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Attached Once, Attached Forever: The Persistent Effects of *Concertaje* in Ecuador

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Abstract

This paper studies the long-run intergenerational effects of *concertaje*, a widespread forced labor system in the Americas from the Spanish colonial era that coerced indigenous workers in rural estates (*haciendas*) after causing them to become indebted. We collected and digitized the universe of historical individual-level tax records (1800) in what is today Ecuador and connected them to likely descendants using the universe of contemporary (2010s) tax returns and census registries via surnames. We find that descendants from *concertaje* earn 16 percent less formal labor income vis-à-vis descendants from uncoerced indigenous workers. Because of the distortions created by the institution, descendants from *concertaje* are less educated, more likely to work in agriculture and the informal sector, and less prone to migrate. However, the effects of *concertaje* on immigrants are milder, suggesting migration acted as a mitigation channel.

JEL classification

N36, O10, O43, J62

Keywords

institutions, persistence, forced labor, intergenerational mobility, Ecuador

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At the end of the year, he is in debt for more than he earns, without having touched money with his hands nor entered into his power anything worth it; the master acquires rights over his person, forces him to continue in his service until he pays the debt, and it being physically impossible for the poor Indian to do so, he remains a slave for his entire life; and contrary to all natural law and of people, the children are compelled to pay with their work an inevitable debt of their father.
Translated from *Noticias Secretas de America* (1747)
Jorge Juan y Antonio de Ulloa

“Besides, if you don’t want to buy”...
“Well ... want to ... as for wanting to,”...
“With the Indians, too?”
“Of course. You understand that the lands without the Indians are worth nothing.”
“And what Indians! All yours, all **conciertos**, very meek. You can do what you like with those people.”
“I have heard that nearly all of them are single. An unmarried Indian is only worth half. Without children, without wife or family.”
“And so what?”
“It seems you don’t understand. Who’ll take care of ...”
“I see. There are more than five hundred.... Now they are ready for ...” He was going to say, for “sale”, but the word seemed too harsh, and, so after a slight hesitation, he continued, “For work. You’ll see. The young Indians will cost you very little; they’ll be almost a gift.”
“Yes. It seems ...”
-Jorge Icaza, *Huasipungo* (1934).

1 Introduction

A central question in economics is why economic development remains elusive for many regions and individuals. While there is no single explanation, a large body of research emphasizes the role of institutions in shaping long-run development outcomes (Acemoglu et al. (2005); Caselli (2005)). Within this literature, considerable attention has been devoted to how historical institutions continue to influence contemporary economic conditions (e.g., Acemoglu et al. (2001); Dell (2010); Nunn (2008a); Banerjee and Iyer (2005)). Much of this work, however, focuses on regional or aggregate outcomes. Comparatively less is known about how historical institutions affect individuals and their descendants, and in particular about the extent to

which coercive labor institutions generate persistent intergenerational economic consequences.

This paper studies the long-run and intergenerational effects of a forced labor institution created during the Spanish colonial regime in Ecuador known as *concertaje*. Under *concertaje*, indigenous workers (*conciertos*) were tied to rural estates (*haciendas*) through debt contracts that were rarely extinguished in practice. Landlords advanced subsistence goods or cash, recorded the debt, and then extracted labor to “repay” it. Because wages were low and debts were routinely rolled over, the system bound workers to the estate for extended periods, often for life. Moreover, debt obligations were frequently inherited, so descendants could remain attached to the same estate across generations. *Concertaje* was formally abolished in 1918, but coercive labor relations persisted informally well into the twentieth century.

Although our empirical setting is Ecuador, *concertaje* reflects a broader pattern of coercive labor relations across Latin America.¹ More broadly, forced labor has historically been a common labor arrangement in many societies (Acemoglu and Wolitzky (2011); Acemoglu et al. (2021)). Despite its prevalence, empirical evidence on the long-run and intergenerational consequences of forced labor institutions remains limited, in part because individual-level historical data rarely allow researchers to link past exposure to contemporary outcomes.

We address this limitation by combining novel historical data with modern administrative records. We collect and digitize more than 2,000 pages of late-colonial tax records (1780–1830) that list taxpayers and explicitly indicate whether indigenous individuals were subject to coercive labor arrangements. These records include full names, which allows us to link historical individuals to modern data using surnames and geographic information. We match surnames and locations to the universe of individuals in the confidential 2010 Ecuadorian census and to ten years of administrative tax records (2007–2016). This linkage enables us to trace the descendants of individuals exposed to *concertaje* and to study a wide set of contemporary outcomes, including income, occupational structure, educational attainment, and geographic mobility.

Empirically, we exploit surname-based variation in historical exposure while holding ethnicity largely constant. Because most surnames in the historical records are of indigenous origin, our baseline comparisons do not contrast indigenous and Spanish surnames. Instead, we compare indigenous surnames historically associated with *concertaje* to indigenous surnames recorded in the same sources but not linked to the institution. This design leverages the fact that workers’ surnames differ from those of landlords, allowing us to trace descendant lineages without conflating worker and elite families. More broadly, surnames contain infor-

¹The *conciertos* of Ecuador are comparable to *terrazgueros* in Colombia, *inquilinos* in Chile, *yanacunas* in Peru, and *colonos* in Bolivia (Oberem (1981a)). It also resembles the indentured labor system in the Caribbean (Dippel et al. (2020)). Debt peonage through employer stores (*tienda de raya*) was common in Mexico (Villegas (2008)). Unlike institutions abolished earlier in the region, coercive labor arrangements of this kind persisted deep into the twentieth century, including in Bolivia (Choque (1997)).

mation about ancestry and have been used to study long-run persistence and intergenerational mobility (Olivetti and Paserman (2015); Clark and Cummins (2015)). Our baseline estimates therefore compare individuals with similar ethnic origins who differ in their historical exposure to coercive labor.

The intergenerational effects we document point to a mechanism through which the consequences of coercive institutions may persist. Because labor coercion often spans several generations, the inheritance of coercion status—combined with the late abolition of the institution—can generate durable differences in opportunities and outcomes. This mechanism is consistent with evidence suggesting that intergenerational mobility may be substantially lower than previously believed (Clark and Cummins (2015); Barone and Mocetti (2020)), increasing the likelihood that shocks affecting one generation shape outcomes for descendants. Intergenerational mobility also plays a central role in shaping long-run inequality (Solon (2004); Corak (2013)) and efficiency (Galor and Tsiddon (1997)), two outcomes emphasized in the literature on institutions and development (e.g., Easterly (2007); Nunn (2008a); Banerjee and Iyer (2005)). To formalize this intuition, we develop a parsimonious model that highlights the intergenerational transmission of labor coercion. The model shows how inherited labor constraints and limited economic opportunities can generate persistent disparities in human capital and income, while also allowing coercive labor institutions to shape regional conditions and migration decisions.

As exposure to *concertaje* was not randomly assigned. Estates may have relied more heavily on coercion in places with particular economic fundamentals, and families may have sorted across space in response to shocks, land scarcity, or estate expansion. To address this concern, we implement a second identification strategy based on an instrumental variable that exploits variation in agricultural labor requirements. In the Ecuadorian highlands, crop suitability varies over short distances due to agro-climatic conditions. Because crops differ in labor intensity, this variation shaped landlords' incentives to rely on coerced labor. We use spatial data from the GAEZ-FAO project on potential crop yields, which are determined by agro-climatic conditions and are therefore orthogonal to human intervention (IIASA/FAO (2012)). Specifically, we use the relative productivity of maize (labor intensive) versus potatoes (less labor intensive) as an instrument for *concertaje* intensity. Regions with a comparative advantage in maize production historically relied more heavily on coerced indigenous labor. To mitigate remaining concerns, we control flexibly for geography and overall agricultural productivity and examine alternative channels, including differential population trajectories and nutrition-driven development.

Across both strategies, we find large and persistent effects. Descendants of individuals historically subjected to *concertaje* earn substantially lower incomes than comparable individuals whose ancestors were not exposed to the institution. A 10 percentage point (pp) increase in the share of an individual's ancestors exposed to *concertaje* reduces current formal labor income by

about 1.6 percent. Evidence from census registries indicates even larger impacts when broader measures of well-being are considered. Individuals whose ancestors were entirely subject to *concertaje* are 10 pp more likely to be poor, measured using the a poverty indicator, and 6 pp less likely to work in the formal sector. Because our income estimates rely on administrative tax records and therefore capture only formal workers, these results suggest that the income effects represent a lower bound. We confirm these findings through a series of robustness checks designed primarily to address potential concerns about the accuracy of the historical linkages. Placebo regressions in which surnames are randomly reshuffled produce neither statistically nor economically significant effects. In contrast, restricting the analysis to rarer surnames—where the probability of mislinkage is lower—yields even larger estimated impacts, reinforcing the credibility of the baseline results. Our instrumental-variable framework provides additional support for the intergenerational estimates. We show that a 10 pp increase in *concertaje* intensity around 1800 (roughly half a standard deviation) increases the current incidence of extreme poverty by 4.2 pp. These estimates remain robust when controlling for initial population levels and for proxies of late-colonial economic conditions constructed from newly digitized historical sources, which further mitigates concerns about reverse causality. Importantly, we find that *concertaje* was positively correlated with (proxied) output per capita around 1800, indicating that the institution was concentrated in relatively productive districts rather than in initially underdeveloped ones. Its subsequent negative effects are therefore consistent with a reversal of fortune (Acemoglu et al. (2002)) and align with a growing body of evidence documenting the detrimental long-run consequences of labor coercion for economic development in the Americas (Nunn (2008a); Acemoglu et al. (2012); Fujiwara et al. (2017)) and in other historical contexts (Lowes and Montero (2021a); Buggle and Nafziger (2021); Nunn (2008b)).

With these two identification strategies in hand, we collect data to study channels of persistence. In particular, we disentangle these mechanisms by investigating how *concertaje* specifically and labor-coercive systems generally operated. Firstly, the intergenerational transmission of coercion gave landlords the means to retain the offspring of *conciertos* and maintain a stable labor force, forcing the children to forgo schooling in favor of early work, which meant they never became literate. As illiterate individuals could not vote, their ability to engage in public affairs was limited, reducing their opportunities to advocate for public goods. Moreover, due to landlords' ability to retain workers, *concertaje* became a barrier to labor mobility across industries and locations, likely leading to labor misallocation.

Consistently, we document both for descendants and districts an adverse effect of *concertaje* on human capital and labor mobility across sectors and regions. In line with the macrodevelopment literature (Caselli (2005)), these two forces can explain most of the observed income differences. For example, given the standard estimates of school returns, the human capital channel alone explains half of the intergenerational impact of *concertaje*, with barriers to allo-

cating labor across sectors (Hsieh et al. (2019)) or regions (Tombe and Zhu (2019)) potentially accounting for the rest. As a corollary of low migration, we document a strong assortative mating between individuals who descend from labor coercion. That is, we show that descendants from *conciertos* intermarried between them, further strengthening any intergenerational mechanism. Although the literature has studied the human capital channel, other channels have not been explored to the best of our understanding, despite the fact that limited mobility naturally induces labor market distortions. The joint study and quantification of these forces provide a novel understanding of the channels of persistence of labor coercion.

Studies investigating the historical origins of underdevelopment in the Americas have indeed stressed the negative association between labor coercion and economic development, although through different channels. Notoriously, Engerman and Sokoloff (1997) see land inequality as the first-order consequence of labor coercion and the crucial channel that explains its adverse effects. Alternatively, Dell (2010) provides evidence of a negative long-run effect of coercion from Peru’s mining *mita* but attributes a positive role to *haciendas* in economic development as providers of public goods. In contrast, the evidence in this paper suggests that these two mechanisms were absent in Ecuador.

On the one hand, the lower provision of public goods from *haciendas* was due to the state’s reliance on public labor to construct them, particularly during the 19th century. Although they had incentives to lobby for the latter, landlords were also cautious about providing *conciertos* for these public enterprises, concerned that they could lose control of their labor force. Consequently, we show that *concertaje* led to a lower historical provision of public goods (roads), with descendants of the institution living further away from them. On the other hand, the evidence in this paper suggests that historical labor coercion, instead of land inequality itself, is critical to explaining contemporary development, principally because labor coercion does not necessarily have to be accompanied by land inequality. Rather, labor coercion, usually transmitted across generations, has typically been characterized by education restraints and subsequent labor mobility restrictions, which better explain its detrimental long-term effects.

This paper contributes to several strands of literature. First, it adds individual-level evidence to the literature on the long-run effects of historical institutions on economic development (Acemoglu et al. (2001); Dell (2010); Nunn (2008a); Banerjee and Iyer (2005); Acemoglu et al. (2002)). While much of this work focuses on regional outcomes, our analysis traces persistence across families and lineages using micro data. Second, we contribute to the literature on forced labor and coercive institutions (Acemoglu and Wolitzky (2011); Acemoglu et al. (2021)), documenting persistent effects in a setting where coercion was mediated through debt and transmitted across generations. Finally, our findings speak to research on intergenerational mobility and the origins of persistent inequality (Solon (2004); Solon (2018); Aaronson and Mazumder (2008); Björklund and Jäntti (1997); Hertz (2008); Hertz (2009)).

Understanding these long-run effects is important for both historical interpretation and contemporary policy. Many societies continue to grapple with the legacy of coercive institutions that shaped inequality and opportunity. By documenting the persistent effects of *concertaje* and using complementary strategies to address endogeneity, we provide evidence that labor coercion can leave durable economic imprints that remain visible in descendants' outcomes today.

We organize this paper as follows: the next section offers historical background; section 3 describes the estimation framework and the data; sections 4 and 5 present the identification strategies and the long-run impacts of *concertaje* across generations and regions, respectively; section 6 discusses mechanisms of persistence and *un-persistence*; and section 7 concludes.

2 Historical Background

2.1 *Concertaje*

During the colonial regime in Ecuador, Spanish authorities organized the allocation of indigenous labor in the highlands primarily through the *mita*, a pre-Hispanic institution adapted to colonial rule. The *mita* was a system of forced but remunerated labor that assigned roughly one-fifth of the male indigenous population between ages 18 and 55 to specific tasks for a pre-determined period (Cushner, 1982, p. 119). Its enforcement rested with local chiefs (*caciques*), who could lose their authority or face financial penalties if they failed to supply the required workers (Cushner, 1982, p. 121). Importantly, *caciques* themselves were exempt from the *mita* tribute and therefore were not part of the pool of potential workers (Oberem, 1981a, p. 306). Because deposits of gold and silver were largely absent in the region, *mita* labor in the Ecuadorian highlands was allocated mainly to landlords engaged in agricultural and textile production.²

The rotational nature of the *mita* created strong incentives for landlords to secure a more stable labor force (Cushner, 1982, p. 128). The most important mechanism was the extension of credit to indigenous workers, which gradually evolved into the system known as *concertaje*. Once employed, workers received nominal wages, but landlords deducted a range of charges associated with alleged benefits, including access to a small plot of land, the payment of tribute obligations, and the provision of food and clothing. Because wages rarely covered the cost of these items, workers frequently accumulated debts with the *hacienda* and were required to extend their service in order to repay them. As a result, it was the exception rather than the

²Deposits of precious metals were rapidly depleted during the early colonial period (Andrien, 1995, p. 18). Because most landlords combined textile production (*obrajes*) and agricultural activities within their *haciendas* (Oberem, 1981b, p. 347), it is difficult to distinguish precisely how labor was allocated across these activities. See Perez (1947) for further discussion of *mita* arrangements in this region.

rule that *mita* workers completed their service without becoming indebted (Oberem, 1981a, p. 314).³

Landlords actively reinforced this cycle of work and debt. With the exception of tribute payments, they controlled both the valuation and provision of the benefits deducted from workers' wages. The plots of land granted to workers were typically too small to sustain subsistence, while the prices charged for food and clothing often exceeded market levels (Oberem, 1981a, pp. 314–315; Cushner, 1982, p. 134). In addition, losses incurred during activities such as shepherding could be added directly to workers' debts (Andrien, 1995, p. 123). Through these practices, debt became an effective instrument for securing a permanent labor force. Cushner (1982, pp. 123–128), for example, documents the case of an indigenous worker in a Jesuit *hacienda* who began with a debt of 27 pesos in 1701, worked for fifteen additional years, and ultimately accumulated a debt of 48 pesos.

Once attached to a *hacienda*, workers had limited opportunities to leave. Those who attempted to flee were often pursued and forcibly returned, and escape attempts frequently increased workers' indebtedness because landlords charged the costs of recapture directly to them (Oberem, 1981a, p. 316). The system also reinforced labor attachment through intergenerational debt transmission: children inherited their parents' debts and were therefore required to work on the same estates. Historical accounts report multiple generations of the same family residing and working on a single *hacienda* (Alchon, 1991, p. 85), a pattern reflected in the repeated appearance of surnames in estate records (Poloni, 2006, p. 382). This combination of restricted mobility and inherited debt effectively tied entire family lineages to the same estates for generations.⁴ Consistent with this historical evidence, we observe strong persistence of *concertaje* across families in the archival records: using microdata for the provinces of Ibarra (1783–1818) and Loja (1785–1792), the share of individuals living in *haciendas* within each surname is highly correlated across periods (Figure 1b). Together with the regional persistence shown in Figure 1a, these patterns suggest that uncoerced workers—who were primarily town residents (Tyrer, 1988, p. 326)—generally maintained their free status over time rather than transitioning into *hacienda* labor.

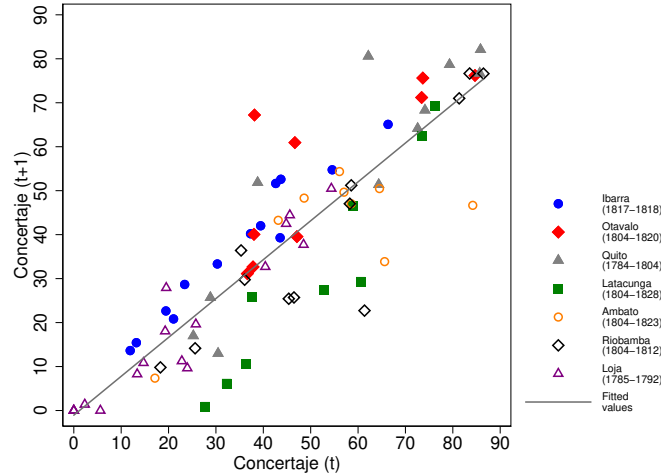
The progressive indebtedting of indigenous workers gave rise to the institution known as *concertaje*, a system of debt peonage that effectively tied workers to Spanish *haciendas*. Workers subject to this arrangement, known as *conciertos*,⁵ were not legally classified as slaves because

³For instance, wages ranged between 18 and 25 pesos, while tribute payments and the cost of food and clothing averaged approximately 5–6 and 5–10 pesos, respectively (Andrien, 1995, p. 122). Paying workers partly in goods also helped landlords address liquidity constraints, a persistent problem in the region that became more acute during the eighteenth century (Tyrer, 1988, p. 100).

