central Brazil for which the prevalence of slavery was low. Overall, we interpret these regressions as showing that conflict with quilombolas constitutes an altogether different mechanism than proximity to freedom, and this mechanism does not seem to significantly influence voting decisions.

In additional regressions (not presented), we similarly attempt to falsify our results by including (endogenous) controls capturing abolitionist movements, the existence of pro- or anti-abolitionist interest groups, as well as activist newspapers, which are also likely to be correlated with local cultural traits that might drive pro-abolition voting behavior. Our results remain un-altered by the inclusion of any of these covariates.

Table A.20: Destroyed quilombos

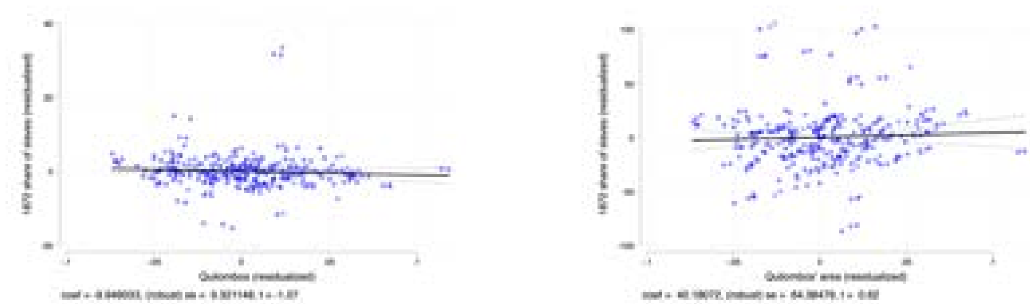
	$\mathbb{1}$(Abolition vote)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1872 Share of enslaved	-0.898***	-0.911***	-0.318	-0.750***	-0.912***	-1.001***	-0.514*	-1.185***
	(0.219)	(0.206)	(0.505)	(0.172)	(0.230)	(0.222)	(0.302)	(0.419)
Destroyed quilombos	-0.0052	-0.036	-0.027	-0.0060	-0.0051	-0.027	-0.020	0.0015
	(0.011)	(0.076)	(0.025)	(0.015)	(0.011)	(0.077)	(0.025)	(0.019)
Sh. enslaved × Destroyed quilombos		0.172				0.105		
		(0.387)				(0.405)		
Av. quilombo area					-0.00012	-0.0012	0.0027	0.0037***
					(0.00040)	(0.00076)	(0.0023)	(0.0013)
Sh. enslaved × Av. quilombo area						0.0087*		
						(0.0048)		
Observations	1,284	1,284	335	453	1,263†	1,263†	335	453
R-squared	0.421	0.421	0.397	0.399	0.418	0.420	0.399	0.408
Controls and FE	All	All	All	All	All	All	All	All

Note: *p<0.1; **p<0.05; ***p<0.01; † two outlier districts dropped, i.e. 26 obs. out of 1586, and 21 out of the 1284 for which a vote is registered. District-vote two-way clustered standard errors in parentheses. This table presents a test of the sensitivity of proximity to freedom to including a measure of conflicts with quilombolas (the district-level number of destroyed quilombos). Columns 1-4 include it in the full sample (alone and interacted in cols. 1-2), in the subsample where this measure is nonzero (col 3), and in the Sudeste (col 4), in each case without our preferred measure of proximity to freedom. Columns 5-8 proceed similarly but including our measure of proximity to freedom. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

Finally, a remaining possible concern is that quilombos and slavery might be colliders, as the number of quilombos and their size can conceivably constitute an outcome of the prevalence of slavery. For this to be an issue, there would however need to be a reverse relationship between abolition voting and quilombos, which seems improbable in this setting. Moreover, quilombos are widespread across districts irrespective of the prevalence of slavery in 1872, and once we control for our usual vector of covariates and FE, quilombos and slavery are at most marginally correlated, as illustrated in Figure A.4.

A.2.4 Selection

In this subsection, we briefly address a possible concern that our results may be driven by a selection of legislators through their presence during the vote, or by a



(a) Quilombos (b) Quilombos' area

Figure A.4: Slavery and quilombos

selection of emancipation bills. We may be worried that the decision to call the roll on an emancipation-related bill was in some way dependent on the anticipated distribution of the votes. Even if bills relating to emancipation were particularly sensitive, strongly publicized and surrounded by heated debate, so that the roll was called for every emancipation bill that passed through the Câmara dos Deputados during our sample's time frame, the bills discussed in parliament were sometimes divided in several sub-parts, which may individually be the object of nominative voting. To account for this possibility, in columns 1 to 3 of Table A.21, we report our main specifications using only the three most important emancipation bills. In doing so, we reduce the size of our sample from 1,284 to 290 district × bill observations. This does not change substantially our results and suggests that our conclusions are not driven by a specific selection of bills.

We may also be worried that legislators from specific districts would display consistent patterns of absence and abstention on (scheduled) bills related to emancipation, thus biasing our results. For example, it is possible that some legislators supporting a given side were systematically absent when a bill was scheduled to be voted and they thought their side had no chance to win (or on the contrary, if they thought the matter was already won). At all times, a considerable number of legislators was absent from the Chamber (close to a fifth of the assembly). Absences were supposed to be justified, and they sometimes were. Most of the time, however, absent legislators did not provide justification. In practice, when we regress a dummy for absence on our full vector of controls and explanatory variables (not presented), the only control that we find correlates with absenteeism is longitude (i.e. westernmost legislators within a province might be more inclined to absenteeism), but overall this does not

Table A.21: Selection

	𝟙(Abolition vote)					
	Selection on bills			*Selection on legislators*		
	(1)	(2)	(3)	(4)	(5)	(6)
1872 share of enslaved	−0.901 (0.492)* {0.519}*	−0.945 (0.527)* {0.537}*	−1.116 (0.592)** {0.554}**	−0.770 (0.295)*** {0.364}**	−0.719 (0.309)** {0.359}**	−0.849 (0.301)*** {0.363}**
1872 share of free foreigners		−2.941 (2.040) {1.243}**			−1.300 (1.395) {0.879}	
1890 share of foreigners		2.192 (0.687)*** {01.060}**			2.027 (1.137)* {0.795}**	
Av area quilombola			−9.0e-4 (8.9e-4) {9.4e-4}			−0.0011 (6.2e-4)* {7.7e-4}
Sh. enslaved × Av. area quil.			0.013 (0.0061)*** {0.0054}**			0.0079 (0.0047)* {0.0046}*
Controls and FE	All	All	All	All	All	All
Observations	290	290	286†	1586	1586	1560†
R^2	0.402	0.407	0.406	0.284	0.287	0.287

