Journal of Comparative Economics 40 (2012) 534–564

Contents lists available at SciVerse ScienceDirect

Journal of Comparative Economics

journal homepage: www.elsevier.com/locate/jce

Finding Eldorado: Slavery and long-run development in Colombia

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ARTICLE INFO

Article history:
Received 12 June 2012
Available online 30 August 2012

JEL classification:
H41
N96
O10
O54

Keywords:
Slavery
Public goods

ABSTRACT

Acemoglu, Daron, García-Jimeno, Camilo, and Robinson, James A.—Finding Eldorado: Slavery and long-run development in Colombia

Slavery has been a major institution of labor coercion throughout history. Colonial societies used slavery intensively across the Americas, and slavery remained prevalent in most countries after independence from the European powers. We investigate the impact of slavery on long-run development in Colombia. Our identification strategy compares municipalities that had gold mines during the 17th and 18th centuries to neighboring municipalities without gold mines. Gold mining was a major source of demand for slave labor during colonial times, and all colonial gold mines are now depleted. We find that the historical presence of slavery is associated with increased poverty and reduced school enrollment, vaccination coverage and public good provision. We also find that slavery is associated with higher contemporary land inequality. *Journal of Comparative Economics* **40** (4) (2012) 534–564. Massachusetts Institute of Technology, Building E52, Room 380B, Cambridge, MA 02142, United States; University of Pennsylvania, 528 McNeil Building, 3718 Locust Walk, Philadelphia, PA 19104, United States; Government Department, Harvard University, Institute of Quantitative Social Science, Room N-309, 1737 Cambridge Street, Cambridge, MA 02138, United States.

1. Introduction

Throughout most of world history, labor repression of different forms has played a key role in shaping the economic structures of society. In the classical world, for instance, probably 35% of the population of Roman Italy were slaves (Bradley, 1994, p. 12), while 25% of the population of ancient Athens were slaves (Morris and Powell, 2006, p. 210). Closer to our time, slavery has been even more prevalent in some societies and has lasted until recently. In 1680 2/3 of the people on the Caribbean island of Barbados were slaves (Dunn, 1969). In 1860 slaves still made up about 13% of the entire population of the United States, and almost 50% of the population in the US South. In large parts of West Africa slaves made up 50% of the population in the 19th century (Lovejoy, 2000), and in Sierra Leone slavery was abolished by the British colonial state only in 1928.

Slavery was not of course the only form of labor repression. Though slavery vanished from Western Europe in the early Medieval period,[1] it was replaced by feudalism where the serfs who made up probably 90% of the population were also coerced

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[1] The Doomesday Book, the great census of England conducted by the Norman king William the Conquerer recorded that in 1086 about 10% of the population were slaves. By 1400 this was down to zero in England.

0147-5967/$ - see front matter
http://dx.doi.org/10.1016/j.jce.2012.07.003

and were subject to great restrictions on movement and occupational choice. Elsewhere similar systems arose, for example in Ethiopia and India which more or less resembled slavery. Russian serfdom, for example, allowed serfs to be sold just like slaves, which was not characteristic of serfdom in Western Europe. This labor repression probably also had a major impact on the economic development of these societies. Finlay (1965), for example, argued that it was precisely the fact that the economy of the classical world was based on slavery which made it so undynamic technologically. Slaves had little incentive to innovate or work creatively. Brenner (1976) made a similar argument about the lack of technological change in feudal Europe. The consequences of labor repression for economic development have perhaps been most intensively debated in the Americas. A long intellectual tradition argued that the relative economic backwardness of the US South compared to the rest of the country (in terms of income per-capita, urbanization, manufacturing industry, and infrastructure) was directly a consequence of the slave economy (Genovese, 1965, Wright, 1978, Bateman and Weiss, 1981, Ransom and Sutch, 2001). This could be so even if slave production was itself highly profitable (Fogel and Engerman, 1974) since slave plantations may have exerted all sorts of negative externalities on the wider economy.

Engerman and Sokoloff (1997) placed labor repression at the heart of their comparative theory of the long-run development of the Americas. In their argument, conditions conducive to crops which exhibited economies of scale and could be profitably produced with slaves, such as sugarcane and cotton, led to poor economic development in Latin America compared to North America. When a society had such factor endowments it developed a very unequal hierarchical society which impeded development.

Despite this plethora of hypotheses about the pervasive role of labor repression and slavery in retarding economic development, there have been few systematic empirical studies. Neither Finlay nor Brenner provided systematic evidence for their claims, while the work on the US South and that by Engerman and Sokoloff has been at the level of broad correlation. Most notably, Dell (2010) examined the long-run impact of the largest system of coerced labor used in colonial Latin America, the Andean mining mita in modern day Peru. Although the mita was abolished at independence, almost 200 years ago, using regression discontinuity techniques she showed convincingly that in villages which today lie within the former catchment area, average household consumption is 1/3 lower. She showed this was due to less participation of primarily agricultural households in the market. A major difference is that Dell focuses on a specific form of corvée labor, whereas we focus on slavery, which has been probably more widespread across societies, time, and economic environments. This paper complements Dell (2010) by studying the long-run implications of labor coercion, focusing both on the different type of coercion (slavery instead of corvée labor) and a different source of variation.

We do so in the context of Colombia where the national census of 1843 (when the country was called New Grenada) provides complete municipality level data on the incidence of slavery (the last before slavery was abolished in 1851). We investigate the long-run impact of slavery in terms of current development outcomes but also at two intermediate dates, 1918 and 1938. The biggest empirical challenge in conducting such a test is that the location of slaves in 1843 was endogenous and determined by the characteristics of municipalities which may be determinants of current development outcomes. For example, the location of slaves might have been determined by agricultural productivity, or it might have been more attractive to use slaves in places which were less healthy, or perhaps in places which were more remote. In addition, the presence of slaves may have been correlated with other important features, such as the presence and strength of state institutions, which are difficult to control for but which persist and impact current development outcomes. The crux of the paper is therefore our identication strategy. This is based firstly on the observation that in colonial Colombia one predominant use of slaves was in gold mining (Colmenares, 1973, 1979, Jaramillo, 1974). Nevertheless, by the mid 19th century gold production from deposits exploited during colonial times was negligible. This does not, however, make the mere presence of colonial gold mining an appealing instrument for slavery in 1843. This is because gold deposits (both vein and placer ones) were not randomly distributed across the country, but rather concentrated in the basin of the Cauca River, in the Upper Magdalena River valley, and on the Pacific Coast (West, 1952). Empirically this is problematic because these gold-mining regions are also very different to other regions of the country in several dimensions.

To solve this problem, we implement an empirical strategy in the spirit of a matching methodology (Angrist and Pischke, 2009), and compare directly neighboring municipalities (municipalities sharing a border), with and without colonial gold mines. These neighbors are likely to have faced similar colonial state presence, and are likely to be very similar across any other unobservables. Moreover, using neighbor-pair fixed effects, we can directly control for any unobservables that are common across the boundary. Thus, the effect of slavery on current outcomes is identified by the variation in slavery across neighboring municipalities with and without colonial gold mines.

Formally, our main specifications use the presence of a gold mine in the 17th and 18th centuries as an instrument for slavery in a sample consisting of gold-mining municipalities and their neighbors (and include a full set of fixed effects for each cluster of gold-mining municipality and its neighbors). Our focus on the extensive margin of variation in slavery is motivated by two considerations. First, the persistent effects of slavery are likely to be due largely to the institutional complex supporting slavery. Second, our data provide only a noisy measure of the number of slaves in a municipality. We verify that the OLS correlation between contemporary outcomes and slavery is driven mostly by the extensive margin. The clustered nature of our data raises some questions about inference. To overcome this problem, we also implement an alternative strategy which includes random effects that allow for a within-cluster and cross-cluster correlation structure for our sample of municipalities. Our IV random-effects estimates are close to the IV neighbor-pair fixed effects estimates, and allow us to be confident about the degree of precision of the effects we find.

Our basic finding is that across a comprehensive set of economic development indicators, slavery has a robust negative effect. Slavery in 1843 is associated with greater poverty, lower educational attainment, lower vaccination coverage, and lower public good provision in the form of aqueduct and electricity coverage around the 1990s and 2000s. When looking at development outcomes in the early 20th century, we also find that slavery is associated with reduced literacy, educational attainment and vaccination coverage. Moreover, slavery is also strongly associated with increased contemporary land inequality. We find that the magnitude of the effects is economically important, and in line with estimates from Dell (2010) who looks at an alternative coerced-labor institution. For example, relative to the sample means, municipalities with slaves in 1843 have 23% higher poverty rates, 16% lower secondary school enrollment rates, 33% less vaccination coverage, 15% less aqueduct coverage, and 5% larger land gini coefficients. Interestingly, historical slavery does not appear to have significant effects on contemporary state presence measured by the size of public bureaucracies, tax collection or public goods such as police posts, courts or health centers, or on contemporary sectoral specialization.

Though there has not been a convincingly identified study of the impact of slavery on economic development, several papers have examined part of the issue. McLean and Mitchener (2003) showed that at the level of US states the extent of pre-civil war slavery was negatively correlated with subsequent economic growth. Lagerlof (2005) showed at the level of southern US counties that higher slavery in 1860 is strongly associated with lower income per-capita in the 1990s. He tackled the issue of the endogeneity of slavery by instrumenting it with elevation above sea level, average annual temperature, and precipitation (rainfall), but the use of such geographical instruments is problematic. Other related work is by Canaday and Tamura (2009) and Alston and Ferrie (1993) who explore some mechanisms of persistence of slavery in the context of the US South during the "southern redemption" decades. Canaday and Tamura (2009) study discrimination in education provision, while Alston and Ferrie (1993) look at the emergence of paternalistic labor contracts between white landed elites and former slaves and their descendants which, in their argument, retarded the adoption of welfare programs in the South.

Bruhn and Gallego (2010) classified slavery during the colonial period of the Americas as a bad type of economic activity which they showed, using cross-national and within-country variation, was negatively correlated with contemporary GDP per-capita. Nunn (2008) also showed that within the Americas there is a negative correlation between historical slavery and contemporary development outcomes. Summerhill (2010), however, found using variation within the Brazilian state of Sao Paulo, no correlation between the extent of slavery in 1872 and contemporary income-per capita or human capital outcomes.

Others have used historical slavery as an instrument for various types of outcomes, though under exclusion restrictions that are likely to be violated. For instance Bashera and Lagerlof (2008) used slavery as an instrument for human capital in a study of within US and Canada income variation. Easterly and Levine (2003) and Easterly (2007) do this more indirectly using the presence of land suitable for growing slave crops like sugarcane and cotton as an instrument for institutions and inequality respectively.

This paper proceeds as follows. In Section 2 we provide a historical discussion of slavery and gold mining in New Grenada during the colonial period. Section 3 presents the data collected and used in this study, Section 4 then discusses the empirical strategy, Section 5 then discusses the main results and explores the robustness of our findings, and Section 6 concludes.

2. Slavery in Colombia

2.1. Conquest, settlement, and gold deposits

In this section we discuss the historical background of slavery in Colombia. Our purpose is to motivate our empirical strategy, which was to a large extent suggested by the historical experience of slavery and gold-mining during the colonial period. The interest in precious metals, best exemplified by the quest for Eldorado during the Spanish conquest of South America, was one of the driving forces in the occupation and settlement of the Spanish colonies. In fact, most of the largest precious metal deposits in the Americas were found shortly after the Spanish arrival. For example, the Potosí silver deposits were discovered in 1545, while the first silver mines in Michoacán, Taxco, and Zacatecas in Mexico, were found in 1525, 1534, and 1546, respectively (Bakewell, 1971, 1997, Dell, 2010, Wagner, 1942, West, 1952).

In the northern Andes silver deposits are scarce, but gold deposits are much more abundant. As a result, in New Grenada gold mines both from vein and placer deposits were also rapidly located by Spanish conquistadors. Exploration of New Grenada started in the mid 1530s, and reports document that by 1544 mining was well established in the upper Cauca River region. By 1547 Spaniards were already aware of the rich gold deposits of Anserma and Cartago, 200 miles north of Cali down the Cauca River (West, 1952). Finding precious metal deposits was one of the main motivations for the initial exploration of the territory, which makes their rapid discovery unsurprising.

The distribution of gold deposits in New Grenada was determined by the geo-morphological features of the northern Andes. These traverse the country from south to north, subdividing into three mountain chains, namely the Western Cordillera, the Central Cordillera, and the Eastern Cordillera. The most significant gold deposits are concentrated in three regions around the Andes: the drainage basin of the Cauca River, flowing between the Western and Central Cordilleras, the upper Magdalena River, flowing between the Central and Eastern Cordilleras, and the Pacific coast lowlands, running between the Pacific Coast and the western slopes of the Western Cordillera (see Fig. 2). The nature of the gold deposits in these three regions also varies somewhat. Most of the gold deposits in the Pacific Coast basin are placer deposits, located along the riverbeds that flow down