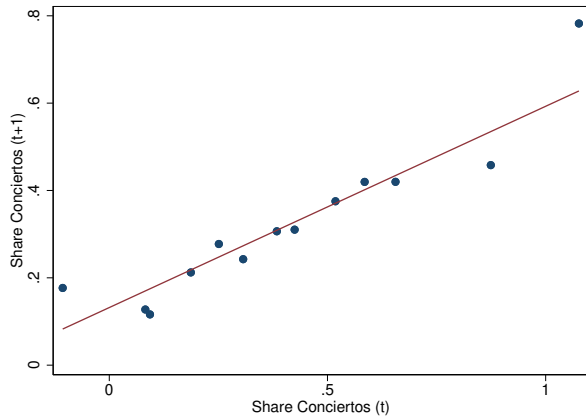
⁴*Concertaje* continued to affect children even after the formal abolition of debt inheritance in 1833, as landlords often indebted minors to secure their labor once they reached adulthood (Oberem, 1981a, p. 315).

⁵Ironically, *concierto* derives from the Spanish verb *concertar*, meaning “to agree.” In a literal sense, a *concierto* is an individual who has agreed to a contract (Oberem, 1981a, p. 309).

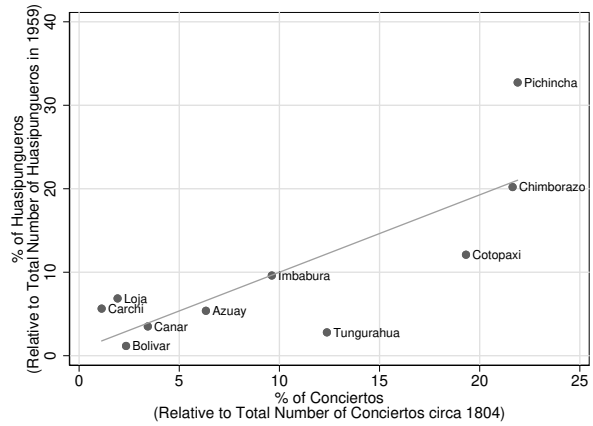
Figure 1: Persistence of *Concertaje*



(a) Across regions



(b) Across families



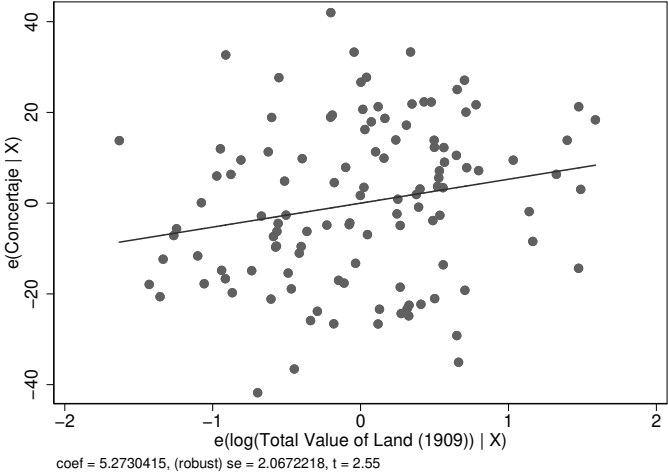
(c) In the long-run

Notes: Figure A presents the correlation between *concertaje* in period t and *concertaje* in period $t + 1$ across regions. Figure B presents a binscatter between *concertaje* in period t and *concertaje* in period $t + 1$ across families (surnames) for the colonial provinces of Ibarra and Loja, controlling for colonial province fixed effects. Figure C presents the correlation between *concertaje* in 1800 and *huasipungo* in 1959 across provinces. Data about huasipungueros come from Barsky (1984) (p.73). See appendix A for a detailed explanation of the source of *concertaje* data.

landlords did not formally own them. In practice, however, legal mechanisms such as debtor’s prison allowed landlords to retain workers who attempted to leave. Moreover, workers’ debts were treated as assets attached to the estate (Poloni, 2006, p. 377). As a result, *conciertos* were effectively transferred with the *hacienda* when properties were sold, even though landlords could not move them across estates because they lacked formal property rights over the workers themselves (Oberem, 1981a, p. 310; Baud, 2007, p. 76). By securing a permanent workforce in this way, landlords not only stabilized labor supply but also increased the economic value of their land.

To provide evidence of this facts, we assemble and digitize parish-level data on the total value of rural landholdings in 1909, when *concertaje* was still in place. These data show that land values are positively correlated with historical exposure to *concertaje* (Figure 2). The institution therefore linked the value of land to the availability of bound labor, a pattern with deep historical roots: during the colonial period, access to *mita* labor itself constituted a substantial component of a *hacienda*’s value (Oberem, 1981a, p. 307). Because indebted workers effectively represented part of landlords’ wealth, landowners had strong incentives to oppose reforms that threatened the system and frequently used their political influence to delay its abolition.⁶

Figure 2: Partial Correlation between Log(Total Value of Land (1909)) and *Concertaje* (circa 1800)



Notes: The regression controls for colonial province fixed effects. Standard errors are robust. Data about land values come from land tax records reported in Compañía Guía del Ecuador (1909) and described in appendix C. See appendix A for a detailed explanation of the source of *concertaje* data.

⁶See chapter I of Prieto (2004) for a detailed discussion of the political debate surrounding the abolition of *concertaje*.

Concertaje continued after Ecuador’s independence in 1822. In 1918 authorities sought to legally abolish it by prohibiting imprisonment for debt, but once that de jure mechanism disappeared, landlords continued to coerce rural people de facto by giving them the right to farm a small plot of land in a *hacienda* in return for labor obligations, a variation of *concertaje* known as *huasipungo* (Oberem, 1981a, pp. 321–322). As the latter was simply a renaming of the institution, unsurprisingly the distribution of *huasipungo* workers in 1959 strongly correlates with *concertaje* in 1800 (Figure 1c). Ecuador’s rural workers finally witnessed the disappearance of all traces of *concertaje* in 1964, when the agrarian reform abolished all forms of labor coercion. With the first records of *concertaje* dating from 1601 (Oberem, 1981a, p. 309); (Peñaherrera de Costales and Costales Samaniego, 1964, p. 4), this system lasted for 300–360 years and as such was one of the most, if not the most, persistent economic institutions in the Ecuadorian highlands.⁷

2.2 Labor Requirements and Coercion

Historical evidence suggests that landlords’ decisions to coerce labor were shaped largely by economic considerations (Andrien, 1995, p. 124). In particular, landlords sought to secure a stable labor force when the rotational *mita* system failed to provide sufficient workers for agricultural production. Labor demand varied substantially across crops. Maize cultivation, for instance, required intensive labor during the three to four months of plowing and tillage (Andrien, 1995, p. 124) and had a crop cycle lasting roughly ten months (Cushner, 1982, p. 73). Sugar cane, similarly, was highly labor intensive and historically associated with coercive labor arrangements. Consequently, landlords operating in areas well suited to crops with high labor requirements—such as maize or sugar cane—had stronger incentives to maintain a permanent workforce, in this case through the use of *conciertos*.

Historical accounts illustrate this pattern. The Jesuit *hacienda* of Tanalagua in San Antonio (Quito), for example, recorded both a large number of indebted workers and substantial maize production. By contrast, the Jesuit *hacienda* of Llanagua in Santa Rosa (Ambato), which specialized in crops with lower labor requirements, retained few indebted workers (Andrien, 1995, p. 124). The fact that the same owners adopted different labor arrangements across estates depending on the crops produced suggests that coercion was most attractive where labor demand was especially high.

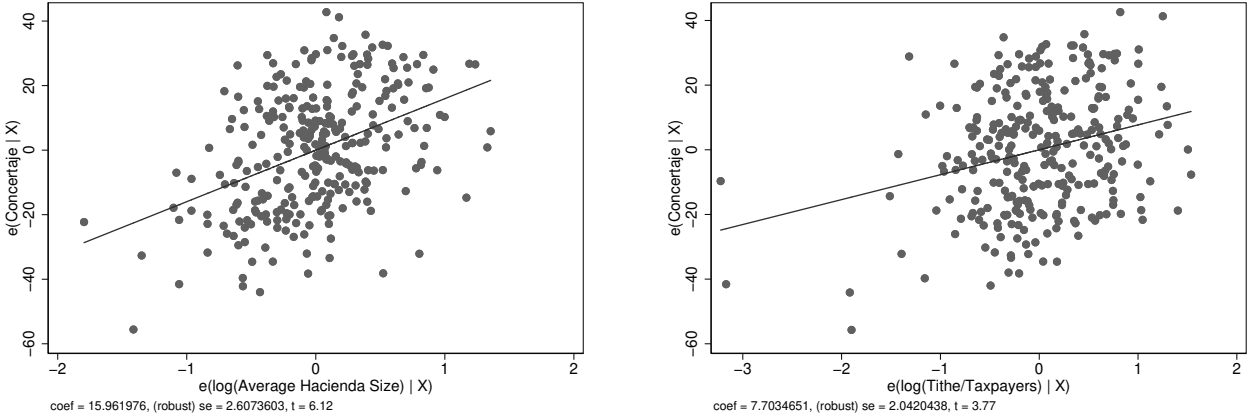
Although *concertaje* helped secure labor supply, it was not costless. Spanish agro-pastoral production in the region was largely commercial (Cushner, 1982, p. 176), and landlords had to weigh the costs of maintaining attached workers. At a minimum, they were required to provide *conciertos* with food, clothing, and the payment of their tribute obligations, all of

⁷Indeed, the historical relevance of this institution was such that, unsurprisingly, Jorge Icaza’s book *Huasipungo* (1934) is perhaps the most famous Ecuadorean novel globally speaking.

which represented a nontrivial expenditure (Andrien, 1995, p. 134). While landlords could manipulate workers’ accounts to inflate debts, they still faced the underlying cost of sustaining a permanent labor force, and failing to do so could lead to severe financial losses (Andrien, 1995, p. 108). These considerations indicate that *concertaje* emerged primarily where the economic returns to stable labor supply exceeded these costs.

To examine whether *concertaje* was indeed associated with higher production, we digitize archival tithe auction data covering 1780–1830. The tithe was a mandatory tax equal to 10 percent of output and payable to the Catholic Church. Because the church delegated its collection to private individuals who bid on towns before production occurred, tithe auctions capture expectations about a district’s future agricultural output (Andrien, 1995, p. 90).⁸

Figure 3: *Concertaje* and Productivity (1780–1830)



(a) Partial Correlation between Log(Average Hacienda Size) and *Concertaje*

(b) Partial Correlation between (Proxied) Output per Capita and *Concertaje*

Notes: Partial correlations between *concertaje* and Log(Average hacienda size) are shown in panel a and (proxied) Output per Capita in panel b. The regressions control for time (decade) and colonial province fixed effects. Standard errors clustered by district. See online appendix A for a detailed explanation of the data sources.

Figure 3b presents the partial correlation between *concertaje* and (log) tithe per capita after controlling for time (decade) and colonial province fixed effects. Because a price deflator is unavailable, time effects capture inflationary trends, while province effects account for systematic differences in tithe collection across regions (Andrien, 1995, pp. 91–92). The results indicate that parishes with greater exposure to *concertaje* tended to exhibit higher per capita tithe payments, suggesting that the institution was associated with higher agricultural production and, by extension, greater local wealth.

⁸See Ouweneel and Bijleveld (1989b), Brading (1989), Coatsworth (1989), Lindo-Fuentes (1989), and Ouweneel and Bijleveld (1989a) for a discussion of the usefulness of tithe data as a proxy for rural production in the Mexican context.

These patterns align with evidence that coercive labor systems may raise short-run productivity despite generating socially inefficient outcomes (Fogel and Engerman, 1974; Acemoglu and Wolitzky, 2011). Figure 3a also shows a positive correlation between *concertaje* and *hacienda* size, which can reflect higher agricultural productivity (Adamopoulos and Restuccia, 2014). Historical accounts likewise indicate that landlords typically controlled the most productive land (Coronel Feijoo, 1991, p. 77; Ortiz de la Tabla Ducasse, 1993, p. 193; Tyrer, 1988, p. 327). These findings suggest that coercive labor arrangements emerged in economically valuable areas rather than in marginal regions.

At the same time, the negative long-run effects of *concertaje* documented later in the paper imply that a reversal of fortune occurred between the nineteenth and twentieth centuries (Acemoglu et al., 2002). As economic activity diversified beyond agriculture, institutions designed to secure coerced labor likely became obstacles to modern economic development.

This interpretation is also consistent with the hypothesis of Engerman and Sokoloff (1997), which emphasizes the role of initial factor endowments in shaping long-run development paths. While their framework focuses primarily on plantation economies producing crops such as sugar cane, the historical evidence discussed here suggests a broader mechanism. More generally, crops that required sustained labor inputs over extended periods encouraged landlords to adopt coercive labor arrangements, a point also emphasized by Earle (1992).⁹

These considerations imply that regions with agro-climatic conditions favorable to crops requiring fewer days of labor—such as potatoes or wheat—likely relied less on *concertaje*. In contrast, areas suited to more labor-intensive crops, such as maize or sugar cane, were more likely to adopt coercive labor institutions. In Section 3.1, we exploit this variation in agricultural suitability to identify the long-run effects of *concertaje*. Because these geographic conditions primarily shaped landlords’ demand for labor, the framework also suggests that the underlying abilities of *conciertos* and free workers were broadly similar, reflecting landlords’ demand for raw labor rather than differences in worker productivity, consistent with patterns observed in other contexts of historical labor coercion (e.g., Naidu and Yuchtman, 2013; Bobonis and Morrow, 2014; Dippel et al., 2020).

3 Data and Estimation Framework

The persistence of *concertaje* may have had important implications for the long-run development of the Ecuadorian highlands. This section describes the data, presents a stylized model of how labor coercion may generate persistent effects, and outlines the empirical strategies

⁹Farms specializing in crops requiring extended labor attention were often larger. In the Chillos Valley, for example, a maize-producing region, religious orders controlled more than half of the farmland (Andrien, 1995, pp. 83–84). Large landowners could also reduce the costs of coercion by compensating workers partly through access to small plots of land.

used for the individual- and regional-level estimations. The model captures parsimoniously the potential intergenerational transmission of coercion and the regional economic distortions it may have created. More relevant, it provides a framework for interpreting the empirical results presented in the paper.

3.1 Data

3.1.1 *Concertaje*

Concertaje data come from colonial tax records. Spanish authorities collected taxes by visiting each town twice a year and reported the amount paid by every indigenous worker from 18 to 55 years old (Villegas (2008)). Each tax administration (colonial province) organized these records in books. Most of them are lost, with only a few preserved in Ecuador’s National Archive of History (Archivo Nacional de Historia). We collected at least 1 book for 9 of the 10 tax administrations that comprise today’s Ecuadorian highlands.¹⁰

These records are for different years between 1780 and 1830, the last period of the colonial administration and the first years after independence, when *concertaje* was already a consolidated system (Morelli, 2005, p. 165). Critically, each book clearly distinguishes between free workers and those living as *conciertos* in *haciendas*, as the latter were considered as such when the landlord paid their taxes (Tyrer, 1988, p. 325), avoiding confusion with temporary workers, if any. Hence, records show first of all a list of the names of free workers, and then for each *hacienda* in the town the name of the owner, the names of the indigenous workers living there, and the tax paid (although the latter did not differentiate between free and *hacienda* workers). We manually digitized all this information, accounting for over 2000 pages of tax records.

Historians such as Tyrer (1988) and Poloni (2006) have used some of these records to document regional rates of *concertaje*, although never for the totality of colonial districts, let alone to report individualized data. Oberem (1981b) is a notable exception, who reports a partial list of *concertaje* rates around 1805, but only for towns with the three highest and lowest rates in each tax administration. Unfortunately, the source of this data is lost. Thus, we included Oberem’s list in our dataset to complement regional accounts. The final database covers virtually all colonial districts (144/147) and totals more than 50,000 tributaries, giving us a complete snapshot of the labor conditions of the indigenous population at the time.

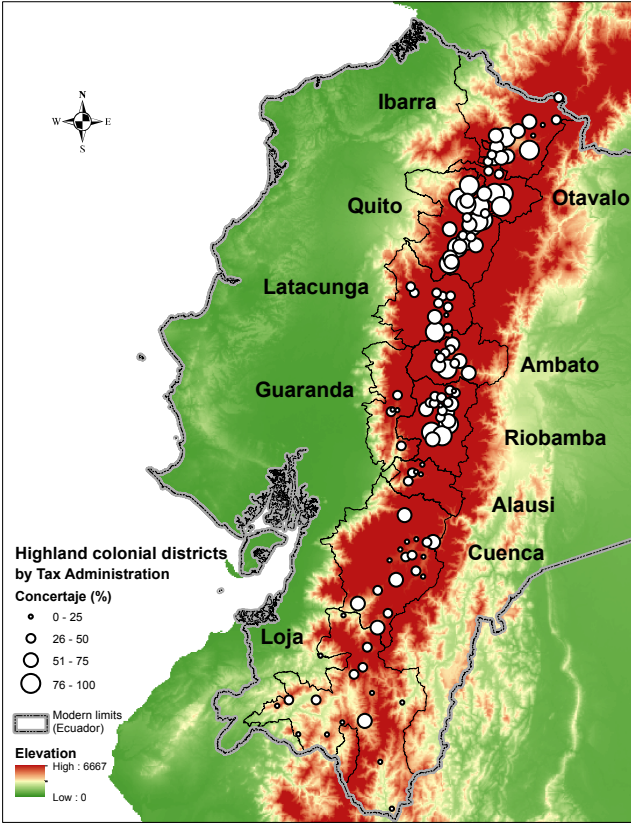
Tyrer (1988) mentions that data quality is better for earlier periods, because there were stronger incentives to know the actual number of workers and their exact location. Thanks to the availability of books for different years, we can observe whether there were dramatic differences in the number of workers and rates over time. Overall, as noted in section 2,

¹⁰Taxes were paid in “San Juan” (June) and “Navidad” (December). Women, people with disabilities, and older ones were exempted, yet in practice abuses were committed (Juan and Ulloa, 2011, pp. 255–258).

concertaje rates did not vary much, although they seemed higher for earlier years. Some differences reflect the fact that officers collected taxes seasonally and sometimes postponed their collection to the next visit. So, whenever two sources for the same province exist, we select the earliest one, following the observation of Tyrer (1988).¹¹

For regional estimations, we aggregate data by colonial district and match them with contemporary ones (parishes, the smallest political units), using several sources to reconstruct the approximate colonial territory. The fact that most modern districts have retained their colonial names facilitates this procedure. The matching process leaves a sample of 125 aggregated districts. We discuss further details in appendix A. Figure 4 shows a map of the studied region, the location of each tax administration, and the intensity of *concertaje* in each district.