Note: *p<0.1; **p<0.05; ***p<0.01. † two outlier districts dropped. District-vote two-way clustered standard errors in parentheses, except for column 3, which shows district-level clustered standard errors. Conley standard errors (with a 250km window) in curly brackets. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

suggest any obvious pattern in the selection of legislators. In columns 4 to 6 of Table A.21, we follow the traditional approach in development economics, and reproduce our main specifications with the dependent variable coded as a dummy equal to zero when a legislator did not vote in favor of abolition, whether they were present or not at the time of the vote. This yields qualitatively similar results.

A.2.5 Combining instrumental variable strategies

We consider pairwise instrumentation exercises in Tables A.22 and A.23, and all instruments together in Tables A.25 and A.26 (second and first stages respectively).

In Table A.22, we instrument the 1872 prevalence of slavery and 1890 immigration with *i)* our standard instrument for each of these variables, and *ii)* both the latter and heteroskedasticity-based instruments. Using only standard instruments raises weak identification issues, but combining them with heteroskedasticity-based instruments dissipates these concerns and yields results similar to their counterparts in Table 4.

In Table A.23, we instrument the 1872 prevalence of slavery and proximity to freedom with *i)* our standard instrument for each of these variables, and *ii)* also heteroskedasticity-based instruments. We might be concerned about weak identification concerns with only standard instruments (second stage estimates are consistent with their Table 5 analogues, but imprecisely estimated), but complementing standard instruments with heteroskedasticity-based ones alleviates these concerns and yields numerically indistinguishable results.

In Tables A.24, A.25 and A.26, we focus on jointly evaluating immigration-driven substitution and proximity to freedom effects. In particular, Table A.24 provides OLS specifications simultaneously including immigration and proximity to freedom as explanatory variables in addition to the prevalence of slavery (without and with interaction terms respectively). These results mirror closely the corresponding estimates in Tables 2 and 3.

Columns 1 to 3 of Table A.25 provide the second stages of alternative 2SLS specifications. In column 1, the 1872 prevalence of slavery, 1890 immigration, proximity to freedom, and the interaction between proximity to freedom and the prevalence of slavery are instrumented using their respective standard instrument.[36] This is a particularly demanding specification (four endogenous variables, one of which is instrumented with the interaction between two other instruments) and it results in weak first stages and imprecisely estimated coefficients (first stage regressions are presented in columns 1 to 4 of Table A.26). However, supplementing standard instruments with heteroskedasticity-based ones in column 2 of Table A.25 (corresponding first stages are provided in columns 5 to 8 of Table A.26) largely solves the weak identification issue and produces results comparable to the ones obtained from the simpler specifications presented in Tables 4 and 5. Finally, column 3 of Table A.25 is similar to column 2 but now controlling but each un-interacted component of the standard instruments as a robustness check (corresponding first stages are provided in columns 9 to 12 of Table A.26) . Again, this yields results consistent and similar to those obtained from simpler specifications.

[36]The interaction between proximity to freedom and the prevalence of slavery being instrumented by the interaction between these variables' respective instruments.

Table A.22: Multi-instrumentation – Prevalence of slavery and 1890 immigration

	First stages				*Second stages*	
	Sh. ensl.	1890 imm.	Sh. ensl.	1890 imm.	$\mathbb{1}$(Abolition vote)	
	(1)	(2)	(3)	(4)	(5)	(6)
Pred. 1872 share of enslaved					−2.758	−1.191
					(0.905)***	(0.544)**
					{1.270}**	{0.546}**
Pred. 1890 sh. of foreigners					−0.117	3.776
					(2.313)	(0.994)***
					{3.162}	{1.173}***
Ln 16th rep. area	1.4e-5	2.0e-6	1.7e-5	2.9e-6		
× Dist. Gold Paths	(3.4e-6)***	(1.2e-6)*	(4.1e-6)***	(9.0e-7)***		
	{2.8e-6}***	{1.3e-6}	{3.4e-6}***	{6.5e-7}***		
Z_{1890}	0.195	−0.104	0.133	−0.085		
	(0.052)***	(0.019)***	(0.036)***	(0.014)***		
	{0.041}***	{0.020}***	{0.028}***	{0.011}***		
Het. instr. 1 (Sugar)			−0.020	0.011		
			(0.012)*	(0.0033)***		
			{0.014}	{0.0029}***		
Het. instr. 1 (Rain)			−0.00046	−0.000060		
			(0.00041)	(0.000073)		
			{0.00045}	{0.000072}		
Het. instr. 1 (Dist. coast)			−0.326	−0.0050		
			(0.109)***	(0.019)		
			{0.164}**	{0.0145}		
Het. instr. 1 (HMI)			−16.13	−2.898		
			(3.804)***	(0.757)***		
			{5.306}***	{0.674}***		
Het. instr. 2 (Sugar)			0.061	0.0076		
			(0.021)***	(0.011)		
			{0.013}***	{0.013}		
Het. instr. 2 (Rain)			0.00044	0.00097		
			(0.00096)	(0.00034)***		
			{0.00081}	{0.00036}**		
Het. instr. 2 (Dist. coast)			0.0027	−0.361		
			(0.237)	(0.059)***		
			{0.248}	{0.055}***		
Het. instr. 2 (HMI)			0.888	−19.46		
			(6.481)	(2.249)***		
			{5.471}	{1.085}***		
1872 share of free foreigners	−0.335	1.002	−0.240	0.970	0.062	−3.286
	(0.147)**	(0.048)***	(0.116)**	(0.031)***	(2.372)	(1.234)***
	{0.105}***	{0.043}***	{0.103}**	{0.029}***	{2.988}	{1.065}***
Controls & FE	All	All	All	All	All	All
Observations	1,284	1,284	1,284	1,284	1,284	1,284
Mean dep. var.	0.148	0.019	0.148	0.019	0.560	0.560
F-stat					5.769	32.65
B-P p-value			0.000	0.000		
P-H p-value						0.643
Hansen J p-value						0.214
Endog. test p-value						0.654

