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Alex Rivadeneira
Banco de México

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Attached once, attached forever: The persistent effects of concertaje in Ecuador *

Alex Rivadeneira†
Banco de México

Abstract: This paper studies the long-run effects of concertaje, a forced labor system from the Spanish colonial era in Ecuador that coerced indigenous workers in rural estates after indebting them. I collected and digitized historical tax records (1800) and connected them to contemporary ones (2010s) via surnames. Employing a TS2SLS approach, I find that a 10 percentage point (pp) increase in a surname's concertaje rate reduces the current formal income of (pseudo) descendants by 1.7%. On a regional scale, I establish a causal relationship by leveraging variations in concertaje intensity due to differences in crop labor requirements. IV estimates show that a 10 pp rise in a district's concertaje rate increases contemporary poverty by 4.2 pp. As channels of persistence, I show that concertaje historically led to lower education levels, reduced public goods provision, increased agricultural employment, and inequality. Concertaje also limited mobility, although its effect on immigrants is milder, suggesting migration acted as a mitigation channel.
Keywords: Institutions; Persistence; Forced Labor; Intergenerational Mobility; Ecuador
JEL Classification: N36, O10, O43, J62

Resumen: Este artículo estudia los efectos a largo plazo del concertaje, una institución colonial española en Ecuador que coaccionaba a los trabajadores indígenas en las haciendas tras endeudarlos. Recopilé y digitalicé datos históricos (1800) y los conecté con registros fiscales contemporáneos (2010) vía apellidos. Estimaciones TS2SLS muestran que un aumento de 10 puntos porcentuales (pp) en la tasa de concertaje del apellido de un (pseudo) descendiente reduce el ingreso laboral formal actual en un 1.7%. A nivel regional, establezco una relación causal usando variaciones en la demanda de mano de obra de los cultivos. Los resultados (IV) muestran que un aumento de 10 pp en la tasa de concertaje de un distrito aumenta la pobreza contemporánea en 4,2 pp. Como canales de persistencia, muestro que el concertaje condujo a menores niveles educativos, menor provisión de bienes públicos, mayor empleo agrícola y desigualdad. También limitó la movilidad, aunque su efecto en los inmigrantes es menor, lo que sugiere que la migración actuó como un canal de mitigación.
Palabras Clave: Instituciones; Persistencia; Trabajo Forzado; Movilidad Intergeneracional; Ecuador

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† Dirección General de Investigación Económica. Email: arivadeneiraa@banxico.org.mx

[…] *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 yet open question in economics is why economic development is elusive for many regions and individuals. Though no consensus exists, recent literature has focused on institutions as a critical element in explaining it (Acemoglu et al. (2005)). Notably, this research agenda has placed special attention on the role of historical institutions in determining contemporary economic development. However, the channels through which institutions may have shaped it remain to be understood. While the literature has explored regional mechanisms like inequality, land tenure, or public goods, there needs to be more understanding concerning various others, such as human capital, migration, or structural transformation. Moreover, there is little evidence about the persistence of institutions' effects on individuals, particularly regarding intergenerational impacts on the descendants of those who lived under these institutions.

In this article, I study the regional and intergenerational effects of a forced labor institution created by the Spanish in the Ecuadorian highlands named *concertaje*. This institution forced indigenous workers to provide labor to landlords to pay off past debts. Because indebted workers usually could not pay their obligations, this situation implied a lifetime of service on the rural estates (*haciendas*) where they worked. Additionally, workers transmitted this condition over generations via inherited debt. These mechanisms endured for more than 300 years, continuing even after the end of the colonial regime in 1822. *Concertaje* was legally abolished in 1918, although it survived *de facto* through other coercive mechanisms until 1964, making it the region's most persistent economic colonial institution.

Although I focus on Ecuador, *concertaje* is similar to other lasting forced labor relations in Latin America[1]. Furthermore, forced labor has been the most common type of labor relation throughout history (Acemoglu and Wolitzky (2011)). However, empirical evidence about its effects is scarce due to data limitations. I circumvented this challenge by collecting and digitizing more than 2,000 pages of historical tax records from the late Spanish colonial regime (1780-1830). These records clearly distinguish indigenous workers' coercion status, allowing me to build a unique dataset of almost the universe of taxpayers at the time, covering all the colonial districts where the institution existed. In turn, these districts account for the totality of the modern highlands, where half of the country's population currently lives. Moreover, as these historical records report the full names of payers, I can match them to current tax records to document the long-run effects of labor coercion on surname-shared descendants.

Previous studies have not delved into the intergenerational implications of labor coercion, primarily due to data constraints. This is unfortunate, as the intergenerational channel can explain why the effects of forced labor systems persist over time. Additionally, intergenerational mobility has important implications for both inequality (Solon (2004), Corak (2013)) and efficiency (Galor and Tsiddon (1997)), the two main sets of long-run outcomes explored by the

[1]The *conciertos* of Ecuador are comparable to *terrazgueros* in Colombia, *inquilinos* in Chile, *yanacunas* in Peru, or *colonos* in Bolivia (Oberem (1981a)). It also resembles the indentured labor system in the Caribbean (Dippel et al. (2020)). To indebt workers during agricultural duties (*tienda de raya*) was also common in Mexico (Villegas (2008)). In contrast to the *mita* (abolished in 1812) or slavery, abolished in 1888 in the Americas (Brazil), systems such as *concertaje* existed until the 20th century (e.g., up to 1954 in Bolivia (Choque (1997)).

literature (e.g., Easterly (2007), Nunn (2008a), Banerjee and Iyer (2005)). Moreover, recent research has evidenced that the degree of long-run intergenerational mobility is much lower than commonly suspected (Clark and Cummins (2015), Barone and Mocetti (2020)), adding further relevance to this mechanism.

To address this research gap, I link individuals from historical records with current taxpayers (2007-2016) through their surnames. Specifically, I use a two-sample, two-stage least squares (TS2SLS) approach (Inoue and Solon (2010)), widely used in studies of intergenerational mobility (e.g., Björklund and Jäntti (1997), Aaronson and Mazumder (2008), Olivetti and Paserman (2015)). The estimates show that a 10 percentage point (pp) increase in a surname's *concertaje* rate reduces formal labor income by 1.7%. I confirm this result via several robustness checks, mainly to account for the accuracy of the linkages. For instance, placebo regressions in which surnames are randomly reshuffled show neither statistically nor economically significant effects. Instead, weighting for surname rarity shows larger ones.

Disentangling the ethnic component from the institutional one is often challenging in many other contexts, given that labor coercion frequently involves entire ethnic groups. However, several features in this case help me in that regard. First, most surnames in the historical tax records are of indigenous origin. Second, even for workers in *haciendas*, their surnames do not coincide with those of landlords. Hence, instead of comparing Spanish versus indigenous surnames, I compare which ones were more or less associated with the *concertaje* system within the universe of indigenous surnames. Thus, by comparing individuals with similar ethnic backgrounds, I can better attribute any effect to the role of labor coercion.

While the intergenerational effects of *concertaje* are significant, examining the institution's impact on regional economic development is also crucial. Additionally, the absence of individual-level data for specific relevant outcomes makes it necessary to exploit cross-regional differences. To properly connect both, I provide a parsimonious model that guides the interpretation of the results. The model stresses intergenerational mechanisms to explain persistence, although it recognizes that regional conditions may also shape long-run effects. In that sense, I identify a causal relation at a regional level, guided by both the Ecuadorian historiography and the well-known hypothesis that natural endowments influenced the formation of forced labor institutions (Engerman and Sokoloff (1997)). Due to the peculiar characteristics of the Ecuadorian highlands, there are non-negligible variations in crop suitability within short distances. In turn, crops' labor requirements determined landlords' incentives to coerce labor.

I use potential crop-yield data from the GAEZ-FAO project to compute relative crop productivity and use it as an exogenous variation for *concertaje*. Crucially, these yields consider only agro-climatic conditions, orthogonal to human intervention, instead of agro-ecological ones. Specifically, I use the relative productivity of maize (a native crop with high labor requirements) over potatoes as an instrument for *concertaje*. I document that, on average, regions with a comparative advantage in producing maize over potatoes tended to coerce more indigenous labor. To mitigate concerns regarding the exogeneity of the instrument, I control for a variety of geographical factors, including the overall agricultural productivity of a region. The idea is that while the instrument exploits a comparative advantage, the latter controls for an absolute one. Moreover, I also show that other channels through which the instrument may have operated, like population divergence due to dietary differences, are neither visible before *concertaje* was well-established nor when it was, let alone for other recent periods.

Using contemporary census data under this instrumental variable framework, I find that an increase of 10 pp in *concertaje* around 1800 increased the 1990s extreme poverty rate of a locality by 4.2 pp. This result remains robust even when including controls such as initial population or measures that approximate late-colonial economic conditions, further mitigating concerns about reverse causality. I also show that an increase of 1 pp in *concertaje* reduced average night light intensity (a proxy for economic development (Henderson et al. (2012))) in 1992 by 4.6%. However, I also report that the effects of *concertaje* have diminished over time.

I further collect historical data to unravel channels of persistence. In particular, I disentangle the mechanisms behind the detrimental effects of *concertaje* by understanding how the system shaped economic incentives. As indebted workers, in addition to guaranteeing a stable labor force, were an asset to the property, they became an intrinsic part of the landlords' profit strategy. Thus, supporting and perpetuating the institution was in their best interest. Consistently, within colonial times, the rates of *concertaje* varied little, while 150 years later, the regional distribution of *huasipungo* workers, a variation of *concertaje*, remained similar to the colonial one. Altogether, this suggests that labor coercion must have been profitable to sustain it.

Indeed, *concertaje* positively correlates with (proxied) output per capita in 1800, implying the system concentrated in more productive districts rather than underdeveloped ones. Additionally, *concertaje* was strongly associated with larger *haciendas*, which are typically more productive. Even a century later, areas where *concertaje* prevailed continued to have higher total land values. These facts suggest that landlords retained workers as a business strategy to profit from their labor and maintain the value of the land. However, *concertaje* became an obstacle to economic development over time, as evidenced by lower educational levels and reduced provision of public goods in the regions where it prevailed. Furthermore, *concertaje* led to a larger workforce in the agricultural sector and limited migration.

This sort of reversal of fortune indicates that during the transition from an agricultural economy, *concertaje* became an obstacle to development by creating substantial barriers to human capital accumulation and labor mobility. Firstly, the intergenerational transmission of debts provided landlords with the means to retain the offspring of *conciertos* and maintain a stable labor force. This situation forced the children of *hacienda* workers to forgo schooling in favor of early work, conducing them to illiteracy. As illiterate individuals could not vote, their ability to engage in public affairs was limited, reducing their opportunities to advocate for public goods. Lastly, due to landlords' efforts to retain workers, *concertaje* became a barrier to labor mobility across industries and locations, likely leading to labor misallocation.

Studies investigating the historical origins of underdevelopment in the Americas have 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 long-run economic development. Mainly because, throughout history, forced labor relations have been prevalent on large plantations. The authors suggest that higher rates of inequality induced by forced labor systems hampered the formation of broad, deep markets, which failed to create the right incentives to invest in public goods.

Instead, Dell (2010) evidences a negative long-run effect of coercion from Peru's mining *mita* and attributes a positive role to *haciendas* in economic development. She argues that regions that did not contribute labor to this system allowed the expansion of large landowners, who had secure property rights and were, therefore, more likely to invest in and lobby for public goods. In contrast, the evidence in this paper suggests that these mechanisms were absent in Ecuador. Behind this could be that the Ecuadorian State, especially during the 19th century, relied on public labor to construct public goods. Despite having 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 stable labor force. While this result may be particular to this context, it raises the question of how generalizable the public goods channel is to understanding the long-run effects of labor coercion.

Surprisingly, within the spectrum of colonial institutions (from missions (Valencia Caicedo (2018)) to slavery (Acharya et al. (2016))), systems like *concertaje* have yet to receive proper attention despite being widespread throughout Latin America. Moreover, unlike other coercive labor systems, *concertaje* persisted much longer, making it more likely to have lasting effects. Additionally, compared to rotational systems like the *mita*, *concertaje* resulted in semi-permanent coercion for individuals and their descendants, naturally putting more weight on intergenerational mechanisms. In this regard, *concertaje* resembles slavery, although a crucial distinction is that landlords did not have legal ownership over the workers. Instead, the latter employed more *de facto* mechanisms to coerce workers, which, in addition to proving successful in conserving the system, also had enduring consequences.

Nonetheless, the results in this paper coincide with the generalized view regarding the detrimental role of labor coercion on economic development not only in the Americas (e.g., Nunn (2008a), Acemoglu et al. (2012), Fujiwara et al. (2017)) but in other contexts (e.g., Lowes and Montero (2021a), Buggle and Nafziger (2021), Nunn (2008b)). Moreover, the evidence in this paper supports the idea that historical labor coercion, instead of land inequality itself, is critical in explaining contemporary development. Principally because labor coercion does not necessarily have to be accompanied by land inequality. In lieu, labor coercion, usually transmitted across generations, has been typically characterized by education restraints and subsequent labor mobility restrictions, which better explains its detrimental long-run effects.

In addition, the evidence presented in this paper sheds light on the colonial origins behind the income gaps of groups that have faced historical discrimination[2]. For instance, Collins and Wanamaker (2022) suggest that most of the black-white income inequality observed in the United States attributes to racial differences in intergenerational mobility, which is lower for African-Americans (Solon (2018), Chetty et al. (2019))[3]. Nonetheless, it is unclear to what extent historical labor-coercive institutions, like slavery, shaped these differences. In particular, because disentangling the ethnic component from the institutional one is challenging, at least in the context of black slavery. However, as mentioned above, in the case of *concertaje*, I can better differentiate the institutional effects from the ethnic ones since not all indigenous workers were part of the *hacienda* system. Therefore, by comparing individuals with similar ethnic backgrounds, I can directly attribute part of the current income gaps across groups to the role of historical institutions.

Similarly, in a recent work, Althoff and Reichardt (2022) track family trees of African-American and document that those descending from individuals who were freed earlier have higher levels of education, income, and wealth. However, their emphasis is on the posterior Jim Crow laws that, they argue, are critical to explain this divergence. In particular, they show that the State in which a slave was freed, rather than slavery by itself, mostly explains the "Free-Enslaved gap" due to posterior exposure to segregation laws. This work complements

[2]See Margo (2016) for a discussion about the evolution of black-white income gaps in the United States.

[3]See also Duncan (1968), Corcoran et al. (1992), Hertz (2008), Hertz (2009), and Mazumder (2014).

theirs by showing how enduring labor coercion, even without direct discriminatory legislation, can also generate intergenerational persistence due to educational and mobility-wide barriers.

Finally, this research makes a novel contribution to the existing literature by examining the role of specific factors in mitigating the long-term effects of historical institutions. While previous studies have predominantly focused on documenting its persistent effects, I also provide suggestive evidence of a potential mechanism to alleviate them. Specifically, I emphasize the importance of migration as a channel of *un-persistence*. For instance, I document that the effects of descending from a *concierto* are less pronounced when including individuals born outside the *concertaje* region, i.e., the Highlands. While this implies that some of the effects of labor coercion expand beyond the specific areas where it existed, it also offers a positive interpretation. Despite mobility barriers, individuals who managed to migrate have overcome part of the labor coercion legacy, suggesting that pro-migration policies can serve as a tool to lessen the consequences of historical institutions.

I organized this paper as follows. The next section offers a historical background. Section 3 describes the estimation framework and the data. Sections 4 and 5 present the identification strategies and the results for the long-run impact of *concertaje* across generations and regions, respectively. Section 6 discusses mechanisms of persistence. Section 7 concludes.

2 Historical Background

2.1 *Concertaje*

During the colonial regime, the Spanish organized the distribution of indigenous labor in the Ecuadorian highlands, mainly via a pre-Hispanic system known as *mita*. The *mita* was a system of forced but paid labor that assigned one-fifth of the male population between 18 and 55 years old to work on a specific task for a certain period (Cushner, 1982, p. 119). Its enforcement was in the hands of local chiefs, who could lose their power or be obliged to pay with their wealth if they did not supply the solicited workers (Cushner, 1982, p. 121). Given the absence of gold and silver mines in the region, the Spanish mainly distributed *mita* workers to landlords for agricultural and textile activities[4].

Due to the rotational nature of the *mita* system, landlords developed mechanisms to secure a stable labor force (Cushner, 1982, p. 128). The most important one was to push indigenous workers to acquire debts with the *hacienda* to retain them indefinitely. Once employed, landlords deducted a combination of alleged benefits from their wages, which mainly included the right to use a small plot of land, the guarantee of tribute payment, and the provision of food and clothes. However, as typical wages hardly covered the supposed costs of these expenses, workers ended up contracting a series of obligations with the *hacienda*. Consequently, they had to extend their original service period to pay these induced debts. Thus, it was the exception rather than the rule that *mita* workers did not end up indebted during their serving period (Oberem, 1981a, p. 314)[5].