Figure 4: Map of the Studied Region with its Colonial Tax Administrations



Lastly, regarding the name transcription from historical records, it is important to mention two things. First, by the time these records were created (1780–1830), Catholicism, and thus, the baptism tradition, had been taken up fully by locals. This implies all indigenous workers already had surnames. Second, these surnames are, in their majority, of indigenous origin.

¹¹See online appendix A for a detailed discussion regarding the quality of each source.

This contrasts with other contexts where labor-coerced individuals shared the surname of the coercer, such as enslaved persons in the USA. This differentiation reduces surname mismatch concerns. We provide additional details about the treatment of historical surnames in section 4.1 and appendix A.

3.1.2 Tax Records

The Ecuadorian Tax Authority (Servicio de Rentas Internas; SRI) facilitated our access to anonymized individual tax records. These correspond to the universe of employees' yearly declarations for 2007–16. Aside from income information, they include the corresponding employer's identifier and characteristics of the firms, such as location and industry. In addition, SRI provided us with a restricted list of taxpayers, which included the IDs, full names, and the corresponding province of birth of individuals. As an intermediate step, we use this data to match current and historical tax records. Posteriorly, we join these data with Civil Registry records that include demographic information. Online appendix A provides additional details.

3.1.3 Population Census Registries

The Ecuadorian Statistical Office (Instituto Nacional de Estadísticas y Censos; INEC) provided access to individual census registries. While the microdata are publicly available, INEC granted access to confidential records, which included the surnames of individuals, as the latter is recorded in the census though it is not reported because of the nature of the information. The main advantage of the census is that it covers the universe of individuals and includes a larger battery of variables but does not capture income information. Moreover, the census includes information on both the birth and residence locations, which facilitates the analysis of immigrants. Online appendix A provides additional details.

3.1.4 Regional Outcomes

These data come mainly from censuses. SIISE (2011) provides disaggregated information by parish from 1990 onward and we obtained data before 1990, particularly from 1950 to 1982, by digitizing tabulates from older censuses. Online appendixes A and B provide additional details and summary statistics, respectively. Data from other regional outcomes are described in the corresponding sections.

3.2 The Model

To formalize the intergenerational mechanism described above, we consider a simple three-period model spanning the colonial era ($t = 0$), the early republican period ($t = 1$), and the

present ($t = 2$). Let

$$\begin{aligned}
 y_{id',2} &= \alpha_{id',2} + \gamma_2 w_{d,1} + \beta_2 y_{id,1} + \epsilon_{id',2} \\
 y_{id,1} &= \alpha_{id,1} + \gamma_1 w_{d,0} + \beta_1 c_{id,0} + \epsilon_{id,1} \\
 c_{id,0} &= \alpha_{id,0} + \gamma_0 z_d + \epsilon_{id,0},
 \end{aligned}
 \tag{1}$$

where $y_{id,t}$ denotes the income (or another proxy for economic well-being) of individual i living in district d in period t , and $y_{id,t-1}$ captures the economic well-being of that individual's ancestor. The term $w_{d,t-1}$ represents local economic conditions in district d during the period in which the individual was raised, $\alpha_{id,t}$ captures unobserved individual characteristics, and $\epsilon_{id,t}$ is assumed to be i.i.d. In this framework, an individual's economic outcomes depend on parental economic status, the environment in which the individual was raised, and individual-specific attributes.

Individual characteristics evolve across generations according to the AR(1) process $\alpha_{i,t} = \rho\alpha_{i,t-1}$. The model also allows for migration: the district in which an individual currently resides (d') may differ from the district where that person was raised (d). For simplicity, we assume that migration prior to period 1 is negligible.¹²

Following the historical evidence presented above, we assume that in period 0 labor coercion was shaped by exogenous local conditions z_d , such as agro-climatic suitability for labor-intensive crops. These factors affected the probability that an individual became subject to coercive labor arrangements but were unrelated to individual ability, so $Cov(\alpha_{id,0}, z_d) = 0$. In other words, even if unobserved characteristics influenced individual labor outcomes, cross-regional variation in *concertaje* arose primarily from geographic and economic conditions rather than differences in worker ability.

Labor coercion may also have affected local economic conditions. We capture this channel by assuming $w_{d,0} = \delta \bar{c}_{d,0}$, where $\bar{c}_{d,0}$ denotes the prevalence of coercion in district d . Local conditions then evolve according to the persistence process $w_{d,t} = \lambda w_{d,t-1}$. This formulation captures the possibility that coercive institutions generated enduring regional effects. For example, Engerman and Sokoloff (1997) argue that coercive labor systems could shape long-run development through their effects on inequality and institutional structures.

Within this framework, the coercion status of an individual's ancestor, $c_{id,0}$, generates persistent effects through an intergenerational channel. This mechanism provides a simple explanation for why institutions such as *concertaje* may have long-run consequences. Historically, indigenous workers frequently transmitted their coercion status across generations through inherited debt obligations, which effectively tied families to the same estates for extended periods (so c_{id} persisted at least until 1918). More broadly, intergenerational persistence is a defining

¹²As discussed above, the mobility restrictions associated with *concertaje* likely limited migration during earlier periods.

feature of many coercive labor institutions. Given the historical context discussed above, we expect $\beta_1 < 0$, while ρ , λ , and β_2 are assumed to lie in the interval $[0, 1)$.

3.3 Individual-level estimation

As alluded, in an ideal setting one would like to know who descends from whom to estimate the long-run effects of labor coercion at an individual level. Even if that is feasible, obtaining individual-level information about the universe of workers at different periods is challenging. Instead, one can estimate the reduced form equation of the effect of descending from a *concierto* on individuals nowadays as follows:

$$y_{id',2} = \mathcal{B}_2 c_{id,0} + \mathcal{E}. \quad (2)$$

Given the system of equations and the assumptions made, it is straightforward to show that the OLS estimate of \mathcal{B}_2 is¹³

$$\mathcal{B}_2^{OLS} = \beta_1 \beta_2 + (\rho^2 + \beta_2 \rho)(\mu_{\alpha_1} - \mu_{\alpha_0}) + (\gamma_2 \lambda + \beta_2 \gamma_1) \sigma \delta, \quad (3)$$

which can be split into three parts: the direct intergenerational effect of having labor-coerced ancestors, $(\beta_1 \beta_2)$; the intergenerational transmission of abilities, $(\rho^2 + \beta_2 \rho)(\mu_{\alpha_1} - \mu_{\alpha_0})$; and a born-location effect. However, after controlling for a set of regional dummies, the latter is no longer an identification threat, so that the estimate solely captures intergenerational mechanisms, yet limits the use of any *regional* variation for identification as in section (5).

It is unclear whether individuals during colonial times self-selected into *concertaje* or if they were assigned to the system according to their abilities. On the one hand, if mainly low-ability individuals worked in *haciendas*, then $(\mu_{\alpha_1} - \mu_{\alpha_0}) < 0$, making the reduced-form estimate an *upper bound* of the actual effect of descending from a *concierto*. On the other hand, landlords could have retained high-ability individuals, in which case $(\mu_{\alpha_1} - \mu_{\alpha_0}) > 0$, leading the OLS estimate to be a *lower bound*. Although untestable, in what follows we assume that the average abilities between both groups were approximately the same, which is plausible given the discussion in section 2.2 regarding landlords' need for raw labor.¹⁴

Hence, to the extent that one can credibly link individuals across generations, the OLS estimate of (2) will capture the compound effect of intergenerational mobility attached to the coercion status of an individual's ancestors. In a more general way, if one assumes that intergenerational mobility follows a first-order autoregressive AR(1) process, such that $\beta_t = \beta$

¹³See appendix C for proof.

¹⁴To our knowledge, no empirical evidence exists about differences in abilities between coerced and free workers for any institution. The closest evidence is that of Fogel and Engerman (1974), which is related to the productivity of enslaved people in the southern United States, which they show was higher.

$\forall t > 1$, then the reduced form estimate equals $\beta_1\beta^{T-2}$, which one can think of an initial shock that perpetuates across time via an intergenerational mechanism. Moreover, note that migration is not a complication as long as (a) one can properly track descendants even if they have moved to other regions and (b) one has access to the universe of individuals to perform such tracking, as in this case.

3.4 Regional estimation

Due to data limitations, specific outcomes may only be observed aggregated geographically. Hence, if we consider the previous system of equations and take the corresponding district average, we have

$$\begin{aligned} y_{d,2}^{\tilde{}} &= a_{d,2}^{\tilde{}} + g_2 w_{d,1} + b_2 y_{i\bar{d},1}^{\tilde{}} + e_{d,2}^{\tilde{}} \\ y_{d,1}^{\bar{}} &= a_{d,1}^{\bar{}} + g_1 w_{d,0} + b_1 c_{d,0}^{\bar{}} + e_{d,1}^{\bar{}} \\ c_{d,0}^{\bar{}} &= a_{d,0}^{\bar{}} + g_0 z_{d,0} + e_{d,0}^{\bar{}}. \end{aligned} \tag{4}$$

However, as individuals can migrate, the population living nowadays in location d do not necessarily correspond to the descendants of those who lived there during colonial times ($y_{d,2}^{\tilde{}} \neq y_{d,2}^{\bar{}}$). Still, similar to above, one can estimate the reduced-form equation of the long-run effect of *concertaje* at a district level:

$$y_{d,2}^{\tilde{}} = \mathbf{B}_2 c_{d,0}^{\bar{}} + \mathbf{E}, \tag{5}$$

but instrument $c_{d,0}^{\bar{}}$ with z_d . So, the instrumental variable (IV) estimate of \mathbf{B}_2 is,

$$\mathbf{B}_2^{IV} = (g_2\lambda + g_1 b_2)\delta + b_1 b_2 \tag{6}$$

In this case, the estimate will capture any initial effects that *concertaje* may have had on other regional outcomes, even with a valid instrument. So, if $\delta \neq 0$, then \mathbf{B}_2^{IV} will include not only the effect of *concertaje* on contemporaneous poverty through its effect on historical poverty (as $b_1 b_2 = \frac{\partial y_{d,2}^{\tilde{}}}{\partial y_{d,1}^{\bar{}}} \frac{\partial y_{d,1}^{\bar{}}}{\partial c_{d,0}^{\bar{}}}$), but also its additional effects on other historical regional factors. How these extra elements interact with the latter will depend on the sign of δ . If negative, (6) will amplify the poverty-on-poverty persistence channel, but if some reversal of fortune did happen and $\delta > 0$, it will underestimate it. This certainly does not invalidate the estimator, as $b_1 b_2$ is not of unique interest, but it draws attention to how to interpret it.¹⁵

Observe also that estimates of the effect of *concertaje* on the outcomes of earlier periods capture fewer compounded effects. For example, the IV estimate of *concertaje* in the second period, $y_{d,1}^{\bar{}}$, is $\mathbf{B}_1^{IV} = g_1\delta + b_1$. This leads to several observations. First, given the autoregres-

¹⁵This differs from Casey and Klemp (2021) argument, as we observe and use an instrument for *concertaje* at $t = 0$, not at $t = 1$. Note also that we implicitly assume the rest of coefficients other than b_1 and δ are positive.

sive nature of local conditions, $w_{d,t}$, the rate of convergence between individual and regional estimates differs. Second, if $\delta < 0$ ($\delta > 0$), regional estimates of the same outcome will have larger (smaller) effects vis-à-vis individual ones. Third, if *concertaje* had effects on unobservable regional events such as the intensity of land distribution during the agrarian reform, but only in the second period (so $\delta = 0$ and $w_{d,1} = \delta'c_{d,0}$), then earlier estimates will capture its effect on $y_{d,1}$ directly. This issue is particularly relevant if we have unobserved variables through which labor coercion may have operated. Otherwise, $w_{d,t}$ can be seen as regional channels of persistence and directly estimate the (reduced-form) effect of *concertaje* on them, as other omitted variable concerns will be addressed by the instrument. Hence, we collect as much data as possible to investigate these potential channels, particularly for earlier periods.

Finally, note that even with an appropriate exogenous instrument, z_d , the estimation may suffer from a sample bias if migration was heavy. Whether the bias attenuates the estimate or not depends on the type of selection (Borjas (1987)). Although, as mentioned above the estimates for *concertaje* on development measures for earlier periods will be less prone to this issue, particularly for periods before significant migration waves happened.

On top of concerns about omitted variables, other econometric threats justify using an instrument for regional estimates. As discussed below, historical data typically come from many sources that differ in quality. This mixture of data may imply the inclusion of measurement error, leading to the well-known downward bias of the coefficient of interest.¹⁶ Second, one can hypothesize that regions that were already poor in colonial times self-select into *concertaje* (so $c_{d,0} = \delta w_{d,0}$), meaning that any association between *concertaje* and subsequent underdevelopment may not be mediated by the effect of the institution. However, reverse causality concerns seem less of an issue, given the evidence shown in section 2.2.

Thus, we address these concerns by instrumenting *concertaje* with the relative productivity of crops that differ in labor requirements. As mentioned in section 2.2, historical evidence suggests that crops largely determined the demand for labor, although this hypothesis has not been formally tested. In particular, we use information from potential crop yields, exogenously determined by local conditions, to provide consistent regional estimates of the institution's long-run effects. In section 5.1, we discuss this approach in detail.

4 Individual Estimates

We are interested in estimating Equation 2. Unfortunately, we do not know exactly which individuals today descend from those in the 1800 tax records. Hence, to overcome the challenge of linking individuals across several generations, we employ a similar empirical strategy to

¹⁶Without loss of generality, suppose that only the number of *conciertos* is potentially mismeasured. Then, $concertaje^{true} = \frac{conciertos + \mu_e}{tributaries} = concertaje^{obs.} + \mu$, with $E(\mu) = 0$, leading to classical measurement error.

several papers in the intergenerational mobility literature (e.g., Barone and Mocetti (2020)) and link individuals through surnames. Before discussing this approach in detail, we want to show how to interpret our estimates given our data limitations. First, unlike Equation 2, we do not have an indicator variable to distinguish descendants of *conciertos*. Instead, what we can do is compute the *concertaje* rate of each surname s , in location l , as the fraction of the total number of *conciertos* with surname s in location l ($conciertos_{s,l}$) relative to the total number of individuals with surname s in location l ($total_{s,l}$): $\frac{conciertos_{s,l}}{total_{s,l}}$. Since the surnames associated with *concertaje* partially overlap with those of free workers, this rate ranges from 0 to 1. Moreover, as we have data for the universe of indigenous workers in 1800, we interpret this rate as the probability of descending from a *concierto* from the paternal line. For instance, if surname A has a rate of 0.75, it implies that, in the absence of other considerations (surname mutation, for example), an individual nowadays with surname A has a 75% probability that one of his potential ancestors in 1800 was in *concertaje*, since, in principle we have the universe of workers in 1800, that is, all the potential ancestors of A from that time. This point shows why it is critical to have the full distribution of individuals rather than a subsample (e.g., Clark and Cummins (2015)). Thus, a simple way to estimate the intergenerational impact of descending from a *concierto* would be to focus only on individuals today whose surnames have a 0 or 1 *concertaje* rate, as, by definition, they either had all their potential ancestors coerced or not. In turn, this will make the estimation analogous to Equation 2. In this way, we clearly distinguish the set of *concertaje* surnames from the free-workers' one. The following graph shows those estimates.

The province (parish) column shows the estimate when using the average *concertaje* rate for surname s in province (parish) l , thereby controlling for province (parish) fixed effects and matching surnames to the corresponding geographical level. That is, we treat, for example, the surname A in location 1 differently from the same surname in location 2, acknowledging that descendants of A in location 2 may be unrelated to those in location 1 despite sharing a surname. As shown, descending from a *concierto* is associated with 5.42% lower labor income than descending from indigenous free workers.

Alternatively we can use the *concertaje* rate of every surname in our 1800 tax records and run the same regression. Figure show the binscatterplots associated with those regressions. Panel A (B) show the estimates when matching surnames by province (parish). In this case the interpretation of the coefficient changes given the continuous nature of the *concertaje* rate. In this case, increasing the. Still, one could make the extreme assumption of moving individuals from a rate of 0 to 100, which in this case implies a reduction in income of 8.42%, which is about a 3 percentage point larger effect than the previous estimates that fully separate free and *conciertos* surnames. Santavirta and Stuhler (2024) recommend against this practice

4.1 Identification

To overcome the challenge of linking individuals across several generations, we employ a similar empirical strategy as Barone and Mocetti (2020) and use a two-sample, two-stage least squares (TS2SLS) approach to matching individuals by surnames. This methodology is particularly appealing when moments needed for estimation come from two different samples (Angrist and Krueger (1992)), as in this case, where we link tax records separated by more than 200 years. Consequently, studies of intergenerational mobility have heavily relied on this approach.¹⁷

In particular, matching individuals by surname has several advantages over linking them by name (Olivetti and Paserman (2015)) or occupation (for example, Björklund and Jäntti (1997)), which are more likely to be endogenous. Instead, surnames are exogenous markers transmitted across generations, making them good instruments for intergenerational linkages (Güell et al. (2014)). Thus, the identifying assumption is that surnames predict an individual’s socioeconomic status solely through intergenerational effects. A violation of this assumption may occur if, for instance, systematic discrimination based on surnames exists, though it is worth noting that most of these surnames are of indigenous origin. Thus, the estimation captures the effect of descending from a *concierto* to the extent that, if any, discrimination by indigenous surnames does not vary across descendants from *conciertos* vis-à-vis free workers.¹⁸

Formally, we first predict the incidence of *concertaje* on each surname by estimating,

$$concierto_{i,1800} = v + \Psi' S_{i,1800} + \Upsilon' P_i + \epsilon_{i,1800} \quad (7)$$

where $concierto_{i,1800}$ is a dummy that takes the value of 1 if individual i circa 1800 lived in a *hacienda* and 0 if not, and $S_{i,1800}$ is a set of dummies for each surname s . Therefore, Ψ is a vector of coefficients associated with each surname, and so, when estimated, it predicts the fraction of individuals in the past with surname s who were part of the *concertaje* system. Put it simply, each dummy $\psi_s \in (0, 1)$ captures the share of potential ancestors in 1800 who were coerced. For example, if surname A has an estimate of 0.8 it means that 80 percent of the potential ancestors of an individual who today has the surname A were coerced. Hence, a larger fixed effect implies a larger association of that surname with *concertaje*. Lastly, P_i is the location where the worker lived, either province (the largest political unit) or parish (the smallest).

¹⁷See Inoue and Solon (2010) for the estimator’s properties. Other studies using this approach in the context of intergenerational mobility include Fortin and Lefebvre (1998) (Canada), Lefranc and Trannoy (2005) (France), Dunn (2007) (Brazil), Gong et al. (2012) (China), Piraino (2015) (South Africa), Lefranc et al. (2010) (Japan), and Cervini-Plá (2015) (Spain).

¹⁸This is different from ethnic discrimination, as many nonindigenous individuals could have indigenous surnames without identifying themselves as indigenous persons and vice-versa.