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered SE are reported in parentheses, and Conley SE (with a 250km window) are reported in curly brackets. Columns 1, 2, and 5 use our standard instruments for the prevalence of slavery and immigration in 1890, and columns 3, 4, and 6 use both the standard and heteroskedasticity-based instruments. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

Table A.23: Multi-instrumentation – Prevalence of slavery and proximity to freedom

	First stages						*Second stages*	
	Sh. ensl.	Av. area	Ensl. × area	Sh. ensl.	Av. area	Ensl. × area	$\mathbb{1}$(Abolition vote)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pred. 1872 share of enslaved							-1.970	-1.168
							(0.846)**	(0.393)***
							{1.155}*	{0.430}***
Pred. av. area quilombola							-0.0020	-0.0014
							(0.0022)	(0.00079)*
							{0.0028}	{0.00086}
Pred. av. area quil. × Sh. ensl.							0.012	0.0096
							(0.017)	(0.0043)**
							{0.020}	{0.0044}**
Ln 16th rep. area × Dist. Gold Paths	0.000021	-0.0062	-0.00026	0.000025	-0.0066	-0.00015		
	(7.0e-6)***	(0.0074)	(0.0012)	(7.8e-6)***	(0.0029)**	(0.00045)		
	{5.6e-6}***	{0.0075}	{0.00081}	{4.0e-6}***	{0.0018}***	{0.00029}		
Av. TRI	-0.033	22.43	0.050	0.023	-2.043	-2.968		
	(0.028)	(13.36)*	(2.824)	(0.019)	(8.563)	(1.872)		
	{0.019}**	{15.05}	{2.596}	{0.0179}	{7.293}	{1.711}*		
Travel time	-0.00034	1.004	0.111	0.00020	0.418	0.010		
	(0.00025)	(0.266)***	(0.038)***	(0.00021)	(0.098)***	(0.022)		
	{0.00018}**	{0.228}***	{0.032}***	{0.00018}	{0.091}***	{0.020}		
Av. TRI × travel time	0.00084	-0.703	-0.042	-0.00049	-0.201	0.060		
	(0.00055)	(0.356)**	(0.056)	(0.00035)	(0.176)	(0.044)		
	{0.00041}**	{0.409}	{0.052}	{0.00035}	{0.132}	{0.043}		
Av. TRI × Rep. area × Dist. GP	-0.000019	0.0051	-0.0020	-0.00004	0.017	-0.00088		
	(0.000026)	(0.024)	(0.0044)	(0.000026)	(0.011)	(0.0016)		
	{0.000020}	{0.025}	{0.0029}	{0.000021}*	{0.0072}**	{0.0011}		
Travel time × Rep. area × Dist. GP	5.3e-9	0.000099	0.000013	-1.5e-7	0.000060	0.000011		
	(3.0e-8)	(0.000029)***	(5.9e-6)**	(4.2e-8)***	(0.000017)***	(3.8e-6)***		
	{2.3e-8}	{0.000031}***	{7.2e-6}*	{3.5e-8}***	{0.000012}***	{3.4e-6}***		
TRI × trav. time × Rep. area × Dist. GP	-7.4e-8	-0.00028	-0.000042	5.0e-7	-0.00023	-0.000027		
	(2.1e-7)	(0.00020)	(0.000027)	(1.6e-7)***	(0.000085)***	(0.000012)**		
	{1.4e-7}	{0.00023}	{0.000030}	{1.9e-7}**	{0.000065}***	{9.9e-6}**		
Het. instr. 1 (Sugar)				-0.0064	-5.474	-0.305		
				(0.013)	(3.532)	(0.590)		
				{0.015}	{2.016}***	{0.314}*		
Het. instr. 1 (Rain)				-0.0011	0.159	-0.017		
				(0.00044)**	(0.145)	(0.024)		
				{0.00045}**	{0.083}**	{0.015}		
Het. instr. 1 (Dist. coast)				-0.538	-100.9	-11.34		
				(0.094)***	(33.30)***	(5.561)**		
				{0.144}***	{29.53}***	{5.661}*		
Het. instr. 1 (HMI)				-22.02	2108	271.4		
				(3.713)***	(950.6)**	(142.8)*		
				{5.076}***	{634.7}***	{90.85}***		
Het. instr. 2 (Sugar)				0.000088	0.023	0.0056		
				(0.000033)***	(0.013)*	(0.0025)**		
				{0.000018}***	{0.011}**	{0.0019}***		
Het. instr. 2 (Rain)				1.5e-6	-0.00067	-0.00018		
				(6.7e-7)**	(0.00028)**	(0.000050)***		
				{5.4e-7}**	{0.00022}***	{0.000042}***		
Het. instr. 2 (Dist. coast)				0.00080	0.721	0.050		
				(0.00015)***	(0.071)***	(0.018)***		
				{0.00012}***	{0.063}***	{0.015}***		
Het. instr. 2 (HMI)				0.0036	6.987	-0.512		
				(0.0098)	(3.647)*	(0.526)		
				{0.0050}	{3.185}**	{0.338}		
Het. instr. 3 (Sugar)				-0.00049	-0.165	-0.060		
				(0.00026)*	(0.109)	(0.027)**		
				{0.00016}***	{0.083}**	{0.023}***		
Het. instr. 3 (Rain)				2.8e-6	0.0023	0.0014		
				(5.3e-6)	(0.0023)	(0.00051)***		
				{3.7e-6}	{0.0020}	{0.00048}***		
Het. instr. 3 (Dist. coast)				-0.0032	-0.936	-0.042		
				(0.0017)*	(0.770)	(0.226)		
				{0.0013}**	{0.739}	{0.221}		
Het. instr. 3 (HMI)				-0.089	20.19	6.174		
				(0.077)	(29.39)	(6.351)		
				{0.046}**	{24.67}	{6.663}		
Controls	All	All	All	All	All	All	All	All
Province & Vote FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,263	1,263	1,263	1,263	1,263	1,263	1,263	1,263
Mean dep. var.	0.148	23.70	2.901	0.148	23.70	2.901	0.556	0.556
F-stat							2.213	61.92
B-P p-value				0.000	0.000	0.000		
P-H p-value								0.620
Hansen J p-value							0.354	0.332
Endog. test p-value							0.521	0.971