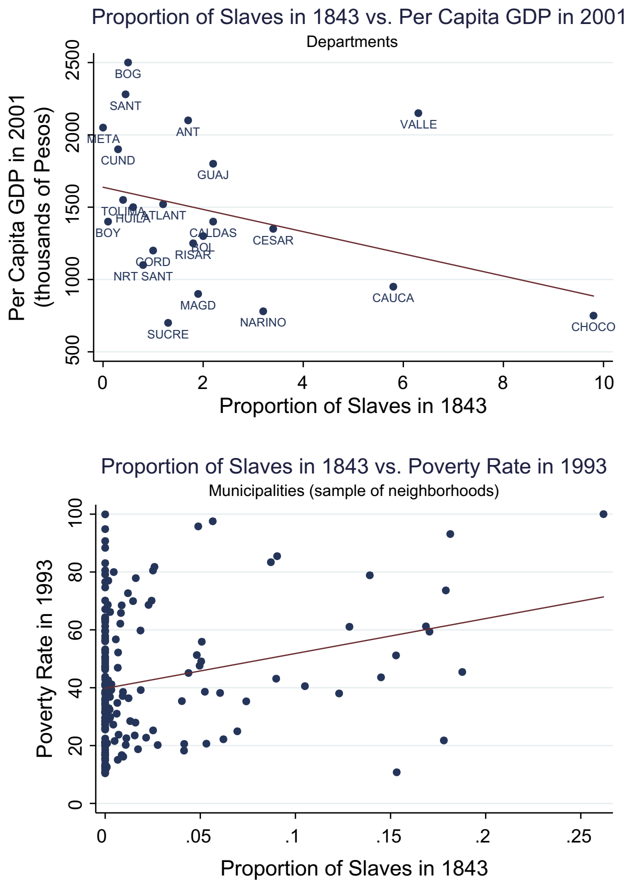


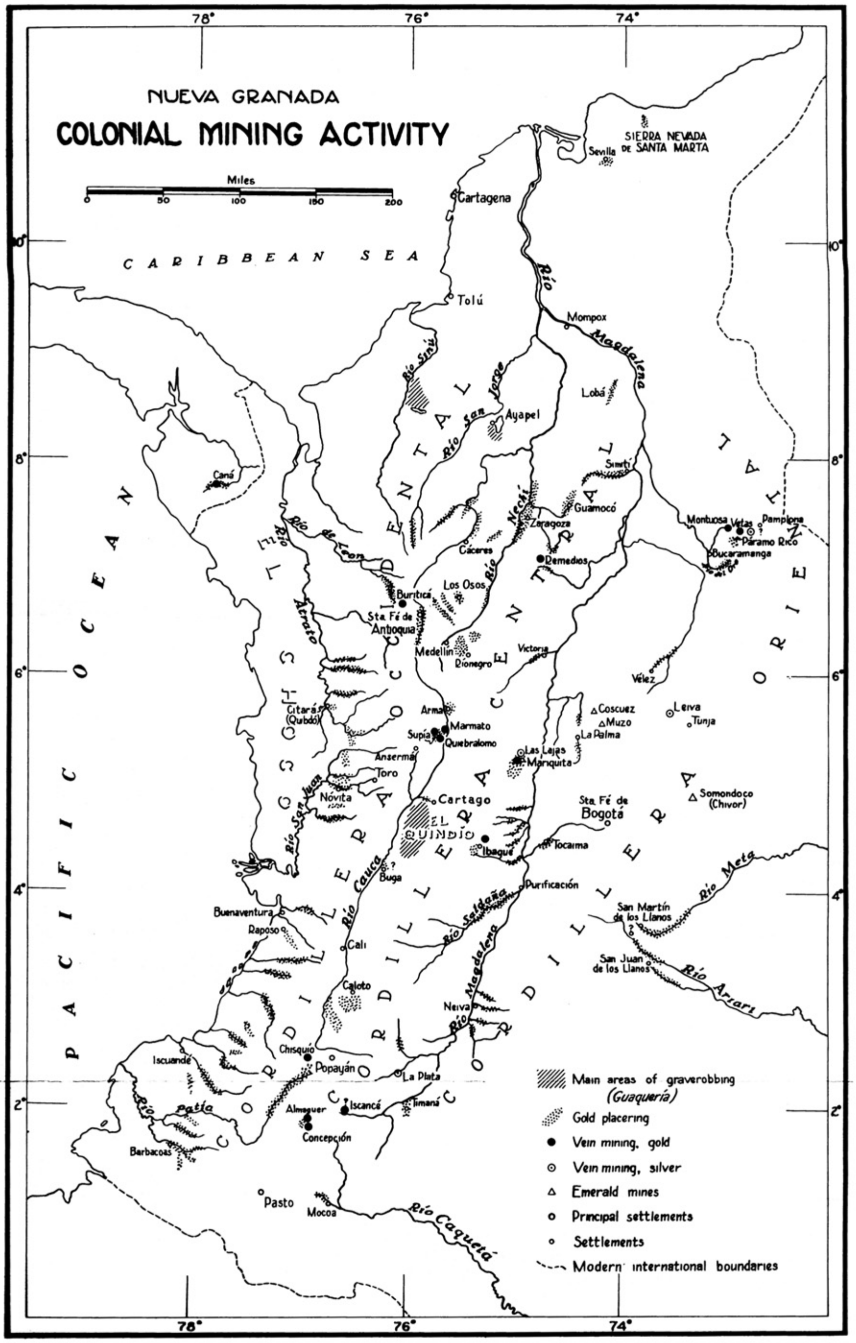
Fig. 1. Proportion of slaves in 1843 vs. per capita GDP in 2001 (top panel). Proportion of slaves in 1843 vs. poverty rate in 1993 (bottom panel).

to the Pacific basin, eroding the mineral deposits along the slope of the Western Cordillera on their way. On the other hand, gold deposits around the Cauca and Magdalena rivers are more varied in nature, with vein as well as placer mines. The northern highlands of the Central Cordillera (in what is today northern Antioquia), for example, were rich in vein deposits such as those in the Buriticá mines, while the rivers flowing east down the slopes of the Central Cordillera around the Ibagué and Mariquita region gave rise to several placer deposits. Finally, it is worth mentioning that although these three regions contained the majority of gold deposits in New Grenada, there were some other localized gold mining areas, for example around the Saratá River region in current Santander, on the slopes of the Eastern Cordillera.

Although the three main gold-mining regions had been identified by the Spanish by the late 16th Century, there was variation in the timing of exploitation of the different locations, both between and within the broadly defined regions. While in some areas gold production declined during the first half of the 17th Century, other mining districts like those in the Chocó along the Pacific lowlands only saw their systematic developmnent in the 18th Century. In fact, two elements explain the spatial dynamics of gold mining in New Grenada during the colonial period. First, the boom-and-bust nature of gold mining. As mines were discovered, they were intensely exploited and rapidly depleted. Technological limitations seem to have played an important role in this respect. For example, talking about the Anserma mines near the Cauca River, West (1952) argues that

> "Because of the pronounced Pyritic character of the ore, at least half of the gold content was lost in washing. Consequently, many of the smaller mines closed down; by 1627 the Negro slaves in the Anserma area had decreased to less than half of their former number. Yet, during the period 1629–35 nearly 190,000 pesos of gold, most of which came from the vein workings of Anserma, was registered at the royal treasury in Cartago. Although with their output reduced, the vein mines continued to produce until the middle of the seventeenth century..." (p. 10)

Indeed, most of the largest gold mines in Antioquia also saw their best years in the late 16th and early 17th centuries. By this time, most of the mines had already been depleted. In 1663 an official survey of Antioquia stated that only a few mines were not yet depleted by that time (Cardona, 1942). Nevertheless, as some mines were depleted, others were discovered. For example, the exploitation of the highland placers around Santa Rosa de Osos in the heart of Antioquia started only around



MAP 3.

Fig. 2. Source: West (1952).

the mid 17th century. Gold mining in the Upper Magdalena region was particularly prone to boom and bust behavior. The discovery of the gold placers in Remedios in 1594, for example, led to a huge gold rush that brought Spaniards from regions as far away as Cartagena on the Caribbean coast. Slave gangs were also relocated from other gold mines to Remedios during this gold rush. According to West (1952), more than 2000 slaves were brought during the first two years. The same pattern was observed further south in the highlands of current Cauca:

> "Throughout the colonial period the old gravels and recent stream alluvium of the Popayan plateau yielded a steady but decreasing flow of gold. Today the bare red slopes of the gravel hills and the almost endless rock and boulder tailings in the narrow valleys [at the] back of Santander [de Quilichao] and Caloto attest to the thorough exploitation that this area has undergone." (West, 1952, p. 13)

Occasionally, towns settled around the depleted mines would be moved towards newly discovered gold deposits. This phenomenon was very recurrent in the Cauca drainage of the Antioquia province, and probably led to very weak incentives for the development of public goods and local infrastructure. According to West (1952), in the mining region of Cáceres

> "Around 1700 depletion of surrounding placer deposits probably caused the transfer of Caceres thirty miles downstream to its present site on the Cauca... At that point gold-bearing terraces bordered the river, the old workings of which can still be seen today. By the end of the eighteenth century, however, even the new site had been almost abandoned. Today the town is still in a miserable state." (p. 25)

The second element explaining the difference in the timing of exploitation of gold deposits in New Grenada was the active opposition of native communities in some areas. This proved to be a major constraint during the early decades of conquest, when Spanish authority was still very weak beyond the main urban centers. It is best exemplified by the Spanish experience in the Nóvita area of the Chocó lowlands. Although by the late 16th Century Spanish mining entrepreneurs had already located and attempted to exploit the placer deposits along the Tamaná river using the local indigenous labor, the native indigenous groups in the area rebelled in the late 1500s forcing the closure of all mining activity. Spaniards were only able to return to the area in the 1630s and large scale exploitation in the Chocó region only started in the late 17th Century. As a result, Chocó became the main gold producing region in New Grenada only in the 18th Century. The resistance of the native communities was also a huge impediment for exploitation of the San Sebastián area in the upper Magdalena River in current Huila (in the highlands around the city of Popayán in current Cauca), and in the Frontino mines of western Antioquia where, according to West (1952),

> "Raids by hostile Chocoes probably caused temporary abandonment of the mines in the closing years of the 16th century, but in 1610 miners from Santa Fé de Antioquia re-established exploitation of the rich placers in the area." (p. 23)

2.2. Slavery and gold mining

Not only was the active resistance of native communities a major obstacle to the Spanish exploitation of gold mines, but it was also one of the main reasons why African slaves were introduced into New Grenada. The distribution of gold deposits in fact appears to be correlated with the presence of indigenous communities, which suffered an acute demographic collapse because of disease and overexploitation in the mines. The difficulty in controlling the indigenous labor force, together with their demise, led to a rapid substitution towards African slave labor. A case in point is the upper Cauca River region, where

> "By 1544 mining was well established in the upper Cauca... At the time, owing to indian rebellions, the Spaniards were already bringing Negro slaves as mine laborers." (West, 1952, p. 11)

This situation was very similar to the Brazilian plantation experience, where indigenous labor was first used and given up as mortality and reistance made it very expensive relative to slave labor (Schwartz, 1985). On the other hand, it contrasts sharply with the mining experiences of Peru and Mexico, where indigenous population densities in relative proximity to the silver deposits were much higher, and easier to control. As a result, most of the labor supply in those silver mines was indigenous. A third factor contributing to the substitution of African slaves for indigenous people was that colonial legislation became much more protective of the indigenous peoples, partly as a response to their demographic collapse. The legal differences in rights between both groups were marked, so that labor coercion on African slaves became less expensive for mining entrepreneurs (Jaramillo, 1974). Slaves often responded to coercion by fleeing the mines and Spanish settlements, retreating to inaccessible locations. Several towns of fled slaves developed around the areas with largest slave populations, of which the town of San Basilio de Palenque on the Caribbean coast is the best known. These towns were effectively not under the control of the Spanish crown and remained so even after independence. Of course, this was a major concern both for mining entrepreneurs and the authorities, and partly explains the harsh nature of the legislation concerning slaves that fled from their owners (Jaramillo, 1974, de Friedman and Cross, 1979).

The Spanish Empire not only tried to control slave labor in New Grenada, but also held a tight control over the slave trade, which increased the cost of slaves for mining entrepreneurs. For example, all slaves imported to New Grenada had to enter through the port of Cartagena. Nevertheless, the incentives for the use of slave labor in the mines were so large, that smuggling of African slaves became a major concern for the authorities, to the extent that the crown forbid all trade through the Atrato River in the Chocó region.[2] This river flows north along the Pacific lowlands, but actually ends its course in the Caribbean Sea, which made it a very convenient route for the introduction of smuggled slaves into the Pacific region. Nevertheless, even in the early period of mining activity the importation of slaves was steady. Original documents in Colombia's National Archives report figures from 1000 to 2400 slaves being imported annually through Cartagena during the 1590s (West, 1952). Of course,

[2] In Spanish, the name Atrato actually means "trade".

the current ethnic distribution of the population in Colombia is highly correlated with the distribution of gold mines during colonial times.

Together with the availability of indigenous labor, the location of gold mines was an important element driving the early distribution of Spanish settlement in New Grenada, and hence, the distribution of colonial State activity (McFarlane, 1993). Interestingly, in terms of our empirical strategy, the colonial state centralized its presence in relatively large Spanish settlements around the gold mining regions, which were intended to provide services and control to neighboring areas. These administrative and supply centers often contained smelter and royal treasury offices, and had political and religious jurisdiction over the surrounding areas. Their typical bureaucracies included an *alcalde de minas* (mines mayor) in charge of enforcing mining ordinances in surrounding camps, approving and recording the registration of new claims, and judging legal cases arising from disputes between miners, a *corregidor de indios* (sheriff for indian affairs) in charge of enforcing labor laws concerning the indigenous people, and often a *procurador general* (general prosecutor) who would be in charge of enforcing tax and commerce laws (West, 1952, p. 107). So for example, Santa Fé de Antioquia, Cali and Popayán, all along the Cauca river, became administrative centers for their respective surrounding mining regions.

3. The data

3.1. Slavery

Our focus of interest is the long-run effect of slavery on contemporary development outcomes. As described above, we explore this relationship by comparing neighboring municipalities in gold-deposit regions which differed in their status as gold mining places during the colonial period. Thus, we first collected historical data on the incidence of slavery. Census data during colonial times in New Grenada exists for 1778, but unfortunately the available data is reported only aggregated at the province level.[3] Nevertheless, slavery was not abolished until 1851, quite a bit after New Grenada achieved independence in 1819.[4] In 1843 the republican government performed a census, in which the number of slaves at the municipality level was reported (del Interior Secretaría, 1843). As a measure of slavery intensity, we focus on two measures; a dummy variable indicating the presence of slaves in a given municipality in 1843, and the percentage of the population who were slaves in 1843. Table 1a presents basic descriptive statistics splitting the sample between the colonial-gold-mine municipalities, their non colonial-gold-mine neighbors, and all other Colombian municipalities. Of course, a natural question that arises is the extent to which the 1843 distribution of slaves is a good proxy for the late colonial period distribution of slaves. The historical account does not mention any important differential trends in manumission or migration of slaves during this period. Nevertheless, we looked directly at the province-level correlation between the proportion of slaves in the 1778 and the 1843 census, and it exceeds 0.8. Fig. 3 illustrates the geographic distribution of slavery in New Grenada, where darker colors imply a larger share of slaves in the population in 1843. As Table 1a shows, by 1843 gold mining municipalities had on average a 4% slave population, twice the fraction of non-gold-mine municipalities. The table also reveals that slavery as a share of total population in the rest of the country was almost an order of magnitude smaller. Although the fraction of remaining slaves by 1843 was low relative to the 1778 census, Fig. 3 reveals that geographic variation was significant. Moreover, it was highly correlated with the 1778 distribution. Similarly, gold-mining municipalities were 1.5 times more likely to have slaves by 1843 than their neighboring municipalities without colonial gold mines.[5]

As mentioned above, we focus on gold mining as our key source of identification, and obtained the information on the location of colonial gold mines from Colmenares (1973), who in turn relied on West (1952) and original historical sources. A total of 42 current municipalities are listed as having gold mines at some point during the colonial period. We then included all the neighboring municipalities to this palette of 42 gold-mining municipalities, making up our base sample of 202 municipalities (consisting of 42 neighborhoods and 206 pairs of municipalities; see Fig. 4).