Indeed, landlords discretely mediated this vicious cycle of work and debt. Except for the tribute payment, they could adjust the extent of the provided benefits. For instance, the parcel of land granted was so small that it barely fulfilled any subsistence requirements, while the prices of food and clothes charged to workers typically exceeded those of the market (Oberem, 1981a, pp. 314-315; Cushner, 1982, p. 134). Moreover, the loss of livestock during shepherding could be added directly to the worker's debt (Andrien, 1995, p. 123). In sum, landlords deliberately employed debt to perpetuate indigenous workers' labor supply. For example, Cushner (1982, pp. 123-128) documents the story of an indigenous worker in a

[4]Deposits of precious metals rapidly depleted during the first colonial years (Andrien, 1995, p. 18). Since most landlords combined in their *haciendas* both textile (*obrajes*) and agricultural production (Oberem, 1981b, p. 347), the distinction between both is unclear. See Perez (1947) regarding the *mitas* in this region.

[5]For instance, while wages ranged between 18-25 pesos, tribute payments and the cost of food and cloth averaged 5-6 and 5-10 pesos respectively (Andrien, 1995, p. 122). By paying workers in goods, landlords also ameliorated liquidity constraints, a constant problem in the region sharpened during the 18th century (Tyrer, 1988, p. 100).

Jesuit *hacienda* who started with a debt of 27 pesos in 1701 and worked there for 15 more years to end up accumulating a debt of 48 pesos by 1716.

Once attached to *haciendas*, workers' chances to escape were low. Those who tried were chased and typically recaptured. Furthermore, the intent to escape, in addition to harsh punishments, implied an increase in the debt since landlords charged prosecution costs directly to workers (Oberem, 1981a, p. 316). Moreover, the descendants of these workers shared the same fate since they inherited their parents' debt. Accordingly, many *haciendas* reported two or three generations of one family living and working together on the same estate (Alchon, 1991, p. 85). Hence, the difficulty of escape and the intergenerational transmission of debts attached entire generations of families to *haciendas*[6].

This process created the institution called *concertaje*, a debt peonage system that forced indigenous workers to become permanent residents of Spanish holdings. These workers, known as *conciertos*, were not considered slaves since landlords did not formally own them. However, the debtor's prison guaranteed landlords a legal mechanism to retain them. Moreover, because the *concierto's* debt was an asset of the property (Poloni, 2006, p. 377), landlords bought and sold them with their *haciendas* (Oberem, 1981a, p. 310; Baud, 2007, p. 76)[7].

Accordingly, in Appendix B, I show that even in 1909, when the system was still in place, the total value of land was positively correlated with the *concertaje*. So aside from supplying a stable labor force, the institution linked land's intrinsic value to its workforce, a deep-rooted practice, as the right to *mita* labor already constituted a considerable part of a *hacienda's* value in colonial times (Oberem, 1981a, p. 307). Thus, as indebted workers were part of the landlord's wealth, they opposed the abolition of *concertaje* and used their political power to perpetuate it[8].

In 1918, authorities legally abolished *concertaje* by prohibiting prison debt. However, once that *de jure* mechanism disappeared, landlords continued to coerce them *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). Ecuador's rural workers just witnessed the disappearance of any residuals of *concertaje* in 1964, when the agrarian reform abolished all forms of labor coercion. Since the first records of *concertaje* date from 1601 (Oberem, 1981a, p. 309); (Peñaherrera de Costales and Costales Samaniego, 1964, p. 4), this means it lasted for 300-360 years, being one, if not the most persistent,

[6] *Concierto* status still transmitted to sons even after the abolition of debt inheritance (1833), as landlords forced children to obtain debts to guarantee their labor supply when adults (Oberem, 1981a, p. 315).

[7] Ironically, *concierto* comes from the Spanish word *concertar*, which means to agree. If translated literally, *concierto* is an individual that has agreed to a contract (Oberem, 1981a, p. 309).

[8] See Chapter I of Prieto (2004) for an analysis of the political debate around the abolition of *concertaje*.

economic institution in the Ecuadorian highlands[9].

2.2 Labor requirements and coercion

Historical analysis suggests that business considerations did determine landlords' decision to coerce labor (Andrien, 1995, p. 124). Depending on their labor requirements, landlords had to secure a stable labor force for their properties, mainly if they believed the *mita* system would not grant them enough workers. Thus, landlords may have considered the pool of workers available to them, given the potential demand of other parties.

In that sense, whenever landlords anticipated high levels of competition of *mita* workers, and thus, smaller quotas for themselves, they could have coerced more labor to secure workers for their production. Instead, in the extreme case of full monopsonistic power, landlords may not have to worry about coercing a significant number of workers, given their high disposal. Accordingly, Figure 1 shows that districts with lower labor market concentration (Herfindahl-Hirschman Index (HHI) based on each *hacienda*'s labor force), and thus, with more competition, also exhibited higher *concertaje* rates. This evidence aligns with Domar (1970)'s idea of labor scarcity and coercion[10].

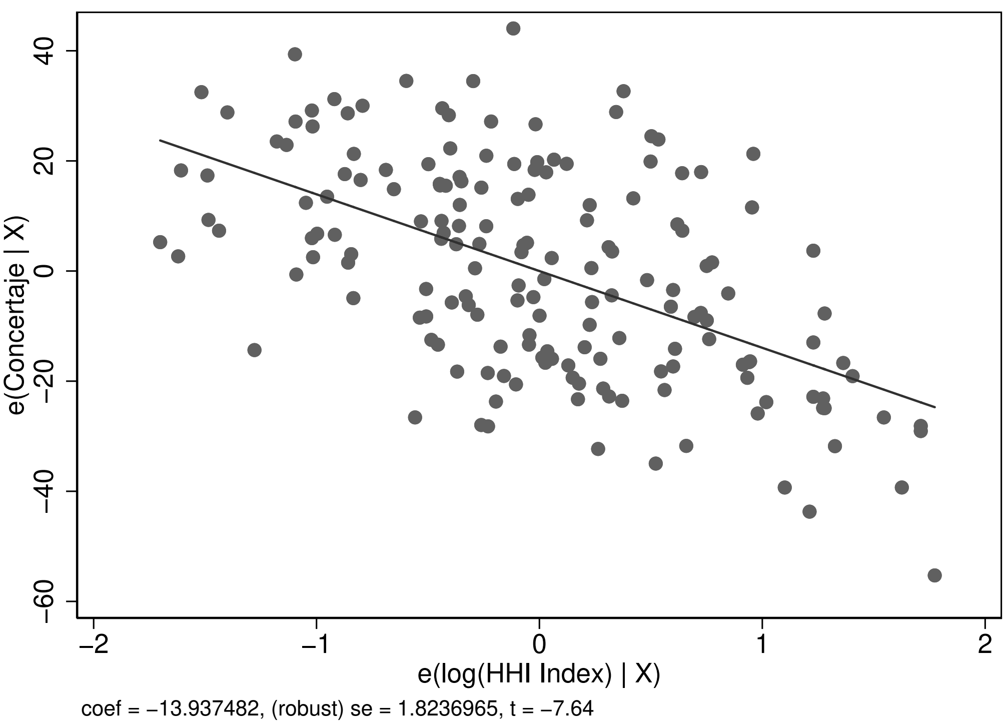
This degree of labor competition, in turn, was directed by the types of crops a region could grow. For instance, maize, as opposed to either potatoes or wheat, requires extensive labor demand, especially during the three to four months of plowing and tillage (Andrien, 1995, p. 124). It also has an extended crop cycle that lasts about ten months (Cushner, 1982, p. 73). Similarly, sugar cane, a crop typical of slave plantations, is quite labor-intensive. Accordingly, landlords in regions more suitable for crops with high labor requirements, like sugar cane or maize, had more incentives to have *conciertos*.

Anecdotal evidence confirms this was the case. For example, accounting records of the Jesuit *hacienda* of Tanalagua, located in San Antonio (Quito), report many indebted workers and a large production of maize. Conversely, the Jesuit *hacienda* of Llanagua in Santa Rosa (Ambato), which did not produce crops with high labor requirements, failed to retain indebted workers (Andrien, 1995, p. 124). The fact that the same owners had different business strategies depending on the crops they could grow suggests that coercion was profitable only when labor was more intensely required.

[9]In Appendix B, I show the persistence rates of *concertaje* during colonial times. Moreover, I also show that the distribution of *huasipungo* workers in 1959 strongly correlates with *concertaje* in 1800. Indeed, the historical relevance of this institution was such that, unsurprisingly, Jorge Icaza's book, *Huasipungo* (1934), is perhaps the most worldwide famous Ecuadorean novel.

[10]Instead, Méndez and Van Patten (2022) show that, in Costa Rica, whenever the United Fruit Company faced additional competition, it invested more in schooling and health care to retain workers.

Figure 1: Partial correlation between Log(HHI Index) and *Concertaje* (1780-1830)



Notes: The regression controls 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.

Since Spanish agro-pastoral production and distribution were eminently commercial (Cushner, 1982, p. 176), landlords had to consider the labor costs around *concertaje* before permanently attaching indigenous workers. After all, landlords had to provide *conciertos* with at least food and clothes, plus their guaranteed tax payment, which altogether represented a non-trivial expenditure (Andrien, 1995, p. 134). Even though landlords could manipulate the costs in their workers' accounts, they had to cover the proper expenses of maintaining a stable labor force. A failure to do so could imply immediate economic ruin (Andrien, 1995, p. 108).

This profit-oriented rationale of *concertaje* was also reflected in higher agricultural productivity. To see this, I collected historical tithe auction data (1780-1830), which could be considered an index of rural production. The tithe was the ten percent mandatory tax given to the Catholic Church. Nonetheless, the Church delegated its collection to privates, who used to bid by each town *before* production did occur. Hence, these auctions represent an expected value of a district's production (Andrien, 1995, p. 90)[11].

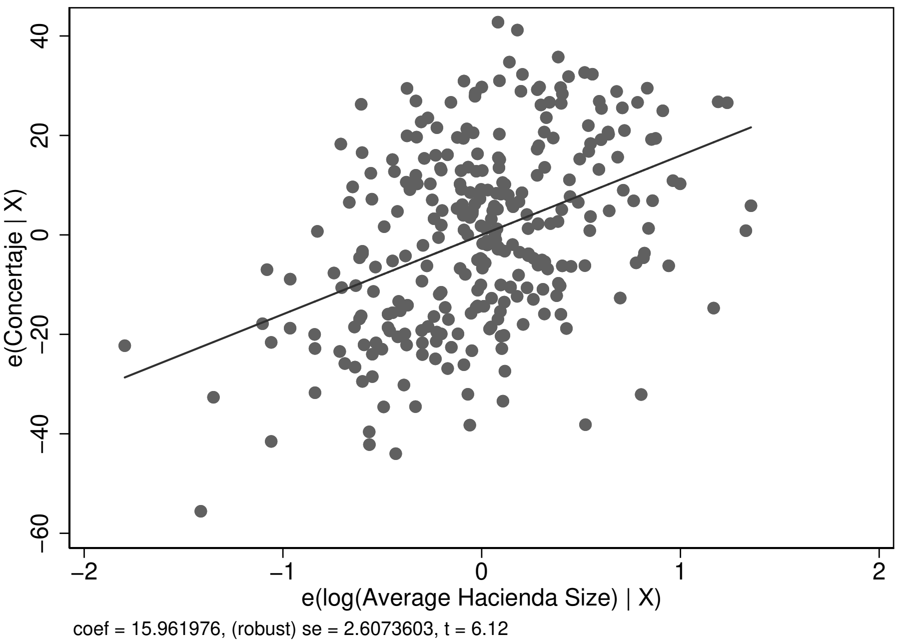
Figure 2b shows the partial correlation between *concertaje* and (log) tithe per capita after controlling for time (decade) and colonial province fixed effects. Without a price deflator, the former takes into account any inflationary trends, while the latter controls for the fact that tithe collection varied significantly across regions (Andrien, 1995, pp. 91-92). Parishes with more *concertaje*, which likely concentrated agricultural production, also showed larger per capita tithe payments on average. Moreover, Figure 2a shows that *concertaje* positively correlates to *hacienda* size, a sign of productivity (Adamopoulos and Restuccia (2014)).

This suggests that the prevalence of *concertaje* was more considerable in districts with relatively higher (proxied) output per capita. Moreover, as the land value was higher in those places (see online Appendix B), it is hard to reconcile with the idea that they were unproductive. Especially given that landlords had the best available land ((Coronel Feijoo, 1991, p. 77); (Ortiz de la Tabla Ducasse, 1993, p. 193); (Tyrer, 1988, p. 327)). However, as shown in Section 5.2, the fact that nowadays we observe a negative long-run relation between current economic development and historical labor coercion indicates that a sort of reversal of fortune (Acemoglu et al. (2002)) occurred somewhere between the 19th and 20th centuries. As agricultural endeavors stopped being the primal economic activity, *concertaje* seemed an obstacle to modern economic development.

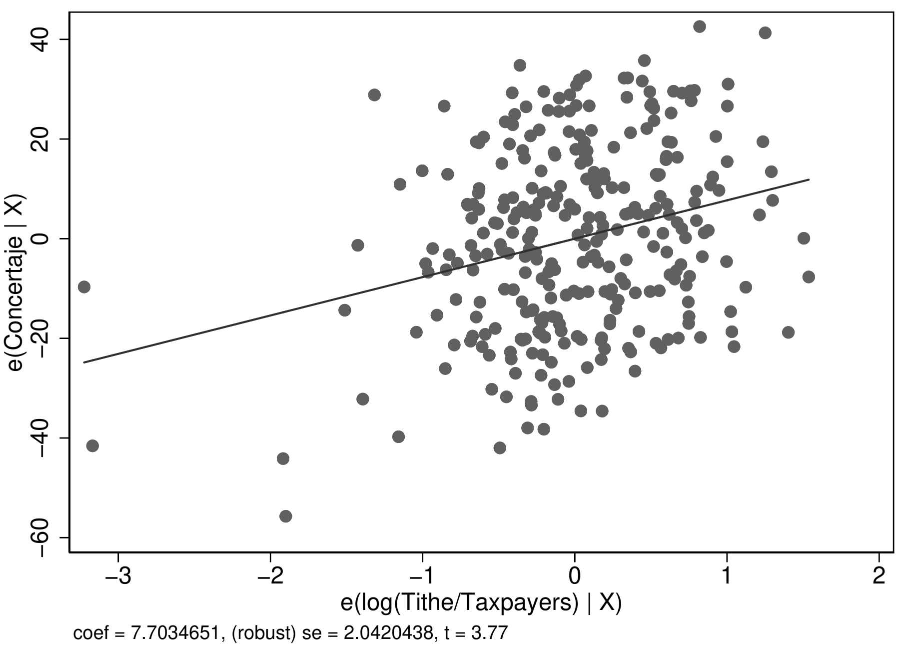
These historical observations are also consistent with the Engerman and Sokoloff (1997) hypothesis that initial factor endowments have determined the development path of a region.

[11] See Ouweneel and Bijleveld (1989b), Brading (1989), Coatsworth (1989), Lindo-Fuentes (1989), and Ouweneel and Bijleveld (1989a) for a debate regarding the usefulness of tithe data as a proxy for rural production for the case of Mexico.

Figure 2: *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: 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.

However, their theory pays special attention to the historical formation of forced labor institutions via large-scale plantations like sugarcane. However, the evidence discussed above suggests that large-scale plantations were not the only ones to induce the formation of these types of institutions. In general, plantations with high labor requirements, particularly in terms of days of attention, favored labor coercion, a point also made by Earle (1992)[12].

Therefore, landlords in regions with natural endowments for crops that require relatively fewer days of attention or are less labor-intensive, such as potatoes or wheat, might have competed less for labor and attached fewer indigenous workers. Conversely, regions suitable for crops with high labor requirements, such as sugarcane or maize, might have experienced more significant labor competition, with the inevitable consequence of higher *concertaje* rates. In Section 3.4, I exploit this idea that exogenous agricultural conditions determined the intensity of labor coercion to identify the long-run effects of the *concertaje* at a regional level.

3 Estimation Framework

The persistence of labor coercion via *concertaje* may have had significant implications in the development process of the Ecuadorian highlands. Thus, in this section, I present a stylized model of the long-run effects of labor coercion. The model captures parsimoniously its potential intergenerational effects and the regional economic distortions it may create. More relevant, the model serves as a guide to understanding the paper's empirical results.