Note: *p<0.1; **p<0.05; ***p<0.01. District-level two-way clustered SE are reported in parentheses, and Conley SE (with a 250km window) are reported in curly brackets. Columns 1, 2, 3, and 7 only use our standard instruments for the prevalence of slavery, proximity to freedom, and their interaction, and columns 4, 5, 6, and 8 use both the standard and heteroskedasticity-based instruments. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

Table A.24: Labor demand and outside option effects together

	$\mathbb{1}$(Abolition vote)					
	(1)	(2)	(3)	(4)	(5)	(6)
Share of enslaved	−0.841	−0.844	−1.330	−1.150	−1.488	−1.013
	(0.307)***	(0.313)***	(0.572)**	(0.534)**	(0.362)***	(0.438)**
	{0.350}**	{0.353}**	{0.556}**	{0.479}**	{0.451}***	{0.511}**
1872 share of free foreigners	−2.569	−2.901	−3.302	−3.674	1.631	1.839
	(1.265)**	(1.249)**	(2.140)	(2.109)*	(1.673)	(1.781)
	{0.703}***	{0.726}***	{1.915}*	{1.956}*	{1.784}	{1.294}
1890 share of foreigners	3.329	3.460	1.485	1.499	3.938	3.591
	(1.063)***	(1.068)***	(2.961)	(2.926)	(0.934)***	(1.144)***
	{0.725}***	{0.770}***	{3.401}	{3.427}	{1.006}***	{1.441}**
Av area quilombola	-0.00014		-0.0011		0.0032	
	(0.00040)		(0.00068)		(0.0011)***	
	{0.00043}		{0.00082}		{0.0016}**	
Ln av. dist. 1 quil.		0.00041		0.010		−0.0058
		(0.0037)		(0.0068)		(0.0080)
		{0.0031}		{0.0076}		{0.0063}
Sh. enslaved × Av. area quil.			0.0080			
			(0.0045)*			
			{0.0048}*			
Sh. enslaved × Ln av. dist. 1 quil.				-0.075		
				(0.042)*		
				{0.049}		
1872 sh. foreigners × sh. enslaved			2.793	3.922		
			(10.71)	(10.79)		
			{9.423}	{9.703}		
1890 sh. foreigners × sh. enslaved			10.04	9.544		
			(15.91)	(15.87)		
			{19.39}	{18.95}		
Controls & FEs	All	All	All	All	All	All
Observations	1263	1263	1,284	1,284	453	453
R-squared	0.425	0.427	0.428	0.430	0.432	0.426
Mean dep. var.	0.56	0.56	0.56	0.56	0.43	0.43

Note: *p<0.1; **p<0.05; ***p<0.01. District-level clustered SE are reported in parentheses, and Conley SE (with a 250km window) are reported in curly brackets. Columns 1, 3 and 5 use the average area of land belonging to quilombolas as a measure of proximity to freedom, alone, interacted with the prevalence of slavery, and focusing on the Sudeste region. Columns 2, 4, and 6 use the average distance to the closest municipality with at least one quilombo. All columns also include immigration in 1872 and 1890, alone (cols 1-2), interacted with the prevalence of slavery (cols 3-4), and focusing on the Sudeste region (cols 5-6). Controls include: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education dummies, share of free colored, literacy, province and vote FEs.

Table A.25: Labor demand and outside option effects together – 2SLS second stages

	$\mathbb{1}$(Abolition vote)		
	(1)	(2)	(3)
1872 share of enslaved	-1.564	-1.007	-0.778
	(0.640)**	(0.382)***	(0.428)*
	{0.862}*	{0.607}*	{0.511}
Av. area quilombola	-0.0017	-0.0011	-0.00074
	(0.0021)	(0.00081)	(0.00089)
	{0.0027}	{0.00098}	{0.00087}
Av. area quil. × Sh. ensl.	0.0098	0.0072	0.0075
	(0.015)	(0.0040)*	(0.0042)*
	{0.019}	{0.0049}	{0.0048}
1872 sh. of free foreigners	-0.495	-2.509	-2.743
	(1.847)	(1.296)*	(1.071)***
	{2.369}	{1.335}*	{0.960}***
1890 sh. of foreigners	0.857	3.168	3.560
	(1.942)	(1.246)**	(0.949)***
	{2.369}	{1.534}**	{1.294}***
Dist. Gold Paths			0.000052
			(0.00017)
			{0.00020}
Ln 16th rep. area			0.0060
			(0.0084)
			{0.0089}
Av. TRI			0.060
			(0.056)
			{0.078}
Travel time			-0.0011
			(0.00080)
			{0.0010}
Controls and FE	All	All	All
Het.-based ID	No	Yes	Yes
Observations	1263†	1263†	1263†
Mean dep. var.	0.556	0.556	0.556
F-stat	3.817	79.52	64.49
P-H p-value		0.621	0.796
Hansen J p-value	0.281	0.176	0.168
Endog. test p-value	0.525	0.983	0.656

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered SE are reported in parentheses, and Conley SE (with a 250km window) are reported in curly brackets. Column 1 uses the standard instruments for the share of enslaved, proximity to freedom, and share of immigrants in 1890. Column 2 adds heteroskedasticity-based instruments. Column 3 controls for the un-interacted components of the standard instruments as a robustness check. First stages are reported in Table A.26. Controls and FE: coffee, sugar, and cotton suitability, rainfall, longitude and latitude, population density, distance to coast and to closest river, average HMI, party affiliation, reelection status, other government appointment, occupation/education (law, medicine, science, civil service, priesthood, and military), share of free colored, literacy, and province and votes FE.