3.2. Main outcomes

We collected data on several development outcomes. We obtain the poverty rate in 1993 an 2005 (percent of housholds classified as poor) from the Colombian Statistics Department DANE, and the secondary school enrollment rate averaged over the 1992–2002 period from CEDE.[6] Some basic differences can already be seen from Table 1a, in the comparison of means between gold mining and non gold mining neighbors. Gold-mining municipalities have almost 15 percentage points higher poverty rates and 10 percentage points lower secondary school enrollment rates. On the other hand, the neighboring municipalities look very similar to the rest of the country, both in the poverty rates and the school enrollment rates. The same is true when looking at child vaccination rates for 2002 from the OCCHA research group at the United Nations. These are 30 percentage points lower in gold-mining municipalities than in their neighbors, which, in turn, are very similar to the rest of the country.

[3] Provinces were sub-units within *gobernaciones*(the analogous to current departments). See Tovar et al. (1994).

[4] The abolition of slavery was accomplished gradually due to the political clout and opposition of slave-owners. In 1821 a *Ley de Vientres*(wombs law) was approved by the Cúcuta Congress, by which the newborn children of slaves would be free when they turned 18. After abolition, the State actually made a commitment to compensate slave-owners for their losses.

[5] About 69% of gold-mining municipalities had slaves in 1843, while 45% of their non-gold-mining neighbors reported slaves in the 1843 census.

[6] CEDE (*Centro de Estudios sobre Desarrollo Economico*), is an economics research center at Universidad de los Andes in Bogotá, Colombia.

Table 1a
Descriptive statistics.

Variable	All municipalities			Colonial gold mines			p-Value for t-test for equality of means	Colonial gold mines neighbors		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.		Obs	Mean	Std. Dev.
Had slaves in 1843	839	0.419	0.493	42	0.690	0.460	0.001	145	0.400	0.490
% Slaves in 1843	839	0.006	0.018	42	0.04	0.06	0.001	145	0.02	0.04
Current development outcomes										
Land gini index, 2002	743	0.680	0.100	34	0.72	0.12	0.940	105	0.72	0.09
% in Poverty, 2005	917	44.457	20.667	42	76.61	39.26	0.000	160	43.52	31.37
% in Poverty, 1993	839	51.760	18.240	42	61.92	25.12	0.000	145	55.35	19.44
Secondary enrollment rate, 1992–2002	828	0.588	0.264	40	0.52	0.25	0.013	145	0.62	0.22
% Homes with aqueduct, 2002	895	67.425	24.086	42	49.40	30.35	0.000	156	72.10	20.17
% Homes with sewage, 2002	895	31.888	24.519	42	35.55	29.90	0.952	156	35.29	23.17
% Homes with electricity, 2002	895	70.678	22.583	42	57.99	28.97	0.000	156	77.13	19.66
% Children vaccinated, 2002	789	0.741	0.267	42	0.41	0.19	0.000	156	0.74	0.22
Geographic										
Longitude	839	−74.55	1.41	41	−75.76	1.41	0.841	145	−75.70	1.33
Latitude	839	5.75	2.50	41	5.47	1.94	0.223	145	5.05	2.10
Land area (sq. km)	839	613.1	1459.7	41	1575.6	1989.2	0.0001	145	738.3	774.5
Altitude above sea level (m)	839	1235.5	920.9	41	790.5	779.0	0.015	145	1125.1	763.0
Annual rainfall (mm)	839	1748.0	866.7	41	3273.3	2043.1	0.001	145	2357.7	1287.1
Distance to department capital (km)	839	109.44	81.71	41	178.37	157.03	0.210	145	147.56	128.13
Density of primary rivers (km/sq. km)	839	33.52	251.30	41	20.52	68.53	0.648	145	32.22	160.21
Density of secondary rivers (km/sq. km)	839	23.51	60.17	41	51.91	133.59	0.050	145	27.21	34.83
Density of tertiary rivers (km/sq. km)	839	18.29	57.60	41	27.77	81.02	0.218	145	17.55	30.18
Per capita land suitable for agriculture (sq. km)	825	0.62	2.10	41	0.37	0.89	0.822	145	0.44	1.61
Demographics										
Population, 2005	836	35872	209691	42	60193	101776	0.441	144	39648	161100
% Indigenous population, 2005	911	5.33	16.28	42	10.35	16.58	0.518	160	7.77	17.79
% Afro-decending population, 2005	911	5.99	14.71	42	33.92	34.19	0.003	160	18.48	28.51
% Other ethnicities, 2005	911	86.37	22.75	42	51.27	41.23	0.010	160	69.25	37.35
Malaria incidence, 2000–2003	912	2.89	16.20	42	13.80	26.74	0.706	158	11.88	29.81

Also from OCCHA we have the percent of households with aqueduct, sewage and electricity coverage in 2002. Although no statistically significant differences can be observed for sewage converage between gold-mining municipalities and their neighbors, the latter have around 20 percentage points higher aqueduct and electricity coverage than the former. The final contemporary development outcome we report in Table 1a is land inequality, as measured by the land gini coefficient in 2002. These land gini coefficients are constructed using the cadastral data from IGAC, the Colombian geographic information department. Land inequality is extremely high in Colombia, with an average land gini coefficient of 0.70. The raw descriptive statistics do not reveal significant differences in this variable across the different groups of municipalities.

Providing satisfactory evidence consistent with the idea that differences in the economic performace of different municipalities are partly due to the historical incidence of slavery requires finding persistent effects over long periods of time. With this purpose in mind, we collected information on intermediate development outcomes by looking at the 1918 and the 1938 National Censuses. Both provide cross-sectional historical data on several development outcomes. From the 1918 Census we have data on total literacy, school enrollment, and vaccination coverage. From the 1938 Census we have data on adult literacy, and on aqueduct, electricity, and sewage coverage of buildings.[7] Descriptive statistics are reported in Table 1b. A pattern very similar to the one for contemporary outcomes can be observed, although the magnitude of the differences is not as large as for current outcomes. Gold-mining municipalities have lower school enrollment rates in 1918, and significantly lower adult literacy, aqueduct, electricity, and sewage coverage in 1938, than their neighbors. Once again, all other municipalities look on average very similar to the neighbors sample. For example, electricity coverage in 1938 (percent of buildings with electricity connection) was 2% for the colonial-gold-mining municipalities, 6% for their neighbors, and 7% for all other municipalities.

3.3. Other municipality characteristics

CEDE also provided us with a set of geographic covariates including longitude, latitude, land area, altitude above sea level, average annual rainfall, distance to the department capital, and the density of primary, secondary, and tertiary rivers.[8] Table 1a presents descriptive statistics for this data. The table highlights that geographic characteristics in Colombia vary

[7] Notice that the contemporary data on aqueduct, electricity and sewage coverage is for households, whereas the 1938 data is for buildings.
[8] Longitude, latitude and altitude above sea level are computed for the *cabecera municipal*, this is, the location of the urban center of the municipality.

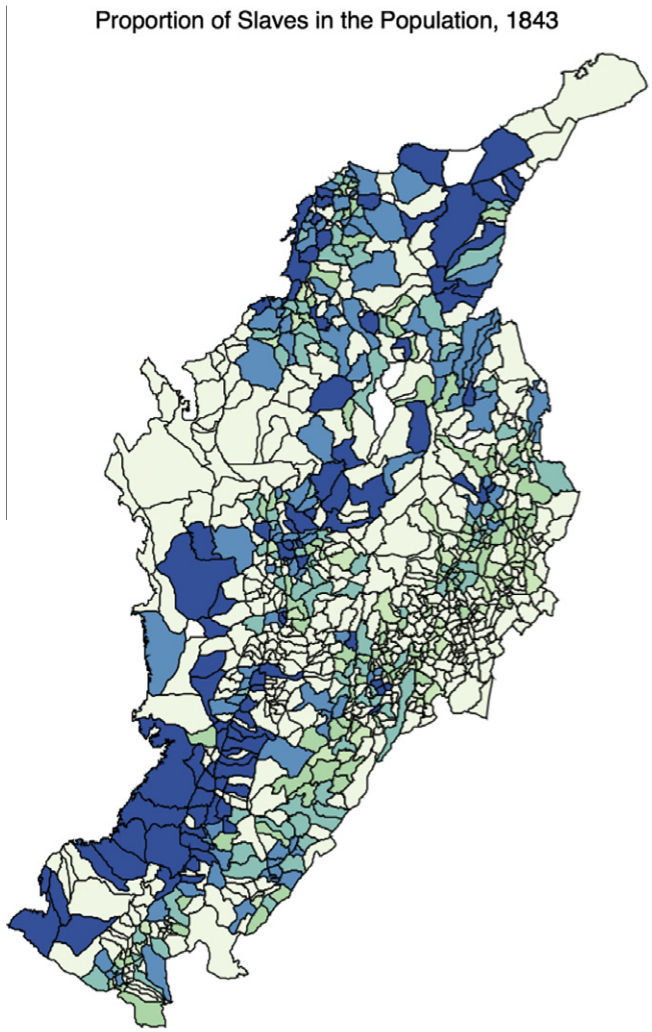


Fig. 3. Proportion of slaves in the population, 1843. Very light green = 0–0.052. Light green = 0.052–0.104. Green = 0.104–0.156. Green blue = 0.156–0.208. Blue = 0.208–0.26. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

considerably across even short distances. Because of the highly variable topographic conditions of the Andean region in Colombia, some significant differences arise between municipalities with colonial gold mines and their neighbors. The former have on average twice the land area, two thirds of the altitude, and 40% more rainfall than the latter. These facts highlight that controlling for observable geographic characteristics in our analysis is important, even when comparing municipalities which share a boundary. On the other hand, both subsets of municipalities do not seem to differ in terms of their distance to main cities, their river density, or their supply of agriculture-suitable land. We also collected data from IGAC on the distribution of soil qualities, soil suitabilities (agriculture, livestock, conservation),[9] and geological features such as the share of fresh water, valleys, mountainous terrain, hilly terrain, and plains as a fraction of total land area.

We also gathered some additional historical data. In particular, Durán y Díaz (1794) provides data on state presence during the late colonial period. This is an original document containing detailed information on the location of colonial state offices and the bureaucracy throughout New Granada in 1794. Based on this source we computed two measures of colonial State presence. Durán y Díaz (1794) reports the municipalities with a tobacco and playing cards *estanco*, an *aguardiente* (liquor) and gunpoweder estanco, an *alcabala*, and a post office.[10] We constructed a "colonial state presence index" taking values from 0 to 4, based on a straightforward count of how many of these local institutions were present in the municipality, which constitutes our first measure of colonial State presence. Durán y Díaz (1794) also reported the number of Crown employees in

[9] Notice this is data on geological classification of soils for their suitability for different activities, not on the actual use of those soils.

[10] An *estanco* was a regional board with monopsonic and/or monopolic power over specific goods produced and traded in the region. Thus for example, the liquor *estanco* would buy all of the local production of anise, necesary for the production of *aguardiente.* An *alcabala*, on the other hand, was the tax-collection office in charge of collecting the indirect taxes levied on consumption goods.

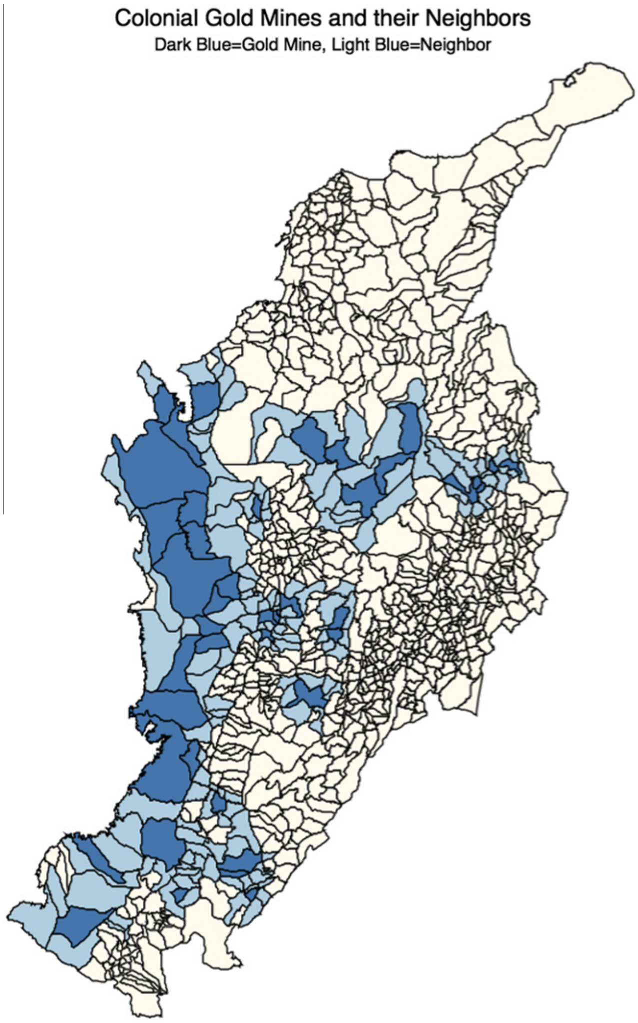


Fig. 4. Colonical gold mines and their neighbors. Dark blue = gold mine. Light blue = neighbor. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

the municipality, and this constitutes our second measure of colonial State presence. While municipalities with colonial gold mines appear to have significantly larger values of the colonial state presence index than municipalities without gold mines (1.26 vs. 0.5), the latter had larger numbers of Crown employees (3.54 vs. 2.17). The state presence index is also correlated with the presence of slaves in 1843 for reasons already discussed above.

We also collected demographic data such as total population, and its self-reported ethnic composition in 2005 from DANE. In Colombia, people can report to be of African descent, indigenous, mixed, raizal, [11] Roma, or not to report an ethnicity. The central feature to highlight is that gold-mining municipalities have on average almost twice the share of population of African descent than their neighbors, which in turn have three times the share of all other municipalities. Interestingly, the former also had twice as many slaves in 1843 than the latter. Population of African descent make up 34% of the population in gold-mining municipalities, they represent 18% of the population of neighbor municipalities, and only 6% of the population of all other municipalities. It is clear that the distribution of African descendants is highly correlated with the places where slavery was most intensively used. One additional covariate we collected is the incidence of Malaria in 2000–2003 (number of cases per 1000, averaged over the 4 years). Malaria incidence is almost 4 times higher in gold-mining municipalities compared to the national average, because these receive higher rainfall, and are warmer (lower altitude) as the table also shows. Nevertheless, gold-mining municipalities are again similar to their neighbors, with no significant difference in this outcome either.