We use as a second sample current tax or census records (2010: 2007–16) and estimate

$$y_{i,2010} = \omega + \mathcal{B}_{2010} \hat{concierto}_{i,1800} + \Omega' \mathbf{P}_i + \mathcal{E}_{i,2010}, \quad (8)$$

where $\hat{concierto}_{i,1800}$ is ancestor’s i *concierto* status, as predicted by (7); $y_{i,2010}$ is a measure of current well-being, like (log) labor income or a poverty indicator, and $\mathcal{E}_{i,2010}$ is the error term. \mathcal{B}_{2010} is the TS2SLS coefficient that captures the intergenerational effect of descending from a *concierto*, the main object of interest. Note that by including location-fixed effects, the predicted *concierto* status varies by surname and location of birth. Hence, the match of pseudo-descendants becomes more accurate as the precision of links across generations is presumably higher at a regional level, although a more disaggregated regional match (for example, by parish) reduces the sample size.¹⁹ However, by including the set of location-fixed effects, we mitigate confounding regional factors, as discussed in section 3.3. So, we only consider individuals born in highland provinces (that is, the *concertaje* region) in this part of the analysis. Nonetheless, the estimation does not exclude today’s immigrants, as we match individuals by their birth location rather than where they currently live. We discuss the implications of ancestors’ migration in section 6.2.2.

We merge current tax records with demographic information from the Civil Registry. We restrict the sample to males, as historical records, by definition, only include information for them, but more critically because surnames transmit from the paternal side, so that is the one that survives over time. Moreover, we only consider nonforeign individuals from 15 to 65 years of age to focus on labor market outcomes. We deflate yearly incomes using the national CPI and compute the average real income from all the periods an individual appears in the dataset (2007–16) to reduce life-cycle income concerns (Lee and Solon (2009), Black and Devereux (2011)). We apply the same cleaning process to population census registries, as they also include demographic information. However, because the census lacks income information, we use as a measure of economic well-being a poverty index called Unsatisfied Basic Needs (Necesidades Básicas Insatisfechas), which considers a household poor (extremely poor) if one (two) or more of the five criteria are satisfied (see appendix A).

Because historical records are written in “old” Spanish, the match of surnames is flexible. In particular, we apply a cleaning process that associates different surnames with minor variations. For instance, it considers the surnames “Sanchez” and “Sanches” or “Alvares” and “Alvarez” to be the same. Moreover, we exclude current surnames with foreign sounds (for example, KT, TH) from potential matches. This approach allows us to match 95 percent of individuals from historical records, implying we find at least one descendant for almost all the tributaries in 1800. Appendix A presents additional details of the cleaning process.

¹⁹Parishes correspond to the harmonized 125 historical locations described in section 3.1.1.

Interestingly, the distribution of *conciertos* is very similar for both the 95 percent matched and the 5 percent unmatched. The overall *concertaje* rate is 42.3 percent, while for the matched it is 42 percent and for the unmatched it is 47 percent. The 5 percent difference suggests that surnames associated with *conciertos* were more prone to disappear, which suggests that, if any, the effect of descending from labor coercion will be *underestimated*, though the magnitude does not seem quantitatively important. Moreover, this match is similar for both tax records and census registries, suggesting that by considering a long data span for the formal economy (10 years in this case), one eventually captures the census distribution. For current tax records (census registries), we match 83 percent (81 percent) of individuals from eligible provinces. Taken at face value, this implies that around 80 percent of the current population in the Ecuadorian highlands descend from someone named in the historical tax records. This large overlap is critical to ensure that the TS2SLS estimates actually capture an intergenerational effect (Santavirta and Stuhler (2024)).

4.2 Results

Table 1 reports the results. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. To start, panel B presents the estimates of the first stage of the abovementioned empirical approach, namely, equation (7), for each of the different second-stage specifications (see below). It can be seen that surnames, in addition to the location fixed effects, strongly predict an individual’s *concierto* status in 1800. Depending on the specification, they explain 18–43 percent of the total variation of the *concierto* status (adjusted R-squared), reflecting the considerable intergenerational component that the institution itself had and that surnames in the past revealed a marked effect of inheritance.²⁰ Moreover, surnames are also jointly statistically significant, although the F statistic looks a priori as one of a ‘weak’ first stage. However, the fact that surnames in both samples overlap substantially, given the census nature of both, makes this less of an issue in this particular context, because the feasible TS2SLS estimator is biased toward the unfeasible OLS that truly connects individuals (Santavirta and Stuhler (2024)).

Panel A in Table 1 presents the estimates of equation (8), the second stage. Column (1) shows the baseline ones when the match is by surname and province, which suggests that on average an increase of 10 pp (about half a standard deviation) in a surname’s *concertaje* rate reduces labor income to descendants by almost 1.64 percent. Put another way, individuals with fully coerced ancestors earn 16.4 percent less than those who descend from fully uncoerced workers. Of course, this is conditional on having a surname listed in the historical records. Similarly, although the match by parish is certainly more restrictive, the estimates in column

²⁰Absent location fixed-effects, the adjusted R-squared lies between 0.14 and 0.39.

Table 1: Long-Run Effect—Descendants: Log Labor Income (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	-.00164*** (.00043)	-.00132*** (.0004)	-.00103** (.00049)	-.00106** (.00051)	-.00154*** (.00041)	-.00177*** (.0004)	-.00178*** (.00042)	-.00165*** (.0004)	-.00171*** (.00038)	-.00123*** (.00041)
Adj. R-squared	.0098	.0549	.0031	.0476	.0058	.0491	.0114	.0583	.0179	.0664
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	744504	185569	744504	185569	420891	137314	667276	152611	744504	185569
N Surnames	2362	1926	2362	1926	2127	1864	2281	1732	2362	1926
Mean Dep. Var.	8.2144	7.9917	8.2144	7.9917	8.286	8.0153	8.2	7.9735	8.2144	7.9917
Panel B: First Stage										
F Statistic	3.0461	2.5684	6.18	3.1488	2.5357	2.2316	3.9559	3.6631	4.9176	6.9731
Adj. R-squared	.1819	.2827	.2945	.3104	.2426	.2903	.2341	.3678	.2661	.4333
FE	Province	Parish	Province	Parish	Province	Parish	Province	Parish	Province	Parish
Observations	44984	44984	44984	44984	16469	31410	43752	39214	44984	44984
N Surnames	3021	3021	3021	3021	2703	2968	2922	2732	3021	3021
Mean Dep. Var.	.423	.423	.423	.423	.4269	.4497	.423	.4152	.423	.423

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

(2) continue to be both economically and statistically significant.

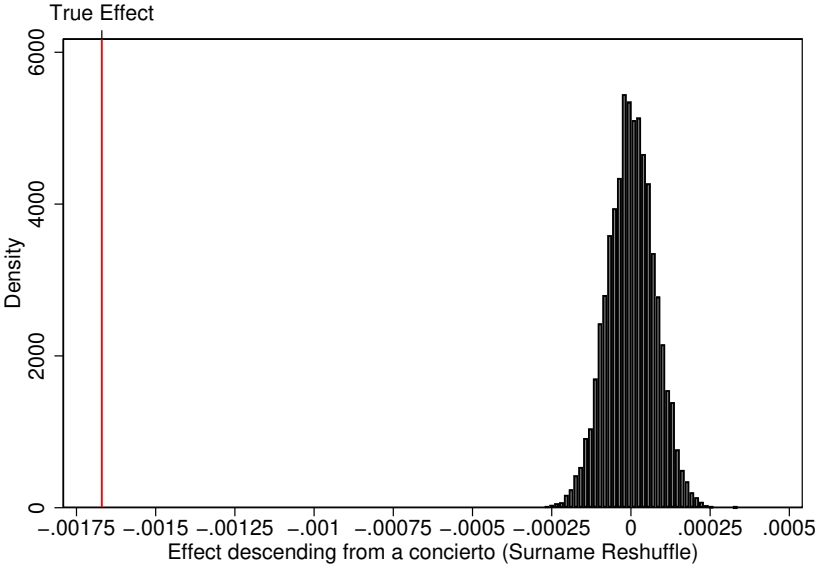
The empirical strategy described above heavily relies on the assumption that sharing a surname implies a family connection. Nevertheless, spurious relations could arise if, for instance, there are erroneous matches or individuals have migrated. Hence, we follow Barone and Mocetti (2020) and apply similar checks to assess the robustness of the results. We present them in columns (3)–(10) of Table 1. Depending on the matching procedure, the weights employed in these checks use either the colonial province or parish distribution of surnames.

Given their more informative nature, we first consider rare surnames. Columns (3) and (4) present the results when we repeat the estimation using the inverse of the relative frequency of past surnames by location as weights, and in columns (5) and (6) we only consider infrequent colonial surnames (< 10). Then, in columns (7) and (8) we use the inverse of a surname's *concertaje* rate standard deviation as weights. The logic behind this check is that incorrect matches between surnames are less troublesome if there is less variability in the coercion status. Given the dichotomous nature of the *concierto* condition, this means putting more weight on surnames with extreme *concertaje* rates. Finally, in columns (9) and (10) we weigh surnames by their local specificity, defined as the share of a surname in a given province or parish relative to the overall colonial surname distribution. Hence, if individuals have migrated, as expected, this check will mitigate incorrect linkages from surnames coming from other regions. In any case, the effect of descending from a coerced worker continues to be negative, statistically significant, and similar to the baseline case. Having larger estimates when considering rare

surnames is consistent with an attenuation bias due to a mismeasurement of family linkages.

Conversely, in Figure 5, we show the distribution of estimates when we randomly reassign the surnames of current taxpayers and match them with the historical records. If this procedure leads to results similar to those in Table 1, it certainly invalidates them. If not, it means surnames contain information about intergenerational linkages.²¹ we repeat this procedure 10,000 times, finding null effects once the linkage is broken. This result reassures that the baseline estimates reflect not some spurious relation but the intergenerational impact of labor coercion. Moreover, appendix B shows that the results are robust to the treatment of surnames.

Figure 5: Estimates when Surnames Are Randomly Reshuffled



Notes: The figure presents the distribution of estimates when we randomly reshuffle surnames and match them with the historical records. The regression is equivalent to that of column (1) in Table 1, though it uses randomly reassigned (fake) linkages (surnames). The solid line is the baseline (true) effect.

Economically, a 16.4 percent gap in labor income between descendants of *conciertos* and free workers is of substantial magnitude, given the 200 years separating both samples, even if we consider that the institution largely disappeared 100 years ago and its remnants 50 years later. Hence, if we take a generation to be 30 years, 7 have passed from the end of colonial times, and 2 to 3 after the abolition of *concertaje* and *huasipungo*, respectively, and its effects have not vanished. This result is even more striking, considering the sample only includes formal workers, making these estimates a lower bound because, on average, individuals in the

²¹Güell et al. (2014) use the same strategy to validate their intergenerational-mobility methodology, namely the informational content of surnames (ICS).

informal sector earn less (La Porta and Shleifer (2008)).

Consistently, Table 2 shows a negative effect of descending from a *concierto* on the (log) number of years an individual appears in the tax records. The idea of using this metric as a proxy for formality is that more years in the tax database reflect a larger attachment to the formal sector. The effect is of considerable magnitude, as individuals with surnames associated entirely with *concertaje* have 5.6 percent fewer years in formal employment vis-à-vis those who do not (column (1)). As a typical working life cycle spans 40 years, the estimates imply a reduction of about 2 years in formality. Similarly, Table 3 shows that the likelihood of being a formal worker, as classified in the census (that is, having access to a form of social security), is lower for descendants of *concertaje*. In this case, the estimates suggest that each 10 pp in a surname’s *concertaje* rate reduces the probability of being a formal worker by 0.62 pp to 0.77 pp (columns (1) and (2)).²²

Table 2: Long-Run Effect—Descendants: Log Years in Tax Records (2010)

	Base Model		Weight inverse Surnames’ frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje’s SD (Colony)		Weight Local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	-.00056*** (.00017)	-.00032 (.00023)	-.00033* (.00018)	-.00017 (.00025)	-.00057*** (.00016)	-.00061*** (.00023)	-.00061*** (.00017)	-.00046* (.00025)	-.00057*** (.00017)	-.00031 (.00026)
Adj. R-squared	.0104	.0481	.0058	.0418	.0078	.0435	.0117	.0503	.0152	.0568
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	744504	185569	744504	185569	420891	137314	667276	152611	744504	185569
N Surnames	2362	1926	2362	1926	2127	1864	2281	1732	2362	1926
Mean Dep. Var.	1.411	1.33	1.411	1.33	1.4344	1.335	1.4065	1.3242	1.411	1.33

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Using population census data, we find additional evidence of the adverse intergenerational effects of *concertaje*. Given that the census includes individuals regardless of their formal condition, this check also reduces concerns, if any exist, regarding the underestimation of the effects in the tax registries, as the 10-year-average sample partially addresses this issue. Because the census lacks income information, Table 4 shows the results when the dependent variable is the national poverty indicator. In this case, the estimates show once again both the statistical and the economic significance of the impact of descending from *concertaje*: an increase of 10 pp in a surname’s *concertaje* rate increases the likelihood of being poor in the

²²The smaller sample compared to that of Table 4 is because we only consider individuals currently in the labor force.

Table 3: Long-Run Effect—Descendants: Formality (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	-.0615*** (.01202)	-.07726*** (.01268)	-.04916*** (.01266)	-.0813*** (.015)	-.0573*** (.01065)	-.08767*** (.01294)	-.05967*** (.01195)	-.07202*** (.01286)	-.05037*** (.01051)	-.04966*** (.01082)
Adj. R-squared	.0508	.0769	.0364	.0704	.0436	.0753	.0539	.0797	.0662	.0863
FE	Province	Parish	Province	Parish	Province	Parish	Province	Parish	Province	Parish
Observations	752102	262208	752102	262208	426039	201926	665666	205150	752102	262208
N Surnames	2476	2153	2476	2153	2263	2105	2385	1946	2476	2153
Mean Dep. Var.	33.9253	24.2933	33.9253	24.2933	36.3676	25.4994	33.4123	22.7672	33.9253	24.2933

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

current era by around 1.1 pp. This is equivalent to the effect of a 3.54 percent GDP per capita growth contraction if we consider standard GDP to poverty semi-elasticity estimates (for example, 0.31 in Benfica and Henderson (2021)).

Table 4: Long-Run Effect—Descendants: Poverty (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	.11118*** (.02047)	.09696*** (.02145)	.08207*** (.01839)	.11668*** (.02358)	.09367*** (.01733)	.11655*** (.02131)	.10984*** (.0205)	.0853*** (.02183)	.1004*** (.01792)	.02856 (.01782)
Adj. R-squared	.073	.1294	.0518	.1264	.0619	.1304	.0767	.1282	.0959	.1261
FE	Province	Parish	Province	Parish	Province	Parish	Province	Parish	Province	Parish
Observations	968271	334705	968271	334705	547417	256707	858266	262793	968271	334705
N Surnames	2502	2175	2502	2175	2288	2126	2411	1968	2502	2175
Mean Dep. Var.	49.1099	65.2551	49.1099	65.2551	44.6586	62.7057	50.1086	67.8774	49.1099	65.2551

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Through the lens of the model, and if we assume socioeconomic status transmits through an AR(1) (as most studies do), the reduced form estimate after seven generations is $\mathcal{B}_{20\bar{1}0} = \beta_{1830} \times (\beta^5)$. Although there is no study to date of intergenerational mobility employing data on income for Ecuador, there have been some attempts to measure it using information

about education, proposing an estimate close to 0.6.²³ Therefore, if we take that number and consider $\mathcal{B}_{20\bar{1}0}$ to be 0.164 (to both map it to the model and facilitate interpretation), the results suggest that the initial *concierto* shock was indeed massive to a dynasty, generating a 215 percent initial income gap between *conciertos* and free workers.

Another answer to why we observe persistent effects of coercion after 200 years could be that intergenerational mobility was quite low or considerably lower than has traditionally been assumed in the past. For instance, if no mobility existed until the first reform of *concertaje* in 1918 ($\beta_t = 1$, $t \in 1860, 1890, 1920$), and then it started to occur at a constant rate of 0.6 per generation, then $\mathcal{B}_{20\bar{1}0} = \beta_{1830} \times (\beta^2)$, in which case *concertaje* generated a 46 percent initial income gap. This is a plausible explanation, because surnames strongly predict labor coercion in 1800, which signals a substantial weight of inheritance factors during that time. Moreover, consistent with the model, the effects are generally larger for older age groups, as seen in Figure 6.

Note that the results above only include those individuals whose surnames appear in the historical records, meaning their ancestors belonged to the lowest socioeconomic group during colonial times: indigenous workers. The estimates presented here separate the ethnicity effect from the institutional one, as most surnames are of indigenous origin. Hence, it is more plausible to interpret this lack of catch-up between descendants from coerced and uncoerced workers as the direct institutional effect rather than as reflecting differences between ethnic groups (for example, white or *mestizo* vs. indigenous people).²⁴ This result contrasts with Sacerdote (2005), who documents a catch-up between African American descendants of former enslaved persons and free persons of color in the United States. This author attributes the phenomenon to massive investments in public education, a policy that was absent in Ecuador, as discussed in section 6.1.

The results presented in this section point to an undocumented mechanism through which institutional persistence operates, namely, the intergenerational channel. This finding implies that part of the long-run effects of labor coercion reported in other contexts can be explained by the direct transmission of low opportunities across generations, including the transmission of the coercion status itself. Once a group has suffered labor coercion systematically, such as the indigenous workers in the *hacienda* system or African-descended peoples in the case of slavery (Althoff and Reichardt (2024)), their descendants carry the consequences even when the forced labor system itself has disappeared.