Table A.26: Labor demand and outside option effects together – 2SLS first stages

	Sh. ensl. (1)	Av. area (2)	Ensl. × area (3)	Sh. for. (4)	Sh. ensl. (5)	Av. area (6)	Ensl. × area (7)	Sh. for. (8)	Sh. ensl. (9)	Av. area (10)	Ensl. × area (11)	Sh. for. (12)
Ln 16th rep. area × Dist. Gold Paths	0.000017**	-0.0065	-0.00022	2.7e-6	0.000024***	-0.0081***	-0.00027	2.9e-6**	0.000017*	-0.0083**	-0.000056	3.7e-6**
	(6.9e-6)	(0.0075)	(0.0012)	(2.4e-6)	(7.9e-6)	(0.0031)	(0.00047)	(2.0e-6)	(0.000010)	(0.0037)	(0.00061)	(0.00001)
Av. TRI	-0.049**	21.47	0.140	-0.0051	-0.025	-8.043	-3.496*	-0.011	-0.0139	-8.260	-3.337	-0.013**
	(0.024)	(15.34)	(3.286)	(0.0080)	(0.023)	(9.827)	(2.119)	(0.0079)	(0.023)	(10.34)	(2.265)	(0.0050)
Travel time	-0.00040*	1.006***	0.109***	0.000099	-0.00030	0.367***	0.013**	1.4e-6	-0.00017	0.421***	0.014	-0.000076
	(0.00024)	(0.280)	(0.041)	(0.000078)	(0.00024)	(0.093)	(0.020)	(0.000093)	(0.00024)	(0.106)	(0.024)	(0.000060)
Av. TRI × travel time	0.0011**	-0.696*	-0.041	-0.00026*	0.00062	-0.045	0.067	-0.00011	0.00030	-0.072	0.068	0.000062
	(0.00053)	(0.398)	(0.064)	(0.00016)	(0.00044)	(0.203)	(0.047)	(0.00015)	(0.00042)	(0.227)	(0.051)	(0.00011)
Av. TRI × Rep. area × Dist. GP	0.000013	0.0082	-0.0025	-1.9e-6	-9.4e-6	0.015**	-0.00024	-6.3e-6	-0.000035	0.026*	0.0018	-0.000033***
	(0.000024)	(0.025)	(0.0047)	(8.1e-6)	(0.000028)	(0.013)	(0.0020)	(7.8e-6)	(0.000033)	(0.013)	(0.0018)	(7.9e-6)
Travel time × Rep. area × Dist. GP	4.0e-8	0.00010***	0.000012**	-2.4e-8***	-7.6e-8*	0.000085***	0.000012***	-2.5e-8**	-6.4e-8	0.000072***	0.000012***	-2.0e-8**
	(3.0e-8)	(0.000030)	(6.0e-6)	(8.6e-9)	(4.5e-8)	(0.000017)	(3.6e-6)	(1.1e-8)	(5.2e-8)	(0.000018)	(4.1e-6)	(9.1e-9)
TRI × trav. time × Rep. area × Dist. GP	-3.6e-7**	-0.00031	-0.000037	9.8e-8*	-3.4e-8	-0.00021*	-0.000030*	1.2e-7*	2.0e-7	-0.00031***	-0.000047***	2.9e-7***
	(1.6e-7)	(0.00021)	(0.000028)	(5.9e-8)	(2.1e-7)	(0.00011)	(0.000016)	(6.5e-8)	(2.2e-7)	(0.00011)	(0.000016)	(6.0e-8)
Z_{1890}	0.244***	24.57	-4.052	-0.112***	0.243***	26.61	2.375	-0.079***	0.168***	38.23*	5.362	-0.119***
	(0.055)	(29.45)	(4.824)	(0.023)	(0.055)	(18.27)	(3.164)	(0.024)	(0.048)	(22.99)	(4.085)	(0.013)
1872 sh. of free foreigners	-0.258	-041.03	8.356	0.975***	-0.300**	-61.36	-0.889	0.941***	-0.285**	-40.47	-3.603	0.970***
	(0.169)	(76.28)	(11.57)	(0.055)	(0.126)	(49.51)	(8.988)	(0.043)	(0.114)	(52.96)	(8.41)	(0.026)
Het. instr. 1 (Sugar)					-0.026*	1.978	0.277	0.012***	-0.017*	-2.709	0.199	0.010***
					(0.014)	(3.433)	(0.629)	(0.0038)	(0.011)	(3.932)	(0.781)	(0.0025)
Het. instr. 1 (Rain)					-0.00021	-0.121	-0.057**	-0.000019	-0.00077	-0.041	-0.040	-0.000040
					(0.00050)	(0.146)	(0.028)	(0.00011)	(0.00050)	(0.146)	(0.029)	(0.000083)
Het. instr. 1 (Dist. coast)					-0.313**	-96.07**	-14.97**	0.023	-0.417***	-84.30**	-11.89*	0.0054*
					(0.134)	(37.95)	(6.699)	(0.033)	(0.104)	(35.77)	(6.582)	(0.014)
Het. instr. 1 (HMI)									-17.08***	2436**	277.0	-3.731***
									(3.751)	(1128)	(192.3)	(0.754)
Het. instr. 2 (Sugar)					0.094***	-23.03***	-2.961**	0.022	0.063***	-14.76	-2.733	0.0064
					(0.024)	(8.048)	(1.474)	(0.014)	(0.023)	(10.56)	(2.196)	(0.0089)
Het. instr. 2 (Rain)					-0.0020	0.022	0.0086	0.0013**	-0.0015*	-0.0029	0.0017	0.00062**
					(0.0012)	(0.434)	(0.084)	(0.00055)	(0.00089)	(0.432)	(0.087)	(0.00029)
Het. instr. 2 (Dist. coast)					-0.317	185.3*	9.785	-0.254**	-0.166	159.0***	-2.252	-0.260***
					(0.280)	(97.46)	(13.93)	(0.107)	(0.231)	(95.41)	(14.63)	(0.054)
Het. instr. 2 (HMI)									-3.666	-294.7	-125.7	-21.09***
									(6.355)	(2724)	(469.0)	(1.795)
Het. instr. 3 (Sugar)					0.000017*	0.030**	0.0072***	9.4e-6	0.000047	0.027**	0.0064**	7.3e-7
					(0.000026)	(0.014)	(0.0023)	(0.000011)	(0.000028)	(0.014)	(0.0029)	(8.6e-6)
Het. instr. 3 (Rain)					1.2e-6**	-0.00096***	-0.00023***	1.4e-7	6.7e-7	-0.00097***	-0.00020***	1.3e-7
					(5.6e-7)	(0.00026)	(0.000040)	(1.9e-7)	(6.1e-7)	(0.00029)	(0.00005)	(1.6e-7)
Het. instr. 3 (Dist. coast)					0.00034**	0.817***	0.060***	0.000065***	0.00064***	0.740***	0.051**	0.00017***
					(0.00016)	(0.075)	(0.020)	(0.000040)	(0.00017)	(0.082)	(0.022)	(0.000047)
Het. instr. 3 (HMI)									0.0030	10.72***	-0.280	-0.0079***
									(0.0080)	(3.449)	(0.508)	(0.0026)
Het. instr. 4 (Sugar)					-0.00015**	-0.234**	-0.066***	-0.000075	-0.00030	-0.156	-0.063**	-0.000041
					(0.00020)	(0.092)	(0.021)	(0.000088)	(0.00022)	(0.109)	(0.028)	(0.000067)
Het. instr. 4 (Rain)					-1.3e-6	0.0062***	0.0019***	-8.2e-7	5.1e-6	0.0042*	0.0015***	4.2e-7
					(2.9e-6)	(0.0010)	(0.00018)	(7.0e-7)	(5.4e-6)	(0.0024)	(0.00047)	(1.2e-6)
Het. instr. 4 (Dist. coast)					-0.0016*	-1.868**	-0.085	-0.00055***	-0.0037*	-1.001	-0.043	-0.002***
					(0.0019)	(0.816)	(0.260)	(0.0010)	(0.0021)	(0.989)	(0.290)	(0.00043)
Het. instr. 4 (HMI)									-0.076	-0.021	5.259	0.015
									(0.069)	(29.74)	(6.202)	(0.018)
Dist. Gold Paths									0.000016	0.0020	0.0019	-0.000031***
									(0.000035)	(0.017)	(0.0022)	(0.000012)
Ln 16th rep. area									0.0029	0.145	-0.178	0.0015***
									(0.0024)	(0.903)	(0.193)	(0.00056)
Controls	All	All	All	All	All	All	All	All	All	All	All	All
Province & Vote FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,263	1,263	1,263	1,263	1,263†	1,263	1,263	1,263	1,263	1,263	1,263	1,263
Mean dep. var.	0.148	23.70	2.901	0.019	0.148	23.70	2.901	0.019	0.148	23.70	2.901	0.019
B-P p-value					0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note: *p<0.1; **p<0.05; ***p<0.01. District-vote two-way clustered SE are reported in parentheses. Second stages are reported in Table A.25.