3.1 Model

Consider the following three-period model (e.g., present times, early republican period, and colonial era),

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

where $y_{id,t}$ represents the income (or any other proxy of economic well-being) of individual i, living in district d, at period t; $y_{id,t-1}$ is the economic well-being of individual's i ancestor; $w_{\bar{d},t-1}$ refers to the economic conditions in location d (where one was born and raised), $\alpha_{id,t}$ captures any unobserved individual characteristics, and suppose $\epsilon_{id,t}$ is *i.i.d.* Hence,

[12]Farms specialized in multiple-day crops were likely larger. For example, in the Chillos Valley, a region characterized by maize production, extensive holdings of religious orders held more than half the farmland (Andrien, 1995, pp. 83-84). Large landowners, in turn, could reduce labor coercion costs, as it was cheaper to retain peasants by paying them via the right to use a small plot of land.

the model assumes that an individual's economic prosperity depends on their parent's economic situation, the environment in which that person was raised, and some innate individual skills. The latter are transmitted across generations according to the following AR(1) law of motion: $\alpha_{i,t} = \rho\alpha_{i,t-1}$. Similarly, assume that local conditions have a persistence pattern: $w_{d,t} = \lambda w_{d,t-1}$. Lastly, suppose that at period 0, some exogenous circumstances at the locality level, z_d (like crop suitability), unrelated to an individual's ability, determined his labor coercion status[13].

Through the model's lens, the labor coercion condition of an individual's ancestor, $c_{id,0}$, leads to persistent effects via an intergenerational channel. This fundamental mechanism could explain why *concertaje*, and other historical labor coercive institutions, may have long-run effects. In this context, indigenous workers transmitted their labor coercion status across generations, mainly via inherited debt (so c_{id} persisted at least until 1918). Still, this intergenerational transmission is a common feature of almost all labor-coercive institutions. This channel takes further relevance as recent research has suggested that the socioeconomic status of ancestors can even influence descendants in the very long run (Barone and Mocetti (2020)). Given the discussion above, it is expected that $\beta_1 < 0$, while it is assumed that $\rho, \lambda, \beta_2 \in [0, 1)$.

Additionally, I assume labor coercion could have affected local economic conditions in the past, so $w_{d,0} = \delta \bar{c}_{d,0}$. This intends to capture any initial regional effect labor coercion may have created. For instance, Engerman and Sokoloff (1997) argue that labor coercion led to inequality, affecting economic development. However, note that this initial effect may not necessarily be detrimental, which could later lead to a reversal of fortune (see below). Finally, note that the model allows for migration. Hence, the location where an individual was raised, d, may be distinct from where that person currently lives, d'. Although, for simplicity, assume that up to period 1, migration does not occur or is negligible[14].

3.2 Individual-level estimation

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 may be challenging. Hence, one might be interested in estimating the reduced form equation of the effect of de-

[13] So, $Cov(\alpha_{id,0}, z_d) = 0$. Hence, even if some unobserved characteristics influenced an individual's labor coercion status, these were unrelated to the local factors that induced cross-regional variation in *concertaje*.

[14] As discussed above, the mobility restrictions associated with *concertaje* may have limited migration.

scending from a *concierto* on individuals nowadays, namely,

$$y_{id',2} = \mathcal{B}_2 c_{id,0} + \mathcal{E} \tag{2}$$

Given the system of equations and the assumptions made, it is straightforward to show that the Ordinary Least Squares (OLS) estimate of $\mathcal{B}_2$ is[15],

$$\mathcal{B}_2^{OLS} = \beta_1\beta_2 + (\rho^2 + \beta_2\rho)\frac{cov(\alpha_{id,0}, c_{id,0})}{var(c_{id,0})} + (\gamma_1\beta_2 + \gamma_2\lambda)\delta\sigma \tag{3}$$

which can be split into three parts: the direct intergenerational effect of having labor-coerced ancestors, the intergenerational transmission of abilities, and a born-location effect (d). However, once controlling for a set of regional dummies, the latter is no longer an identification threat, leaving the estimate to capture solely intergenerational mechanisms.

It is unclear whether individuals during colonial times self-selected into *concertaje* or if they were assigned to the system according to their abilities. Note, for instance, that if low-ability individuals opted to work in *haciendas*, then $Cov(c_{id,0}, \alpha_{id,0}) < 0$, and the reduced form estimate will be an *upper bound* of the actual effect of descending from a *concierto*. On the other side, landlords could have retained high-ability individuals, in which case $Cov(c_{id,0}, \alpha_{id,0}) > 0$, leading the OLS estimate to be a *lower bound*. Although untestable, in what follows, I assume that the initial correlation between abilities and labor coercion is zero[16].

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 an AR(1) process, such that $\beta_t = \beta \; \forall \; t > 1$, 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, the OLS estimates using individual data can overcome confounding geographical forces once appropriate regional fixed effects are used.

[15]See Appendix C for proof; $\sigma = \frac{Var(c_{d,0})}{Var(c_{id,0})}$ is the fraction explained by the between-region variance.

[16]To my knowledge, there is no empirical evidence about this for any labor coercion institution. The closest evidence is that of Fogel and Engerman (1995), related to the productivity of enslaved people in the south of the USA, which they show was higher.

3.3 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,

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

Since individuals can migrate, the population living nowadays in location d does not necessarily correspond to the descendants of those who lived there during colonial times. For now, suppose no migration occurs, so $y_{\tilde{d},2}$=$y_{\bar{d},2}$. So, similar to above, one can estimate the reduced form equation of the long-run effect of *concertaje* at a district level,

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

However, one can instrument $c_{\bar{d},0}$ 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 $b_1 b_2$=$\frac{\partial y_{\bar{d},2}}{\partial y_{\bar{d},1}}\frac{\partial y_{\bar{d},1}}{\partial c_{\bar{d},0}}$) 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 brings attention to how to interpret it[17].

Note also that estimates of *concertaje* on earlier periods outcomes capture fewer compounded effects. For example, the IV estimate of *concertaje* on $y_{\bar{d},1}$ is $\mathbf{B}_1^{IV} = g_1\delta + b_1$. This leads to two observations. First, if δ<0 (δ>0), earlier estimates of the same outcome will have larger (smaller) effects. This is an important point, as will be seen later, to understand why some effects have diminished over time. Second, if *concertaje* had effects on unobservable

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

regional events, like the intensity of land distribution during the agrarian reform, but only in period 1 (so $\delta = 0$ and $w_{d,1} = \delta' c_{\bar{d},0}$), then earlier estimates will capture its effect on $y_{\bar{d},1}$ directly.

Finally, note that 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, I collect as much data as possible to investigate these potential channels, guided by an analysis of Ecuadorian history. In that sense, the main unobserved historical event for which I do not have data is the agrarian reform. Thus, whenever appropriate, I discuss the latter's implications for the results.

3.3.1 Migration

In the previous section, I assume that migration never occurs. Nonetheless, this is undoubtedly not a plausible assumption. Hence, even with an appropriate exogenous instrument, z_d, the estimation may suffer from a sample bias if migration heavily occurred. Whether the bias attenuates or not the estimate depends on the type of selection (Borjas (1987)). Although, as mentioned above, 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 the other hand, individual-level estimates can deal better with migration complications to the extent that one can adequately link individuals across generations, even if they have moved to other regions. In section 6.5, I discuss how I address this issue.

3.3.2 Other

On top of omitted variables concerns, other econometric threats justify the usage of 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[18]. Second, one can hypothesize that regions that were already poor in colonial times self-select into *concertaje* (so $c_{\bar{d},0} = \delta w_{d,0}$), 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.

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

Thus, I 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, I use information from potential crop yields, exogenously determined by local conditions, to provide consistent regional estimates of the long-run effect of the institution. In Section 5.1, I discuss this approach in detail.

3.4 Data

3.4.1 *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*). I collected at least one book for 9 of the 10 tax administrations that comprise today's Ecuadorian highlands[19].

These records correspond to 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). Each book clearly distinguishes between free workers and those living in *haciendas* and is usually organized as follows. First, they report a list of the names of free workers and then describe each *hacienda's* workforce. Hence, records show 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 differ between free and *hacienda* workers). I manually digitized all this information, accounting for over 2000 pages of tax records.

Historians like Tyrer (1988) and Poloni (2006) 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, I included Oberem's list in my dataset to complement regional accounts. The final database covers virtually all colonial districts (144/147) and totals more than 50,000 tributaries, giving me a complete snapshot of the labor conditions of the indigenous population at the time.

[19] Taxes 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).

Tyrer (1988) mentions that data quality is better for earlier periods since there were stronger incentives to know the actual number of workers and their exact location. Thanks to the availability of books for different years, I can observe if there were dramatic differences in the number of workers and rates over time. Overall, as commented in Section 2, *concertaje* rates did not vary much, although they seemed higher for earlier years. Some differences reflect that officers collected taxes seasonally and sometimes deferred their collection for the next visit. So, whenever two sources for the same province exist, I select the earliest one, following the observation of Tyrer (1988)[20].

For regional estimations, I 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 retain their colonial names facilitates this procedure. The matching process leaves a sample of 125 aggregated districts. I discuss further details in Appendix A. Figure 3 shows a map of the studied region, the location of each tax administration, and the intensity of *concertaje* in each district.

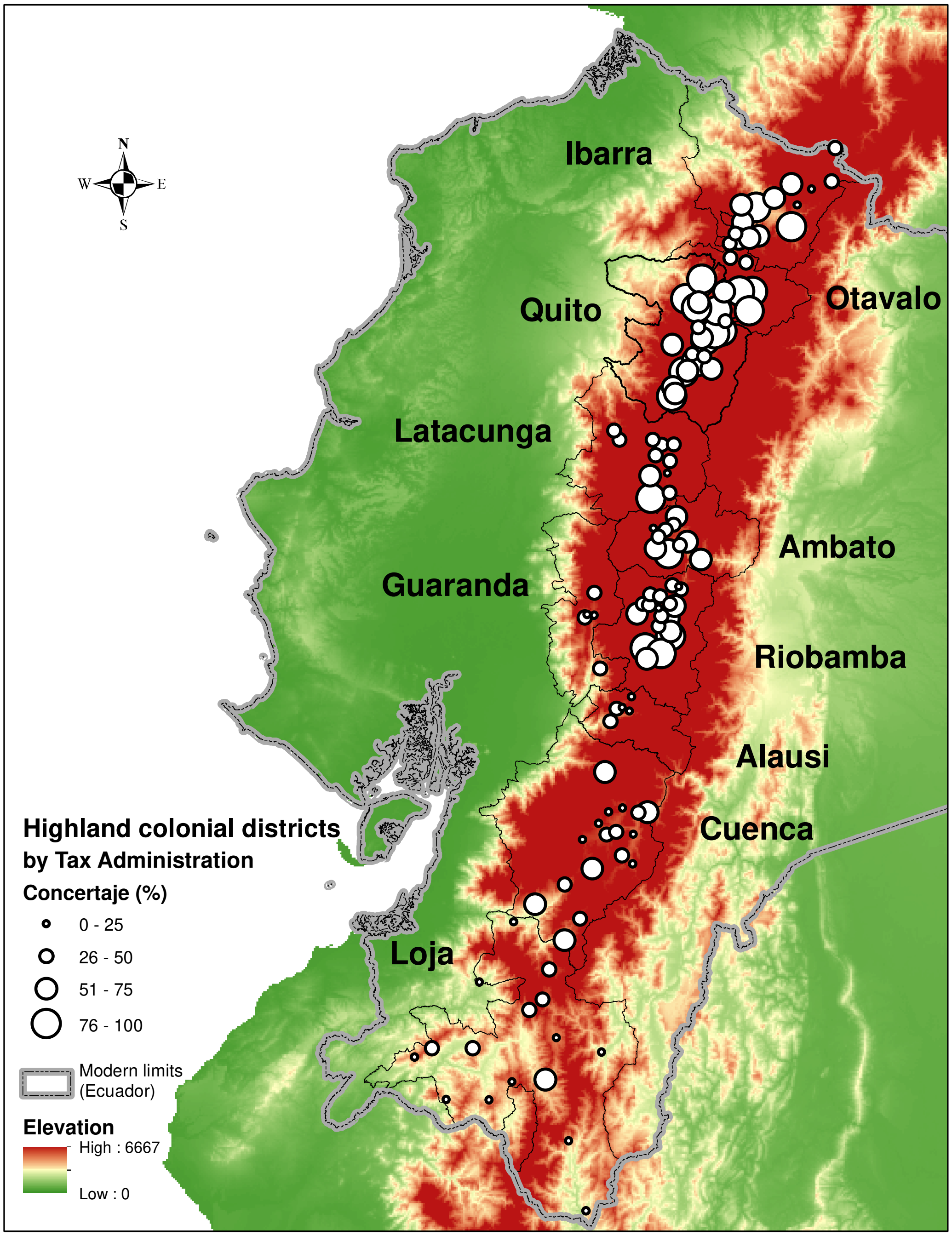
Lastly, regarding the name transcription from historical records, it is important to mention two things. First, by the time these records took place (1780-1830), Catholicism, and thus, the baptism tradition, were fully spread among locals. This implies all indigenous workers already had a surname. Second, these surnames are, in their majority, of indigenous origin. This contrasts with other contexts where labor-coerced individuals shared the surname of the coercer, like slaves in the USA. Moreover, this differentiation reduces surname mismatch concerns. I provide additional details about the treatment of historical surnames in Section 4.1 and Appendix A.

3.4.2 Tax records

The Ecuadorian Tax Authority (*Servicio de Rentas Internas* (SRI)) facilitated anonymized individual tax records. These correspond to the universe of employees' yearly declarations for 2007-2016. Aside from income information, they include the corresponding employer's identifier and characteristics of these firms, like location and industry. In addition, SRI provided me with an undisclosed list of taxpayers, which includes the ID, full name, and the corresponding province of birth of an individual. As an intermediate step, I use this data to match current and historical tax records. Online Appendix A provides additional details.

[20] See online Appendix A for a detailed discussion regarding the quality of each source.

Figure 3: Map of the studied region with its Colonial Tax Administrations.



3.4.3 Regional outcomes

This data comes mainly from censuses. SIISE (2011) provides disaggregated information by parish from 1990 onwards, while I obtained data before 1990 by digitizing tabulates from older censuses. Online Appendices A and B provide additional details and summary statistics, respectively. Data from other regional outcomes are described in the corresponding sections.

4 Individual estimates

4.1 Identification Strategy

To overcome the challenge of linking individuals across several generations, I 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 I link tax records separated by more than 200 years. Precisely, studies of intergenerational mobility have heavily relied on this approach[21].

In particular, matching individuals through surnames possesses several advantages compared to linking them via names (Olivetti and Paserman (2015)) or occupations (e.g., 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 only predict an individual's socioeconomic status via intergenerational effects. A violation of this assumption may occur if, for instance, systematic discrimination via surnames exists. In that sense, it is noteworthy 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-a-vis free workers[22].

[21] See Inoue and Solon (2010) for the estimator's properties. Other studies using this approach in the context of intergenerational mobility include Björklund and Jäntti (1997) (United States), 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).

[22] This is different from ethnic discrimination, as many non-indigenous individuals could have indigenous surnames without identifying themselves as indigenous persons and vice-versa.

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

$$concierto_{i,1800} = \upsilon + \Psi' \mathrm{S}_{i,1800} + \Upsilon' \mathrm{P}_i + \epsilon_{i,1800} \tag{7}$$

where $concierto_{i,1800}$ is a dummy that takes the value of 1 if individual i *circa* 1800 was living in a *hacienda*, 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, predicts the fraction of individuals in the past with surname s that were part of the *concertaje* system. P_i is the province where the worker lived and assumed to be the same as his birthplace.

Then, I use as a second sample current tax records ($2\tilde{0}10$: 2007-2016) and estimate,

$$y_{i,2\tilde{0}10} = \omega + \mathcal{B}_{2\tilde{0}10} \left(\hat{\Psi}' \mathrm{S}_{i,2\tilde{0}10} \right) + \Omega' \mathrm{P}_i + \mathcal{E}_{i,2\tilde{0}10} \tag{8}$$

where $\hat{\Psi}' \mathrm{S}_i^{2\tilde{0}10}$ is the ancestor's *concierto* status, as predicted by (7), although only for matched surnames ($s \in S^{2\tilde{0}10}$); $y_{i,2\tilde{0}10}$ is (log) labor income of individual i, and $\epsilon_{i,2\tilde{0}10}$ is the error term. $\mathcal{B}_{2\tilde{0}10}$ is the TS2SLS coefficient that captures the intergenerational effect of descending from a *concierto*, the main object of interest. Note that by including province-fixed effects, the predicted *concierto* status varies by surname and province. Hence, the match of pseudo-descendants becomes more accurate as the precision of links across generations is presumably higher at a regional level. So, I only consider highland provinces in this part of the analysis. Hence, potential descendants that may have been born out of this region are not part of the estimation. However, this specification includes descendants who have migrated as long as they have moved to another district *within* the same province. Moreover, by including the set of province-fixed effects, I mitigate confounding regional factors, as discussed in Section 3.2[23].

Current tax records do not include any demographic information aside from the province of birth, P_i. Hence, I use classification algorithms based on names to impute gender. I 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. I deflate yearly incomes using the national CPI and compute the average real income from all the periods an individual appears in the dataset (2007-2016) to reduce lifecycle income concerns (Lee and Solon (2009), Black et al. (2011)).