In an attempt to explore potential channels of persistence, we also collected information on contemporary state presence and the sectoral distribution of employment. Fundación Social, a Colombian NGO, collected detailed data on an array of state

[11] Raizal refers to the ethnic population from the Caribbean islands of San Andrés and Providencia.

Table 1b
Descriptive statistics.

Variable	All municipalities			Colonial gold mines			*p*-Value for *t*-test for equality of means	Colonial gold mines neighbors		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.		Obs	Mean	Std. Dev.
Had slaves in 1843	839	0.419	0.493	42	0.690	0.460	0.001	145	0.400	0.490
% Slaves in 1843	839	0.006	0.018	42	0.04	0.06	0.001	145	0.02	0.04
Historical development outcomes										
Literacy rate, 1918	577	0.271	0.124	33	0.28	0.13	0.491	99	0.30	0.12
School enrollment, 1918	577	0.065	0.047	33	0.06	0.03	0.003	99	0.08	0.04
Vaccine coverage, 1918	577	0.299	0.204	33	0.27	0.13	0.634	99	0.29	0.16
Adult literacy rate, 1938	640	0.463	0.164	33	0.35	0.13	0.000	118	0.453	0.13
% Buildings with aqueduct, 1938	641	0.048	0.093	33	0.02	0.02	0.000	118	0.051	0.03
% Buildings with electricity, 1938	641	0.071	0.111	33	0.02	0.03	0.000	118	0.060	0.03
% Buildings with sewage, 1938	641	0.023	0.058	33	0.005	0.01	0.001	118	0.017	0.02
Contemporary State Capacity										
Per capita tax collection, 2002	895	29919	44364	42	27035	27054	0.258	156	22400	22451
Per capita municipality public employees, 1995	837	2.116	1.848	42	1.95	1.68	0.795	144	2.03	2.00
Per 1000 police posts, 1995	801	0.097	0.093	40	0.06	0.07	0.342	142	0.07	0.06
Economic structure										
Official labor share in agriculture, 2008	907	0.054	0.097	42	0.043	0.055	0.727	158	0.048	0.076
Official labor share in mining, 2008	907	0.153	0.164	42	0.190	0.221	0.028	158	0.130	0.136
Official labor share in manufacturing, 2008	907	0.166	0.195	42	0.113	0.144	0.140	158	0.159	0.187
Official labor share in government services, 2008	907	0.117	0.158	42	0.078	0.080	0.117	158	0.124	0.184
Historical state capacity										
Count of number of colonial offices in 1794[a]	840	0.540	0.816	42	1.26	1.17	0.000	145	0.50	0.97
Number of crown employees in 1794	840	6.192	134.95	42	2.17	2.65	0.714	145	3.54	24.27

[a] Colonial public offices include a state monopoly administration for Aguardiente and Gunpowder, a state monopoly administration for Tobbaco and Cards (estancos), a Postal Office, and a Tax Collection Office (alcabala). Thus, this variable can take values from 0 to 4.

presence variables in 1995. These include the number of municipality public employees, police posts, courts, registry offices, public phone service offices, public mail service offices, health centers and hospitals, schools, libraries, fire stations, jails, and tax collection offices. This data draws a very comprehensive picture of the distribution of state presence in the mid 1990s. Additionally, OCCHA also provides data on per capita tax revenue, which is standard in the state capacity literature as a measure of the taxing ability of the state. Table 1b also includes descriptive statistics for the subset of state capacity outcomes which we discuss in the paper. Interestingly, both for the gold-mining sample and for the neighbors sample, average tax collection, number of per capita employees and number of per capita police posts are somewhat lower than for the rest of the country. This is consistent with the conventional wisdom that gold-deposit areas have had a historically low degree of state presence in Colombia. On the other hand, no significant differences arise between the gold-mining sample and the neighbors sample for these variables.

4. Empirical strategy

4.1. General approach

A simple correlation between the fraction of slaves in 1843 and current income per capita (2001) across Colombian departments reveals a strong negative relationship (see the top panel of Fig. 1). In fact, the poorest department of Colombia, ChocÓ, has an income per capita of around a fifth of the richest, Bogotá, and had an order of magnitude more slaves as a share of the population in 1843. Although suggestive, this correlation could be the result of many other factors explaining both the variation in contemporary development outcomes and in the historical incidence of slavery. Indeed, differences across departments are stark along many dimensions. A similar picture arises when looking at the poverty rate in the cross-section of municipalities we use in this paper, which is negatively correlated with the proportion of slaves in 1843 (see the bottom panel of Fig. 1). In the cross section of municipalities slavery is likely to be related with other determinants of long run development (even after controlling for all posible observables). This will be in part because factor endowments, agricultural productivity or distance to markets, among other things, are all likely to be correlated with colonial slavery and could have an impact on the path of development. Although we might be able to control for some of these covariates, remaining unobservables could always be driving the correlations in the data.

In the case of New Grenada, and more generally across the Spanish colonies in America, slavery was used in large-scale agriculture, mining, haciendas, and domestic activities. Geography and natural resources vary considerably even within Colombia, so that regions with different conditions exploited slave labor for different activities and to a different extent.

Throughout the historical discussion in Section 2, we stressed the importance of the gold mining economy as a key source of variation in slavery in New Grenada. This is an attractive source of variation in slavery relative to, say, agricultural productivity in cotton and sugarcane, or proximity to the slave auction markets in the Caribbean, because most colonial gold mines in New Grenada were depleted at some point between the 16th and early 19th centuries. Because other determinants of slavery such as agricultural productivity or relative distance to markets are persistent features throughout long time periods, they are likely to have direct effects on the outcomes of interest. In contrast, gold deposits exploited and depleted during the colonial period, necessarily, cannot have a direct effect on current outcomes.

Nevertheless, the discussion also noted that gold-deposit regions are vastly different from non-gold-deposit regions in many dimensions, and had very different historical experiences during the colonial period. Hence, it is likely that gold deposit-location is correlated with determinants of current outcomes for which we cannot control. In particular, a valid exogenous source of variation should have an effect on current outcomes only through its effect on slavery intensity. Our argument is that gold mines are a more attractive source of variation when the sample is restricted to gold-mining municipalities and their neighbors because this avoids the comparison between gold-deposit regions and the rest of the country.

Fig. 4 presents a map of Colombia (excluding the eastern plains and the Amazon region, which were frontier areas during colonial times) depicting the sample used in this paper, consisting of gold-mining municipalities, shown in dark, and their direct neighbors, shown in a lighter shade. Differences in gold mining status between neighboring municipalities are not likely to be correlated with differences in other determinants of long-run outcomes. The neighborhood structure illustrated in Fig. 4 makes it clear that the validity of our identification strategy requires only that within each neighborhood, the fact that one of the municipalities had colonial gold mines and its neighbors did not, can be considered as good as randomly assigned (conditional on observables). If so, because these colonial mines were depleted long ago, any differences in outcomes between pairs of neighboring municipalities can be plausibly attributed to the difference in the incidence of slavery between them.

A caveat to this strategy is the possibility that colonial gold mines had an effect on slavery intensity beyond the municipality boundaries, and into the surrounding neighborhood. Nevertheless, in this setting the possibility of this spillover would have the effect of weakening the correlation between the presence of colonial gold mines and slavery intensity in the sample, making colonial gold mines a weak source of variation in observed slavery. Another concern is our choice of unit of analysis. By comparing differences in slavery and outcomes across (neighboring) municipalities, we are focusing on long-run effects of slavery working at the municipality level. If, in contrast, the mechanisms operate at a larger level of aggregation (e.g. regional, provincial), so that in practice the effect of slavery spills across all municipalities within a given area, we would underestimate the effects of slavery on economic development.

Out of a total of 1119 Colombian municipalities, our sample is thus restricted to the 42 municipalities which had colonial gold mines according to historical sources, and the 160 municipalities without gold mines adjacent to them. Our first strategy, the neighbor-pair fixed effects estimator, is in the spirit of a paired matching estimator, and compares each gold-mining municipality to each of its neighbors.

Cultural traits, some geographic features, or even violence (an important element in the Colombian context) are all likely to be similar across the boundaries of neighboring municipalities. Neighboring municipalities might even have common labor markets. Key to this empirical strategy is our ability to directly control for all of the factors common to a given pair of neighbors, which we do by directly introducing fixed effects for each neighbor pair. Although our strategy compares pairs of directly adjacent municipalities, being able to control for unobservables shared by the pair is important. An analogy with the randomized control trials literature is helpful to understand why. When the trials sample is stratified, and the randomization of the treatment intent is performed at the subgroup level, fixed effects for the subgroups on which the stratification has taken place should be included. To the extent that the randomization differs, or take-up rates differ across the sub-groups, controlling for this common unobservable within subgroups is necessary. In our setting, each pair of neighbors is analogous to a subgroup, with the difference that we created the groups a fortiori, relying on the assumption that each pair of neighbors was subject to the same regional environment. The reason why one of them ended up with a gold mine in colonial times, and the other one did not, conditional on observables, can be for all practical purposes considered as random. Within the neighbor-pair, we argue that the conditionally exogenous source of variation in slavery is the presence of a colonial gold mine. But gold-mining status for each current municipality is the realization of a stochastic process that depended on municipality characteristics, idiosyncracies of the gold search process during colonial times, and possibly some of the unobservables common to the neighbor pair. Hence, just as in a randomized trials setting one needs to control for unobservable differences in the randomization process across subgroups, here we need to control for analogous differences across pairs of neighbors.

In this setting, the inclusion of pair-specific fixed effects limits our ability to estimate standard errors clustered at the neighborhood level because this would require a large number of observations per cluster, while we only have two (Baum et al. (2003, p. 10)). In fact, we have 4 times as many pair fixed effects as clusters, impeding the computation of such clustered standard errors.

To overcome this difficulty, we take advantage of the fact that we can model the variance structure of unobservables following our construction of the estimation sample. This allows us to estimate random effects models where we allow for within-cluster unobservables, and which, moreover, also allows us to incorporate the specific sources of cross-neighborhood correlation that arise when a given municipality is a member of more than one neighborhood. Reasuringly, the IV estimates from models with neighbor-pair effects and with random effects are very similar.

4.2. Estimation framework

From the sample of all municipalities in Colombia, C, we restrict our analysis to the subset $M \subset C$ of municipalities which, according to the historical record, had gold mines at some point during the colonial period, and the subset N of all their directly adjacent neighbors. Let $K = M \cup N$. We index colonial gold mine municipalities by g, $g = 1, \ldots, 42$, and non-colonial gold mine municipalities by $i, j, k \ldots$ Also, define $N(g) \subseteq N$ to be the subset of non-gold-mining neighbors of gold-mining municipality $g \in M$. In the same way, define $M(i) \subseteq M$ to be the subset of gold-mining municipalities of which non-gold-mining municipality $i \in N$ is a neighbor. Although for most non-gold-mining municipalities $M(i)$ is a singleton (i only belongs to one neighborhood), there are 38 non-gold-mining municipalities bordering more than one gold mining municipality, and hence, for which $M(i)$ has more than one element.

Also, let y_τ be any of our development outcomes, S_τ be our measure of slavery, G_τ be an indicator variable for the presence of colonial gold mines (so that $G_g = 1$ and $G_i = 0$), and let $\boldsymbol{x}_\tau$ be a vector of covariates. $\boldsymbol{x}_\tau$ will include a constant, geographic variables, and department dummies unless otherwise stated.[12]

4.2.1. Neighbor-pair fixed effects

Our first empirical strategy is based on comparing pairs of adjacent municipalities of which one member had colonial gold mines and the other member did not. We posit the following linear model for outcomes: For each pair (g, i), $i \in N(g)$,

$$\begin{aligned} y_g &= \beta S_g + \boldsymbol{\gamma} \mathbf{x}_g' + \xi_{gi} + v_g \qquad g \in M \\ y_i &= \beta S_i + \boldsymbol{\gamma} \mathbf{x}_i' + \xi_{gi} + v_i \qquad i \in N(g) \end{aligned} \tag{1}$$

where ξ_{gi} are the neighbor-pair fixed effects, which represent unobservables common for the neighbor pair (g, i). v_τ are τ-specific unobservables. Of course, we allow for $cov(S, \xi) \neq 0$. Under the assumption that all remaining unobservables are conditionally uncorrelated with slavery, so that $cov(S, v) = 0$, the inclusion of neighbor-pair fixed effects is necessary for consistent estimation of β, our causal parameter of interest. We estimate by OLS models of this form, and call the estimated effect β^{PE}_{OLS} the OLS neighbor-pair fixed-effects estimator.

Even after controlling for common unobservables across municipality borders, and a flexible specification for the geographic controls (we allow for up to a quartic polynomial on our geographic covariates[13]), it is possible that $cov(S, v) \neq 0$. Hence, when allowing slavery to be conditionally correlated with municipality-specific unobservables, to proceed further our identification strategy relies on the assumption that conditional on the common unobservables for a pair of neighboring municipalities, the difference in slavery between them is due to the presence of a colonial gold mine in one of them. Moreover, it assumes that conditional on covariates and on the common-to-the-neighbor-pair fixed effects, the presence of a gold mine at some point during colonial times does not have a direct effect on current outcomes. Of course, the assumption that differences in slavery due only to the presence or absence of a colonial gold mine are uncorrelated with municipality-specific unobservables is a satisfactory assumption only when doing these very local contrasts. Moreover, the assumption that the location of these colonial gold mines is as good as random is also only sensible when comparing nearby areas within gold deposit regions.

Another key choice is whether we should focus on the intensive or the extensive margin of slavery. We focus on the extensive margin for two reasons. First, this is likely to better capture the source of variation relevant for long-run development, which depends not so much on the exact number of slaves but whether a municipality developed economic and political institutions to use and control slaves. The extensive margin is more relevant for the source of variation. The distribution of the proportion of slaves supports our approach. Fig. 5 shows a very skewed slavery intensity distribution in our estimation sample, with most municipalities having either no slaves or a small fraction of them by 1843. Second, our data on slavery is from 1843, which is already late in the history of slavery in New Grenada. At this point the gold mining economy had already been in decline for decades. In several places it had all but dissapeared (Colmenares, 1973). Although the remnant slave distribution in 1843 is likely to be a proxy for the intensity of slavery during colonial times when the gold-mining economy was thriving, we cannot account for any differential trends in the decline of slavery, for example due to manumission.[14] We will also see that in OLS regressions, the extensive margin appears to be the dimension correlated with long-run development outcomes.