A.3 Database construction and variables creation

This section provides additional details on how we proceed to build our legislative database and generate our 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. In order to make it into the final data set, 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.[37] The votes we retain in our 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.)

- 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.)

[37] In other words, the annals must have made clear that voting in favor/against the bill meant being in favor/against the emancipation of slaves.

- 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.)

Among these votes, we retain three into our core legislative data set, one by legislature: the motion of no confidence against Dantas on July 28, 1884 (this precipitates the end of the eighteenth legislature and the annals could hardly be clearer about it being related to emancipation), the vote on the *Lei dos Sexagenários*, and the vote on the *Lei áurea* (these two are the only two bills that become laws in their own right). Our goal in building this secondary data set (which we use only to assess selection issues) is to abstain – as much as possible – from any discretionary judgment, and we thus only keep the most important vote within each legislature.

We believe that together, 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 she represents. This is mostly done using records of *Juntas Verificadoras de Poderes* (special councils that occur during the early months of each legislature), double checked using Nogueira and Firmo (1973). We then link each district with the municipalities and parochias (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 parochias and municipalities into 1882-1889 district-legislator-vote observations.

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

Geo-coded data. We exploit several sources of geo-coded data, most notably 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. In particular, 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 'frontier openness/distance to freedom' proxies (e.g. number and size of quilombos and, in combination with Fundação Palmares (2020), average distance to the closest municipalities with at least k quilombos).

Linking our matched 1872-1881 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: *1)* 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). *2)* 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 by any means, but they do illustrate the geographical configuration of abolition votes.

Finally, we also geo-reference a number of existing maps, in particular from Milliet (1941) and CPDOC (2016). Most notably, this allows us to approximate the location of state-sponsored settlements in a succession of waves and to assess proximity to Caminhos do Ouro, mining sites, insurrection sites, Tupi-Guarani-speaking populations' areas of enslavement, slavery/abolitionist interest groups and abolitionist journals.

A.4 Additional maps

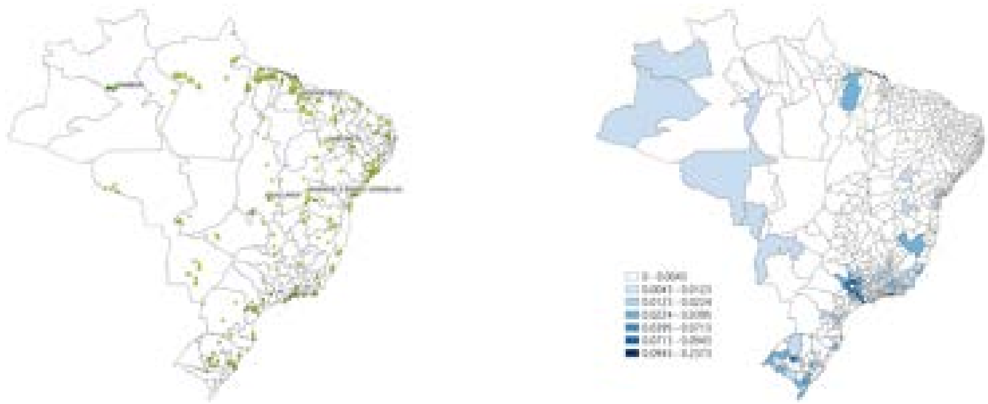


Figure A.5: Quilombos and share of foreigners in 1890 with district- and municipality-level boundaries respectively