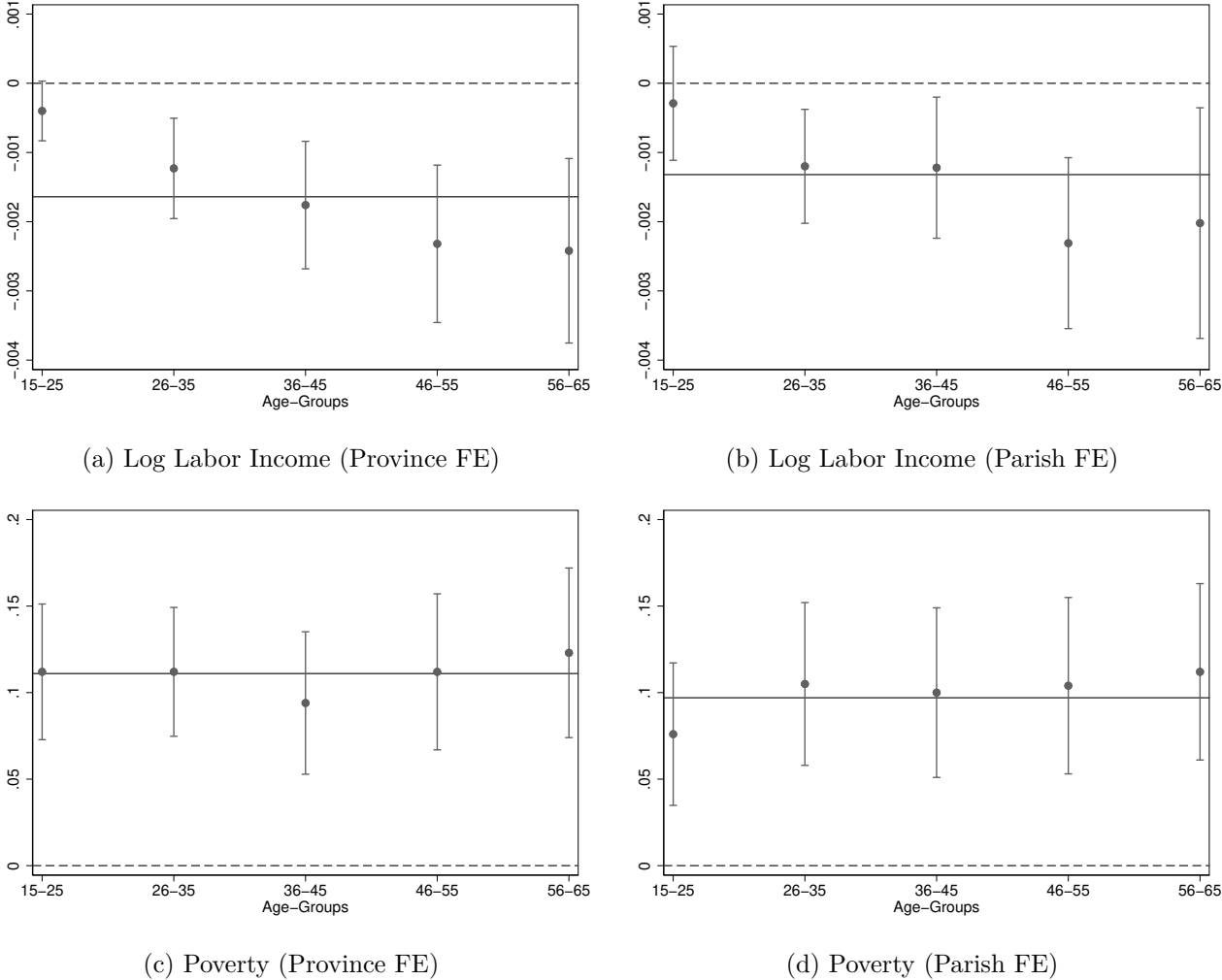
These results are consistent with the historical narrative of several scholars who have dis-

²³See Daude and Robano (2015); Brunori et al. (2013); Neidhöfer et al. (2017).

²⁴Matched workers earn in average 5 percent less than unmatched ones, controlling for province fixed effects. Although imperfect, this could be considered a raw measure of the white/*mestizo*-indigenous income gap.

cussed the adverse impacts of *haciendas* on the rural workers of Ecuador (for example, Oberem (1981a)). Moreover, they suggest that the effects of labor coercion could go beyond the specific places where the institution existed, as those effects are carried by the descendants of those who experienced it. This has relevant implications for studies focusing on long-run institutional effects using aggregate data, for example, at a regional level. In particular, intergenerational mobility can play an amplifying role, such that institutions continue to produce persistent effects beyond their limits of existence. For instance, if one sees *concertaje* as a massive human capital shock, it is unsurprising that its effects appear even in the case of immigrants, as seen in section 6.2.2.

Figure 6: Differential Effects of *Concertaje* across Age Groups



Notes: The estimates are as of those of columns (1) and (2) in Table 1 and Table 4, for different age groups, respectively. The solid line is the baseline effect. Confidence interval: 95 percent (clustered standard errors at the surname level). See section 3.1.4 for variable definition.

5 Regional Estimates

The estimates from the previous section show the quantitatively significant adverse effects of descending from a coerced worker. However, in the absence of microdata for earlier periods, one may also want to estimate the effects of *concertaje* at a regional level. This exercise becomes relevant not only due to data limitations, but also for understanding how the institution may have shaped regional factors. Thus, we provide an identification strategy to estimate such effects.

5.1 Identification

Due to the presence of the Andes and the Equatorial line, the Ecuadorian highlands have non-negligible climate variations within short distances (Basile, 1974, p. 19). Those variations translated into considerable differences in crop suitability in the colonial era. We exploit those variations to predict each town’s historical agricultural labor requirements using spatial data reported by the GAEZ-FAO project (IIASA/FAO (2012)). The GAEZ-FAO provides estimates of potential crop yields (ton per hectare) at a grid resolution of $5' \times 5'$ (approximately 100 km^2) for low, medium, and high inputs, and one can select between rain-fed and irrigation water supply sources. We chose a combination of low inputs and rain-fed water supply to replicate historical conditions²⁵.

One prominent feature of the GAEZ-FAO project is that it reports potential (rather than actual) yields based on agro-climatic conditions, which, as discussed by Galor and Ozak (2016), are arguably exogenous to human intervention. Moreover, none of these estimates of potential yields use information on actual or historical production. Instead, they are based on detailed models incorporating agro-climatic conditions to predict potential crop yields, such as temperature, radiation, and moisture regimes. Precisely because these crop suitability metrics are plausibly exogenous, they have been widely used in the literature. (Easterly (2007); Lowes and Montero (2021b); Acharya et al. (2016); Bobonis and Morrow (2014); Bustos et al. (2016))²⁶

We use these data to test whether districts with a comparative advantage in producing labor-intensive crops saw higher rates of coerced indigenous labor. Specifically, we focus on (highland) maize and (white) potatoes. Both crops, native to the Americas, represent some of the most important ones in the region (Cushner, 1982, p. 72). However, as mentioned earlier, maize has higher labor requirements than potatoes. To compute the relative productivity of both crops in a given district, we first compute the corresponding average potential yields (\bar{p}_d^i)

²⁵In appendix B, we show that the results are robust to selecting intermediate inputs.

²⁶See also Nunn and Qian (2011) and Costinot and Donaldson (2016) for further explanation of the data.

using geospatial software to take their log difference posteriorly:²⁷

$$z_d = \log(\bar{p}_d^i) - \log(\bar{p}_d^j), i \neq j, i \in \{maize\}, j \in \{potato\}. \quad (9)$$

We use this instrument to predict the historical rates of *concertaje*. However, because the endogenous explanatory variable is a fraction (workers living in *haciendas* relative to the tax population), it is possible to use the two-step IV method (Wooldridge, 2010, p. 939). Thus, we first estimate a (fractional) probit model for *concertaje*, namely,

$$concertaje_{d,1800} = \pi + g_{1800}z_d + \Pi' \mathbf{X}_d + \nu_{d,1800}, \quad (10)$$

and then estimate an IV regression of equation (11) using the predicted values of the probit model as an instrument for *concertaje*. This approach has the advantage of improving both the efficiency and precision of the 2SLS estimator and does not require the probit model to be correctly specified for consistency. Moreover, Xu (2021) shows that this procedure improves the first stage *F*-statistic, alleviating weak instrument concerns.

$$y_{d,t} = \phi + \mathbf{B}_t concertaje_{d,1800} + \Phi' \mathbf{X}_d + \nu_{d,t} \quad (11)$$

Because relative crop productivity may reflect differences in other geographic features that directly impact economic development, we control for a comprehensive set of exogenous variables. These include latitude, longitude, altitude, elevation, slope, precipitation, and temperature. Furthermore, we control for a district’s overall agricultural suitability by employing an agro-climatic index reported by the GAEZ-FAO project. Whereas the instrument exploits the *comparative* advantage of certain crops, this index controls for the *absolute* advantage in agricultural activities. Including this control further alleviates concerns regarding the instrument’s direct impact through channels other than the institution itself. These variables constitute the baseline controls, \mathbf{X}_d , in both (11) and (10).²⁸

In this case, the identification assumption is that holding geographical factors and overall agricultural suitability constant, a location’s comparative advantage in producing maize (vs. potatoes) impacts long-term economic development only through its effect on *concertaje*. The main concern with this strategy is that differences in crop suitability may be correlated with local features that affect economic development through channels other than *concertaje*. For instance, differences in crop suitability could lead to differences in caloric consumption, impacting workers’ health and in turn their productivity. However, this is not worrisome, given

²⁷See appendix A for additional technical details. The results are similar if wheat is used instead of potatoes (appendix B). To avoid problems relative to places where a crop is not suitable at all, we follow Easterly (2007) and Lowes and Montero (2021b) and add a constant (1) to the average potential yields.

²⁸Due to their skewness, we take the natural log of them. See online appendix A for extra details.

that maize and potatoes are high-calorie food sources (US Department of Agriculture (2016)). Furthermore, including the overall agricultural suitability index as a control further alleviates concerns in this direction.

Still, the health conditions of *conciertos* have been documented as precarious, in particular, due to inadequate and scarce alimentation, on top of the unsanitary life and harsh working conditions they faced (Oberem, 1981a, p. 324). However, this is more likely the consequence of labor coercion rather than the unavailability of certain crops. For instance, Suárez (1942) clearly distinguishes significant food expenditure differences between rural workers and those in *haciendas* despite their having access to the same agricultural products. This issue is particularly relevant given the pervasiveness of infant malnutrition in Ecuador, which is more prevalent in the Highlands (Gutiérrez et al. (2017)). Because low height is a reflection of malnutrition, in online appendix B, we show estimates of *concertaje* on the proportion of stunting children (Freire and Bacallao (1992)). However, we find no (statistically significant) effect, likely due to the ubiquity of maize and potatoes in addition to the historical fertile conditions of the region.

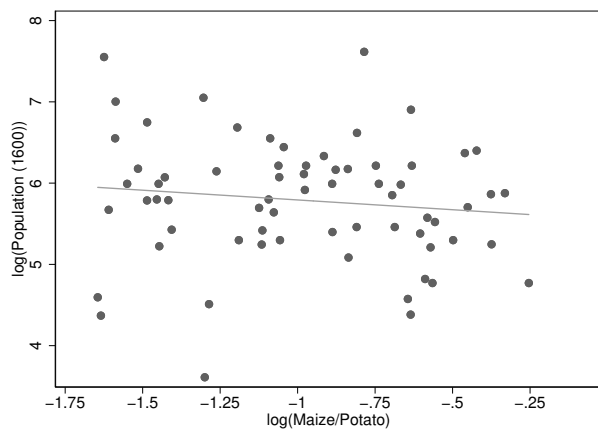
Indeed, contrary to other regions like Peru, Ecuador faced less population collapse during colonial times, which historians have attributed partly to better agricultural conditions (Tyrer, 1988, p. 76). To see if this can be traced to maize-to-potatoes productivity differences, we further collected and digitized historical population data. However, as Figure 7 shows, there is no correlation between the instrument and (log) population levels in any periods (1600, 1800, and 1950). Moreover, other initial colonial conditions (tax per capita in 1600), which could have affected economic organization later on, are also unrelated to the instrument.²⁹ This lack of population differences contrasts with the evidence presented by Nunn and Qian (2011) that the ease of potato adoption accelerated urbanization in Europe. We conjecture this did not occur in native-potato regions such as Ecuador because most crops are sowed and harvested year-round (Tyrer, 1988, p. 76), in contrast to Europe’s more seasonal agricultural production. Lastly, other channels potentially impacted by the instrument, such as gender roles via ploughing practices, seem less of a concern as maize and potatoes are both “plough-negative crops” (Alesina et al. (2013)) and so they should positively impact female labor force participation.

5.2 Results

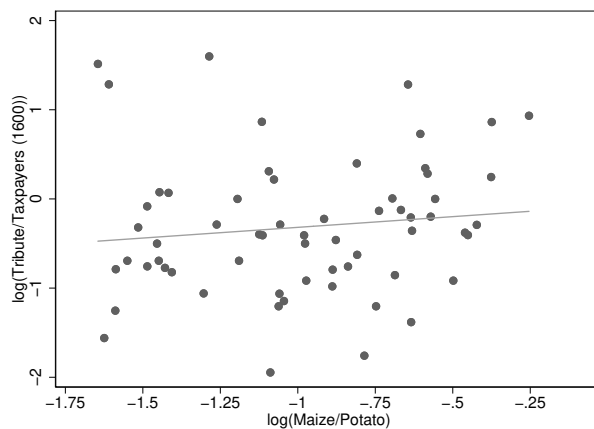
Table 5 shows the IV and OLS estimates of (11) using as a dependent variable 1990 extreme poverty rates, the earliest ones available. All columns (except (3)) include the comprehensive set of geographical controls described in section 5.1. The F -statistic, reported in panel B,

²⁹Keep in mind that early population data are of low quality (Ortiz de la Tabla Ducasse (1980)).

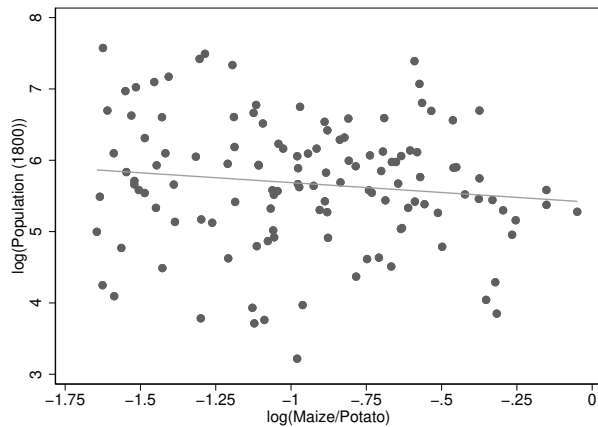
Figure 7: Relation between the Instrument and Population



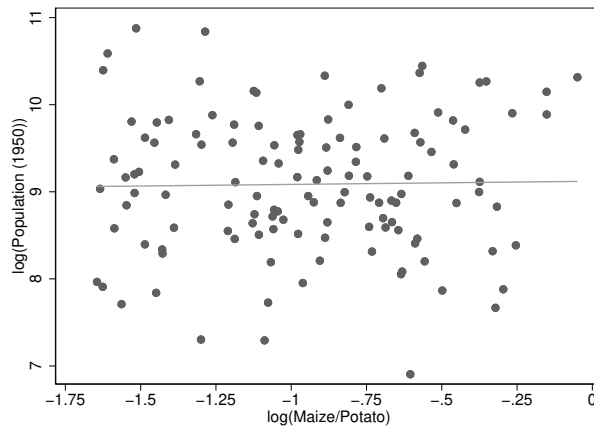
(a) Population (1600)



(b) Tax per Capita (1600)



(c) Population (1800)



(d) Population (1950)

Notes: See appendix C for a detailed explanation of the source of the population data. Maize to potato is computed as explained in Equation 9.

ranges between 11 and 15 depending on the specification, which suggests a relatively strong first stage and thus reduces weak instrument concerns. It also confirms the hypothesis that crop suitability did determine the intensity of labor coercion. Because spatial dependence naturally arises in this setting, we also report Conley (1999) standard errors on top of robust ones.³⁰

Table 5: Long-Run Effect: Extreme Poverty (1990)

	Base Model (1)	Proxy output Per Capita (Colony) (2)	Province Fixed effects (Colony) (3)	Province Capital (Colony) (4)	Fraction Indigenous (Colony) (5)	Number Taxpayers (Colony) (6)
Panel A: 2SLS						
Concertaje	.4207 (.1976)** [.1903]**	.432 (.2017)** [.2029]**	1.0085 (.2956)*** [.415]**	.3819 (.2025)* [.2111]*	.4613 (.2578)* [.273]*	.4443 (.2107)** [.2156]**
Panel B: First Stage						
Instrument	101.8123 (26.4564)***	101.6979 (26.6423)***	102.6682 (29.0584)***	101.6014 (26.9144)***	102.2855 (30.7597)***	102.1098 (27.4322)***
F Statistic (Robust)	14.8095	14.5707	12.4832	14.2505	11.0577	13.8552
Panel C: OLS						
Concertaje	.0798 (.0729) [.0812]	.0823 (.0738) [.0821]	.2271 (.0754)*** [.0747]***	.0455 (.07) [.0816]	.0718 (.076) [.0787]	.0918 (.0735) [.0843]
Geo. Controls	Yes	Yes	No	Yes	Yes	Yes
Observations	125	125	125	125	125	125
Mean Dep. Var.	64.9141	64.9141	64.9141	64.9141	64.9141	64.9141

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in Appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Overall, the estimates in Table 5 confirm, at the regional level, the negative long-run effect of *concertaje*. The IV coefficient reported in column (1) of panel A shows that *concertaje*'s impact on extreme poverty in 1990 is around 0.4. This statistically significant result suggests that on average an increase of 10 pp in the rate of *concertaje* circa 1800 (about half a standard deviation) increased extreme poverty 190 years later by 4 pp. To put it another way, a district with all its workers attached to *haciendas* around 1800 had in 1990 an extreme poverty rate 40 pp higher than one where *concertaje* was wholly absent, a considerable economic effect. To assess the robustness of this result, columns (2)–(5) show the estimates when we include additional controls to the baseline regression.

Column (2) reports the results when we control for the proxied colonial output per capita discussed in section 2.2, that is, tithe per capita, to address potential concerns of reverse causal-

³⁰Conley standard errors are computed using the *acreg* Stata package (Colella et al. (2019)) at the cutoff distance of 15 km. The latter is the one that maximizes standard errors for the 1990 extreme poverty rate.

ity. Similarly, column (4) shows estimates when we include a categorical variable as a control for whether the town was the capital of the colonial tax district.³¹ Next, columns (5) and (6) present the estimates when we add as controls the fraction of the indigenous population circa 1780 and the (log) total population, respectively, to observe whether *concertaje* is capturing the effect of the institution instead of differences in the colonial population distribution. As can be seen, the inclusion of these controls barely changes the baseline estimates.

Turning to province fixed effects, column (3) shows the estimates when we include province fixed effects to capture other time-invariant unobservable regional differences. However, we omit geographical controls to avoid overfitting the regression, as there is less geographical variation within provinces given the satellite resolution. More importantly, because tax records correspond to colonial provinces, adding fixed effects helps to control for any systematic misreporting of *concertaje* by tax administration. Interestingly, the IV and OLS estimates are the largest in this case, with the latter also turning out to be statistically significant.