Following this discussion, we also posit a first-stage relationship of the form:

$$\begin{aligned} S_g &= bG_g + \mathbf{c}\mathbf{x}_g' + \zeta_{gi} + \epsilon_g \qquad g \in M \\ S_i &= bG_i + \mathbf{c}\mathbf{x}_i' + \zeta_{gi} + \epsilon_i \qquad i \in N(g) \end{aligned} \tag{2}$$

[12] Although limited in number, some neighborhoods straddle department boundaries. Hence, there is some variation in department status within neighborhoods and pairs.

[13] Indeed, given the discontinuous nature of the source of identification we are using (a boundary where colonial-gold-mine status changes), controlling for a flexible specification on covariates is very important. If they induce any strong nonlinearities on the outcomes, failing to take them into account might lead us to mistake these effects for the effect of the difference in slavery intensity (Angrist and Pischke, 2009). This is in the same spirit of regression discontinuity models, which generally control flexibly for other covariates around the discontinuity.

[14] One caveat is that as Kane et al. (1999) show in the context of the estimation of returns to schooling, when an endogenous explanatory variable is a discrete proxy of a continuous variable, this introduces non-classical measurement error which biases IV estimates even when the instrument is valid. As explained in the text, the slavery dummy is not a proxy for slavery intensity but the relevant aspect of slavery. Nevertheless, if the intensive margin also matters, a similar bias might still arise, though the OLS results below suggest that the intensive margin may not be very important in practice.

Fig. 5. 1843 Slavery intensity distribution. Sample of neighbors.

where S_τ is a dummy for slavery, ζ_{gi} represents common unobservables for the neighbor pair (g,i), and ϵ_τ represents municipality-specific unobservables. Notice that the discrete nature of our instrument G_τ implies that the Instrumental Variables estimator of β, which we call the IV neighbor-pair fixed-effects estimator β_{IV}^{PE}, is equivalent to a Wald estimator. This equivalence makes the interpretation of the source of identification very clear. β is identified off the average difference in outcomes between municipalities with colonial gold mines and municipalities without them (their neighbors), normalized by the difference in slavery between both groups, conditional on covariates and neighbor-pair unobservables:

$$\beta_{IV} = Wald = \frac{E[y|G=1,\boldsymbol{x},\xi] - E[y|G=0,\boldsymbol{x},\xi]}{E[S|G=1,\boldsymbol{x},\zeta] - E[S|G=0,\boldsymbol{x},\zeta]}$$

4.2.2. Random effects

Our concern about correct inference leads us to our alternative estimation strategy. We posit a random effects structure on the variance of unobservables in our sample. Specifically, we assume that there is one unobservable ξ for each neighborhood, and that all of these are drawn independently from a common distribution with zero-mean and variance σ_ξ^2. Thus, the random effects model is

$$\begin{aligned} y_g &= \beta S_g + \gamma \mathbf{x}_g' + \xi_g + v_g \qquad g \in M \\ y_i &= \beta S_i + \gamma \mathbf{x}_i' + \sum_{g \in M(i)} \xi_g + v_i \qquad i \in N \end{aligned} \tag{3}$$

The correlation structure is assumed to take the following form: $E[\xi_g^2] = \sigma_\xi^2$, $E[\xi_{g_1}\xi_{g_2}] = 0$, $E[v^2] = \sigma_v^2$, $E[\xi v] = 0$, and $E[v_i v_j] = 0$. The analogous first stage of the Random Effects model takes the form:

$$\begin{aligned} S_g &= bG_g + c\mathbf{x}_g' + \xi_g + \epsilon_g \qquad g \in M \\ S_i &= bG_i + c\mathbf{x}_i' + \sum_{g \in M(i)} \xi_g + \epsilon_i \qquad i \in N \end{aligned} \tag{4}$$

where the correlation structure is the same as the one for the second stage. Defining $u_g = \xi_g + v_g$ and $u_i = \sum_{g \in M(i)} \xi_g + v_i$, the above implies that

$$\begin{aligned} E[u_g^2] &= \sigma_\xi^2 + \sigma_v^2 \\ E[u_{g_1} u_{g_2}] &= 0 \\ E[u_i^2] &= |M(i)|\sigma_\xi^2 + \sigma_v^2 \end{aligned}$$

$$E[u_g u_i] = \begin{cases} \sigma_\xi^2 & \textit{if } g \in M(i) \\ 0 & \textit{if } g \notin M(i) \end{cases}$$

$$E[u_i u_j] = |M(i) \cap M(j)|\sigma_\xi^2$$

Using these moments we construct the covariance matrix for the residuals $\boldsymbol{\Omega} = E[\mathbf{u}\mathbf{u}']$. This correlation structure takes into account both the within-clusters residual correlation, and the cross-cluster correlation induced by the non-gold-mining municipalities in common across neighborhoods. Appendix A illustrates the structure of Ω with a simple example. Of course, the feasible random effects estimator requires prior estimation of Ω. Using the standard OLS residuals from Eq. (3), Appendix A describes the construction of the estimated variance matrix $\widehat{\Omega}$.[15]

We first estimate the model in Eq. (3), and call the estimated effect β^{RE}_{OLS} the OLS random effects estimator. As is standard in a GLS setting, defining **X** = [**S**:**x**], the OLS Random effects estimator is given by

$$\beta^{RE}_{OLS} = (\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{X})^{-1}(\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{y}) \tag{5}$$

Defining the matrix **W** as

$$\mathbf{W}(i,j) = \begin{cases} u_i u_j & \text{if } \mathbf{\Omega}(i,j) \neq 0 \\ 0 & \text{if } \mathbf{\Omega}(i,j) = 0 \end{cases}$$

the within-cluster heteroskedasticity-robust variance matrix of this estimator is given by

$$V(\beta^{RE}_{OLS}) = (\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{X})^{-1}\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{W}\widehat{\mathbf{\Omega}}^{-1}\mathbf{X})(\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{X})^{-1} \tag{6}$$

For the IV random effects estimator β^{RE}_{IV}, we follow White (1989), who discusses Instrumental Variables Generalized Least Squares estimators. Defining **Z** = [**G**:**x**], the IV random effects estimator is given by[16]

$$\beta^{RE}_{IV} = (\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z})^{-1}\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{X})^{-1}(\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z})^{-1}\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{y}) \tag{7}$$

In analogous way as in Eq. (6), the within-cluster heteroskedasticity-robust variance matrix of this estimator is given by

$$\begin{aligned} V(\beta^{RE}_{IV}) = {} & (\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z})^{-1}\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{X})^{-1}(\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z})^{-1}\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{W} \\ & * \widehat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z})^{-1}\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{X}(\mathbf{X}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z})^{-1}\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{X})^{-1} \end{aligned} \tag{8}$$

5. Estimation results

5.1. Similarity of neighbors

Our strategy compares gold-mining with non-gold-mining municipalities. As such, it resembles matching methodologies, which often are based on matching on observables, and similarly requires both municipalities in a pair not to differ significantly on key observable characteristics. Thus, we begin by presenting a set of "similarity" regressions in Table 2. The table reports the estimation results of running OLS neighbor-pair fixed effects regressions of the different measures of soil qualities and soil characteristics, on the colonial gold mine dummy G_τ:

$$\begin{aligned} T_g &= \phi + \pi G_g + \delta_{gi} + \epsilon_g \qquad g \in M \\ T_i &= \phi + \pi G_i + \delta_{mi} + \epsilon_i \qquad i \in N(g) \end{aligned} \tag{9}$$

In Eq. (9), T_τ can be any of the shares of soil qualities (classified from quality 1 to quality 8 by IGAC), the shares of soil suitabilities, or the shares of land under different topological conditions. Table 2 shows that across all of these characteristics, no significant differences arise between the different pairs of adjacent municipalities. The reported regressions in the table do not include department fixed effects, but results including them make the estimates of π even smaller and less significant. These results make us confident we are comparing pairs of municipalities that are indeed very similar to each other along key geographic features.

5.2. Development outcomes

5.2.1. Current outcomes: OLS results

We begin the exposition of our findings by focusing on four contemporary development outcomes. Table 3 presents OLS results for the 1993 poverty rate, average secondary enrollment rates between 1992 and 2002, the fraction of children vaccinated in 2002, and the land gini coefficient in 2002. For each outcome, the first four columns of the table present results for the neighbor-pair fixed effects model in Eq. (1), while the fifth to eighth columns present analogous results for the random effects model in Eq. (3). Odd-numbered columns present the estimates of a model focusing on the extensive margin of slavery, so that the explanatory variable is a dummy taking the value of 1 for those municipalities that had slaves in 1843.

[15] For the IV models, either the standard OLS or the IV residuals can be used for consistent estimation of $\widehat{\sigma^2_\xi}$ and $\widehat{\sigma^2_V}$ (White (1989, p. 190)). Throughout we present results using OLS residuals.

[16] White (1989) notices that the standard IV analogue $\hat{\beta} = (\mathbf{X}'\mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'\mathbf{X})^{-1}(\mathbf{X}'\mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'\widehat{\mathbf{\Omega}}^{-1}\mathbf{y})$ is not in general correct in the presence of heteroskedasticity and/or autocorrelation. This expression is also not an efficient estimator in the presence of more than one instrument, when residuals are spherical (White (1989, p. 178)). We nevertheless also computed this estimator, which yields results almost identical to those from Eq. (7) (omitted to save space).

Even-numbered columns include a horse race between the intensive and extensive margins, adding the fraction of slaves in 1843 on the right-hand side.

In line with the discussion above, Table 3 suggests that the extensive margin is much more important as a correlate of contemporary development outcomes. In particular, the introduction of the fraction of slaves (in the even-numbered columns) does not alter the magnitude or significance of the estimated coefficient on the slavery dummy (in the odd-numbered columns). Indeed, the slavery dummy is statistically significant throughout the table, except for the random effects estimates on the land gini models, where the slavery dummy is not significant prior to the introduction of the proportion of slaves variable. Moreover, the fraction of slaves is either statistically insignificant, or its significance is lower than the significance level of the slavery dummy. The exception is column (10) looking at secondary enrollment in the neighbor-pair fixed effects specification, where both the slave dummy and the fraction of slaves are significant and the latter has a larger t-statistic. Nevertheless, the fraction of slaves loses its significance in the random effects model. Of course, the fact that the fraction of slaves remains significant in some specifications (particularly in the children vaccination rate models) does not allow us to completely rule out the possibility that the intensive margin might play a role of its own.

Focusing on column (1) in Table 3, municipalities with slaves in 1843 appear to have on average 15.9 (s.e. = 2.45) percentage points higher poverty rates that their neighbor pairs in 1993. The estimated magnitude falls only slightly to 12.1 (s.e. = 2.73) when including the battery of geographic controls (see column (3)). This is around a quarter of the mean poverty rate, or two thirds the standard deviation of the poverty rate across Colombian municipalities. The magnitude of the difference falls to around a half in the random effects models. The estimate in column (7) implies that within a cluster of municipalities around a colonial gold mine municipality, municipalities with slavery (in 1843) have on average 5.7 (s.e. = 2.6) percentage points higher poverty rates than those without. This is in fact a general pattern: in the OLS estimates, we typically see a fall of about 50% when moving from the neighbor-pair fixed effects to the neighborhood random effects specifications. We do not have an explanation for this pattern. But reassuringly, as we will see below, the estimated magnitudes in the instrumental variables models, which are our main focus, are very similar between the neighbor-pair fixed effects and the neighborhood random effects specifications.

Together with increased poverty rates, columns (11) and (15) show that the presence of slaves is associated with between 10 (s.e. = 4.2) and 6.5 (s.e. = 3.3) percentage points lower secondary enrollment rates (for the neighbor-pair fixed effects and neighborhood random effects, respectively). The latter estimate implies an effect that is 10% of the average and 23% of the standard deviation in secondary enrollment rates across Colombia. Across outcomes, children vaccination rates appear to be the most strongly correlated with slavery. The random effects estimate in column (23) implies that municipalities with slaves have 14.5 (s.e. = 3.6) percentage points lower vaccination coverage than their neighbors without slaves. This is equivalent to 20% of the average, and almost half a standard deviation of the Colombian distribution of vaccination rates. The fourth panel of Table 3 presents results for the land gini coefficient. Although estimates are statistically significant and positive for all of the neighbor-pair fixed effects models, they lose significance in the random effects specifications. Nevertheless, the coefficient magnitudes remain fairly similar. The point estimate from column (31) implies 0.023 higher land gini indeces in municipalities with slavery relative to their neighboring non-slaveholding municipalities.[17]

The top panel of Table A2.1 in Appendix B presents analogous OLS results for additional contemporary development outcomes. Municipalities with slaves appear to have lower electricity and aqueduct coverage rates in 2002, and somewhat higher poverty rates in 2005.[18] The OLS estimates for both neighbor-pair fixed effects and neighborhood random effects show a robust pattern of economic underperformance of municipalities with slaves in 1843 relative to their adjacent neighbors without any slaves. These results motivate our subsequent instrumental variables strategy.

5.2.2. First stages

The estimates we have presented thus far assume that any differences in slavery status across municipality boundaries is conditionally uncorrelated with unobservables that vary within the pairs of colonial gold-mining/non-gold-mining municipalities, or within the neighborhoods of colonial gold-mining municipalities. Despite our exclusive focus on a sample of municipalities lying in gold deposit regions, and despite our local comparisons of pairs and adjacent neighborhoods, we can actually exploit variation in colonial gold mining status directly as an instrument for slave presence in 1843. The relationship between presence of colonial gold mines and presence of slaves is strong. In our estimation sample, 29 out of 42 municipalities with colonial gold mines had a positive number of slaves in 1843 (70%), while only 73 out of 160 municipalities without colonial gold mines had slaves in 1843 (45%). Table 4 looks more rigorously at this relationship. It presents the first stages of our main instrumental variables models, across different specifications, for the sample used in the 1993 poverty rate models.[19] Columns (1)–(5) present the first-stage estimates for the neighbor-pair effects models, while columns (6)–(10) look at the neighborhood random effects estimates. These first stages are fairly precisely estimated, and the coefficient

[17] Note, however, that we have no data on land ginis for municipalities in the northwestern department of Antioquia, which reduces our sample size for this outcome critically, given the historical importance of gold mining in this department. This may partly explain the reduced significance in slavery here.

[18] We should point out that demographers have raised some concerns regarding the 2005 Population Census from which the 2005 poverty data comes from. The apparent reason is a flawed sampling design. For this reason we focused on the 1993 poverty data.

[19] Table A2.2 in Appendix B presents the benchmark first stages for all the different samples. They are all very close to the ones discussed in this section.