Since historical records are written in "old" Spanish, the match of surnames is flexible. In particular, I apply a cleaning process that associates different surnames with minor variations.

[23]Note that the fixed effects are at a province level, the largest political unit, rather than at the district (parish) level.

For instance, it considers the surnames "Sanchez" and "Sanches" or "Alvares" and "Albarez" to be the same. Moreover, I exclude current surnames with foreign sounds (e.g., KT, TH) from potential matches. This approach allows me to match 89% of individuals and 48% of surnames from historical records. For current tax records, I match 61% of individuals and 20% of surnames from eligible provinces. Appendix A describes additional details.

4.2 Results

Tables 1 and 2 show the results of the abovementioned empirical approach. The former indicates the estimates of the first stage, namely, equation (7), for each of the different second-stage specifications (see below). One can observe that surnames strongly predict an individual's *concierto* status in 1800. Surnames explain 18-29% of the total variation of the *concierto* status (Adjusted R-squared), depending on the specification, and are jointly statistically significant (F Statistic). This reflects the fact that the institution itself had a considerable intergenerational component. However, it also shows that surnames in the past revealed a marked effect of inheritance.

Table 1: Intergenerational Mobility (First Stage): Concierto Status (1780-1830)

	Base Model	Weight Inverse Surnames' Frequency (Colony)	Province Rare Surnames (Colony)	Weight Inverse Concertaje's SD (Colony)	Weight Local Surname Specificity (Colony)
	(1)	(2)	(3)	(4)	(5)
F Statistic	3.0461	6.18	2.5357	3.9559	4.9176
Adjusted R-squared	.1819	.2945	.2426	.2341	.2661
Province FE	Yes	Yes	Yes	Yes	Yes
Observations	44984	44984	16469	43752	44984
N Surnames	3021	3021	2703	2922	3021
Mean Dep. Var.	.423	.423	.4269	.423	.423

Notes: Surnames correspond to their standardized version after using the algorithm described in Appendix A.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2 shows the estimates of equation (8). Column (1) shows the baseline ones, which suggest that, on average, an increase of 10 percentage points (about half a standard deviation) in a surname's *concertaje* rate reduces labor income to descendants by almost 1.7%. Put in another way, the individuals whose ancestors were not coerced at all earn 17% more than those who descend from fully coerced workers. Of course, this is conditional on having a surname listed in the historical records.

The empirical strategy described above heavily relies on the assumption that sharing a

Table 2: Long Run Effect on Descendants (Second Stage): Labor Income (2007-2016)

	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)
Concierto	-.00167***	-.00105**	-.00155***	-.00184***	-.00177***
	(.00043)	(.0005)	(.00041)	(.00042)	(.00038)
Adjusted R-squared	.01	.0032	.006	.0117	.0182
Province FE	Yes	Yes	Yes	Yes	Yes
Observations	762947	762947	431743	683593	762947
N Surnames	2379	2379	2142	2296	2379
Mean Dep. Var.	8.2181	8.2181	8.2901	8.2037	8.2181

Notes: Clustered standard errors by surname in parentheses. Surnames correspond to their standardized version after using the algorithm described in Appendix A.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

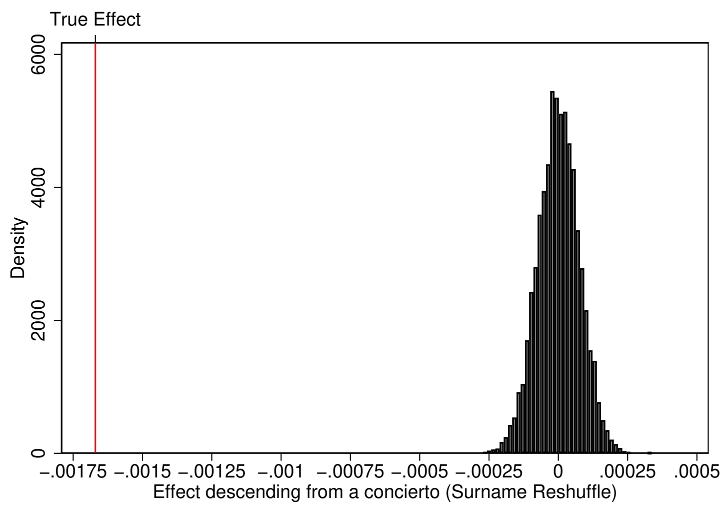
surname implies a family connection. Nevertheless, spurious relations could arise if, for instance, there are erroneous matches or individuals have migrated. Hence, I follow Barone and Mocetti (2020) and apply similar checks to assess the robustness of the results. I present them in Columns (2)-(5) of Table 2. Consistent with the estimation procedure, all the weights and moments employed in these checks are based on the provincial distribution of surnames.

I first consider rare surnames to minimize spurious relations because they are more informative, given that few individuals share them. For instance, in column (2), I present the results when I repeat the TS2SLS procedure but weighting by the inverse of the relative frequency of past surnames. Similarly, in column (3), I report the estimates when I consider only infrequent colonial surnames (<10) in the regression. Both estimates also show a negative and statistically significant effect of descending from a coerced worker. Instead, in column (4), I 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. Indeed, this estimate is the largest of all.

Finally, in column (5), I weigh surnames by their local specificity, defined as the share of a surname in a given province relative to the overall colonial surname distribution. The idea is that more common surnames to certain regions may show more accurate linkages. Hence, if individuals have migrated, as expected, this check will mitigate incorrect linkages from surnames coming from other regions. This specification, along with column (4), leads to larger estimates than the baseline case and is consistent with an attenuation bias due to a mismeasurement of family linkages.

Conversely, in Figure 4, I show the distribution of estimates when I randomly reassign the surnames of current taxpayers and match them with the historical records. If this procedure leads to similar results as Table 2, it certainly invalidates them. If not, it means surnames contain intergenerational linkages' information[24]. I repeat this procedure ten thousand 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. Appendix B shows additional robustness checks.

Figure 4: Estimates when Surnames are Randomly Reshuffled



Economically, the results presented above are of substantial magnitude. A 17% gap in labor income between descendants of *conciertos* and free workers represents a considerable effect given the 200-year span separating both samples. In particular, if we consider the institution itself disappeared 100 years ago and its remnants 50 years later. Hence, if we take a generation to be 30 years, seven have passed from the end of colonial times and two to three after the abolition of *concertaje*, and its effects have not vanished yet. This is even more striking considering I only employ data on formal labor income, making these estimates a lower bound of the actual labor coercion intergenerational effect.

Through the lens of the model, and if we assume socioeconomic status transmits through a first-order autoregressive process (as most studies do), after seven generations, the reduced form estimate is $\mathcal{B}_{\tilde{2010}} = \beta_{1830}\times(\beta^5)$. Although there is no study of intergenerational mobility

[24]Güell et al. (2014) use the same strategy to validate their intergenerational-mobility estimates.

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[25]. Therefore, if we jointly consider these parameters, it suggests that the initial *concierto* shock was indeed massive to a dynasty[26].

Another answer to why we observe persistent effects of coercion after 200 hundred years could be that intergenerational mobility was considerably low, or lower than it is traditionally 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 $\mathcal{B}_{\tilde{2010}} = \beta_{1830} \times (\beta^2)$, in which case *concertaje* generated a 46% initial income gap. This is a plausible explanation since surnames strongly predict the labor coercion condition in 1800, which signals a substantial weight of inheritance factors during that time.

On the other side, 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. In that sense, 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 non-coerced workers as the direct institutional effect rather than differences between ethnic groups (e.g., white or *mestizo* vs. indigenous people)[27]. This result contrasts with Sacerdote (2005), who documents a catch-up between African-Americans' descendants of formerly slaves and non-slaves in the US. He attributes the latter to massive investments in public education, a policy that, as discussed in Section 6.1, was absent in Ecuador.

The results presented in this section pose the existence of an institutional persistence channel not documented before, namely, intergenerational mobility. This 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 blacks in the case of slavery, their descendants carry the consequences even when the forced labor system itself disappears.

[25] See Daude and Robano (2015), Brunori et al. (2013) and Neidhöfer et al. (2017)

[26] As it implies the initial shock generated a 200% income gap between the first generation of descendants from coerced and non-coerced workers. Note that I consider $\mathcal{B}_{\tilde{2010}}$ to be 0.167, instead of 0.00167, to both map it to the model and facilitate interpretation.

[27] In turn, matched workers earn in average 5% 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.

Moreover, these results suggest that the effects of labor coercion could go beyond the specific places where the institution existed, as they 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 district or regional level, as this work does in the following sections. In particular, intergenerational mobility can play like an amplifying mechanism, allowing institutions to display 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 immigrants. I postpone this discussion to Section 6.5.

5 Regional Estimates

The estimates from the previous section show quantitatively significant adverse effects of (pseudo) descent from a coerced worker. Nonetheless, the data requirements to produce such an estimation are demanding. Hence, 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 to understand how the institution may have shaped regional factors. Thus, I provide an identification strategy to estimate such effects in this section.

5.1 Empirical Approach

Due to both 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 yields. I 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 project provides estimates of potential crop yield (ton per hectare) at a grid resolution of $5' \times 5'$ (approximately 100 km^2). Crop yields can be computed for low, medium, and high inputs. Moreover, one can select between rain-fed and irrigation water supply sources. Although, in the interest of replicating historical conditions, I chose a combination of low inputs and rain-fed water supply[28].

One prominent feature of the GAEZ 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. None of the estimates of potential yield reported by the GAEZ project use information on actual or historical production. Instead, they are based on detailed models incorporating agro-climatic conditions to predict potential crop yields. These climate characteristics include, among others, temperature, radiation, and mois-

[28]In online Appendix B, I show that results are robust to selecting intermediate inputs.

ture regimes. Nunn and Qian (2011) and Costinot and Donaldson (2016) further explain these data.

I use the GAEZ data to test whether districts with a comparative advantage in producing labor-intensive crops coerced higher rates of indigenous labor. Specifically, I 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, I first compute the corresponding average potential yields ($\bar{p}^i_d$) using geospatial software to take their log difference posteriorly[29]:

$$z_d = log(\bar{p}^i_d) - log(\bar{p}^j_d), i \neq j, i \in \{maize\}, j \in \{potato\}$$

I use this instrument to predict the historical rates of *concertaje*. However, since 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, I first estimate a (fractional) probit model for *concertaje*, namely,

$$concertaje_{d,1800} = \pi + g_{1800} z_d + \Pi' \mathrm{X}_d + v_{d,1800} \tag{9}$$

and then estimate an IV regression of equation (10), but 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 two-stage least square (2SLS) estimator, and it 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.

$$y_{d,t} = \phi + \mathbf{B}_t concertaje_{d,1800} + \Phi' \mathrm{X}_d + v_{d,t} \tag{10}$$

Since relative crop productivity may reflect differences in other geographic features that directly impact economic development, I control for a comprehensive set of exogenous variables. These include (center) latitude, longitude, and altitude, and (mean) elevation, slope, precipitation, and temperature. Furthermore, I control for a district's (mean) overall agricultural suitability by employing an agro-climatic index reported by the GAEZ-FAO. In that

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

sense, while 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, X_d, in both (10) and (9). However, due to their skewness, I take the natural log of them[30].

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, which could impact workers' health, and, thus, their productivity. However, this is not worrisome given that maize and potatoes are high-calorie food sources (US Department of Agriculture (2016)). Plus, 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 seems to be more the consequence of labor coercion rather than the unavailability of certain crops. In fact, Suárez (1942) clearly distinguishes significant food expenditure differences between rural workers in *haciendas* vs others. This issue is particularly relevant given the pervasiveness of infant malnutrition in Ecuador, which is more prevalent in the Highlands (Gutiérrez et al. (2017)). As malnutrition typically reflects in low height, in online Appendix B, I show estimates of *concertaje* on the percent of stunting children (Freire and Bacallao (1992)). However, I find no (statistically significant) effect. Herein, the ubiquity of maize and potatoes, in addition to the historical fertile conditions of the region, suggest the causes of malnutrition are other.

Indeed, contrary to other regions like Peru, the Ecuadorian highlands 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, in online Appendix B, I show the correlations between the instrument and (log) population levels for different periods (1600, 1800, and 1950), finding no significant relation between both. Moreover, I also show that other initial colonial conditions (tax per capita around 1600), which could have affected economic organization later on, are unrelated to the

[30]See online Appendix A for extra details about these geographical variables.

instrument. However, this result should be taken cautiously since early population data is of low quality (Ortiz de la Tabla Ducasse (1980)). However, the lack of population differences contrasts with Nunn and Qian (2011) evidence that the easiness of potato adoption accelerated urbanization in Europe. I conjecture this did not occur in native-potato regions like Ecuador since seasons strongly condition European agricultural production. In contrast, most crops in this region are sowed and harvested year-round (Tyrer, 1988, p. 76). Lastly, other channels potentially impacted by the instrument, like gender roles via plough practices, seem less of a concern as maize and potatoes are both "plough-negative crops" (Alesina et al. (2013)), so, if any, they should positively impact female labor force participation.

Since FAO-GAEZ crop suitability metrics are plausibly exogenous, they have been widely used in the literature. For example, Easterly (2007) uses the relative suitability of wheat versus sugarcane as an instrument of inequality to test its effect on economic development. However, he does not study its direct effect on the formation of forced labor institutions as I do. Lowes and Montero (2021b) instead use the relative suitability of cassava relative to millet as an instrument for medical campaigns in Africa to understand its current effects on medical trust. Other works following a similar approach are Acharya et al. (2016), who study the political legacy of slavery in the United States, and Bobonis and Morrow (2014), who investigate labor coercion's role in human capital accumulation in Puerto Rico, and Bustos et al. (2016) that examines the role of agricultural productivity in structural transformation in Brazil.

5.2 Results

Table 3 shows the IV, and OLS estimates of (10) 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, ranges between 11-15, depending on the specification, which suggests a relatively strong first stage, reducing weak instrument concerns. It also confirms the hypothesis that crop suitability did determine the intensity of labor coercion. Since spatial dependence naturally arises in this setting, I also report Conley (1999) standard errors on top of robust ones[31].

The IV coefficient reported in column (1) of Panel A shows that the long-run effect of *concertaje* on extreme poverty is around 0.4. The statistically significant result suggests that, on average, an increase of 10 percentage points (pp) in the rate of *concertaje* circa 1800 (about

[31] Conley 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. This poverty index, known as Unsatisfied Basic Needs (*Necesidades Básicas Insatisfechas*), is commonly used in Latin America. It classifies a household as poor or extremely poor if one or more than one of the five proposed criteria is satisfied, respectively.

Table 3: Long-run Effect: Extreme Poverty (1990)

	Base Model	Proxy Output Per Capita (Colony)	Province Fixed Effects (Colony)	Province Capital (Colony)	Fraction Indigenous (Colony)	Number Taxpayers (Colony)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Concertaje	.4207	.432	1.0085	.3819	.4613	.4443
	(.1976)**	(.2017)**	(.2956)***	(.2025)*	(.2578)*	(.2107)**
	[.1903]**	[.2029]**	[.415]**	[.2111]*	[.273]*	[.2156]**
Panel B: First Stage						
Instrument	101.8123	101.6979	102.6682	101.6014	102.2855	102.1098
	(26.4564)***	(26.6423)***	(29.0584)***	(26.9144)***	(30.7597)***	(27.4322)***
F Statistic (Robust)	14.8095	14.5707	12.4832	14.2505	11.0577	13.8552
Panel C: OLS						
Concertaje	.0798	.0823	.2271	.0455	.0718	.0918
	(.0729)	(.0738)	(.0754)***	(.07)	(.076)	(.0735)
	[.0812]	[.0821]	[.0747]***	[.0816]	[.0787]	[.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

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include, in logarithms, (center) altitude, latitude longitude; (average) elevation, slope, total annual precipitation, mean annual temperature, and agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

half a standard deviation) increased extreme poverty in 1990 by 4 pp. To put it another way, a district with all its workers attached to *haciendas* around 1800 nowadays has an extreme poverty rate 40 percentage points higher than one where *concertaje* was wholly absent.

In column (2), I control for the proxied colonial output *per capita* discussed in Section 2.2, i.e., tithe *per capita*, to see if the initial (1800) economic conditions still affect today's outcomes. Nevertheless, its inclusion barely changes the long-run effect of *concertaje*. As discussed earlier, relatively productive agricultural regions were where *concertaje* concentrated. This result further reduces the concerns of reverse causality. Instead, columns (3)-(6) report the estimates when I add other controls. Overall, the point estimates for the IV coefficients remain close to the base model, except in column (3), which is larger, when I include colonial province fixed effects.

These fixed effects capture other time-invariant unobservable regional differences, although when included, I omit geographical controls to avoid overfitting the regression. More importantly, since tax records correspond to colonial provinces, adding fixed effects helps to control for any systematic misreport of *concertaje* by the tax administration. Including these fixed effects, without geographical controls, reduces the instrument's strength, as there is less crop variation within provinces. However, the OLS estimates become larger (and statistically significant), suggesting the likely existence of measurement error in *concertaje* data.