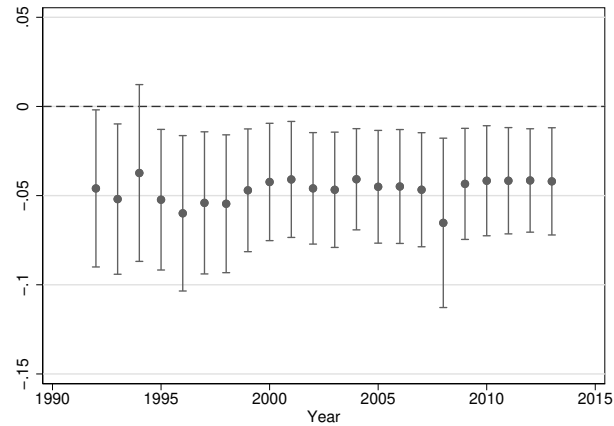
In all columns of Table 5, the IV estimates are larger than the OLS ones. As mentioned, one possible reason is the existence of (classical) measurement error in the *concertaje* data. A second explanation has to do with omitted variables. Consistently, the effects are larger when regional dummies, which capture unobserved invariant factors, are included. Also noteworthy are the large confidence intervals. Although typical in IV estimation, this is undoubtedly partly due to the small number of observations. On the other hand, it can also result from a weak first stage (Andrews et al. (2019)). Thus, to verify the robustness of the inference we show the corresponding Anderson Rubin (AR) confidence intervals of Table 5 in appendix B. While somewhat larger, they still confirm the results presented in this section.

Finally, in Figure 8 we present estimates of the effect of *concertaje* on poverty and extreme poverty for different years to see whether its effect has vanished over time. In addition, we include, as a robustness check as well, estimates of *concertaje* on average night light intensity, a proxy for economic development (Henderson et al. (2012)). Figures 8b and 8c show that the effect of *concertaje* on all of the poverty measures has decreased. Considering the recent governments' poverty alleviation policies, one can expect these efforts to be more concentrated in places with higher rates, some historically affected by *concertaje*. In contrast, Figure 8a shows that the effect on nightlight intensity displays no such trend, as this metric is not a public policy objective.

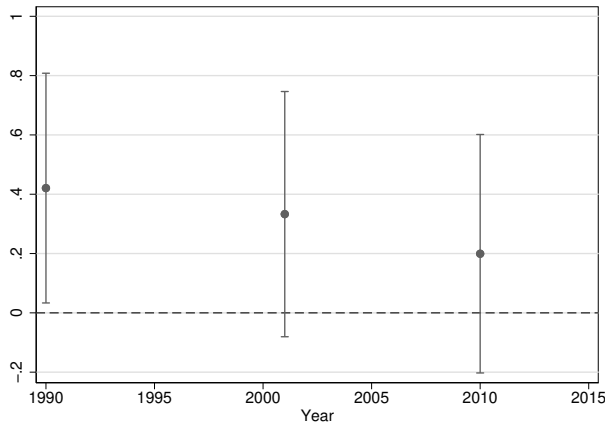
In practice both estimators are not fully comparable. Yet, the fact that we find significant effects at the individual level for the 2010 poverty index—but not at the regional level, even using the same parish-level data—serves as a cautionary note. This is especially relevant for regional institutional studies, particularly those exploring intergenerational dynamics. As

³¹The heads of *corregimientos*, according to the 1809 territorial division, as reported by Morelli (2005).

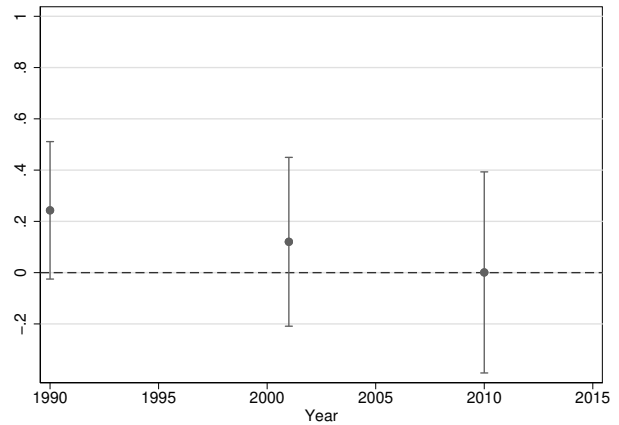
Figure 8: *Concertaje's* Effect on Long-run Economic Development for Different Years



(a) Night Light Intensity (IV)



(b) Extreme Poverty



(c) Poverty

Notes: The estimates as those of column (1) in Table 5 for different years. Confidence interval: 95 percent (robust standard errors). See section 3.1.4 for the definition of poverty and extreme poverty rates. Satellite night lights data from National Geophysical Data Center (2010).

discussed in section 3.2, regional estimates are better suited for earlier periods because confounding effects naturally arise over time. Still, the pattern of a massive historical shock that slowly converges appears in studies such as Valencia Caicedo (2018) and Iyer (2010), though as the result of different colonial interventions.

In any case, the overall negative regional effects of *concertaje* align with other regional investigations like Banerjee and Iyer (2005) study of India's landlord system, as well as with Acemoglu et al. (2012), Fujiwara et al. (2017), and Dell (2010) for other forced labor institutions in Latin America, such as slavery in Colombia and Brazil and Peru's mining *mita*, respectively. However, they differ from the latter in terms of the mechanism, as seen in the next section.

6 Mechanisms

As discussed in Section 2, *concertaje* and its subsequent variations persisted until the mid-20th century, primarily because they gave landlords the means to ensure a cheap labor supply. Furthermore, it allowed them to incorporate workers as assets in their properties. However, as discussed earlier not all individuals and regions were under the shadow of this institution in the same way. Thus, in this section we study the channels that explain how variations in the incidence of *concertaje* led to a divergence in the development paths across individuals and regions.

Our main argument is that the long-run economic effects of labor coercion are better understood if seen as a barrier to accumulating human capital and reallocating labor across sectors or regions. Indeed, both forces have important aggregate implications for economic development. Human capital differences are critical when it comes to accounting for income differences across countries (Hendricks and Schoellman (2017)) and labor misallocation are important for explaining significant differences in aggregate productivity (Hsieh and Klenow (2009)), for instance, via limited migration across regions (Tombe and Zhu (2019)) or lack of reallocation across sectors (Hsieh et al. (2019)).

However, while the adverse effects of forced labor systems on schooling have been documented (Nunn (2008a)), those on labor mobility have not, despite being key to these types of institutions. Indeed, the limiting of labor mobility to reduce workers' outside options is a central concept in theories of labor coercion (Acemoglu and Wolitzky (2011), Naidu and Yuchtman (2013), Dippel et al. (2020)). As it was profitable for landlords to maintain *concertaje* while agriculture was the main economic activity, they refused to make the required investments, such as the provision of public schooling, to develop other economic activities. At the same time, the system limited workers' mobility precisely because it would have brought about a reduction in the landlords' economic power. The limited labor mobility in turn slowed the process of structural transformation and restricted migration, two key drivers of economic growth and development (Herrendorf et al. (2014); Bryan and Morten (2019)).

Jointly, limited labor mobility and human capital affectation can explain most of the adverse effects of labor coercion. Consequently, we find quantitative significant effects of *concertaje* on human capital, sectoral composition, and migration across both regions and descendants. We also test other mechanisms of a more regional nature that the literature has discussed, such as inequality (Engerman and Sokoloff (1997)) and public goods provision (Dell (2010)). We show that the latter, proxied by the current density of roads, is negatively related to *concertaje*, which suggests landlords blocked instead of fostered infrastructure investments. Historical information about farms' road availability confirms this was the case. Instead, despite the strong link between land inequality and *concertaje*, the former alone is not associated

with lower levels of contemporary economic development. Hence, we interpret these results as a generalization of the overall adverse effects of coercion rather than of its operation through inequality.

6.1 Human capital accumulation

There is a wide consensus in the economic literature about human capital's importance in accounting for income differences across individuals or regions (Deming (2022)). However, less is known about the historical origins of human capital differences. As it was central to *concertaje* that the education of *hacienda* workers and, more critically, of their descendants be limited, it makes sense to study this channel. For instance, the wives and sons of indebted workers were also part of the *hacienda* labor force (Oberem, 1981b, p. 352). Landlords usually employed women as housekeepers, while the sons of *conciertos* started working very early, because of either inherited or newly acquired debts. The early disposition of children to work limited their education, if any was available. Accordingly, in 1934 80 percent of indigenous workers living in *haciendas* were illiterate, as opposed to 40 percent of rural workers overall (Oberem, 1981a, p. 323).

In this spirit, we test as a channel of persistence the effects of *concertaje* on human capital using several education metrics. We first report in Table 6 the regional estimates using 1962 census data, the first year that includes comprehensive education information.³² Column (1) shows that, on average, an increase of 10 pp in *concertaje* during colonial times increased the illiteracy rate of a district by 3.8 pp in 1962. We obtain the same negative relation if we use (log) years of schooling as a dependent variable, as reported in column (2). The point estimate, -0.011, means that an increase of 10 pp in *concertaje* around 1800 leads to a reduction in average years of school in 1962 of 11 percent. This effect is also quantitatively significant, given that the mean school years for the population above 6 years in a district in 1962 was around 2 ($\exp(0.63)$). Similarly, as shown in column (3), *concertaje* had a negative effect on school attendance in 1962, reducing it by almost 5 pp for each 10 pp increase in labor coercion.

While individual census registries are absent for 1962, one can estimate the impact of *concertaje* on descendants in 2010. As shown in Table 7, *concertaje* still affects descendants' education nowadays. An increase of 10 pp in a surname's *concertaje* rate decreases the years of school of an individual in 2010 by 0.95 percent (column (1)). This result is robust to the different checks presented before. Thus in the most extreme case the estimates show that descendants whose surnames are 100 percent related to *concertaje* have 9.5 percent fewer years of school than those fully unrelated. Because the average school years in the sample is 9 years, this implies a reduction of about 1 year, or 9 percent lower labor income, given standard

³²The 1950 census only includes information on illiteracy.

Table 6: Mechanisms: Human Capital

	Illiteracy (1962) (1)	Years of school (1962) (2)	Enrollment (1962) (3)
Panel A: 2SLS			
Concertaje	.3763 (.1892)** [.2547]	-.011 (.0052)** [.006]*	-.4888 (.1796)*** [.2229]**
Panel B: First Stage			
Instrument	101.5519 (24.7501)***	101.5519 (24.7501)***	101.5519 (24.7501)***
F Statistic (Robust)	16.8354	16.8354	16.8354
Panel C: OLS			
Concertaje	.0855 (.078) [.108]	-.0021 (.0023) [.0029]	-.1254 (.0708)* [.0927]
Geo. Controls	Yes	Yes	Yes
Observations	124	124	124
Mean Dep. Var.	44.3842	.6398	51.1558

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

estimates of school returns (Psacharopoulos and Patrinos (2018)). With the effect of *concertaje* on income being around -16 percent, taken at face value this channel alone could thus explain half of the long-run effects of *concertaje*.

These results stress the substantial adverse effects of labor coercion on education. Because for landlords their workers' returns on education are negligible, as their main economic activity is agriculture (Galor et al. (2009)), they had few incentives to care about their workers' schooling. Moreover, the elevated illiteracy rates observed in districts where *concertaje* was prevalent reflect the poor incentives individuals had to attend school, if any was available, and the impediment they faced due to work obligations. Wherever *haciendas* had a strong presence, children had only a few alternatives to starting work early, either because they had inherited their parents' debts or were indebted themselves soon enough.

The concerns about the low education of *conciertos* were acknowledged and historically several legal initiatives were undertaken to improve the situation. For example, in 1899 Congress passed a law that made it mandatory for *haciendas* to send all children between 10 and 14 years old to school. Furthermore, it also required *haciendas* with more than 20 *conciertos* to build a school (Oberem, 1981a, p. 324). However, landlords' power over the local authorities made complying with these mandates difficult.

Landlords also had strong incentives to limit the education of their workers as a strategy

Table 7: Long-Run Effect—Descendants: Log Years of School (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	-0.0095*** (.0002)	-0.0092*** (.00023)	-0.0069*** (.00018)	-0.012*** (.00023)	-0.0083*** (.00018)	-0.0125*** (.00021)	-0.0093*** (.00021)	-0.0084*** (.00024)	-0.008*** (.00019)	-0.0043** (.00021)
Adj. R-squared	.0519	.0833	.0406	.089	.0481	.0869	.0536	.0808	.0594	.0747
FE	Province	Parish	Province	Parish	Province	Parish	Province	Parish	Province	Parish
Observations	944475	326644	944475	326644	534451	250713	836947	256218	944475	326644
N Surnames	2502	2171	2502	2171	2288	2122	2411	1964	2502	2171
Mean Dep. Var.	2.3034	2.1519	2.3034	2.1519	2.3472	2.1783	2.2944	2.1288	2.3034	2.1519

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

to preserve *concertaje*. Because persons who were not literate had no right to vote until 1978 (Corkill (1985)), landlords likely blocked their workers' participation in public affairs by limiting their education rather than coercing them to vote for specific candidates (for example, Baland and Robinson (2008)). Hence, workers had little voice in matters like the provision of public goods, such as education infrastructure. Moreover, persons who were illiterate could be more easily retained by altering their debt accounts.

Altogether, the results in this section make a case for how colonial institutions, particularly labor-coercive ones, can explain variations in human capital within a country, a mechanism also emphasized by Mariscal and Sokoloff (2000). While positive historical interventions can have lasting effects on education in regions or among descendants (Valencia Caicedo (2018); Wantchekon et al. (2014)), these results show the other side of the coin: extractive institutions, such as *concertaje*, instead created educational barriers and distorted the incentives to invest in human capital, negatively affecting economic development.

6.2 Limited Labor Mobility

A central feature of *concertaje* and of labor coercion systems in general is the limited mobility of workers. This reality has the potential to generate substantial labor misallocation. Consistently, differences in labor mobility generate positive effects for otherwise potentially coercive employers (Méndez and Van Patten (2022)). As discussed above, landlords lacked property rights over *conciertos*, so the latter were attached to the land and a specific activity, agriculture. Thus, two natural channels to study are sectoral composition and migration.

6.2.1 Sectoral Composition

Nowadays, the more-significant disparities in productivity across countries are in the agricultural sector, with developing countries allocating more employment to agriculture than developed ones (Caselli (2005); Restuccia et al. (2008)). A common explanation of this “food problem” is that developing countries allocate much of their labor to produce food for subsistence needs because of their low productivity (Schultz (1953)). However, the allocation of workers in agriculture is also mediated by distortions in labor markets, some of them linked to historical institutional arrangements. Indeed, the mechanisms developed by *concertaje* were designed to impede labor mobilization, which might have crowded out the development of other economic sectors. To test this hypothesis, we estimate the effects of *concertaje* in sectoral composition for the year 1990, the first for which there are disaggregated sectoral data and present the results in Table 8.

Table 8: Mechanisms: Sectoral Composition

	Agriculture (1990) (1)	Manufacture (1990) (2)	Wholesale Retail (1990) (3)
Panel A: 2SLS			
Concertaje	.3688 (.2579) [.2067]*	-.2477 (.1122)** [.116]**	-.0445 (.0491) [.0341]
Panel B: First Stage			
Instrument	101.8123 (26.4564)***	101.8123 (26.4564)***	101.8123 (26.4564)***
F Statistic (Robust)	14.8095	14.8095	14.8095
Panel C: OLS			
Concertaje	.0151 (.0934) [.0793]	-.0823 (.0467)* [.0446]*	-.0169 (.0156) [.0117]
Geo. Controls	Yes	Yes	Yes
Observations	125	125	125
Mean Dep. Var.	54.7253	10.5299	5.536

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In column (1), the effect, statistically significant at 10 percent, implies that an increase of 10 pp in the rate of *concertaje* increases the share of workers allocated in agriculture by 3.7 pp. Critically, our results are not driven by natural advantages in food production, given the overall agricultural suitability control. Instead, in columns (2) and (3), we report the estimates on the share of workers in the manufacturing and service sectors, respectively. While the former

suggests that a 10 pp increase in *concertaje* reduces the share of workers in the manufacturing sector by 2.5 pp, the latter is not statistically significant, though it is negative.

We confirm these results via individual estimates using census registries data, as can be seen in table 9. As is clearly depicted in all the columns, descendants of *conciertos* are more likely to work in the primary (agricultural) sector. In this case, a 10 pp increase in a surname's *concertaje* rate increases the likelihood of working in the agricultural sector by 0.70 pp (column (1)).³³

Table 9: Long-Run Effect—Descendants: Agriculture (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight Inverse Concertaje's SD (Colony)		Weight local Surname Specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	.06985*** (.01415)	.08428*** (.01645)	.05554*** (.01578)	.09684*** (.01932)	.06048*** (.01294)	.09149*** (.01712)	.06538*** (.01497)	.07473*** (.01727)	.0583*** (.01505)	.04425*** (.01659)
Adj. R-squared	.0711	.1634	.0589	.1567	.0639	.1636	.0731	.1712	.0887	.1706
FE	Province	Parish	Province	Parish	Province	Parish	Province	Parish	Province	Parish
Observations	752102	262208	752102	262208	426039	201926	665666	205150	752102	262208
N Surnames	2476	2153	2476	2153	2263	2105	2385	1946	2476	2153
Mean Dep. Var.	21.2657	31.0124	21.2657	31.0124	19.2543	29.9119	21.7286	32.2983	21.2657	31.0124

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

These results are consistent with the slow process of industrialization in the Ecuadorian highlands (Saint Geours (1994)). Moreover, they shed light on how institutions may delay the process of structural transformation by creating barriers to the reallocation of labor. A notable anecdotal example is the case of the neighboring towns of Cayambe and Otavalo, where the minor presence of large *haciendas* and landlords in the latter contributed to the formation of indigenous trade specialists, who are renowned because of their entrepreneurial talent (Valarezo (2002)). This author shows that these differences in sectoral composition occurred as early as the mid-19th century. For instance, using census data for 1862, the author reports that Otavalo had 38 percent of its population working in the agricultural sector, while Cayambe had 82 percent.

Finally, the results presented in this section also reflect that labor coercion has historically been closely related to agriculture. Even nowadays, most forced labor relations occur in low-technology, labor-intensive activities such as agriculture (Andrees and Belser (2009)). In

³³The smaller sample compared to Table 4 is because we only consider individuals currently in the labor force.

Latin America for instance, labor coercion has historically been concentrated in agricultural and mining activities. While mines were rapidly depleted, coercion in the agricultural sector remained for a more extended period. Accordingly, it created barriers to the reallocation of labor and limited the development of other economic activities. These distortions later on had critical implications for economic development.

6.2.2 Current Migration

The mobility barriers imposed by *concertaje* suggest that migration is a critical channel to study. However, as discussed in section 3, the regional identification strategy has certain limitations. Thus, we mainly focus on the individual-level estimation. Because the census records include the locations of both birth and residence, we build a categorical variable that takes the value of one if an individual lives in the same province where he was born and zero otherwise. Table 10 presents the results when using this individual-level estimator.