Table 2
Land characteristics in gold-mine and non-gold-mine municipalities: OLS models.

Neighbor-pair fixed effects models	Soil quality shares							
	Quality 1 (1)	Quality 2 (2)	Quality 3 (3)	Quality 4 (4)	Quality 5 (5)	Quality 6 (6)	Quality 7 (7)	Quality 8 (8)
Colonial gold mines dummy	−0.003 (0.004)	−0.001 (0.001)	−0.013 (0.017)	−0.015 (0.021)	0.01 (0.012)	0.02 (0.046)	−0.079 (0.066)	0.082 (0.063)
Department fixed effects	*N*	*N*	*N*	*N*	*N*	*N*	*N*	*N*
Observations	354	354	354	354	354	354	354	354
R-squared	0.69	0.50	0.69	0.57	0.65	0.71	0.67	0.67
	Soil suitability shares			Topology shares				
	Agriculture (9)	Livestock (10)	Conservation (11)	Water (12)	Valleys (13)	Mountains (14)	Hills (15)	Plains (16)
Colonial gold mines dummy	−0.025 (0.061)	−0.017 (0.040)	0.03 (0.056)	0.013 (0.010)	−0.021 (0.024)	−0.038 (0.048)	0.047 (0.045)	0.000 (0.001)
Department fixed effects	*N*	*N*	*N*	*N*	*N*	*N*	*N*	*N*
Observations	354	354	354	354	354	354	354	354
R-squared	0.66	0.6	0.74	0.51	0.68	0.76	0.74	0.54

Standard errors are robust to arbitrary heteroskedasticity.
Constant and coefficients on controls not reported.
Soil qualities 1–8 are classified by IGAC according to their natural aptitude for agriculture in the following way:
Quality 1 soils are those with "none of very few use limitations, apt for crops and need only ordinary management practices to conserve their productivity".
Quality 2 soils are those with "slight use limitations either due to their soil, topography, drainage, or climate. Require some practices to avoid erosion".
Quality 3 soils are those with "moderate limitations due to soil depth, erosion, fertility, slope, drainage or climate. They require intensive conservation practices".
Quality 4 soils are those with "severe limitations due to steep slopes, low soil depth, low fertility, poor drainage, frequent flooding or saltiness. Require intensive conservation practices".
Quality 5 soils are those with "limitations due to flooding or rocky surfaces that limit usage to grasslands or forest".
Quality 6 soils are those with "permanent unalterable limitations such as very steep slopes, high erosion susceptibility, rockiness, low soil depth, poor drainage, frequent flooding, low humidity retention, excessive salt or sodium. Their use is limited to grasslands or forest".
Quality 7 soils are those with "severe limitations due to extreme slopes, low soil depth, rockiness, high salt and/or sodium contents poor drainage and unfavorable climate. Their use is limited to forest".
Quality 8 soils are those with "extremely severe limitations due to soil, topography, drainage or climate that limit their use to conservation".
Soil suitabilities and topology are also from IGAC.
Source: Instituto Geográfico Agustín Codazzi IGAC (n.d.). *Manual para el Uso de las Estadísticas de Zonas Físicas Homogeneas, Carreteras y Suelos.*

Table 3
Long-run effect of slavery on development outcomes: OLS models.

	Poverty rate 1993								Secondary enrollment rate average 1992–2002							
	Neighbor-pair fixed effects models				Random Effects models				Neighbor-pair fixed effects models				Random Effects models			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Had slaves in 1843	15.992	12.401	12.126	10.090	6.533	5.526	5.726	6.340	−0.122	−0.063	−0.100	−0.067	−0.071	−0.064	−0.065	−0.056
	(2.454)	(2.748)	(2.731)	(3.062)	(2.940)	(3.458)	(2.680)	(2.993)	(0.043)	(0.031)	(0.042)	(0.031)	(0.035)	(0.031)	(0.033)	(0.029)
Fraction of slaves in 1843		69.246		38.787		25.891		15.564		−1.085		−0.616		−0.198		−0.247
		(29.349)		(37.514)		(41.892)		(47.515)		(0.347)		(0.389)		(0.431)		(0.443)
σ_ξ^2					13.374	14.246	9.031	8.366					0.000	0.000	0.000	0.000
σ_ν^2					259.23	261.80	220.47	225.22					0.043	0.044	0.039	0.039
R-squared	0.482	0.487	0.639	0.639	0.443	0.443	0.560	0.560	0.257	0.272	0.378	0.392	0.200	0.204	0.327	0.330
Geographic controls	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Observations	352	352	352	352	179	179	179	179	336	336	336	336	172	172	172	172

	Percent children vaccinated 2002								Land gini 2002							
	Neighbor-pair fixed effects models				Random Effects models				Neighbor-pair fixed effects models				Random Effects models			
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
Had slaves in 1843	−0.311	−0.229	−0.291	−0.230	−0.139	−0.108	−0.145	−0.105	0.056	0.039	0.033	0.033	0.029	0.027	0.023	0.017
	(0.035)	(0.036)	(0.038)	(0.041)	(0.034)	(0.033)	(0.036)	(0.037)	(0.016)	(0.018)	(0.014)	(0.017)	(0.019)	(0.021)	(0.016)	(0.015)
Fraction of slaves in 1843		−1.561		−1.185		−0.887		−1.035		0.271		0.418		0.394		0.481
		(0.401)		(0.505)		(0.430)		(0.522)		(0.310)		(0.329)		(0.320)		(0.251)
σ_ξ^2					0.000	0.000	0.000	0.000					0.000	0.000	0.000	0.000
σ_ν^2					0.041	0.039	0.040	0.038					0.007	0.007	0.005	0.005
R-squared	0.333	0.371	0.428	0.457	0.337	0.370	0.389	0.414	0.387	0.429	0.523	0.563	0.314	0.342	0.504	0.539
Geographic controls	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Observations	352	352	352	352	179	179	179	179	248	248	248	248	129	129	129	129

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.
Partial *R*-squared for the neighbor-pair fixed effects models, which excludes the contribution of the fixed effects to the fit of the model.

Table 4
First stages of the iv models: slave presence on colonial gold mines presence.

	Had slaves in 1843[a]									
	Neighbor-pair fixed effects models					Random Effects models				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Colonial gold mines presence	0.378 (0.048)	0.384 (0.057)	0.371 (0.061)	0.237 (0.060)	0.375 (0.061)	0.369 (0.065)	0.363 (0.072)	0.399 (0.089)	0.259 (0.083)	0.400 (0.089)
Colonial state presence index, 1794				0.143 (0.027)					0.146 (0.025)	
Crown employees, 1794					0.0026 (0.0014)					0.002 (0.0009)
R-squared	0.254	0.352	0.515	0.558	0.520	0.187	0.249	0.415	0.481	0.422
Geographic controls	N	Y	Y	Y	Y	N	Y	Y	Y	Y
Quartic on geographic controls	N	N	Y	Y	Y	N	N	Y	Y	Y
Observations	352	352	348	348	348	179	179	177	177	177

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.
Partial *R*-squared for the neighbor-pair fixed effects models, which excludes the contribution of the fixed effects to the fit of the model.
[a] 1st Stages for the poverty rate 1993, poverty rate 2005, vaccination rate 2002, electricity coverage 2002, and aqueduct coverage 2002 regression samples.

estimates are only marginally affected by the introduction of geographic controls (columns (2) and (7)), a full quartic polynomial on the geographic controls (columns (3) and (8)), and the number of Crown employees in 1794 as a measure of colonial State presence (columns (5) and (10)). The introduction of the colonial state presence index in 1794 in columns (4) and (8) reduces the magnitude of the colonial gold mines dummy from around 0.37 (s.e. = 0.06) in column (6), to 0.26 (s.e. = 0.08) in column (9), but the estimate remains highly significant. Overall, the first stage results show that the presence of colonial gold mines within these gold deposit regions is associated with a 30% higher likelihood of still having slaves in 1843. These results are very similar between the neighbor-pair fixed effects and random effects models.

5.2.3. Current outcomes: IV results

Table 5 then presents the main results of our paper. It reports the instrumental variables estimates of the effect of slavery on our main set of contemporary development outcomes. The table reports specifications without and with geographic controls, for both the neighbor-pair fixed effects and the neighborhood random effects models. It also presents the F-statistic for joint significance of the associated first stage, showing it is always strong. Odd-numbered columns present the baseline specification, and even-numbered columns then introduce geographic controls. Estimates for the slavery dummy are fairly stable both to the inclusion of geographic controls, and across models. The magnitude of the neighbor-pair fixed effects IV estimates is very close to that of the corresponding OLS estimate. Only for the land gini models the IV estimates appear to be around 50% larger in magnitude. In line with our concern regarding appropriate inference, the neighborhood random effect standard errors for the IV estimates are consistently larger than the corresponding neighbor-pair fixed effects standard errors. Given the similar magnitudes of the estimates of both kinds of specifications, below we will focus on describing the random effects results.

The estimate from column (4) in Table 5 implies that the presence of slavery in 1843 has led to 13.1 percentage points (s.e. = 6.9) higher poverty rates within neighborhoods of municipalities in gold deposit regions around a colonial gold mining municipality. This estimate is statistically significant at the 5% level. The point estimate for secondary enrollment rates from column (7) is −0.127 (s.e. = 0.06), also significant at the 5% level. Nevertheless, the inclusion of geographic controls reduces its magnitude to =0.11 (s.e. = 0.06) making it marginally insignificant. Moving onto the children vaccination rate in column (12), the estimate of −0.25 (s.e. = 0.1) implies that the presence of slavery has led to 25 percentage points lower vaccination rates, which is close to a full standard deviation decrease in vaccination coverage. Lastly, column (16) reports the IV random effects estimate for the land gini. In contrast to the OLS result, here the point estimate is significant at the 5% level. The presence of slavery in 1843 is associated with 0.04 (s.e. = 0.02) points higher land inequality as measured by the gini coefficient. This pattern of results for the land gini suggests that the OLS estimate is subject to attenuation bias due to measurement error, not only because the IV estimate is larger in magnitude, but also because the IV standard errors increases by less than the coefficient estimates, making the IV estimates statistically more significant. For completeness, the top panel of Table A2.3 in Appendix B presents the IV estimates for our additional contemporary outcomes. Although the pattern of signs is consistent with slavery having a negative effect on development, the random effects estimates remain significant after the introduction of geographic controls only for aqueduct coverage (see column (6)).

Table 5
Long-run effect of slavery on development outcomes: IV models.

	Poverty rate 1993				Secondary enrollment rate average 1992–2002			
	Neighbor-pair fixed effects models		Random Effects models		Neighbor-pair fixed effects models		Random Effects models	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Had slaves in 1843	12.938	11.356	14.628	13.142	−0.114	−0.087	−0.127	−0.106
	(3.929)	(3.935)	(7.095)	(6.892)	(0.047)	(0.052)	(0.063)	(0.062)
σ^2_ξ			13.374	9.031			0.000	0.000
σ^2_ν			259.23	220.47			0.043	0.039
1st Stage *F*-statistic	2.190	2.217	3.181	2.345	2.318	2.430	3.069	2.462
p-Value	0.000	0.000	0.000	0.001	0.000	0.000	0.001	0.001
Geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	352	352	179	179	336	336	172	172

	Percent children vaccinated 2002				Land gini 2002			
	Neighbor-pair fixed effects models		Random Effects models		Neighbor-pair fixed effects models		Random Effects models	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Had slaves in 1843	−0.249	−0.251	−0.247	−0.254	0.087	0.045	0.048	0.040
	(0.066)	(0.074)	(0.097)	(0.107)	(0.017)	(0.015)	(0.024)	(0.021)
σ^2_ξ			0.000	0.000			0.000	0.000
σ^2_ν			0.041	0.040			0.007	0.005
1st Stage *F*-statistic	2.190	2.217	3.181	2.345	2.312	2.200	3.233	1.963
p-Value	0.000	0.000	0.000	0.001	0.000	0.000	0.001	0.015
Geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	352	352	179	179	248	248	129	129

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.

Table 6
Long-run effect of slavery on intermediate development outcomes: OLS models.

	School enrollment 1918				Vaccine coverage 1918			
	Neighbor-pair fixed effects models		Random Effects models		Neighbor-pair fixed effects models		Random Effects models	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Had slaves in 1843	−0.020	−0.016	−0.010	−0.010	−0.029	−0.017	−0.016	−0.015
	(0.008)	(0.010)	(0.007)	(0.008)	(0.034)	(0.034)	(0.028)	(0.031)
σ_{ξ}^2			0.000	0.000			0.000	0.000
σ_{ν}^2			0.001	0.001			0.023	0.020
R-squared	0.379	0.582	0.322	0.532	0.206	0.375	0.162	0.347
Geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	216	216	111	111	216	216	111	111
	Literacy rate 1938				Aqueduct coverage 1938			
	Neighbor-pair fixed effects models		Random Effects models		Neighbor-pair fixed effects models		Random Effects models	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Had slaves in 1843	−0.069	−0.047	−0.033	−0.030	−0.017	−0.013	−0.0074	−0.0062
	(0.015)	(0.021)	(0.019)	(0.018)	(0.006)	(0.007)	(0.0038)	(0.0047)
σ_{ξ}^2			0.001	0.001			0.000	0.000
σ_{ν}^2			0.011	0.007			0.001	0.001
R-squared	0.487	0.649	0.444	0.638	0.245	0.375	0.221	0.321
Geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	242	242	123	123	242	242	123	123

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.
Partial R-squared for the neighbor-pair fixed effects models, which excludes the contribution of the fixed effects to the fit of the model.

5.2.4. Intermediate outcomes

In the subsection above we have reported finding an effect of mid-19th century slavery on late 20th century and early 21st century development outcomes. By themselves, those results are quite remarkable. Nevertheless, it would be reasuring to observe similar patterns at intermediate dates. With this purpose in mind, Table 6 presents OLS results for a subset of outcomes from the 1918 and the 1938 population censuses. The sample of municipalities available for these dates is smaller than the one used for contemporary outcomes. Albeit weaker, we nevertheless find some suggestive results. Table 6 presents results for the 1918 school enrollment rate and vaccination coverage in the top panel, and for the 1938 literacy rate and aqueduct coverage in the bottom panel. Following the format of previous tables, the first two columns for each outcome present neighbor-pair fixed effects estimates, and the last two columns present neighborhood random effects estimates. Odd-numbered columns are the benchmark specifications, and even-numbered columns include geographic controls. Table 6 shows that the pattern of signs is robust. Slavery in 1843 is associated with lower school enrollment and vaccination rates in 1918, and with lower literacy and aqueduct coverage rates in 1938. Neighbor-pair fixed effects estimates are statistically significant at the 5% level for both of the 1938 outcomes (see columns (10) and (14)), and for school enrollment in 1918 prior to the introduction of geographic controls (see column (1)). Random effects estimates, although close in magnitude to neighbor-pair fixed effects estimates, are in general statistically insignificant. An exception is the estimate for slavery in the 1938 literacy rate model, which is significant at the 10% level (−0.03 with s.e. = 0.018). The bottom panel of Table A2.1 in Appendix B presents complementary results looking at the literacy rate in 1918, sewage, and electricity coverage rates in 1938.