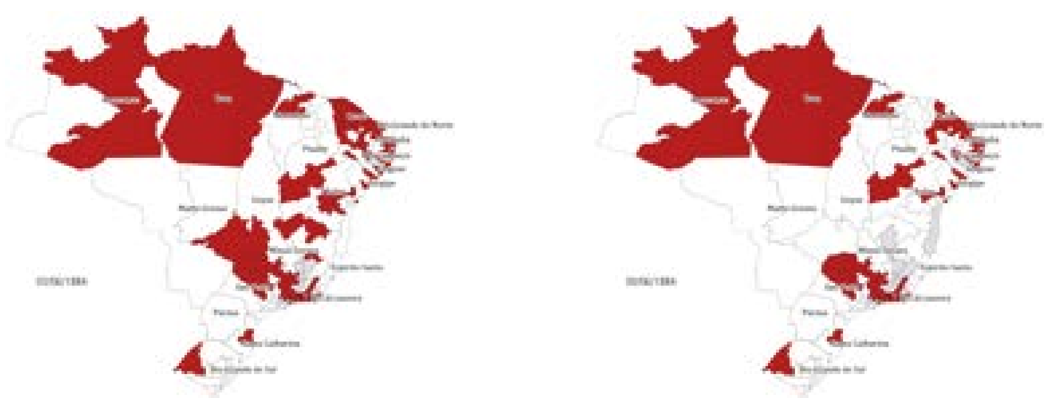


Figure A.6: Abolition maps – other bills (1)

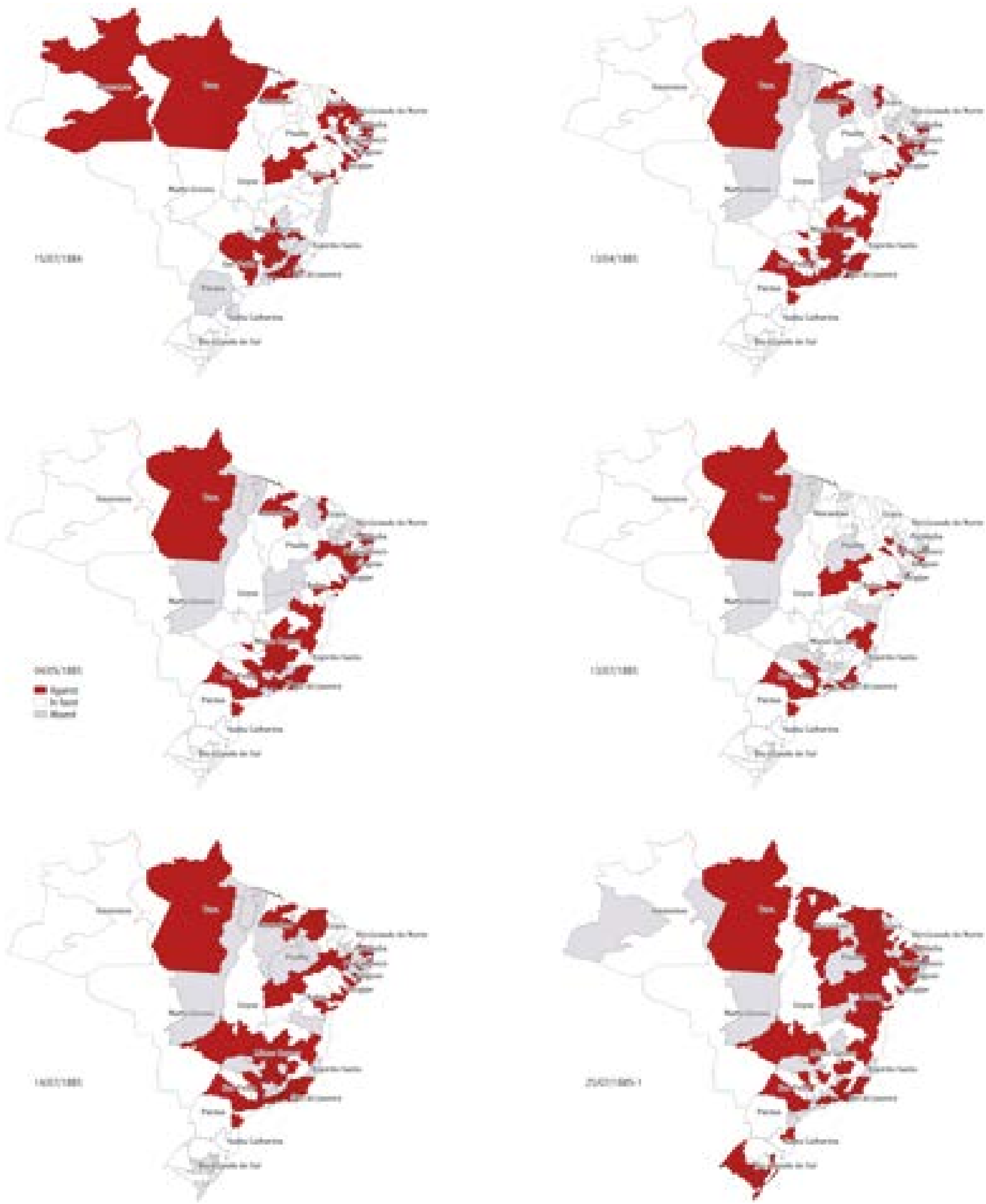


Figure A.7: Abolition maps – other bills (2)

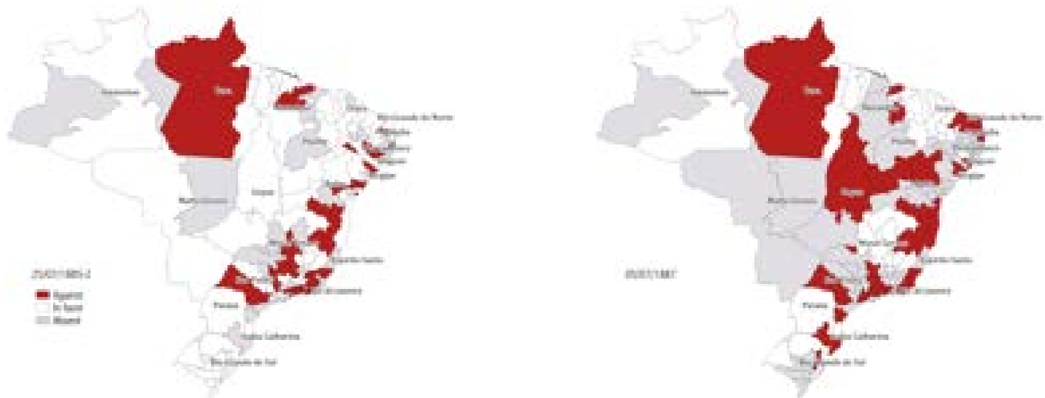


Figure A.8: Abolition maps – other bills (3)