Table 10: Long-Run Effect—Descendants: Migration (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	-0.05869*** (.01558)	-0.07996*** (.01762)	-0.05078*** (.01891)	-0.11015*** (.02436)	-0.05548*** (.01543)	-0.09763*** (.01876)	-0.04883*** (.01489)	-0.05903*** (.01597)	-0.02755** (.01081)	-0.01494* (.00888)
Adj. R-squared	.0704	.0804	.0971	.0969	.0893	.0864	.0635	.071	.043	.064
FE	Province	Parish	Province	Parish	Province	Parish	Province	Parish	Province	Parish
Observations	968271	334705	968271	334705	547417	256707	858266	262793	968271	334705
N Surnames	2502	2175	2502	2175	2288	2126	2411	1968	2502	2175
Mean Dep. Var.	19.2941	15.7464	19.2941	15.7464	22.8785	17.5846	17.8453	13.3485	19.2941	15.7464

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In this case, being a pseudo-descendant of a *concierto* has a detrimental effect on the probability of migration. From the first two columns, the estimates suggest that an increase of 10 pp in a surname's *concertaje* rate reduces the probability of migration by 0.6 to 0.8 pp (columns 1 and 2), implying that mobility restrictions created by the institution still exert an effect nowadays. This result holds in all the specifications.

6.2.3 Historical Migration

As with the rest of the outcomes, individual data that include both surnames and migration information for earlier periods are unavailable, thus limiting the study of historical migration.

However, INEC has an application in which one can search for the total number of individuals registered (\approx born) with a surname s in province p in year t . The source of this information is administrative data from the Civil Registry.³⁴ While a member of the public can search for a single surname, the use of the whole database is restricted. Thus, INEC provided us access through their labor dynamics laboratory.

Using this dataset, we first aggregate the total number of registries by decade to construct a panel by surname-province. Then, we compute a measure of a surname’s regional presence as follows:

$$pres_{s,p}^t = \frac{register_{s,p}^t}{\sum_p register_{s,p}^t}. \quad (12)$$

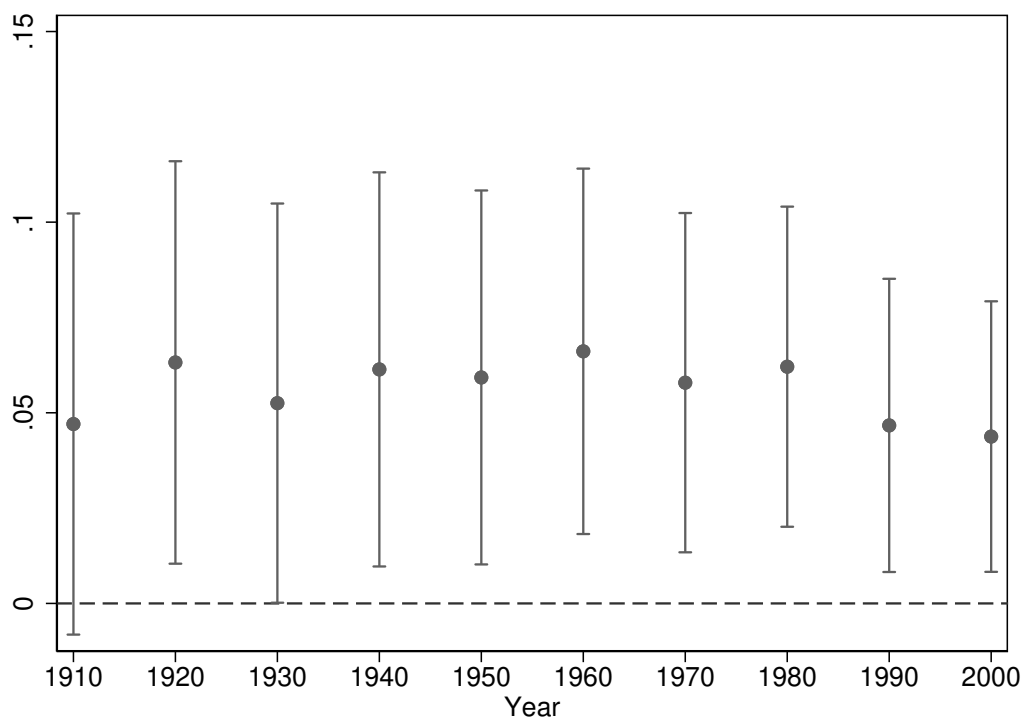
This metric is intended to capture the regional distribution of a given surname. The idea is that surnames associated with low migration are overrepresented in certain regions. For instance, take two regions and suppose the total number of individuals registered with surnames A and B in a given year are 100 and 500, respectively. If in region 1, the number of individuals with surname A is 90, while the number with surname B is 100, this means that region 1 (2) is inhabited by 90 percent (10 percent) and 20 percent (80 percent) of As and Bs, respectively. Albeit imperfect, this suggests that less migration of As has occurred in region 1, because they have a larger presence there, whereas Bs have migrated in greater numbers from region 1 as they have a more significant representation in other regions. Thus, it is expected that *concertaje* is positively associated with this measure.

The estimates presented in Figure 9 demonstrate that is the case, confirming that *concertaje* caused less migration. As for the magnitude, the 2000 estimate suggests, for example, that a 10 pp increase in a surname’s *concertaje* rate incremented by 0.5 pp the relative presence of a surname in a given province. The coefficients correspond to those of Equation 8, although note that the data are not individual, as we aggregated it by surname-province. Interestingly, in all years except 1910, the effect is statistically different from zero, showing a somewhat downward trend in the last years. The lack of a significant change after the agrarian reform (1964) can be explained by the two competing forces that the reform set in motion. As asset transfers reduce credit constraints, facilitating the migration of land recipients’ descendants, as in Colombia (Galán (2020)), the required authorization to buy, rent, and sell the granted properties during the reform surely limited migration at the same time (Barsky, 1984, pp. 338-343). Moreover, the close links of indigenous workers to their *hacienda* communities probably also disincentivized migration (Mörner (1978)).

The adverse effects on migration documented in this section stress the importance of limited mobility as an important channel of persistence, a mechanism the literature has not adequately documented yet. For instance, Dell (2010) provides evidence that during colonial times, mi-

³⁴See https://aplicaciones3.ecuadorencifras.gob.ec/VDATOS2-war/paginas/vrad/nom_ape.xhtml.

Figure 9: Estimates of *Concertaje* on Migration Proxy



Notes: Estimates from a regression equivalent to Equation 8 for different years, though aggregated at the surname-province level. Surnames correspond to their standardized version after using the algorithm described in appendix A. The regression controls for province fixed effects. The outcome is the (lack) of migration proxy described in Equation 12. Confidence interval: 95 percent (clustered standard errors at the surname level).

gration was higher in mining *mita* districts, though it should be noted this author relies on a limited sample of locations. In the same vein, Carpio and Guerrero (2021) show that the *mita* region nowadays has fewer surnames, a proxy for long-term migration and depopulation. However, they argue that this demographic change likely occurred during colonial times. In contrast, in the case of Ecuador there were no substantial early population differences associated with *concertaje*, as discussed above. Instead, the effects on migration only begin to appear in the early 20th century, with their posterior association with economic development likely arising due to labor misallocation.

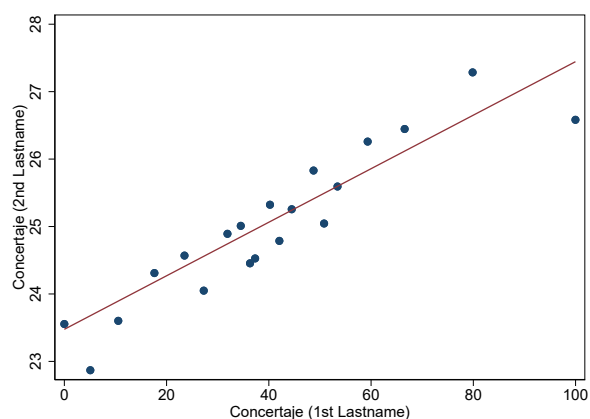
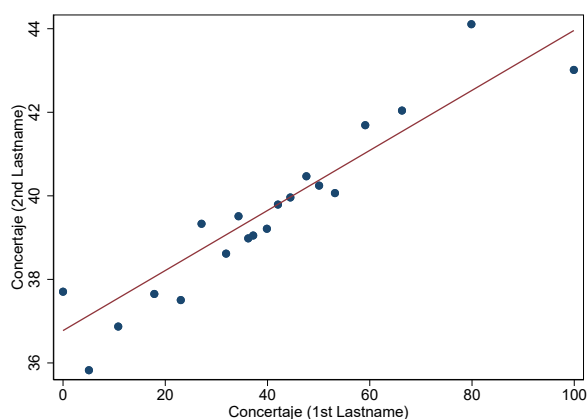
6.2.4 Assortative Mating

A corollary of the reclusion of indigenous workers in the *haciendas* is that eventually individuals married and had descendants between them. This channel naturally strengthens the intergenerational mechanism of labor coercion, with the inheritance of any effect coming from both the paternal and maternal sides. To study this channel, we take advantage of the Spanish naming convention, which, as mentioned above, dictates that the first surname is that of the father and the second that of the mother. Accordingly, we measure the degree of assortative mating between descendants of historical records by merging the incidence of *concertaje* with the second surname and then computing the correlation between them. Hence, as before our sample consists of individuals whose first surname (the one that has survived over time) matches the historical data. However, we also include the corresponding *concertaje* rate of the second surname. We present the results in Figure 10.

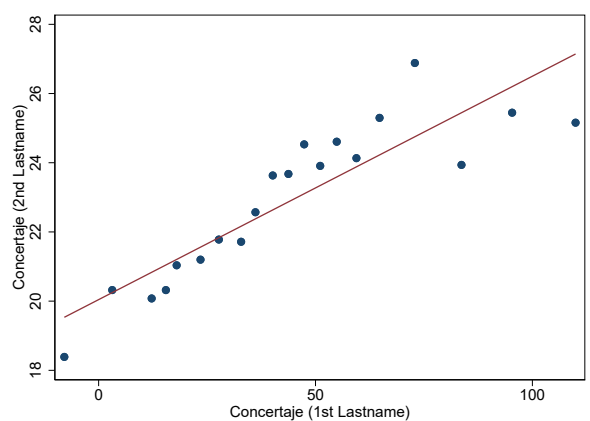
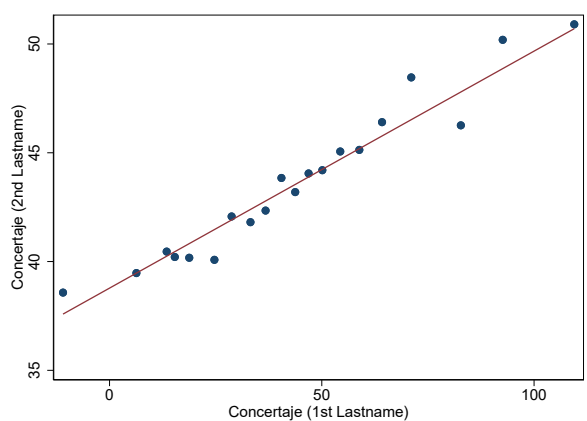
Panels (a), (b), and (c) in Figure 10 present different binscatter plots that correlate the incidence of *concertaje* between the first and the second surnames when the match is done without regional considerations, by province, and by parish, respectively. Each scatter shows correlations when controlling for fixed effects at the corresponding geographical level.³⁵ Columns to the left show the results when we focus only on the sample that matches both the first and second last name, while in the right column, we impute the incidence of *concertaje* as zero if the first surname matches with the historical data but the second does not. We do this to account for any sample selection, that is, intermarriage between potential descendants from indigenous taxpayers in historical records with those who did not appear in historical records (that is, white or *mestizo*). In any case, as can be seen there is a strong (and statistically significant) correlation between the average *concertaje* rate in each surname, suggesting an assortative mating between descendants of labor coercion. Because this result naturally holds only for recent descendants, it can be argued that these effects were stronger in earlier periods in light of our previous results.

³⁵We control for province fixed effects in the overall matching case.

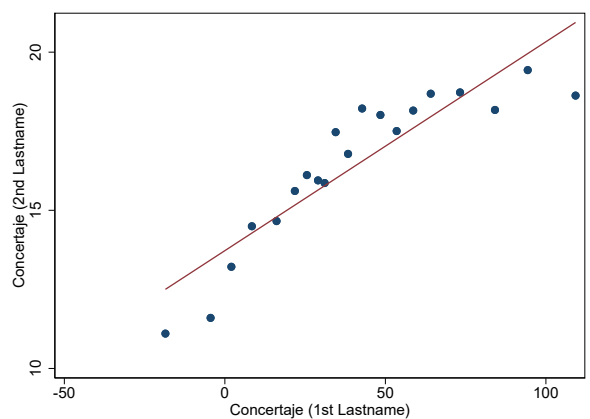
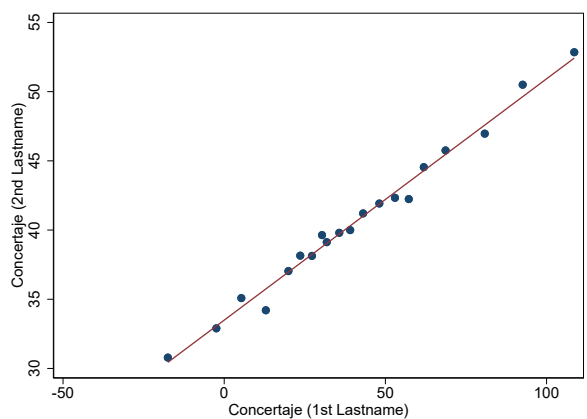
Figure 10: Assortative Mating between Descendants



(a) Overall Matching



(b) Matching by Province



(c) Matching by Parish

Notes: Panels a, b, and c show the conditional correlations in *concertaje* incidence between first and second surnames, based on matches to historical records conducted overall, by province, and by parish, respectively.

Surnames are standardized using the algorithm in Appendix A. Regressions include regional fixed effects, with province fixed effects used for the overall match. Left-column plots use only cases with *concertaje* data for both surnames, while right-column plots impute zeros for missing second surname data.

Our estimates echo Güell et al. (2014), who document a strong assortative mating in Catalonia, Spain, following a similar methodology. Nonetheless, we consider our results to be the consequence of an institutional arrangement, leading to a novel interpretation of the causes of assortative mating in contexts with segregation because of historical conditions. Interestingly, the model of Güell et al. (2014) shows how a larger assortative mating indirectly puts more weight on the standard intergenerational elasticity of socioeconomic status. In a similar spirit, it is possible that our baseline estimates, which do not consider the degree of assortative mating, need to be revised upward. Moreover, this novel channel, not previously explored by the literature, signals how low migration can perpetuate the incidence of historical events.

6.3 Inequality

Engerman and Sokoloff (1997) suggest that the colonial strategy imposed in Latin America, in which officials gave land claims and privileges to a small group of settlers, generated a profoundly unequal society. The social and economic inequality created by colonizers, they argue, posteriorly affected economic development, as this elite extracted rents from natives via forced labor or other forms of taxation in order to concentrate large amounts of wealth in their own hands. In that sense, Ecuador is no exception, as *concertaje* is a reflection of significant societal disparities in which the control of the indigenous population was in the hands of a minor, racially different elite.

To study the distributional effects of *concertaje*, we mainly focus on land inequality, as rural states heavily supported the existence of the *hacienda* system. We first compute land Gini using the 1974 agricultural census, the first one to report land distribution data at the parish level.³⁶ However, as the census was undertaken at a time when agrarian reform was just starting to take effect, it is likely that it still reflected much of the colonial heritage. Thus, we also compute land Gini for 2000 using microdata from the corresponding agricultural census (INEC (2000)). Details about the data and the construction of the Gini are provided in appendix A. Table 11 presents the results.

In column (1), both OLS and IV estimates show a positive and statistically significant relation between *concertaje* and land inequality. The IV estimates suggest that an increase of 10 pp in *concertaje* in 1800 increased land Gini in 1974 by 6.5 pp. Similarly, the coefficient's magnitudes for land inequality in 2000, shown in column (2), are close to those for 1974. The fact that land ownership conditions were almost the same 25 years after the agricultural reform reflects the persistence of *concertaje* via land inequality.

These estimates are consistent with part of the Engerman and Sokoloff (1997) hypothesis

³⁶The agricultural census of 1960 was at the province level.

Table 11: Mechanisms: Inequality

	Land Gini (1974) (1)	Land Gini (2000) (2)	Consumption Gini (2014) (3)
Panel A: 2SLS			
Concertaje	.6544 (.2172)*** [.1996]***	.5517 (.1998)*** [.2232]**	-.0804 (.0384)** [.0466]*
Panel B: First Stage			
Instrument	101.8123 (26.4564)***	101.8292 (26.6605)***	101.8123 (26.4564)***
F Statistic (Robust)	14.8095	14.5884	14.8095
Panel C: OLS			
Concertaje	.2146 (.0699)*** [.0631]***	.1986 (.0575)*** [.0733]***	-.0333 (.0128)*** [.0148]**
Geo. Controls	Yes	Yes	Yes
Observations	125	124	125
Mean Dep. Var.	75.5749	72.9364	31.8472

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

that forced labor institutions such as slavery or *concertaje* induced high levels of inequality and align with the empirical evidence presented by Nunn (2008a) about the positive correlation between slavery and inequality in the United States. However, the evidence to support the second part of the hypothesis of Engerman and Sokoloff (1997), the effect of inequality and development, is mixed.³⁷

For example, while Acemoglu et al. (2008) report a positive correlation between historical and current land inequality and contemporary economic development in Cundinamarca, Colombia, Nunn (2008a) reports that initial land inequality is uncorrelated with current income in the United States. Similarly, in appendix B, we show a null correlation between either historical (1974) or contemporary (2000) land inequality and extreme poverty rates.

However, other dimensions of inequality could be affected by labor coercion. Hence, in column (3), we report the effect of *concertaje* on the consumption Gini index. The latter are small area estimates (Elbers et al. (2003)) from the work of Molina et al. (2015). In this case, an increase of 10 pp in *concertaje* reduces consumption Gini by almost 0.8 pp, a minor effect compared to that of land inequality. Nevertheless, as shown in appendix B the consumption Gini is negatively associated with consumption poverty.

In the long run, labor coercion could affect economic development not only via land or

³⁷Banerjee and Duflo (2003) report a cross-country U-shaped relation between inequality and growth.

consumption inequality, but also through its disparate effects on political representation. For example, Acemoglu et al. (2008) emphasize the role of political, as opposed to economic, inequality in explaining differences in economic performance. This channel is relevant in the present context, considering that 46 percent of the total highland population lived on *haciendas* at the end of colonial times (Oberem (1981b)).

Although no district data exist regarding the nexus between economic and political power, historical and political analysis suggests landlords had substantial influence in local political affairs (Palomeque, 1990, pp. 155–68); (Hurtado, 1985, pp. 45–64), in particular because persons who were not literate could not vote. So, a decided minority presided over local affairs instead of the vast majority of illiterate workers linked to the *hacienda* system. Moreover, because this small elite controlled the political and economic power of the nation, it was not in their best interest to enfranchise the rest of the population, especially the indigenous portion (Quintero López (1987)).