Table 7 follows the same format, and presents instrumental variables estimates as those from Table 6. IV leads to statistically significant estimates for both of the 1938 outcomes. Once more, both IV neighbor-pair fixed effects and neighborhood random effects estimates are similar in magnitude to OLS neighbor-pair estimates. The estimate from column (11) is −0.07 (s.e. = 0.03). It implies that slavery in 1843 led to 7 percentage points lower literacy in 1938. At a time where only 46% of the population in Colombia were literate, this is an effect of considerable economic importance. Similarly, the estimate from column (16) is −0.024 (s.e. = 0.012), and implies that municipalities with slavery would have about half the aqueduct coverage in 1938 of Colombian municipalities without slavery. IV estimates for both 1918 outcomes, though consistently negative, are not statistically significant. Finally, the bottom panel of Table A2.3 in Appendix B presents complementary IV results looking at the literacy rate in 1918, and sewage and electricity coverage rates in 1938. Here, the random effects estimate for the electricity coverage model is significant (−0.02, s.e. = 0.012), implying an electricity coverage by 1938 in municipalities with slavery almost 30% lower than in non-slaveholding municipalities.

Table 7
Long-run effect of slavery on intermediate development outcomes: IV models.

Panel A: Second stage	School enrollment 1918				Vaccine coverage 1918			
	Neighbor-pair fixed effects models		Random Effects models		Neighbor-pair fixed effects models		Random Effects models	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Had slaves in 1843	−0.017	−0.017	−0.021	−0.022	−0.024	−0.018	−0.043	−0.037
	(0.009)	(0.010)	(0.016)	(0.019)	(0.039)	(0.039)	(0.076)	(0.076)
$\boldsymbol{\sigma}_{\xi}^{2}$			0.000	0.000			0.000	0.000
$\boldsymbol{\sigma}_{v}^{2}$			0.001	0.001			0.023	0.020
1st Stage *F*-statistic	2.251	2.458	2.883	1.852	2.251	2.458	2.883	1.852
p-Value	0.000	0.000	0.003	0.026	0.000	0.000	0.003	0.026
Geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	216	216	111	111	216	216	111	111
	Literacy rate 1938				Aqueduct coverage 1938			
	Neighbor-pair fixed effects models		Random Effects models		Neighbor-pair fixed effects models		Random Effects models	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Had slaves in 1843	−0.080	−0.065	−0.069	−0.056	−0.029	−0.028	−0.024	−0.024
	(0.019)	(0.021)	(0.030)	(0.032)	(0.010)	(0.012)	(0.013)	(0.012)
$\boldsymbol{\sigma}_{\xi}^{2}$			0.001	0.001			0.000	0.000
$\boldsymbol{\sigma}_{v}^{2}$			0.011	0.007			0.001	0.001
1st Stage *F*-statistic	1.949	2.286	2.061	1.492	1.949	2.286	2.061	1.492
p-Value	0.000	0.000	0.029	0.097	0.000	0.000	0.029	0.097
Geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	242	242	123	123	242	242	123	123

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.

Table 8
Slavery and colonial state presence: correlations.

Had slaves in 1843	1.000		
Colonial state presence index, 1794	0.406	1.000	
Crown employees, 1794	0.098	0.327	1.000
Obs: 177			

Overall, our results for intermediate outcomes are less precisely estimated, possibly reflecting the smaller sample size and the noisier data, and though consistently in the same direction as our contemporary outcome results, they are typically not statistically significant. This leaves the possibility that the divergence occurred in part in the second half of the 20th century, though our interpretation puts more weight on this being a consequence of smaller sample size and lower data quality.

5.3. Robustness

In this section we present additional robustness exercises. Tables 9 and 10 present OLS and IV estimates, respectively, of some additional specifications on our main contemporary development outcomes. For each outcome the first three columns present results for neighbor-pair fixed effects models, and the last three columns present neighborhood random effects models. For each class of models, the first column presents results when allowing a fully flexible fourth-degree polynomial on our geographic covariates. As we mentioned in section 4, the discontinuous nature of our identification strategy makes it important that we make sure the effects are not being driven by sharp nonlinearities at the municipality boundaries. Reassuringly, although standard errors increase somewhat, the magnitude of the estimates remains stable relative to the specifications without the polynomial terms.

As we discussed in Section 2, natural resources and colonial state presence are likely correlated across Colombia. There we argued that within a neighbor-pair of municipalities, or within a neighborhood of adjacent municipalities, the presence or absence of gold mines during the colonial period was uncorrelated with colonial state presence, which would vary significantly only at the regional level. This is, of course, a key identifying assumption of our empirical strategy. On the other hand, although our data on slavery is for 1843, as we mentioned above, we consider this to be a proxy for the presence of this labor institution during the 2.5 centuries of colonial rule. From this perspective, colonial state presence in the late 18th century (for which we have municipality-level data in 1794), at the eve of the emancipation from Spain, could be a potentially important omitted variable. Table 8 presents a table of correlations between our 1843 slavery dummy and our two measures of colonial state presence in 1794, the colonial state presence index described in section 3, and the number of Crown employees, in our estimation sample. The three variables are positively correlated, but the correlation is stronger between the colonial state presence index and slavery ($\rho = 0.4$) than between the number of Crown employees and slavery ($\rho = 0.1$). Hence, the second columns of Tables 9 and 10 subsequently include the colonial state presence index in 1794, and the third columns use Crown employees alternatively. Focusing on Table 10, as a salient pattern, the magnitude of the estimate for the slavery dummy increases in magnitude when the colonial state presence index is included. For example, for the 1993 poverty rate, the random effects coefficient estimate in column (5) is 20.77 (s.e. = 10.55). Interestingly, our measures of colonial state presence appear to be positively correlated with development (they enter negatively in the poverty rate and land gini models, and positively in the secondary enrollment and vacciantion coverage models). This pattern suggests that our benchmark estimates of the effect of slavery may actually be biased downwards due to a positive correlation between slavery and state presence.

Estimates of the slavery dummy, on the other hand, remain almost unchanged when controlling for Crown employees instead. This suggests that the presence of colonial taxation and public goods institutions, as measured by the State presence index, is qualitatively different from the presence of crown employees. Interestingly, a large fraction of the reported Crown employees were actually members of the militia. Overall, Table 10 does not suggest our results are being driven either by non-linearities on geographic observables, nor by a correlation of slavery with other historical institutions such as the colonial state. If anything, the results suggest that our estimates are lower bounds on the effect of slavery on long-run development. We must notice, nevertheless, that the random effects estimates for the land gini do become insignificant after the introduction of the quartic polynomial in geographic variables. This is a model with a significantly reduced number of degrees of freedom given the smaller sample size for this outcome, and the increased number of covariates.

As an additional set of results, we explored some potential channels of persistence. We do not find strong evidence suggesting their importance. Thus, we leave those results in Appendix B. In particular, following Michaels (2010), who finds patterns of sectoral specialization in a sample of Texan counties as a result of oil discoveries, we explored the possibility that slaveholding municipalities may have endured differential patterns of sectoral specialization. Table A2.4 presents neighbor-pair fixed effects "similarity" regressions (see Eq. (9)) of the sectoral (formal) employment distribution in 2008,[20] on the

[20] Formal employment is defined as the number of workers who are covered by either health, pension, or health and pension benefits. This data accounts only for fomal employment, which is only around 45% of total employment in Colombia. Unfortunately there is no available information on total (formal and informal) employment at the sectoral-municipality level. Albeit imperfect, the data does give a picture of the sectoral distribution of employment. In particular, we use the share of (formal) employment in agriculture, mining, manufacturing, and government services.

Table 9
Long-run effect of slavery on development outcomes: OLS models with additional controls.

	Poverty rate 1993						Secondary enrollment rate average 1992–2002					
	Neighbor-pair fixed effects models			Random Effects models			Neighbor-pair fixed effects models			Random Effects models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Had slaves in 1843	13.972 (3.497)	15.484 (3.608)	14.362 (3.517)	5.786 (3.431)	6.938 (3.407)	6.148 (3.378)	−0.116 (0.046)	−0.141 (0.055)	−0.122 (0.046)	−0.057 (0.044)	−0.078 (0.048)	−0.065 (0.045)
Colonial state presence index, 1794		−1.029 (1.401)			−1.199 (1.406)			0.019 (0.020)			0.022 (0.019)	
Crown employees, 1794			−0.099 (0.040)			−0.069 (0.030)			0.002 (0.001)			0.001 (0.0003)
σ_ξ^2				0.000	0.000	0.000				0.000	0.000	0.000
σ_v^2				195.71	195.81	194.63				0.04	0.04	0.04
R-squared	0.742	0.742	0.744	0.676	0.678	0.680	0.504	0.505	0.515	0.431	0.437	0.446
Quartic on geographic controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	348	348	348	177	177	177	332	332	332	170	170	170

	Percent children vaccinated 2002						Land gini 2002					
	Neighbor-pair fixed effects models			Random Effects models			Neighbor-pair fixed effects models			Random Effects models		
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Had slaves in 1843	−0.308 (0.045)	−0.280 (0.058)	−0.312 (0.046)	−0.169 (0.044)	−0.153 (0.046)	−0.169 (0.045)	0.020 (0.015)	0.020 (0.019)	0.020 (0.015)	0.011 (0.019)	0.007 (0.020)	0.011 (0.019)
Colonial state presence index, 1794		−0.019 (0.023)			−0.016 (0.024)			0.000 (0.008)			0.005 (0.007)	
Crown employees, 1794			0.001 (0.0006)			0.000 (0.0003)			−0.0001 (0.0002)			0.0001 (0.0002)
σ_ξ^2				0.000	0.000	0.000				0.000	0.000	0.000
σ_v^2				0.039	0.04	0.04				0.005	0.005	0.005
R-squared	0.528	0.541	0.529	0.469	0.472	0.469	0.685	0.687	0.685	0.662	0.663	0.662
Quartic on geographic controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	348	348	348	177	177	177	244	244	244	127	127	127

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.
Partial *R*-squared for the neighbor-pair fixed effects models, which excludes the contribution of the fixed effects to the fit of the model.

Table 10
Long-run effect of slavery on development outcomes: IV models with additional controls.

Panel A: Second stage	Poverty rate 1993						Secondary enrollment rate average 1992–2002					
	Neighbor-pair fixed effects models			Random Effects models			Neighbor-pair fixed effects models			Random Effects models		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Had slaves in 1843	12.752	22.330	12.551	11.885	20.774	11.872	−0.089	−0.146	−0.084	−0.091	−0.167	−0.090
	(4.872)	(8.118)	(3.753)	(6.736)	(10.553)	(5.371)	(0.053)	(0.056)	(0.051)	(0.058)	(0.071)	(0.058)
Colonial state presence index, 1794		−3.782			−3.758			0.024			0.034	
		(1.863)			(2.238)			(0.012)			(0.016)	
Crown employees, 1794			−0.043			−0.039			0.001			0.001
			(0.037)			(0.024)			(0.0004)			(0.0002)
$\boldsymbol{\sigma}_\xi^2$				0.000	0.000	0.000				0.000	0.000	0.000
$\boldsymbol{\sigma}_v^2$				195.71	195.81	194.63				0.038	0.038	0.037
1st Stage *F*-statistic	2.651	3.257	2.674	2.408	3.046	2.408	2.807	3.262	2.810	2.349	2.939	2.350
p-Value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Quartic on geographic controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	348	348	348	177	177	177	332	332	332	170	170	170
	Percent children vaccinated 2002						Land gini 2002					
	Neighbor-pair fixed effects models			Random Effects models			Neighbor-pair fixed effects models			Random Effects models		
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Had slaves in 1843	−0.259	−0.414	−0.256	−0.231	−0.360	−0.231	0.073	0.128	0.073	0.037	0.040	0.037
	(0.081)	(0.123)	(0.052)	(0.106)	(0.147)	(0.070)	(0.021)	(0.041)	(0.021)	(0.022)	(0.031)	(0.022)
Colonial state presence index, 1794		0.061			0.054			−0.020			−0.001	
		(0.025)			(0.029)			(0.008)			(0.006)	
Crown employees, 1794			0.001			0.0004			−0.0001			0.0000
			(0.0005)			(0.0003)			(0.0001)			(0.0001)
$\boldsymbol{\sigma}_\xi^2$				0.000	0.000	0.000				0.000	0.000	0.000
$\boldsymbol{\sigma}_v^2$				0.039	0.0389	0.0392				0.005	0.0046	0.0046
1st Stage *F*-statistic	2.651	3.257	2.674	2.408	3.046	2.408	2.416	2.842	2.389	1.594	1.922	1.577
p-Value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.038	0.006	0.041
Quartic on geographic controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	348	348	348	177	177	177	244	244	244	127	127	127

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.

colonial gold mines dummy. Gold mining municipalities do not appear to have a differential size of the agriculture, the mining, the manufacturing or the government employment shares relative to their neighbors without gold mines. Table A2.5 then looks at neighbor-pair fixed effects regressions of the same four employment shares on the slavery dummy, also without any discernible differences between neighboring municipalities with and without slaves. These results suggest that sectoral specialization has not been a channel through which the negative effects of slavery have persisted over time.

Finally, Table A2.6 explores differences in contemporary state presence, once again looking at neighbor-pair fixed effect models. We looked at a large array of variables, but the table only presents the estimation results for the per capita tax revenue in 2002, for the per capita number of municipality public employees in 1995, and for the per capita number of police posts in 1995. The estimate for slavery is negative and significant for the police posts models (see columns (7)–(9)), but it is not for any alternative state presence outcome we looked at. On the basis of these results, our interpretation is that contemporary state presence, though important as a determinant of development in the Colombian context, is not one of the main channels of persistent effects of slavery.

6. Conclusions

Slavery has been a major institution of labor coercion throughout history. It was widely used across the Americas during the colonial period, and the institution remained in place well into the 19th century. Despite the large historical literature on the causes and consequences of slavery, little research has been focused on establishing a causal link between the incidence of this extractive institution and long-run development. In this paper we undertake this task by looking at the historical experience of Colombia. One major source of demand for slave labor in Colombia during the period under Spanish colonial rule was the gold mining economy. These gold mines, nevertheless, were subject to boom and bust cycles and were rapidly depleted. More importantly, within narrowly defined gold-deposit regions, the discovery of gold mines at some point during the colonial period was to a large extent idiosyncratic.