Column (4) reports the results when I control if the town was the capital of the colonial tax district[32]. In this case, the estimates are smaller but still statistically significant. Hence, including historically relevant administrative centers is not driving the results. In column (5), I add as control the fraction of the indigenous population *circa* 1780 to observe if *concertaje* is capturing the effect of the institution instead of differences in the colonial distribution of the indigenous population. However, once again, the results remain mostly the same.

Finally, in column (6), I report the estimates when I control for the (log) number of taxpayers. The premise is the potential heterogeneous effect that *concertaje* may have on districts with different population sizes. However, by adding this control, the results become slightly larger and continue to be in the range of the previous estimates.

Table 4 mimics the estimates presented in Table 3 but shows the results when average night light intensity is the dependent variable. The latter has been established as a good proxy for income, especially useful in settings where no disaggregated income information exists (Michalopoulos and Papaioannou (2013a); Michalopoulos and Papaioannou (2013b)). Hence, given that the census does not report income data, I use night light intensity as a robustness check for the long-run effect of *concertaje*. I obtain night light intensity from US Air Force weather satellites from National Geophysical Data Center (2010). Henderson et al. (2012) provide more details about this data and its relation with income.

I use the satellite light data from 1992 (the earliest available) to compare the results to those in Table 4. To facilitate the interpretation, I take the log of the average satellite night[33]. The baseline estimates (column (1)) suggest that an increase of 1 percentage point of *concertaje* in 1800 reduces average night light intensity by 4.6%. Interpreting the magnitude of the effects must be taken cautiously due to the mapping between light intensity and income. However, a 4.6% drop in (proxied) average income is still a sizable economic effect. The rest of the IV estimates in Table 4 show similar magnitudes to the baseline and are statistically significant. Moreover, they are consistent with the results in Table 3.

In all columns of Tables 3 and 4, 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, when including regional dummies that capture some of these omitted factors, the effects are larger. Nev-

[32]The heads of *Corregimientos*, according to the 1809 territorial division, as reported by Morelli (2005).

[33]Similar to Michalopoulos and Papaioannou (2013b), I add a small constant, 0.01, to keep in the sample observations with average 0 light intensity.

Table 4: Long-run Effect: Night Light Intensity (1992)

	Base Model	Proxy Output Per Capita (Colony)	Province Fixed Effects (Colony)	Province Capital (Colony)	Fraction Indigenous (Colony)	Number Taxpayers (Colony)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2SLS						
Concertaje	-.046	-.0474	-.1524	-.0437	-.0516	-.0533
	(.0225)**	(.023)**	(.0405)***	(.023)*	(.0304)*	(.0244)**
	[.0217]**	[.0223]**	[.0531]***	[.0217]*	[.0315]	[.0245]**
Panel B: First Stage						
Instrument	101.8123	101.6979	102.6682	101.6014	102.2855	102.1098
	(26.4564)***	(26.6423)***	(29.0584)***	(26.9144)***	(30.7597)***	(27.4322)***
F Statistic (Robust)	14.8095	14.5707	12.4832	14.2505	11.0577	13.8552
Panel C: OLS						
Concertaje	-.0048	-.0051	-.0345	-.0027	-.0038	-.0083
	(.008)	(.008)	(.0092)***	(.0082)	(.0086)	(.008)
	[.0091]	[.0092]	[.0112]***	[.0091]	[.009]	[.0098]
Geo. Controls	Yes	Yes	No	Yes	Yes	Yes
Observations	125	125	125	125	125	125
Mean Dep. Var.	-.3024	-.3024	-.3024	-.3024	-.3024	-.3024

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include, in logarithms, (center) altitude, latitude longitude; (average) elevation, slope, total annual precipitation, mean annual temperature, and agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

ertheless, this issue becomes more acute through the model lens if a reversal of fortune has happened, as discussed above, in the Ecuadorian highlands. Something else to point out is the large confidence intervals. Although typical in IV estimation, this is undoubtedly partly due to the small number of observations. Nonetheless, it can also result from a weak first stage (Andrews et al. (2019)). Thus, to verify if appropriate inference is conducted, I show the correspondent Anderson Rubin (AR) confidence intervals of Table 3 in Appendix B. While somehow larger, they still confirm the results presented in this section.

Finally, I estimate the effect of *concertaje* for different years to see if its effect has vanished over time. Figure 5a shows that the effect of *concertaje* on extreme poverty has decreased. Considering the recent governments' policies to reduce poverty, one can expect these efforts to be more concentrated in places with higher rates, some historically affected by *concertaje*. Moreover, this pattern is consistent with the model presented in Section 3.1, which sees labor coercion as a massive historical shock that slowly converges over time. Indeed, other studies like Valencia Caicedo (2018) and Iyer (2010) have documented similar patterns for other colonial interventions. In contrast, Figure 5b shows that the effect on nightlight intensity displays no such trend, likely because this metric is not a public policy objective.

In summary, the estimates reported in this section reveal a quantitatively significant long-run effect of *concertaje* on today's economic prosperity. These results are consistent with the historical narrative of several scholars who have discussed the adverse impacts of *haciendas* on the rural workers of Ecuador (Oberem (1981a)). Furthermore, they align with 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 mechanisms. Precisely, I discuss them in the next section.

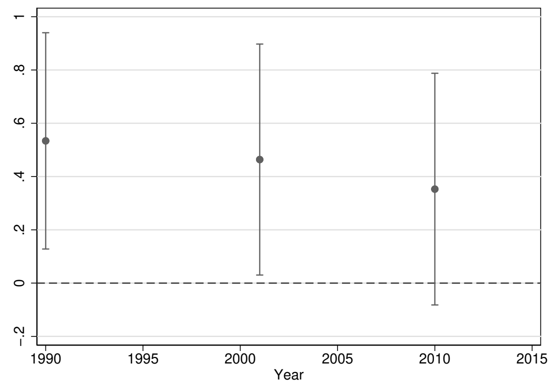
6 Mechanisms

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

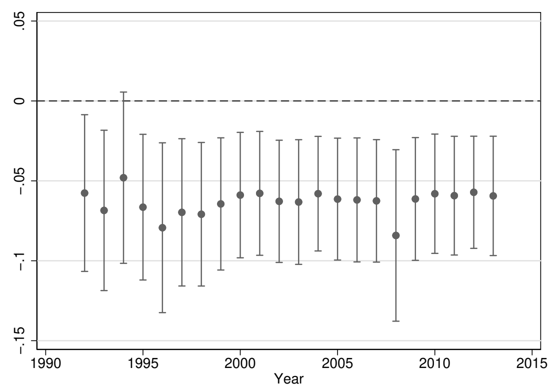
The main argument is that the long-run economic effects of labor coercion are better understood if seen as a barrier to accumulating human capital and reallocating labor across sectors or regions. In that sense, the adverse effects of *concertaje* are explained because it was profitable for landlords to maintain the system while agriculture was the main economic activity. However, by doing so, they blocked and neglected the required investments (like the provision of public schooling) to develop other economic activities. At the same time, the system limited workers' mobility precisely because it would have supposed a reduction in the landlords' economic power. This, in turn, slowed the process of structural transformation and limited migration.

Concertaje, a profitable and likely productive system during colonial times, became an obstacle to economic development between the mid-19th and 20th centuries. This reversal of fortune suggests that during the transition from an agricultural economy, *concertaje* became a significant distortion in the labor market, reducing the incentives to provide schooling and affecting labor reallocation. As landlords retained workers in their *haciendas*, they created barriers to accumulating human capital and reduced the incentives to provide public goods. Moreover, due to the difficulty of escaping from *haciendas*, most workers in regions with

Figure 5: *Concertaje's* effect on Long-run Economic Development for different years



(a) Extreme Poverty (IV)



(b) Night Light Intensity (IV)

Notes: regressions as column (1) in Table 3 and Table 4, respectively. Confidence interval: 95% (robust standard errors). See Section 3.4.3 for the definition of extreme poverty rates. Satellite night lights data from National Geophysical Data Center (2010) a high prevalence of *concertaje* remained in the agricultural sector. Accordingly, I test the effects of *concertaje* on human capital and sectoral composition, finding noteworthy effects on both, especially in earlier periods.

I also test other mechanisms the literature has discussed, like inequality and public goods provision. I 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 land inequality and *concertaje* being strongly linked, inequality alone is not associated with lower levels of contemporary economic development. Hence, I interpret these results as a generalization of the overall adverse effects of coercion instead of its operation through inequality. Finally, I also show that *concertaje* has negatively affected migration. However, I also discuss its role in mitigating the adverse effects of the institution.

6.1 Human capital accumulation

In addition to indebted workers, their wives and sons 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, either because of inherited or newly acquired debts. The early disposition of children to work limited their education, if any was available. Accordingly, in 1934, 80% of indigenous workers living in *haciendas* were illiterate, as opposed to 40% of rural workers (Oberem, 1981a, p. 323).

In that spirit, I test as one of the potential channels of persistence of *concertaje* its effects on human capital using three metrics of education: (1) illiteracy rates, (2) average years of education, and (3) school enrollment. The data come from the 1962 census, the first year for which all this information is available. I report the results in Table 5.

Column (1) shows the results when the dependent variable is the illiteracy rate. The coefficient (statistically significant) suggests that, on average, an increase of 10 percentage points in *concertaje* during colonial times increased the illiteracy rate of a district by 3.8 percentage points in 1962. I obtain the same negative relation between *concertaje* and human capital if, instead, I use as a dependent variable (log) years of school, as reported in column (2). The point-estimate, -0.011, means that an increase of 10 percentage points 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 six years in a district in 1962 was around 2.

Jointly, these results stress the substantial adverse effects of labor coercion on education.

Table 5: Mechanisms: Human Capital

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

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include, in logarithms, (center) altitude, latitude longitude; (average) elevation, slope, total annual precipitation, mean annual temperature, and agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

The elevated illiteracy rates observed in districts where *concertaje* was prevalent reflected the poor incentives individuals had to attend school and the impediment they faced due to work obligations. Wherever *haciendas* had a strong presence, children had only a few alternatives other than to start working early there, either because they were indebted soon enough or they had inherited their parents' debts.

On the other side, landlords had strong incentives to limit the education of their workers as a strategy to preserve *concertaje*. Since illiterates had no right to vote until 1978 (Corkill (1985)), landlords blocked their participation in public affairs by limiting their education. Hence, they had little voice in matters like the provision of public goods, such as education infrastructure. Moreover, illiterates could be more easily retained by altering their debt accounts. So, *concertaje* did not only reduce the demand for schooling but also generated strong incentives in landlords to block the formal education of their workers.

The concerns about the low education of *conciertos* were acknowledged, and historically, there were several legal initiatives to improve this 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 local authorities made it difficult to accomplish all these resolutions. Accordingly, as shown in column (3), *concertaje* harmed school attendance in 1962.

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 (Valencia Caicedo (2018)), these results show the other flip of the coin: extractive institutions, such as *concertaje*, instead created educational barriers and distorted incentives to invest in human capital, negatively affecting economic development[34].

6.2 Sectoral composition

Nowadays, the more significant disparities in productivity across countries are in the agricultural sector (Caselli (2005), Restuccia et al. (2008)). At the same time, as documented by Restuccia et al. (2008), developing countries allocate more employment to agriculture than developed ones. This empirical finding is attributed to Schultz (1953), who argues that developing countries face a "food problem" because they have to allocate much of their labor to produce food for subsistence needs because of their low productivity.

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. I test this hypothesis by estimating the effects of *concertaje* in sectoral composition, specifically in the percent of workers in (1) agriculture, (2) manufacturing, and (3) wholesale and retail sector in the year 1990, the first for which there is disaggregated sectoral data. I show the results in Table 6.

In column (1), the effect, statistically significant at 10%, implies, on average, that an increase of 10 percentage points (pp) in the rate of *concertaje* increased by 3.7 pp the share of workers allocated in agriculture. Critically, the overall agricultural suitability does not drive this result, as the regression also includes this control. Instead, in columns (2) and (3), I report the effect of *concertaje* 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 despite showing a negative effect. I confirm these results via individual estimates (Appendix B) by showing that descendants of *conciertos* are more likely to work in the primary and secondary sectors and less likely to do so in the service one.

[34]Like in Valencia Caicedo (2018), I also find the effects on education have muted over time (see Appendix B).

Table 6: Mechanisms: Sectoral Composition

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

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include, in logarithms, (center) altitude, latitude longitude; (average) elevation, slope, total annual precipitation, mean annual temperature, and agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

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 reallocating 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)). The author shows that these differences in sectoral composition occurred as early as the mid-nineteenth century. For instance, using census data for 1862, he reports Otavalo had 38% of its population working in the agricultural sector, while Cayambe had 82%.

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 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 reallocating labor and limiting the development of other economic activities. These distortions, later on, had critical implications for the economic performance across regions.

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 an unequal society. This situation, they argue, posteriorly affected economic development as this elite extracted rents from natives via forced labor or other forms of taxation while concentrating large amounts of wealth. In that sense, Ecuador is not an exemption to this analysis, 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*, I mainly focus on land inequality, as large rural states heavily supported its existence. I first compute land Gini using the 1974 agricultural census, the first one to report land distribution data at a parish level. However, as it was a period in which agrarian reform was starting to take effect, it is likely that it still reflected much of the colonial heritage. Thus, I 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 7 shows the results.

Table 7: Mechanisms: Inequality

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

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include, in logarithms, (center) altitude, latitude longitude; (average) elevation, slope, total annual precipitation, mean annual temperature, and agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

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 percentage points (pp) in *concertaje* in 1800 increased Land Gini in 1974 by 6.5 pp. Similarly, the coefficient's magnitude for land inequality in 2000, shown in column (2), are close to those for 1974. The fact that land conditions have remained almost the same 25 years after the agricultural reform also reflects the persistence of *concertaje* via land inequality.

These estimates are consistent with part of the Engerman and Sokoloff (1997) hypothesis that forced labor institutions, like slavery or *concertaje*, induced high levels of inequality and aligned with the empirical evidence presented by Nunn (2008a) about the positive correlation between slavery and inequality in the United States. However, evidence for the second part of their hypothesis, the effect of inequality and development, is mixed[35].

For example, Acemoglu et al. (2008) report a positive correlation between historical and current land inequality and contemporary economic development in Cundinamarca, Colombia. Instead, Nunn (2008a) reports that initial land inequality is uncorrelated with current income in the United States. Similarly, in Appendix B, I 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), I 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 percentage points (pp) in *concertaje* reduces consumption Gini by almost 0.8 pp, a minor effect compared to the one of land inequality. Nevertheless, as shown in Appendix B, the consumption Gini is negatively associated with consumption poverty. So overall, *concertaje* has significantly affected economic inequality, but its persistent effects on poverty do not operate through that channel.

In the long run, labor coercion could affect economic development not only via land or consumption inequality but also through its disparate effects on political representation. For example, Acemoglu et al. (2008) emphasize the role of political inequality instead of economic one in explaining differences in economic performance. This is a channel that is relevant in this context, considering that 46% of the total highland population lived in *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 interference in local political affairs (Palomeque, 1990, pp. 155-168); (Hurtado, 1985, pp. 45-64), in particular, because illiterates, could not vote. So, a reduced minority decided over local affairs instead of the vast

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

majority of illiterate workers linked to the *hacienda* system. Moreover, as 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 ones (Quintero López (1987)).

Even though *concertaje* did not lead directly to greater political inequality, land inequality could have undoubtedly reinforced the link between the two through education. For instance, Galor et al. (2009) argue that land inequality can adversely affect economic development by blocking human capital-promoting institutions. Mainly because for landlords, as opposed to capitalists, the returns on education on its main economic activity (agriculture) are negligible. While the low educational levels of *conciertos* certainly signal its low returns in *haciendas*, the results presented here also emphasize the direct adverse effects of labor coercion, which might or might not be accompanied by land inequality.

For example, *concertaje* was almost absent on the Coast of Ecuador but exhibited land concentrations similar to the Highlands[36]. Due to a small labor force and the risks of tropical diseases, the Coast was not heavily populated during colonial times[37]. Agricultural activities there were not of particular interest until the prices of cacao and the availability of some labor force generated enough conditions to attract new colonizers. In this case, land availability and a reduced population created a different landlord class more prone to wage compensation. For other historical reasons, the Coast also showed high land inequality but not the adverse effects of labor coercion. Thus, the inconclusive empirical relation between inequality and economic development is probably due to the lack of understanding of the mechanisms that lead to inequality. In some cases, inequality might capture the effect of labor-coercive institutions, while in others, it might reflect other historical processes.

6.4 Public goods provision

Instead of suggesting a negative relation between the historical *hacienda* system and contemporary economic development, Dell (2010) states that under 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, she proposes an alternative hypothesis to Engerman and Sokoloff (1997) theory to explain the development path in the Americas, where she emphasizes the potential positive role landlords had in providing public goods, like roads, and securing property rights.