The results presented here emphasize how it is critical to account for the origins of inequality in order to understand its effects on economic development. For example, *concertaje* was almost absent on the coast of Ecuador, yet that region exhibited land concentrations similar to the highlands.³⁸ Due to a small labor force and the risks of tropical diseases, the coast was not heavily populated during colonial times.³⁹ Agricultural activities on the coast were not of particular interest until the prices of cacao and the availability of some labor force made the region attractive new colonizers. In this case, land availability and a reduced population created a different landlord class who was more prone to wage compensation. For other historical reasons, the high land equality of the coast did not produce the adverse effects of labor coercion.

6.4 Public goods provision

Instead of attributing a negative role to the historical *hacienda* system, Dell (2010) states that in the presence of the mining *mita*, landlords in Peru protected indigenous workers by offering an escape from the cruel conditions of the Potosi silver mines. Hence, this author proposes an alternative to the theory of Engerman and Sokoloff (1997) to explain the development path in the Americas, where she emphasizes the potential positive role landlords played in providing public goods such as roads and securing property rights.

Accordingly, we test whether places with higher rates of *concertaje* and thus with a larger *hacienda* influence also show a higher presence of public goods. Table 12 presents the results. Following Dell (2010), column (1) shows the effect on (log) road density in 2010, defined as the total length of roads in a given district divided by its surface area. In this case, an increase

³⁸The land Gini for the highlands and the coast in 1979 were 0.64 and 0.62, respectively (Barsky (1984)).

³⁹Salmoral (1994) reports that 88 percent of the total population circa 1784 lived in the highlands.

of 10 pp in *concertaje* led to a decrease of 2.3 percent in road density. Because contemporary data on roads might reflect different conditions from historical roads due to recent substantial government investments in infrastructure, column (2) shows the results when we use data from the 1974 agricultural census to compute for each district the fraction of farms farther than 5 km from a passable road. However, once again the IV estimates show a negative effect. If landlords in this region produced goods to sell to local markets, why did they not invest in or lobby for roads?

Table 12: Mechanisms: Public Goods Provision

	Road density (2010) (1)	Farms farther than 5 km (1974) (2)	Public sector workers (1990) (3)
Panel A: 2SLS			
Concertaje	-.0237 (.0076)*** [.0091]***	.6033 (.2354)** [.2641]**	-.1455 (.0786)* [.0701]**
Panel B: First Stage			
Instrument	101.8123 (26.4564)***	101.8123 (26.4564)***	101.8123 (26.4564)***
F Statistic (Robust)	14.8095	14.8095	14.8095
Panel C: OLS			
Concertaje	-.0046 (.002)** [.0023]**	.0761 (.0817) [.1161]	-.0343 (.0249) [.0215]
Geo. Controls	Yes	Yes	Yes
Observations	125	125	125
Mean Dep. Var.	5.9768	19.204	7.9973

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

One explanation is that during most of the 19th century, the Ecuadorian government relied on unpaid draft labor for public works (Ackerman (1977)). Because *conciertos* were exempted from it (Palomeque, 1990, pp. 171–172), public infrastructure was likely more challenging to build in regions dominated by *haciendas*. Even when some of those regulations were removed, landlords greatly opposed the requisition of their workers by the state (Palomeque, 1990, pp. 181–182). As *conciertos* were an asset, landlords limited access to them as a labor supply, even for activities that could have benefited them (the landlords), like road construction. Hence, landlords faced a coordination problem: their private interest in retaining workers conflicted with the public interest in building infrastructure.

For instance, while constructing a road system near Otavalo, state officials complained that *haciendas* were the principal obstacle to labor recruitment (Williams, 2007, p. 51). Official

records also show governors complaining about landlords' requesting infrastructure but denying the participation of *conciertos* in public works (Palomeque, 1990, p. 184). Despite the fact that more infrastructure was in their best interests, landlords seemed most concerned about retaining their workers. The temporary assignment of workers to road construction implied more chances to escape and was certainly a diversion from agricultural duties. It is thus unclear whether any significant provision of public goods could exist in districts dominated by *haciendas*. Moreover, labor abundance allowed landlords to transport objects without the need to invest in transportation, as exemplified in 1925 when 3000 indigenous workers were mobilized to move some electrical equipment a distance of 150 km (Albornoz, 1971, p. 80).

The significant levels of labor coercion may have also affected the state's capacity. Notably, column (3) shows that *concertaje* is associated with a lower share of workers in the public sector. Hence, providing public goods may have been more challenging in places with a lower state presence, especially when we consider the large number of people living on *haciendas* who were illiterate and as such could not vote and had few chances to demand an active state role. On the contrary, it was in the landlord's best interest to limit the state's capacity and government workers' presence to exert more influence over local affairs and avoid unwanted forms of regulation. Hence, *concertaje* did not necessarily incentivize landlords to promote a more significant state presence and a higher provision of public goods—on the contrary, it discouraged them from doing so.

Because public goods vary within a district, the individual-level estimates can provide further evidence if descendants have different levels of access to them. To generate such estimates, we use census data and construct a dummy variable that takes the value of one if the individual's dwelling has direct access to a paved road and zero otherwise. As seen in Table 13, the effect of *concertaje* on this proxy for public goods' access is statistically and economically significant: a 10 pp increase in a surname's *concertaje* rate increases the likelihood of descendants' living in a dwelling with a paved road by 0.94 to 1.22 pp (columns (2) and (1)). These estimates show not only that regions with more *concertaje* had a lower provision of public goods, but also that the *hacienda* system displaced *conciertos* within those regions to locations with lower access to them. These results confirm the historical observation that landlords allocated their *conciertos* in remote sites, far from the central location of their *haciendas* (Poloni, 2006, p. 385). Consistently during the agrarian reform there were plenty of complaints that the land assigned to its beneficiaries was of lower quality ((Barsky, 1984, p. 65, 71)), as it was located where *conciertos* historically lived.

Table 13: Long-Run Effect—Descendants: Access to Paved Road (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	-.12163*** (.02189)	-.09388*** (.021)	-.09353*** (.01959)	-.12248*** (.02306)	-.10143*** (.01835)	-.11051*** (.02166)	-.11859*** (.02153)	-.07401*** (.02119)	-.10943*** (.01901)	-.01661 (.01703)
Adj. R-squared	.0732	.0992	.0589	.1037	.0703	.1047	.0767	.0973	.0852	.0821
FE	Province	Parish	Province	Parish	Province	Parish	Province	Parish	Province	Parish
Observations	968271	334705	968271	334705	547417	256707	858266	262793	968271	334705
N Surnames	2502	2175	2502	2175	2288	2126	2411	1968	2502	2175
Mean Dep. Var.	49.1997	32.4402	49.1997	32.4402	53.7755	34.7244	48.2028	29.9867	49.1997	32.4402

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.5 A Channel of Unpersistence

A large body of research has strongly emphasized the lasting impact of historical institutions. However, less is known about the channels that could mitigate their persistent effects. More particularly, is there any mechanism that could counteract the intergenerational persistence of labor-coercive institutions? A natural force to explore in that direction is migration, not only due to the potential economic gains it offers (Lagakos et al. (2020)), but also because the distortions *concertaje* created, such as limiting the supply of schooling or slowing structural transformation, were intrinsically regional. In this Section, we study the role of migration as a potential channel of unpersistence.

We first test whether recently migrated individuals experience different effects. Although *concertaje* reduced migration, descendants of coerced workers likely moved more recently, given the less-restrictive mobility mechanisms in play nowadays. Hence, we reestimate regression (8), but interact \mathcal{B}_{2010} with the migration categorical variable defined in section 6.2.2. Table 14 shows the results. In general, recently migrated individuals have a lower effect than those who stayed. The difference can be large enough that the effect is reduced by more than half, as seen in column (1), where descendants from *concertaje* who migrated have a 4.6 percent (11.2-6.6) likelihood of being poor and descendants who stayed have an 11.2 percent likelihood.

In the second place, we test whether individuals whose ancestors migrated are associated with different effects. While the identification strategy described in section 4 exploits historical linkages via combinations of locations and surnames, an alternative is to match surnames indistinctly to their birth location. This approach can hint at the potential differential effects of being born inside or outside of the *concertaje* region (the highlands) while having the same

Table 14: Long-Run Effect—Descendants: Poverty (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname Specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	.00112***	.00077***	.00083***	.0009***	.00093***	.00093***	.00112***	.00068***	.00103***	.0002
	(.0002)	(.00019)	(.00018)	(.00022)	(.00018)	(.0002)	(.0002)	(.0002)	(.00018)	(.00017)
× Current Migration	-.00066***	.00009	-.0003	-.00002	-.00047***	-.00005	-.00072***	.00026	-.00059***	.00059*
	(.00016)	(.00025)	(.0002)	(.00029)	(.00018)	(.00026)	(.00018)	(.0003)	(.00018)	(.00032)
Adj. R-squared	.0985	.1653	.0727	.1635	.0838	.1659	.1022	.1619	.1182	.1525
FE	Parish	Parish	Parish	Parish	Parish	Parish	Parish	Parish	Parish	Parish
Observations	968271	335759	968271	335759	547417	257304	858266	263798	968271	335759
N Surnames	2502	2177	2502	2177	2288	2128	2411	1970	2502	2177
Mean Dep. Var.	.4911	.6533	.4911	.6533	.4466	.6277	.5011	.6796	.4911	.6533

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

pseudo-ancestors. Under this specification, one can compare in the present individuals with surnames with a similar incidence of *concertaje* but who were born in different regions under the implicit assumption that they share common ancestors. Thus, as the original empirical strategy already accounts for current individuals who have migrated, this new strategy instead accounts for individuals who were not born in the *concertaje* region, making them likelier to be descendants of migrants from the previous generation. The downside of this approach is that the precision of linkages may be lower, given that we are matching surnames at a national level instead of a local one. To mitigate these concerns, we again report results in tandem with the same battery of robustness checks as above, although employing the overall distribution of colonial surnames. Table 15 shows the results when current tax records are used. We also use present-day surname local specificity indexes instead of the historical ones, given that we do not have information about the historical distribution of surnames outside the highlands.

In this case, the estimates are smaller than those in Table 1. Nonetheless, all columns show a negative and statistically significant effect of descending from a *concierto* on current labor income. The lower quality of pseudo-linkages can explain these smaller effects due to the omission of regions when matching surnames. Nevertheless, the effect is substantially more prominent when we correct the accuracy of linkages based on the locality of surnames (columns (9) and (10)). Hence, these estimates suggest that individuals born outside the *concertaje* region are less affected by the coercion status of their ancestors. Indeed, the estimates shown in Table 16, which use census registries and the poverty index as an outcome, confirm this result.

Table 15: Long-Run Effect—Descendants: Log Labor Income (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	-.00075**	-.00055**	-.00063**	-.00046**	-.00074**	-.00051**	-.00083**	-.0006***	-.00142***	-.00081***
	(.00034)	(.00023)	(.00032)	(.00021)	(.00033)	(.00022)	(.00033)	(.00022)	(.00031)	(.00022)
× Ancestor's Migration	.00063*	.00042*	.00043	.00021	.00059	.00032	.00075**	.00049**	.00104**	.0005*
	(.00035)	(.00024)	(.00034)	(.00023)	(.00039)	(.00026)	(.00035)	(.00024)	(.00041)	(.0003)
Adj. R-squared	.0185	.0605	.023	.0665	.0248	.067	.0184	.0606	.0185	.0726
FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1939480	1939345	1939480	1939345	560679	560504	1939480	1939345	1939480	1939345
N Surnames	2529	2529	2529	2529	1592	1592	2529	2529	2529	2529
Mean Dep. Var.	8.1083	8.1083	8.1083	8.1083	8.1286	8.1286	8.1083	8.1083	8.1083	8.1083

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: Long-Run Effect—Descendants: Poverty (2010)

	Base Model		Weight inverse Surnames' frequency (Colony)		Rare Surnames (Colony)		Weight inverse Concertaje's SD (Colony)		Weight local Surname specificity (Colony)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: TS2SLS										
Concierto	.00058***	.00026***	.00035**	.00017**	.00042***	.0002**	.00059***	.00027***	.00109***	.00025***
	(.00015)	(.00009)	(.00014)	(.00008)	(.00015)	(.00009)	(.00015)	(.00009)	(.00016)	(.00008)
× Ancestor's Migration	-.00074***	-.00034**	-.00038*	-.00014	-.00042*	-.00015	-.00076***	-.00036***	-.00125***	-.00023
	(.00021)	(.00014)	(.00019)	(.00013)	(.00023)	(.00015)	(.0002)	(.00014)	(.00022)	(.00017)
Adj. R-squared	.061	.1566	.0689	.1602	.0697	.1609	.0608	.1567	.0743	.1751
FE	Province	Parish	Province	Parish	Province	Parish	Province	Parish	Province	Parish
Observations	2948796	2948796	2948796	2948796	839166	839164	2948796	2948796	2948796	2948796
N Surnames	2645	2645	2645	2645	1704	1704	2645	2645	2645	2645
Mean Dep. Var.	.551	.551	.551	.551	.546	.546	.551	.551	.551	.551

Note: Clustered standard errors at the surname level in parentheses. Surnames correspond to their standardized version after using the algorithm described in appendix A. Odd (pair) columns control for province (parish) fixed effects and thus match samples accordingly. Weights and distributions are considered at the corresponding geographical level of analysis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

These differences could be attributed to being raised in a location with fewer economic distortions. Nonetheless, they do indicate that migration could mitigate the adverse effects of labor coercion, even though *concertaje* itself limited mobility. In that sense, migration could be seen as a channel of un-persistence, particularly for historical labor coercion. The Great Migration in the United States stands out as an excellent example of this in the case of slavery. While the general equilibrium response of the recipient locations has yet to be documented for Ecuador, as it has for the United States (Derenoncourt (2022)), there is a widespread consensus that immigrants moving from historically coercive locations increased their income (Boustan (2017)). In a glimpse of hope, the results presented in this section suggest that, although labor coercion limited migration, policies that may reverse its barriers could help alleviate the persistent effects of this class of historical institutions.

7 Conclusion

In this paper, we hand-collected historical records circa 1800 to study the intergenerational long-run effects of *concertaje*, a forced labor institution established in colonial Ecuador that became the most common and persistent one in Latin America. To document the long-run intergenerational effects of labor coercion we match the universe of current (2010) administrative and census registries with the universe of historical ones via surnames. We show that an increase of 10 pp in a surname's *concertaje* rate reduces the labor income of descendants by 1.6 pp and increases the likelihood of being in the informal sector by 0.62 pp and of falling into poverty by 1.1 pp. The results are robust to several specifications, including controlling for the accuracy of pseudo-linkages assumed by surnames. Because the historical records were associated with indigenous workers specifically, these estimates capture the institutional effect of labor coercion rather than ethnic differences.

At the regional level, we identify a causal effect by instrumenting *concertaje* with the relative productivity of crop yields that differ in labor requirements. Both historiography and the hypothesis that geographic conditions influenced the incentives to coerce labor support the use of this instrument. Moreover, the instrument is plausibly exogenous, as those yields consider only agro-climatic conditions, which are orthogonal to human intervention. To further mitigate concerns about the exclusion restriction, we control for a comprehensive set of geographical factors and show that other channels through which the instrument may have operated are absent. We estimate that on average an increase of 10 pp in the rate of *concertaje* increased the probability of being in extreme poverty in 1990 by 4.2 pp.

Furthermore, due to the close relationship between *concertaje* and the labor market, we investigate whether this labor-coercive institution created barriers to human capital accumulation and generated distortions that affected labor mobility across regions or across sectors.

We estimate that, on average, full descendants from *concertaje* have one less year of education. Given the standard estimates of human capital returns, this channel alone can explain half of the long-run income effects. Mobility restrictions likely determine the other half. We estimate that descendants from *concertaje* are 7 percent more likely to work in agriculture and 5.9 percent less likely to migrate. Regional estimates confirm these results, with larger effects observed at earlier points in time (1962). These results confirm the role of historical institutions in creating persistent market distortions.

Using data about land distribution, we also document a strong relation between the institution and land inequality that is in line with the hypothesis of Engerman and Sokoloff (1997) regarding the connection between endowments, institutions, and inequality. However, we find no association between land inequality and economic development. Instead, we emphasize that labor coercion by itself, which is not necessarily accompanied by land inequality, was detrimental to economic progress. This evidence points to the relevance of understanding the underlying nature of historical inequality when attempting to assess its relation with economic development.

Finally, using data on contemporary roads, we report a negative effect of *concertaje* on public goods provision. These results differ from Dell (2010), who highlighted the positive role of Peru's *haciendas* in providing access to roads. Instead, the results presented here do not favor a generalization of the hypothesis of Dell (2010) that landlords shielded individuals against extractive institutions. In the case of Ecuador, where the Spanish depleted gold and silver mines very early, a more homogenous setting to study the effects of institutions promoted by landlords is available. Within that setting, there is more evidence favoring the historical analysis that *haciendas* constituted extractive institutions that affected economic development. Overall, we emphasize that labor coercion itself, either through *mita*, *concertaje*, or slavery, was a critical institution that generated divergences in economic development across regions.

Under the lens of the evidence presented in this paper, the nature of coercion explains its adverse effects. Labor coercion usually implies that workers are retained in a particular activity (for example, agriculture) and in a specific place, so that it functions as a barrier to reallocating labor across sectors and regions. Moreover, coercers perpetuate their workers' conditions by preventing those workers from taking advantage of more-desirable outside options low enough. They typically achieve this via legal channels (for example, debtor's prison) and by reducing the incentives to acquire human capital formally. To accomplish this last point, coercers either use their political power to block public school investments or deny access to education. Furthermore, to keep a stable labor force over time, the next generation needs to be coerced from an early age, which induces a substitution of school for labor. First, their illiteracy means workers cannot participate in the political process via voting, and second, their inability to do so perpetuates the lower provision of public goods and reinforces the barriers

to their escaping from coercion.

In addition, the intergenerational nature of labor coercion amplifies these persistent effects. On the one hand, the low level of migration of individuals caused by the institution generates a positive assortative mating between coerced individuals, further strengthening any inheritance effect. On the other, the direct effect on human capital suggests that the effects of labor coercion can extend beyond the region where it occurs. Indeed, we document that the impact of descending from a coerced worker is present even for individuals born outside the *concertaje* area, that is, the highlands. However, the effect is less pronounced for this group, suggesting migration could undermine historical institutions' persistent effects.

A more detailed analysis of the results presented in this paper requires obtaining *concertaje* data for the early Republican period (19th century) to understand better at which point regions started to diverge in their development paths. In addition, other channels such as trust or conflict are worth exploring. Future research may also consider the findings in this paper as motivation to develop models in which institutions explicitly play a role in impeding the process of structural transformation by creating labor mobility barriers, which could lead to resource misallocation. Moreover, further empirical evidence about the intergenerational effects of historical institutions is needed to understand how their effects extend beyond the regions where they existed. It is also necessary to study how migration and policies oriented toward historical persistence could mitigate it. This is an agenda that shows promise for disentangling the fundamental causes of long-run economic development.

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