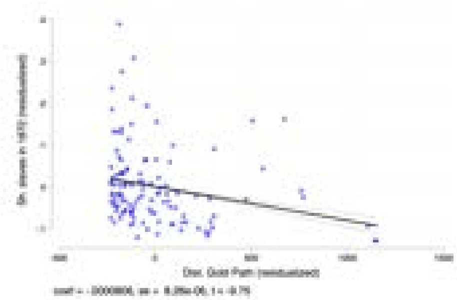
Figure A.9: Eighteenth-century mining activities and Indigenous enslavement (CPDOC, 2016).

Figure A.10: Some historical *Caminhos do Ouro* maps from Scarato (2009)

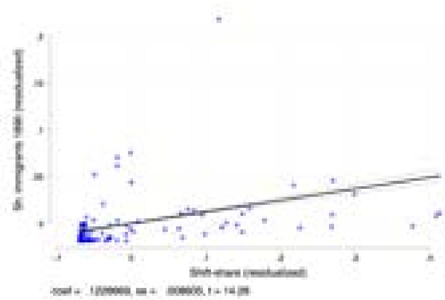
A.5 Additional figures and tables

Table A.27: 1872 nationality-religion matching

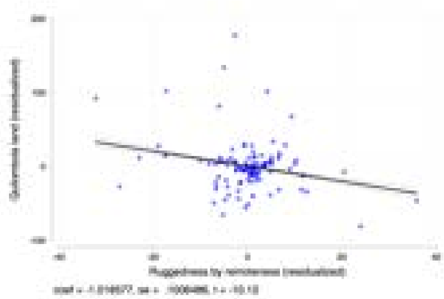
Nationality	Cath. VS N.-Cath.	Predicted religion	Nationality	Cath. VS N.-Cath.	Predicted religion
German	Cath. N.-Cath.	CR Prot.	Italian	Cath. N.-Cath.	CR Prot./NA
Austrian	Cath. N.-Cath.	CR Prot.	Japanese	Cath. N.-Cath.	CR Ath./NA
Argentinian	Cath. N.-Cath.	CR Prot./NA	Mexican	Cath. N.-Cath.	CR Prot./NA
Belgian	Cath. N.-Cath.	CR Prot./NA	North- American	Cath. N.-Cath.	CR Prot.
Bolivian	Cath. N.-Cath.	CR Prot./NA	Oriental	Cath. N.-Cath.	CR M
Chinese	Cath. N.-Cath.	CR Ath./NA	Paraguayan	Cath. N.-Cath.	CR Prot./NA
Danish	Cath. N.-Cath.	CR Prot.	Persian	Cath. N.-Cath.	CO M
French	Cath. N.-Cath.	CR Prot.	Peruvian	Cath. N.-Cath.	CR Prot./NA
Greek	Cath. N.-Cath.	CO Prot./NA	Portuguese	Cath. N.-Cath.	CR Prot./NA
Spanish	Cath. N.-Cath.	CR Prot./NA	Russian	Cath. N.-Cath.	CO M./NA
Dutch	Cath. N.-Cath.	CR Prot.	Swiss	Cath. N.-Cath.	CR Prot.
Hungarian	Cath. N.-Cath.	CR Prot.	Swedish	Cath. N.-Cath.	CR Prot.
English	Cath. N.-Cath.	CR Prot.	Turkish	Cath. N.-Cath.	CO/NA M.



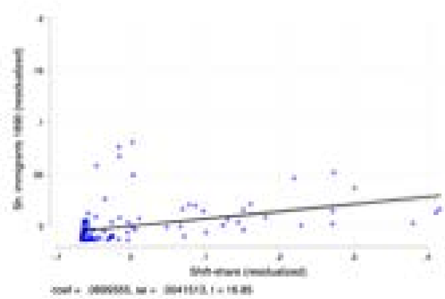
(a) Slavery



(b) Immigration



(c) Quilombos



(d) Immigration, w/o Rio de Janeiro

Figure A.11: First stage plots

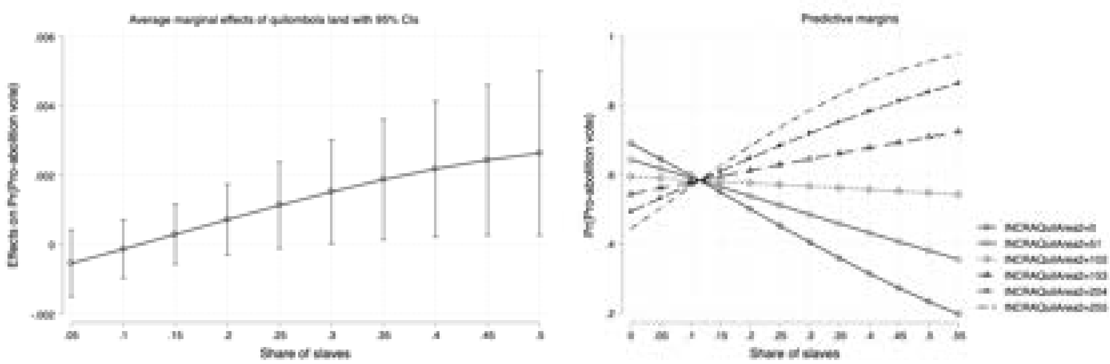


Figure A.12: Exit and votes - marginal effects and predictive margins

Notes: Average marginal effects of quilombola land (left) and predictive margins for different levels of quilombola land (right) by slavery-intensity. The right panel does not include confidence intervals for ease of readability. Note that each curve is in general not statistically different from the closest curves.

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

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Figure A.13: Example biographic record from Nogueira and Firmo (1973)

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