Accordingly, I test if places with higher rates of *concertaje* also show a higher presence of public goods, which I proxy through road density. Specifically, I follow the same strategy

[36]The land Gini for the Highlands and the Coast in 1979 was 0.64 and 0.62, respectively (Barsky (1984)).

[37]Salmoral (1994) reports that 88% of the total population *circa* 1784 lived in the Highlands.

to measure the latter as Dell (2010) and compute the total length of roads (2010) in a given district and divide it by its surface area. Appendix A explains additional details. Table 8 shows the results.

Column (1) shows the effect of *concertaje* on (log) road density. In this case, an increase of 10 percentage points in the historical rates of *concertaje* led to a decrease of 2.3% in road density. These results are contrary to those presented by Dell (2010), who hypothesizes that large landowners with secure property rights had higher incentives to invest in public goods. In contrast, I observe that public goods, reflected in road density, are negatively related to forced labor in *haciendas*.

Table 8: Mechanisms: Public Goods Provision

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

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include, in logarithms, (center) altitude, latitude longitude; (average) elevation, slope, total annual precipitation, mean annual temperature, and agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Contemporary data on roads might reflect quite different conditions from historical ones due to recent substantial government investments in infrastructure. To see if that is the case, I use data from the 1974 agricultural census to compute, for each district, the fraction of farms further than 5km to a passable road. The results are shown in column (2). Once again, the IV estimates suggest that higher rates of *concertaje* are associated with fewer public goods. In this case, an increase of 10 percentage points of *concertaje* increased by around 6 percentage points the share of farms whose passable roads are further than 5km. If landlords in the Ecuadorean highlands produced to sell to local markets, why did they not invest or lobby for roads?

One explanation is that during most of the 19th century, the State critically relied on unpaid draft labor for public works (Ackerman (1977)). Since *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 their workers' disposal (Palomeque, 1990, pp. 181-182). As *conciertos* were an asset, landlords limited their labor supply, even to activities that could have benefited them, 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 about *haciendas* as the principal obstacle for 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 their best interests in more infrastructure, landlords also seemed careful about retaining their workers. A temporary assignment to road construction implied more chances to escape and a diversion from agricultural duties. Then, it is unclear whether a significant provision of public goods could exist in districts with more *haciendas*. Moreover, labor abundance allowed landlords to transport objects without the need to invest in transportation, as exemplified in 1925 when 3000 indigenous workers mobilized electric equipment by 150 km. (Albornoz, 1971, p. 80).

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 to accomplish in places with less State presence. Especially if we consider the large number of illiterates living in *haciendas* who could not vote and, thus, 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. Instead, it seems the opposite happened.

6.5 The role of 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 poses certain limitations. Hence, I return to individual-level estimation, although with some modifications. While the identification strategy described in Section 4 exploits historical linkages via combinations of provinces and surnames, an alternative is to match surnames indistinctly to their birth province. This approach can hint at the potential differential effects of being born in or out of the *concertaje* region (i.e., the Highlands) despite having the same pseudo-ancestors. Under this specification, one can compare, nowadays, 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. 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. Hence, to mitigate these concerns, I again report results considering the same battery of robustness checks as above, although employing the overall distribution of colonial surnames. I also use present-day surname local specificity indexes instead of the historical ones, given that I do not have information about the historical distribution of surnames outside the Highlands. Table 9 shows the results.

Table 9: Long Run Effect on Descendants: Labor Income (2007-2016). All Provinces

	Base Model	Weight Inverse Surnames' Frequency (Colony)	Rare Surnames (Colony)	Weight Inverse Concertaje's SD (Colony)	Weight Local Surname Specificity (Present)
	(1)	(2)	(3)	(4)	(5)
Concierto	-.00048*	-.00046*	-.00046*	-.00051**	-.00104***
	(.00026)	(.00025)	(.00028)	(.00025)	(.00025)
Adjusted R-squared	.0182	.0226	.0245	.0182	.0183
Province FE	Yes	Yes	Yes	Yes	Yes
Observations	1978884	1978884	571459	1978884	1978884
N Surnames	2571	2571	1629	2571	2571
Mean Dep. Var.	8.1133	8.1133	8.1334	8.1133	8.1133

Notes: Clustered standard errors by surname in parentheses. Surnames correspond to their standardized version after using of the algorithm described in Appendix A.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

In this case, the estimates are smaller than those in Table 2. 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 I correct the accuracy of linkages based on the locality of surnames (Column (5)). Hence, these estimates suggest that individuals born outside the *concertaje* region are less affected by the coercion status of their ancestors. Indeed, Table 10 confirms this result. There, I report estimates when I include in the estimation the interaction between a surname's *concertaje* rate and a dummy that takes the value of 1 if the individual was born in a Highland province and 0 otherwise[38]. These results prove that the most considerable effect is on descendants born in the Highlands.

Table 10: Long-Run Effect on Descendants: Labor Income (2007-2016). Regional Differences

	Base Model	Weight Inverse Surnames' Frequency (Colony)	Rare Surnames (Colony)	Weight Inverse Concertaje's SD (Colony)	Weight Local Surname Specificity (Present)
	(1)	(2)	(3)	(4)	(5)
Concierto	-.00015	-.00025	-.00018	-.00011	-.00043
	(.00027)	(.00029)	(.00035)	(.00027)	(.00033)
ConciertoxHighlands	-.00066*	-.00044	-.0006	-.00078**	-.00107**
	(.00035)	(.00034)	(.00039)	(.00035)	(.00042)
Adjusted R-squared	.0183	.0227	.0245	.0182	.0184
Province FE	Yes	Yes	Yes	Yes	Yes
Observations	1978884	1978884	571459	1978884	1978884
N Surnames	2571	2571	1629	2571	2571
Mean Dep. Var.	8.1133	8.1133	8.1334	8.1133	8.1133

Notes: Clustered standard errors by surname in parentheses. Surnames correspond to their standardized version after using the algorithm described in Appendix A.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

These differences could be attributed to being raised in a location with fewer economic distortions. Nonetheless, they also indicate that migration could mitigate the adverse effects of labor coercion, even though *concertaje* itself could have limited mobility. To understand this channel further, I estimate its effect on migration. Since personal tax records can be matched with those of their employers, I assume an individual has migrated if he works for a firm registered in a province different from where he was born. Table 11 shows the results[39].

In this case, being a pseudo-descendant of a *concierto* has a detrimental effect on the probability of migration. This result holds when I also control for working province fixed effects (Column (2)), and once again, I show there is a differential and larger effect for descendants born in the Highlands (Column (3)). From the first two columns, the estimates suggest that an increase of 10 percentage points in a surname's *concertaje* rate reduces the probability of migration by 0.14 to 0.16 percentage points. This effect is undoubtedly small, given that almost 43% of the sample has migrated. However, aside from the potential measurement error

[38]Since I do not have historical records for the highland province of Bolivar, its dummy value is cero.

[39]One caveat is that firms could be registered in a region different from where they operate. Still, this imperfect metric could hint at the effect descending from a *concierto* could have on migration.

Table 11: Long-run Effect on Descendants: Migration (2007-2016)

	All Provinces	Desination Fixed Effects	Interaction Highlands
	(1)	(2)	(3)
Concierto	-.00014**	-.00016***	0
	(.00006)	(.00005)	(.00004)
ConciertoxHighlands			-.00033***
			(.00008)
Adjusted R-squared	.2398	.4091	.4092
Province FE	Yes	Yes	Yes
Observations	1978884	1978884	1978884
N Surnames	2571	2571	2571
Mean Dep. Var.	.4298	.4298	.4298

Notes: Clustered standard errors by surname in parentheses. Surnames correspond to their standardized version after using the algorithm described in Appendix A.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

in the migration status, it implies that mobility restrictions created by the institution still exert an effect nowadays.

This result is unsurprising given that it was indeed more difficult for coerced workers to move across regions. However, it is curious that the effect is so small. One explanation is that we observe current migration rates rather than historical ones. However, this can also be explained by other historical factors that could have mitigated migration barriers. For instance, Galán (2020) documents that in Colombia, descendants of land recipients during the agrarian reform were more likely to migrate despite restrictions on land sales. The author explains that a reduction in credit constraints causes this due to an asset shock. Following the same logic for Ecuador, it is likely that some descendants of *conciertos* benefited from land grants during the agrarian reform. Thus, this could have facilitated migration, even though the required authorization to buy, rent, and sell the granted properties may have somehow limited it (Barsky, 1984, pp. 338-343). In Appendix B, I use (albeit imperfect) imputed migration rates from population data to suggest this seems to be the case.

In that sense, migration could be seen as a channel of *un-persistence*, particularly in historical labor coercion. The Great Migration in the USA 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 USA (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, I hand-collected historical records *circa* 1800 to document the long-run effects of *concertaje*, a forced labor institution in the Ecuadorian highlands, at regional and individual levels. For the latter, I match current administrative records with historical ones via surnames to document the long-run intergenerational effects of coercion. I show that an increase of 10 pp in a surname's *concertaje* rate reduces the labor income of pseudo-descendants by 1.7%. The results are robust to several specifications, including controlling for the accuracy of pseudo-linkages assumed by surnames. As historical records corresponded to indigenous workers in 1800, these estimates capture the institutional effect of labor coercion rather than ethnic differences.

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

Due to the close relationship between *concertaje* and the labor market, I investigate if this labor-coercive institution created barriers to human capital accumulation and generated distortions that affected the sectoral composition of labor. I estimate that, on average, an increase of 10 pp in the rate of *concertaje circa* 1800 increased the illiteracy rate of a district in 1962 by 4.7 pp and the share of workers in agriculture by 3.9 pp. These results support the role of historical institutions as a cause of persistent market distortions.

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

Finally, using data on contemporary roads, I report a negative effect of *concertaje* in 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 Dell's hypothesis that landlords shielded individuals against extractive institutions. In the case of Ecuador, where the Spanish depleted gold and silver mines very early, it is possible to have a more homogenous setting to study the effects of institutions promoted by landlords. Within that setting, there is more evidence favoring the historical analysis that *haciendas* constituted extractive institutions that affected economic development. Overall, I emphasize that it is labor coercion itself, either through *mita*, *concertaje*, or slavery, 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 (e.g., agriculture) and a specific place, becoming a barrier to reallocating labor across sectors and regions. Moreover, coercers perpetuate their workers' conditions by pushing their workers' outside options low enough. They typically achieve this via legal channels (e.g., debtor's prison) and by limiting and 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. As illiteracy relegated workers from political participation via voting, this perpetuates the lower provision of public goods and reinforces the barriers to escaping from coercion.

In addition, the intergenerational nature of labor coercion amplifies these persistent effects, extending them beyond the region where it occurs. Indeed, I document that the impact of descending from a coerced worker is present even for individuals born outside the *concertaje* area, i.e., the Highlands. However, the effect is less pronounced for them, suggesting migration could undermine historical institutions' persistent effects. Nonetheless, due to mobility barriers, descendants of *conciertos* are less likely nowadays to migrate, although in a small magnitude. I show that an increase in a surname's *concertaje* rate in 10 pp reduces the probability of migration nowadays by 0.14 pp.

A more detailed conclusion of the results presented in this paper requires obtaining *concertaje* data for the early Republican period and having living standards data for the 19th century. This is to understand better at which point regions started to diverge in their development paths. Future research topics may also consider the findings in this paper as a motivation to develop models where institutions explicitly play a role in 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 expand beyond the regions where they existed. It is also necessary to study how migration and policies oriented toward it could mitigate historical persistence. This is a promising agenda to disentangle the puzzle about the fundamental causes of long-run economic development.

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Online Appendix for Attached once, attached forever: The persistent effects of *concertaje* in Ecuador

A Data

A.1 Concertaje

Concertaje and *haciendas* data come from two sources. The main one is the colonial tax records in the National Archive of History of Ecuador (Archivo Nacional de Historia (ANH)). The second one is Oberem (1981b). I describe the treatment of the data below.

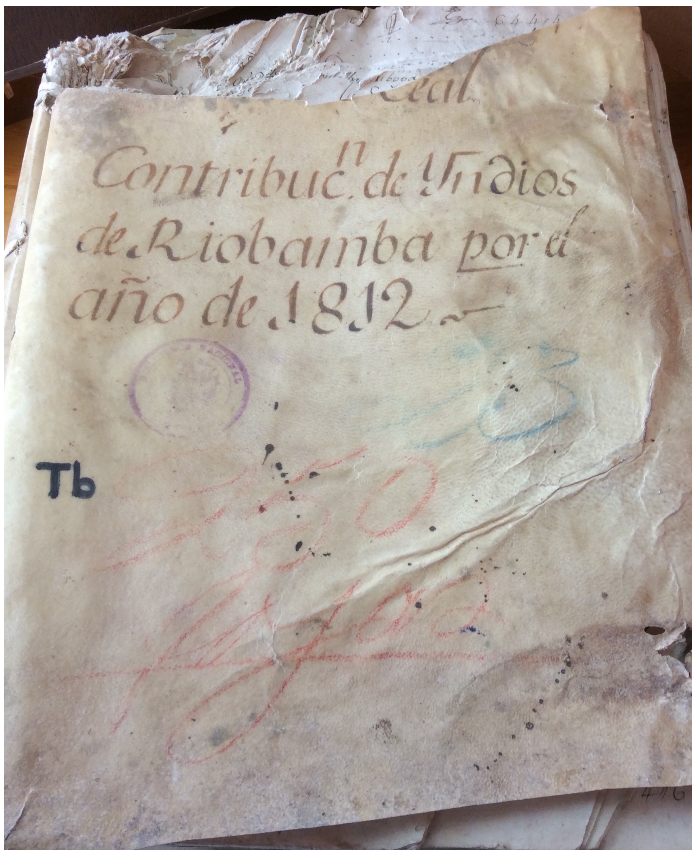
A.1.1 National Archive of History

As mentioned in the main text, tax records include information on individualized payments of indigenous workers by town. Moreover, as landlords paid their workers' taxes, these records distinguish the *hacienda* where workers were living and the *hacienda* owner's name. In turn, tax collectors organized these records in books by tax administration. I found at least one book for each administration, except for Guaranda. I took photographs of those records and digitized them. Figure A.1 shows examples of how these tax records look like.

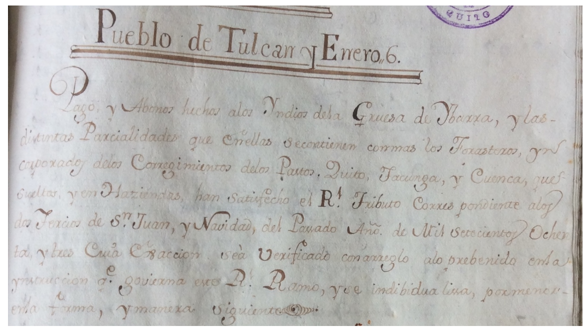
Since tax collection was in charge of local (colonial province) authorities, the books differ in quality and sometimes raise questions about their comparability. Hence, I minimize these concerns by employing the following criteria. First, although most books are relatively easy to read, there are some cases where the ink has vanished, making it hard to recover the information written there. If other sources for the data are available, I disregard data coming from books that are difficult to read to minimize transcription errors.

Second, most books, particularly the earliest ones, seem comparable among them, as they explicitly mention the payments of indigenous workers, free and living in *haciendas* for the periods of *San Juan* and *Navidad*. The books after 1822, when the region became independent, tended to omit the tax collection season or reflect partial payments for a different one. Hence, I prioritize books that explicitly mention the collection of taxes for the period mentioned above. Moreover, I am particularly cautious with the information after 1822 since the independence wars could have temporarily affected the distribution of workers, while the new political control of landlords could have allowed them to skip the taxation of their workers. Tyrer (1988) also suggests that tax records quality is better for earlier years, so generally, these are preferred. Additional details of each book, as well as their location in the Archive, are described below.

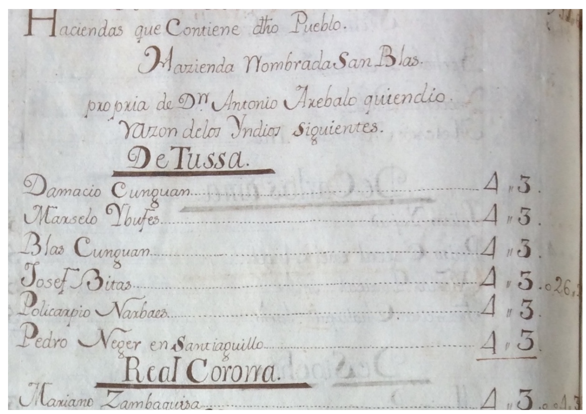
Figure A.1: Colonial Tax Records



(a) *Contribution of the indigenous workers from the province of Riobamba year 1812.*



(b) <u>... Town:</u> *Payments by indigenous workers ...* ***free*** *and in* ***Haciendas****...*



(c) <u>*Haciendas of this town ...*</u>: ***Hacienda... owned*** *by* <u>... with the following workers...</u>

Notes: Example of Tax Records. Figure (a) is from the book of Riobamba (1812). Figures (b) and (c) are from the book of Ibarra (1784). Source: Archivo Nacional de Historia del Ecuador (ANH). See Appendix A.1.1 for a detailed explanation of these books and their location in ANH.