Thus, we exploit variation in colonial gold mining status across pairs of directly adjacent municipalities, and within clusters of neighboring municipalities to identify the effect of slavery on economic performance. We argue that our strategy successfully allows us to separately identify the effect of slavery from other possible confounds. Factors such as the historical presence of the state or unobservables such as cultural differences are likely very similar within these narrowly defined gold-deposit regions. Our estimates indicate that the presence of slavery has led to increased poverty, lower public good provision and higher land inequality today. The finding of systematic differences across a wide set of development outcomes makes us confident about our findings. Moreover, by looking at similar development outcomes in the early 20th century, we also find evidence that these effects have been present for at least the last 100 years. The effects, nevertheless, appear to be stronger in more recent times.

Understanding the channels of persistence of coercive institutions such as slavery is the most important next step. In this work we were able to partially rule out some potential channels such as sectoral composition and contemporary presence of state institutions, though our understanding of these channels of influence remains imperfect.

Acknowledgments

We are grateful to Josh Angrist, María Angélica Bautista, Daniel Berkowitz, Manuel Fernández, Ángela María Fonseca, Ana María Ibañez, Carlos Prada, Pascual Restrepo, and Victoria Eugenia Soto for their help with this project, and to the participants in the Journal of Comparative Economics Conference held in Pittsburgh in October 2011 for their comments.

Appendix A. Random effects correlation structure

As a simple illustration, if the sample had two gold-mine municipalities g_1 and g_2, $\{a,b,c\}$ share a border with g_1 and $\{a,c,d,e\}$ share a border with g_2, then $M(a) = M(c) = \{g_1,g_2\}$, $M(b) = \{g_1\}$, and $M(d) = M(e) = \{g_2\}$. In this case, the Random Effects covariance matrix would take the form:

$$\mathbf{\Omega} = \begin{bmatrix} & g_1 & a & b & c & g_2 & d & e \\ g_1 & \sigma_\xi^2+\sigma_\nu^2 & \sigma_\xi^2 & \sigma_\xi^2 & \sigma_\xi^2 & 0 & 0 & 0 \\ a & \sigma_\xi^2 & 2\sigma_\xi^2+\sigma_\nu^2 & \sigma_\xi^2 & 2\sigma_\xi^2 & \sigma_\xi^2 & \sigma_\xi^2 & \sigma_\xi^2 \\ b & \sigma_\xi^2 & \sigma_\xi^2 & \sigma_\xi^2+\sigma_\nu^2 & \sigma_\xi^2 & 0 & 0 & 0 \\ c & \sigma_\xi^2 & 2\sigma_\xi^2 & \sigma_\xi^2 & 2\sigma_\xi^2+\sigma_\nu^2 & \sigma_\xi^2 & \sigma_\xi^2 & \sigma_\xi^2 \\ g_2 & 0 & \sigma_\xi^2 & 0 & \sigma_\xi^2 & \sigma_\xi^2+\sigma_\nu^2 & \sigma_\xi^2 & \sigma_\xi^2 \\ d & 0 & \sigma_\xi^2 & 0 & \sigma_\xi^2 & \sigma_\xi^2 & \sigma_\xi^2+\sigma_\nu^2 & \sigma_\xi^2 \\ e & 0 & \sigma_\xi^2 & 0 & \sigma_\xi^2 & \sigma_\xi^2 & \sigma_\xi^2 & \sigma_\xi^2+\sigma_\nu^2 \end{bmatrix}$$

The feasible random effects estimator requires prior estimation of $\mathbf{\Omega}$. Given the covariance structure of the unobservables, σ_ξ^2 and σ_ν^2 can be estimated as follows:

$$\widehat{\sigma_\xi^2} = \frac{|N|+|M|}{|N|+|M|-K}\left(\frac{\sum_{i\in N}\sum_{j\in N, j\neq i}\widehat{u_i}\,\widehat{u_j}}{\sum_{i\in N}\sum_{j\in N, j\neq i}|M(i)\cap M(j)|} + \frac{\sum_{i\in N}\sum_{g\in M(i)}\widehat{u_i}\,\widehat{u_g}}{\sum_{i\in N}M(i)}\right) \tag{10}$$

where K is the number of covariates. The term $\frac{|N|+|M|}{|N|+|M|-K}$ is a degrees-of-freedom adjustment.

Now define a to be the sum of the diagonal of $\mathbf{\Omega}$. Thus,

$$a \equiv (|N|+|M|)\sigma_v^2 + \left(|M| + \sum_{i\in N}|M(i)|\right)\sigma_\xi^2 \tag{11}$$

By defining $\hat{a}$ to be the empirical analogous of a,

$$\hat{a} = \sum_s \widehat{u_s^2} \tag{12}$$

We can use the definition of a to solve for σ_v^2:

$$\sigma_v^2 = \frac{a - \left(|M| + \sum_{i\in N}|M(i)|\right)\sigma_\xi^2}{|N|+|M|} \tag{13}$$

Replacing (10) and (12) in (13), we obtain an estimate of σ_v^2:

$$\widehat{\sigma_v^2} = \frac{|N|+|M|}{|N|+|M|-K}\,\frac{\hat{a} - \left(|M| + \sum_{i\in N}|M(i)|\right)\left(\widehat{\sigma_\xi^2}\,\frac{|N|+|M|-k}{|N|+|M|}\right)}{|N|+|M|} \tag{14}$$

Thus, the estimated variance matrix $\widehat{\Omega}$ uses Eqs. (10) and (14) in $\mathbf{\Omega}$.

Appendix B

See Tables A2.1–A2.6.

Table A2.1
Long-run effect of slavery on additional development outcomes: OLS models.

	Poverty rate 2005		Electricity coverage 2002		Aquedcut coverage 2002	
	Neighbor-pair fixed effects (1)	Random Effects (2)	Neighbor-pair fixed effects (3)	Random Effects (4)	Neighbor-pair fixed effects (5)	Random Effects (6)
Had slaves in 1843	5.117 (2.549)	2.753 (3.306)	−12.698 (3.292)	−3.713 (2.476)	−12.746 (3.413)	−6.925 (2.549)
σ_ξ^2		31.823		0.000		0.000
σ_v^2		318.51		240.47		268.98
R-squared	0.640	0.543	0.641	0.599	0.617	0.560
Geographic controls	Y	Y	Y	Y	Y	Y
Observations	352	179	352	179	352	179
	Literacy rate 1918		**Sewage coverage 1938**		**Electricity coverage 1938**	
	Neighbor-pair fixed effects (7)	Random Effects (8)	Neighbor-pair fixed effects (9)	Random Effects (10)	Neighbor-pair fixed effects (11)	Random Effects (12)
Had slaves in 1843	0.005 (0.025)	−0.025 (0.022)	0.001 (0.005)	0.000 (0.003)	−0.012 (0.008)	−0.008 (0.0057)
σ_ξ^2		0.001		0.000		0.000
σ_v^2		0.008		0.0004		0.001
R-squared	0.582	0.532	0.250	0.194	0.341	0.308
Geographic controls	Y	Y	Y	Y	Y	Y
Observations	216	111	242	123	242	123

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.
Partial R-squared for the Neighbor-Pair Fixed Effects models, which excludes the contribution of the fixed effects to the fit of the model.

Table A2.2
First stages of the iv models: slave presence on colonial gold mines presence.

	Had slaves in 1843[a]				Had slaves in 1843[b]			
	Neighbor-pair fixed effects models		Random Effects models		Neighbor-pair fixed effects models		Random Effects models	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Colonial gold mines presence	0.378	0.384	0.369	0.363	0.339	0.350	0.360	0.374
	(0.048)	(0.057)	(0.065)	(0.072)	(0.048)	(0.058)	(0.065)	(0.075)
R-squared	0.254	0.352	0.187	0.249	0.249	0.359	0.188	0.267
Geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	352	352	179	179	336	336	172	172

	Had slaves in 1843[c]				Had slaves in 1843[d]			
	Neighbor-pair fixed effects models		Random Effects models		Neighbor-pair fixed effects models		Random Effects models	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Colonial gold mines presence	0.329	0.339	0.321	0.272	0.309	0.300	0.350	0.357
	(0.058)	(0.065)	(0.084)	(0.083)	(0.056)	(0.064)	(0.079)	(0.081)
R-squared	0.299	0.383	0.224	0.292	0.260	0.351	0.170	0.237
Geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	216	216	111	111	242	242	123	123

	Had slaves in 1843[e]			
	Neighbor-pair fixed effects models		Random Effects models	
	(17)	(18)	(19)	(20)
Colonial gold mines presence	0.369	0.370	0.380	0.384
	(0.054)	(0.064)	(0.070)	(0.082)
R-squared	0.304	0.376	0.215	0.267
Geographic controls	N	Y	N	Y
Observations	248	248	129	129

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.
Partial *R*-squared for the neighbor-pair fixed effects models, which excludes the contribution of the fixed effects to the fit of the model.

[a] 1st Stages for the poverty rate 1993, poverty rate 2005, vaccination rate 2002, electricity coverage 2002, and aqueduct coverage 2002 regression samples.
[b] 1st Stage for the secondary enrollment rate 1992–2002 regressions sample.
[c] 1st Stage for the 1918 regressions sample.
[d] 1st Stage for the 1938 regressions sample.
[e] 1st Stage for the land gini 2002 regressions sample.

Table A2.3
Long-run effect of slavery on additional development outcomes: IV models.

Panel A: Second stage	Poverty rate 2005		Electricity coverage 2002		Aquedcut coverage 2002	
	Neighbor-pair fixed effects (1)	Random Effects (2)	Neighbor-pair fixed effects (3)	Random Effects (4)	Neighbor-pair fixed effects (5)	Random Effects (6)
Had slaves in 1843	5.388	7.307	−7.541	−7.179	−9.792	−11.036
	(3.301)	(5.848)	(3.510)	(5.736)	(3.714)	(6.108)
σ_ξ^2		31.823		0.000		0.000
σ_v^2		318.51		240.47		268.98
1st Stage *F*-statistic	2.217	2.345	2.217	2.345	2.217	2.345
p-Value	0.000	0.001	0.000	0.001	0.000	0.001
Geographic controls	Y	Y	Y	Y	Y	Y
Observations	352	179	352	179	352	179
	Literacy rate 1918		Sewage coverage 1938		Electricity coverage 1938	
	Neighbor-pair fixed effects (7)	Random Effects (8)	Neighbor-pair fixed effects (9)	Random Effects (10)	Neighbor-pair fixed effects (11)	Random Effects (12)
Had slaves in 1843	0.031	0.010	−0.009	−0.009	−0.022	−0.020
	(0.026)	(0.050)	(0.007)	(0.008)	(0.010)	(0.012)
σ_ξ^2		0.001		0.000		0.000
σ_v^2		0.008		0.0004		0.001
1st Stage *F*-statistic	2.458	1.852	2.286	1.492	2.286	1.492
p-Value	0.000	0.026	0.000	0.097	0.000	0.097
Geographic controls	Y	Y	Y	Y	Y	Y
Observations	216	111	242	123	242	123

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Standard errors for the Random Effects models are robust to arbitrary within-neighborhood heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.

Table A2.4
Sectoral composition of employment in gold-mine and non-gold-mine municipalities: OLS models.

Neighbor-pair fixed effects models	Employment share 2008							
	Agriculture		Mining		Manufacturing		Government	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Colonial gold mines dummy	−0.006	−0.017	0.065	0.056	−0.017	0.003	−0.049	−0.029
	(0.015)	(0.012)	(0.049)	(0.054)	(0.032)	(0.049)	(0.021)	(0.023)
Department fixed effects	N	Y	N	Y	N	Y	N	Y
Quartic on geographic controls	N	Y	N	Y	N	Y	N	Y
Observations	364	358	364	358	364	358	364	358
R-squared	0.53	0.73	0.56	0.73	0.69	0.84	0.6	0.77

Standard errors are robust to arbitrary heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
The quartic polynomials include distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.

Table A2.5
Effect of slavery intensity on employment structure: OLS models.

Neighbor-pair fixed effects models	Agriculture employment share 2008			Mining employment share 2008		
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)
Had slaves in 1843	−0.005	−0.010	−0.009	0.022	0.028	0.030
	(0.011)	(0.012)	(0.013)	(0.028)	(0.034)	(0.039)
Geographic controls	N	Y	Y	N	Y	Y
Quartic on geographic controls	N	N	Y	N	N	Y
Observations	364	364	358	364	364	358
R-squared	0.60	0.66	0.73	0.64	0.66	0.72
Effect of slavery intensity on employment structure	Manufacturing employment share 2008			Government employment share 2008		
	OLS (7)	OLS (8)	OLS (9)	OLS (10)	OLS (11)	OLS (12)
Had slaves in 1843	−0.008	−0.006	0.001	0.022	0.017	0.016
	(0.024)	(0.027)	(0.026)	(0.027)	(0.022)	(0.026)
Geographic controls	N	Y	Y	N	Y	Y
Quartic on geographic controls	N	N	Y	N	N	Y
Observations	364	364	358	364	364	358
R-squared	0.75	0.77	0.84	0.59	0.74	0.77

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.

Table A2.6
Effect of slavery intensity on state capacity outcomes: OLS models.

Neighbor-pair fixed effects models	Per capita tax collection 2002			Per capita municipality employees 1995			Per 1000 police posts 1995		
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)	OLS (9)
Had slaves in 1843	17164.8	16825.9	8602.9	−0.135	−0.288	−0.076	−0.036	−0.027	−0.024
	(5117.3)	(4957.3)	(5521.7)	(0.340)	(0.337)	(0.319)	(0.009)	(0.010)	(0.013)
Geographic controls	N	Y	Y	N	Y	Y	N	Y	Y
Quartic on geographic controls	N	N	Y	N	N	Y	N	N	Y
Observations	352	352	352	348	348	348	340	340	340
R-squared	0.65	0.74	0.80	0.67	0.72	0.77	0.74	0.76	0.81

Standard errors for the neighbor-pair fixed effects models are robust to arbitrary heteroskedasticity.
Constant and coefficients on controls not reported.
Geographic controls include: Share of cultivable land per capita, density of primary rivers, density of secondary rivers, density of tertiary rivers, distance to the department capital, millimeters of annual rainfall, elevation above sea level, land area, latitude, and longitude.
All models include department fixed-effects.

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