Alausi

Book 1: Cobro de Capitación de 1826

- **Source:** ANH. Sección: Empadronamientos. Caja 12. Documento 11.
- **Notes:**
    1. This book does not specify if the payments belong to *San Juan* and *Navidad* periods and is quite disordered. However, a major problem with this source is that it does not specify the location of the *haciendas*. Instead, it reports them jointly at the end of the book. Hence, I used other sources to identify their location. This procedure recovers *concertaje* rates similar to those presented by Oberem (1981b). However, I completely disregard this data due to concerns about its quality.

Ambato

Book 1: Libro Público del Ramo de Única Contribución de 1823

- **Source:** ANH. Sección: Tributarios. Caja: 35.
- **Notes:**
    1. This book does not specify if the payments belong to *San Juan* and *Navidad* periods. The number of indigenous workers in this book is also inferior to those reported by Oberem (1981b), but the *concertaje* rates are relatively similar (see Figure A.2). Hence, I consider this source to be of relatively good quality.
    2. These records also include some extra classifications of taxpayers, namely young entrants, recently dead workers, and other contributors for whom taxes were not mandatory, like older people. Since they represent a tiny fraction of workers, and the records do not specify their free or *concierto* status, I omit them from the analysis.

Book 2: Catastro del Cantón de Ambato de 1825

- **Source:** ANH. Sección: Empadronamientos. Caja 29. Documento 21
- **Notes:**
    1. This book does not specify if the payments belong to the periods of *San Juan* and *Navidad*. This book also contains data about other regional districts, but it is very difficult to read since the ink has vanished. Consequently, I did not recover information from it.

Cuenca

Book 1: Pagos de San Juan y Navidad de 1792

- **Source:** ANH. Sección: Presidencia de Quito. Caja 132. Libro 305. Documento 8612.
- **Notes:**
  1. This book does not have its last pages. So, the information about the towns of San Blas and San Sebastian (modern parish of Cuenca) is not included.

Ibarra

Book 1: Pagos de San Juan y Navidad de 1783

- **Source:** ANH. Sección: Presidencia de Quito. Caja 83. Libro 207. Documento 8607.
- **Notes:**
  1. Information about unpaid taxes from the colonial administration of Otavalo is also included in this book. I did not use it since it is aggregated instead of split by town.
  2. Puntal split into Puntal and El Angel after 1783. For the sake of completeness, I impute the number of free workers of El Angel by dividing the information of Puntal using the corresponding relation between both towns in 1817-1818 as a reference. I divide each town's *conciertos* according to their posterior *hacienda* location.

Book 2: Copia Fiel de Pagos de San Juan y Navidad de 1817

- **Source:** ANH. Sección: Tributarios. Caja 29. Documento 27.
- **Notes:**
  1. This book explicitly mentions that the records are a copy of the original. This can also be noted by the book's lack of detail and neatness compared to others.
  2. The book also includes a list of absent taxpayers by town, although it does not specify if they belonged to *haciendas* or were free. Since they represent very few cases, I omit them from the analysis.

Book 3: Pagos de San Juan y Navidad de 1818

- **Source:** ANH. Sección: Tributarios. Caja 35. Documento 12.

Latacunga

Book 1: Cobranza del 2do Semestre de la Contribución de Indígenas de 1828

- **Source:** ANH. Sección: Presidencia de Quito. Caja 259. Libro 645. Documento 13228.
- **Notes:**
  1. This book is a partial list of the tax collected for the second semester of the year. Opposite to the headlines of most other books, this one refers to the "contribución personal" (personal contribution) instead of tax collection for San Juan and Navidad. One can also note that this is an incomplete and partial list since *concertaje* rates are systematically lower than those reported by Oberem (1981b) (see Figure A.2). This is at odds with the well-known fact that this region had a substantial presence of *concertaje* (Oberem, 1981a, p. 323).
  2. This book does not have its last pages. So, the information about the town of San Sebastian is incomplete, while that of the district of Latacunga is not included. The absence of the latter is not a significant loss since, at the beginning of the book, the index indicates that the data of that district only comprises two pages. Instead, the information for San Sebastian, although incomplete, is preserved since only two pages from the *hacienda* section are missing. So, I assume those reported 25 workers (the average of the previous pages).
  3. Sigchos split into Sigchos and Chugchilan after 1805, while Saquisili split into Saquisili and Poalo. Due to the closeness of those districts, I merged them back to compare with the information from Oberem (1981b) and have a more accurate report of the *concertaje* rates given the partial report of the book.
  4. Similar to Ambato (1823), I omit other small categories of workers mentioned in the records for the same reasons described above.

Loja

Book 1: Pagos de San Juan y Navidad de 1785

- **Source:** ANH. Sección: Presidencia de Quito. Caja 96. Libro 233. Documento 8610.

Book 2: Pagos de San Juan y Navidad de 1792

- **Source:** ANH. Sección: Presidencia de Quito. Caja 132. Libro 305. Documento 8611.

Otavalo

Book 1 (Pagos de San Juan y Navidad de 1784)

- **Source:** ANH. Sección: Presidencia de Quito. Caja: 83. Libro: 207. Documento: 8608.
- **Notes:**
  1. As with the case of Otavalo in Ibarra, this book includes some information about the tax administration of Ibarra. I do not use Ibarra's data for the same reasons mentioned before.
  2. This book does not have its first pages. So, the information about the towns of Tocache, Tabacundo, Cayambe, and San Pablo is not included, while that of Cotacachi is incomplete.
  3. I preserve the information on Cotacachi since it only omits the number of free workers, which can be calculated using the total number of *tributarios* reported at the end of the book.

Book 2: Pagos de San Juan y Navidad de 1820

- **Source:** ANH. Sección: Tributarios. Caja: 35.

Book 3: Cobranza de la Contribución de Indígenas de Otavalo de 1830

- **Source:** ANH. Sección: Indigenas. Caja: 174. Documento: 7.
- **Notes:**
  1. This source is just a summary of *haciendas*, *conciertos*, and free workers.
  2. It does not include information on Urcuqui, a town typically reported in the Otavalo region.

Quito

Book 1: Pagos de San Juan y Navidad de 1784

- **Source:** ANH. Sección: Presidencia de Quito. Caja: 96. Libro: 235. Documento: 8610.
- **Notes:**
  1. This book does not have its last pages. So the information about the town of Catedral is missing.

Riobamba

Book 1: Tributos de los Indios de Encomiendas y Forasteros de 1782

- **Source:** ANH. Sección: Presidencia de Quito. Caja: 83. Libro: 207. Documento: 8606.
- **Notes:**

  1. The data seems incomplete as it refers to the tax collection of indigenous workers in certain *encomiendas*. Moreover, information before 1797 is problematic since the earthquake of that year implied the reallocation of several towns (Egred (2000)). Hence, I dismiss this information.

Book 2: Pagos de San Juan y Navidad de 1812

- **Source:** ANH. Sección: Tributarios. Caja: 34.
- **Notes:**

  1. The book includes separately the same extra categories of taxpayers as the book of Ambato (1823). So, following the same logic, I omit them from the analysis.

A.1.2 Oberem

In his essay, Oberem mentions that the original document from which he took the data refers to 1804/1805 and includes information regarding 121 colonial towns of 9 colonial administrations[1]. He reports the aggregate number of *conciertos*, *haciendas* and *tributarios* for each colonial tax administration. However, he does not fully report the information for all the towns. Instead, he published four types of data.

The first one includes districts with their respective number of *conciertos*, *tributarios*, and *concertaje* rates. This information is only reported for districts with the three highest and three lowest *concertaje* rates in each colonial administration. The second type of data is also a partial list of districts that only includes the number of *conciertos* and *haciendas*, with a different measure of the incidence of the institution, namely *conciertos* per *hacienda*. As with the first one, this measure is only reported for those districts with the three highest and three lowest *conciertos* per *hacienda* rates in each administration. The third type is a list of districts with *concertaje* rates above 75% or below 25%, although it does not include the number of *conciertos* or *tributarios*. The fourth type is a list of towns mentioned for other reasons, although no information about them is presented.

[1]The information about Cuenca is absent, while seven parishes of Quito are deliberately left out of the analysis because they do not distinguish between *conciertos* and free workers.

The lists do not fully overlap, and they represent a total number of 86 districts. Hence, 35 towns are never mentioned at all, although I can tell which they are by using tax records and other colonial data as reference information. Since I have complete information regarding the *concertaje* rates of 53 districts (three largest and three smallest rates in each tax administration), I implicitly know the *concertaje* rates boundaries for the remaining 68 districts. The same is true for the *conciertos* per *hacienda* rates.

Given these limitations, I address the problem of incomplete data in the following way. I first collect information about the total number of *tributarios* from the *Archivo General de Indias de Sevilla* (AGI). As explained in Section A.3.3, this data is essentially a snapshot of the tax records in each colonial province, as they include the number of *tributarios* and the total payments of the indigenous workers in each district. For the years I have data from both the AGI data and the tax records, the total number of *tributarios* is virtually the same, which gives me confidence about the comparability of the data.

Using this information, I compute the annual share of the tributary population of each town relative to their respective colonial province and then take the average of the last five available years[2]. Then, I compute the total number of *tributarios* in the missing districts by simply multiplying the average shares to the aggregate number of *tributarios* in each tax administration as reported by Oberem[3]. Conversely, when I do not know a district's number of *haciendas*, I use the numbers reported in the tax records closest to 1804.

Once I imputed the total number of *tributarios*, I computed the *concertaje* rates for 28 observations for which I know the total number of *conciertos*. For the remaining 40 observations where I do not know the latter, I impute them by using as reference the implicit boundaries of both *concertaje* and *conciertos* per *hacienda* rates.

For example, if I know town A has 100 workers (imputed) and 4 *haciendas* (known from the tax records), and should have a *concertaje* rate between 0.25 and 0.40, as well as a *conciertos* per *hacienda* rate between 3 and 8, that means the number of *conciertos* should lie between 25 and 32 (l_b=max(3*4,0.25*100), u_b=min(8*4, 0.40*100)). So, I impute the number of *conciertos* by taking the middle point of the interval. I finally accommodate the data so that all rates and totals are consistent with the aggregates. This procedure leaves me with three classes of *concertaje* rates: 1) those with complete information, 2) those where only the number of *tributarios* was imputed, and 3) those where both *conciertos* and *tributarios* were imputed. A similar logic follows the imputation of *conciertos* per *hacienda*.

[2] In most cases, these years are 1796-1800. For the case of Riobamba, I use instead as reference the 1812 tax records, given the concerns described in the previous section.

[3] For the observations where I can observe the total number of *tributarios*, the average percentage difference between the actual number and the one that will have been from this procedure is -0.44%.

Figure A.2 shows the differences between the Oberem data and the tax records one[4]. As can be seen, both sources show large similarities. The correlation between the *concertaje* rates of both sources is 0.80, while that of *conciertos* per *hacienda* is 0.74.

A.1.3 Summary

To sum up, I have *concertaje* data for all the 10 colonial tax administrations of the Ecuadorian highlands. Following the criteria I discussed at the beginning of this section and the details mentioned above, I completely disregard the books of Alausi (1826) and Ambato (1825) due to their bad quality and deterioration. I also disregard the book of Riobamba (1782) due to concerns regarding its comparability, given both the type of taxes it reports and the change in location of several towns after the 1797 earthquake. This leaves me with 306 observations for 144 towns. The missing towns are Catedral in Quito, and San Blas and San Sebastian in Cuenca. Table A.1 summarizes the available data, their sources, and their quality rank based on the discussion above and in the main text. The baseline sample of all estimations corresponds to the top-ranking one (1).

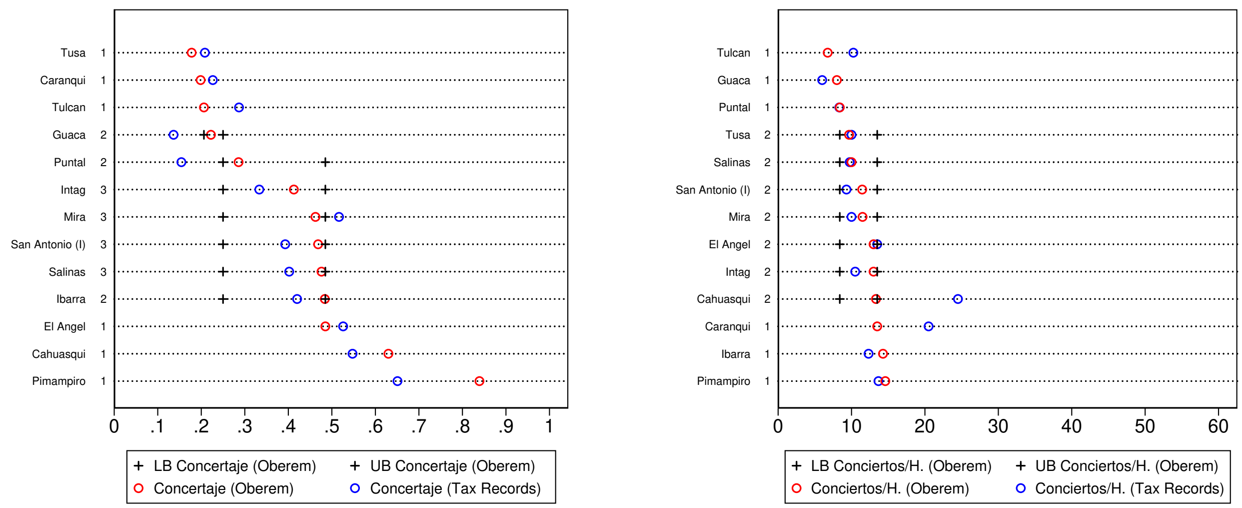
Table A.1: Summary of *Concertaje* Data

	Tax Administration	Year	Source	Quality	No. Towns	Conciertos	Tributarios	Haciendas
1	Alausi	1804	Oberem	1	5	348	1855	25
2	Ambato	1804	Oberem	1	9	2892	5627	270
2	Ambato	1823	Tax Records	2	9	1282	3312	115
3	Cuenca	1792	Tax Records	1	17	2282	8202	107
4	Guaranda	1804	Oberem	1	9	586	1951	108
5	Ibarra	1783	Tax Records	1	13	1030	2136	84
5	Ibarra	1804	Oberem	4	13	925	2175	76
5	Ibarra	1817	Tax Records	3	13	899	2452	77
5	Ibarra	1818	Tax Records	2	13	1013	2595	85
6	Latacunga	1804	Oberem	1	13	4515	8282	242
6	Latacunga	1828	Tax Records	2	13	2285	6734	175
7	Loja	1785	Tax Records	1	17	695	2823	100
7	Loja	1792	Tax Records	2	17	610	3061	90
7	Loja	1804	Oberem	3	17	465	2832	64
8	Otavalo	1784	Tax Records	4	4	1445	3093	80
8	Otavalo	1804	Oberem	2	9	2737	5705	97
8	Otavalo	1820	Tax Records	1	9	2661	5782	116
8	Otavalo	1830	Tax Records	3	8	2570	6315	82
9	Quito	1784	Tax Records	1	31	5365	8687	363
9	Quito	1804	Oberem	2	25	3968	7422	323
10	Riobamba	1804	Oberem	2	21	4673	8826	229
10	Riobamba	1812	Tax Records	1	21	4054	8746	231

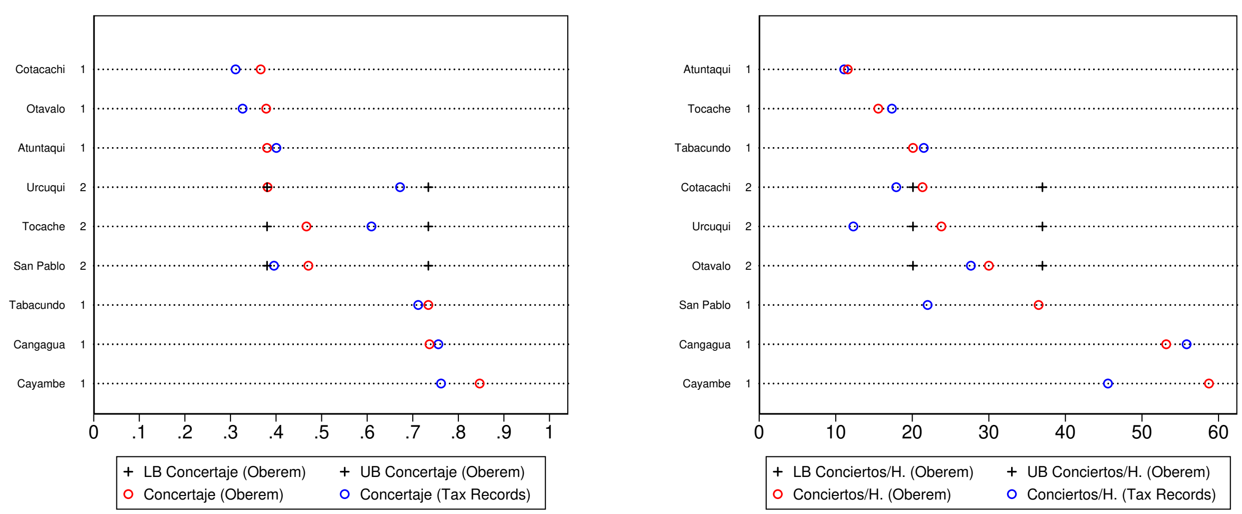
Notes: This Table summarizes all the available *concertaje* data. The source and treatment of each piece of information are described in detail in Appendix A.1.1-A.1.2. See also the discussion in the latter to understand the quality classification.

[4]The selected tax records for comparison are those closer to 1804: Ibarra (1818), Otavalo (1820), Quito (1784), Latacunga (1828), Ambato (1823), Riobamba (1812), Loja (1792). The towns of Celica, Valladolid, Chito, and Zamora are omitted in the latter as they have 0 *conciertos*.

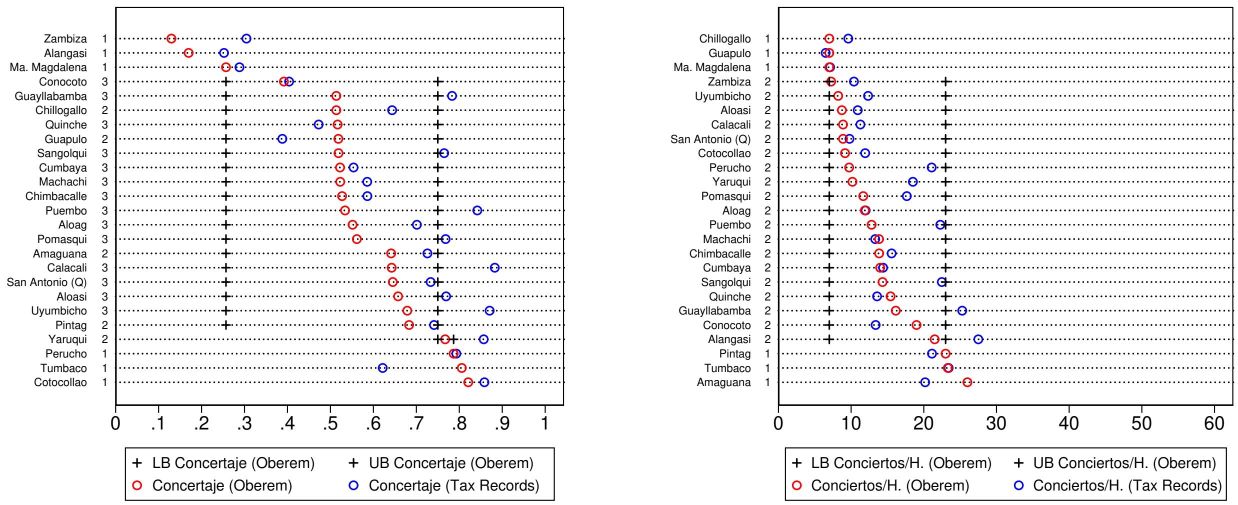
Figure A.2: Comparison of *concertaje* and *conciertos/hacienda* between Oberem and Tax Records



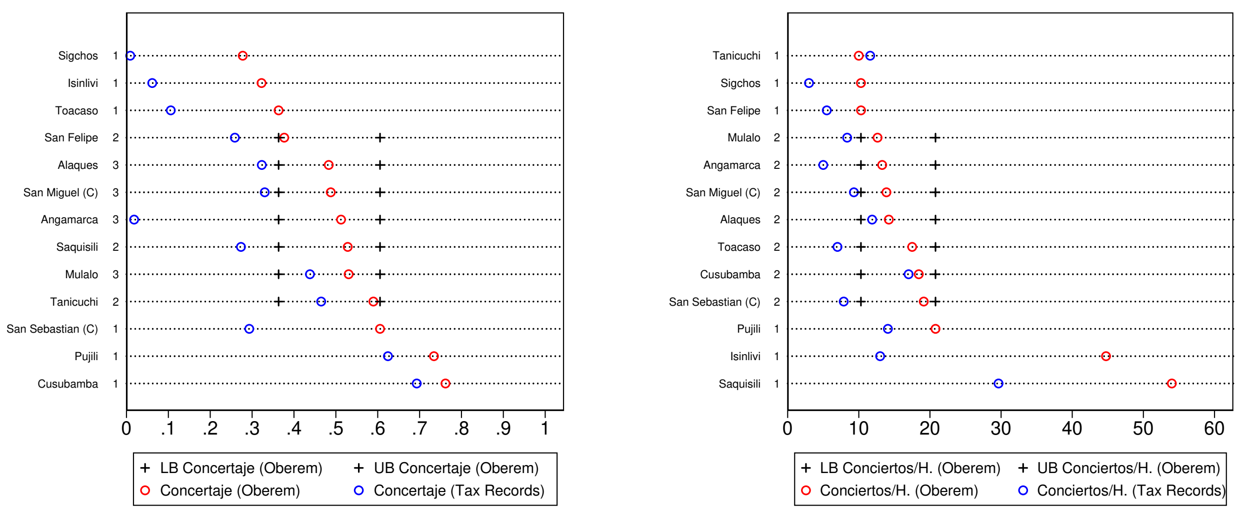
(a) Ibarra



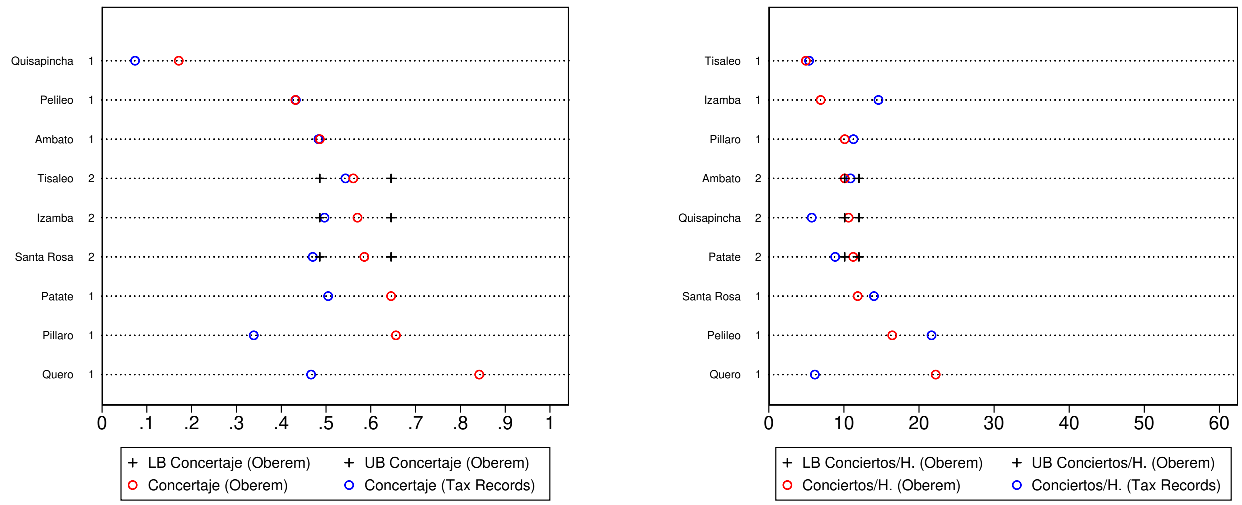
(b) Otavalo



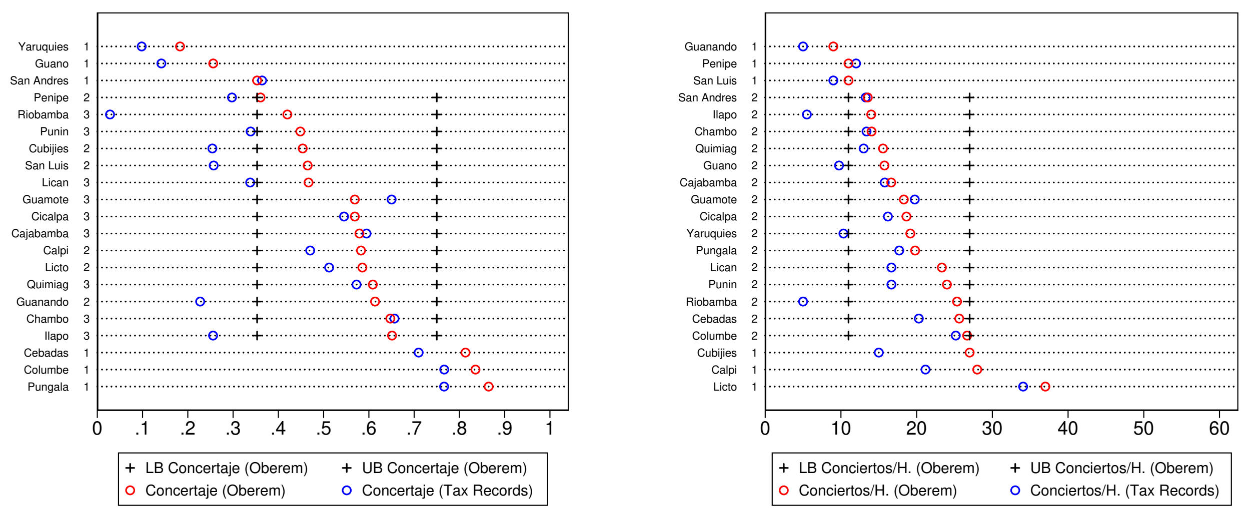
(c) Quito



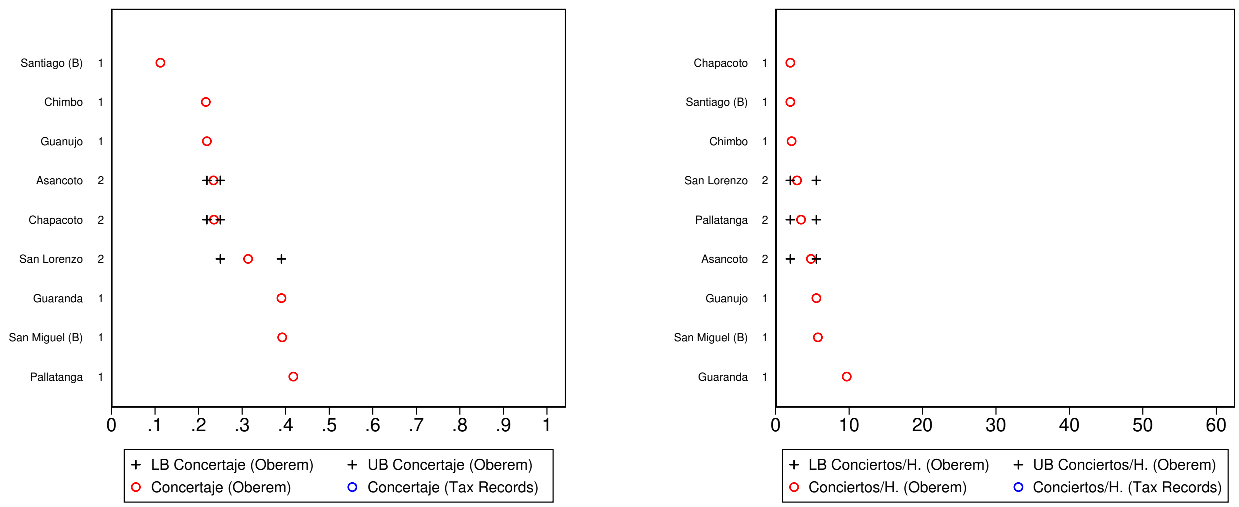
(d) Latacunga



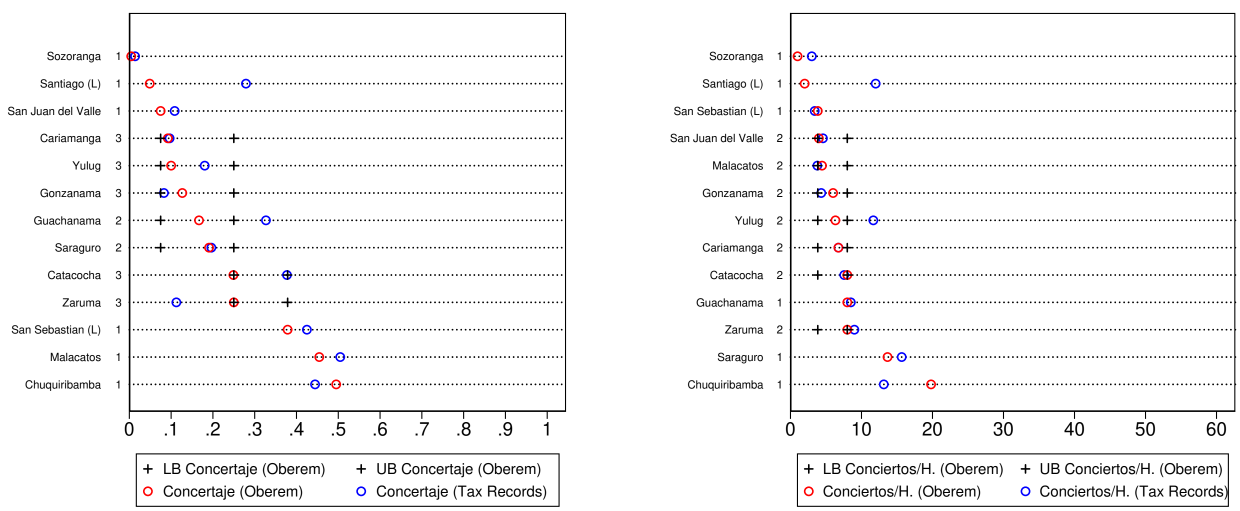
(e) Ambato



(f) Riobamba



(g) Guaranda



(h) Loja

Notes: See Appendix A.1.1 for a detailed explanation of the *concertaje* data coming from Tax Records. The numbers next to the names of towns correspond to the three classes of data derived from Oberem's report: 1) full information; 2) only the number of tributarios (haciendas) missing; 3) both conciertos and tributarios missing.

A.2 Matching

From the 144 towns for which I have *concertaje* data, I disregard 10 districts of the colonial province of Quito that would have matched with the contemporary parish of Quito. I do so because of the following reasons. First, its tax records are incomplete, both in the records of 1794 and in the list of Oberem. More critical, Oberem (1981b) mentions that the numbers of Quito are of low quality since many indigenous workers camouflaged as *mestizos* (mixed between Spanish and Indigenous person). Finally, the city of Quito has been historically the main administrative center of the country, where the landlord elite lived, so its development process corresponds to a different dynamic. That leaves us to match 134 districts.

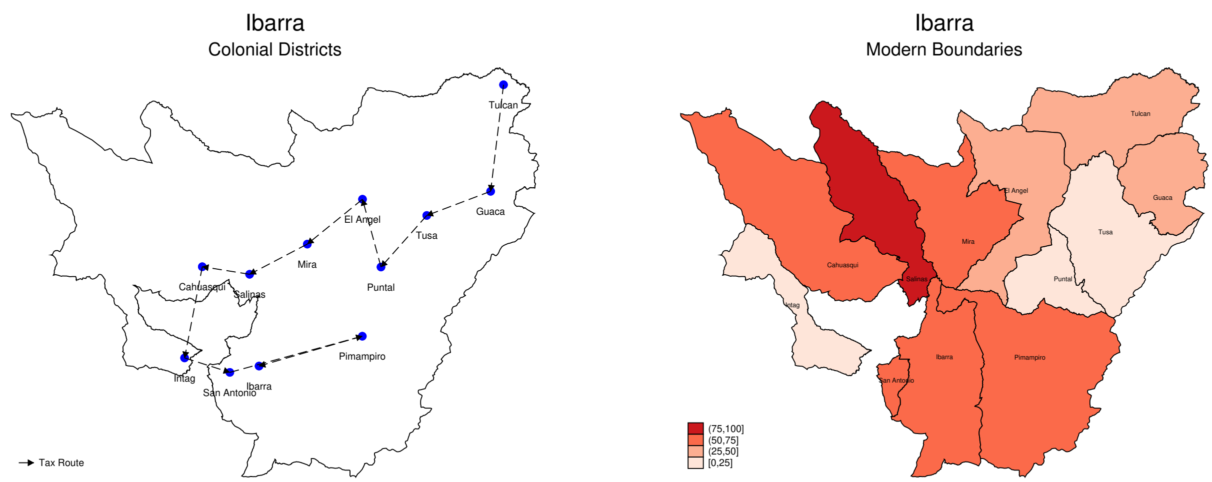
Those 134 districts turn into 128 time-invariant units by proceeding as follows. First, I use as a reference the 2010 official political division of Ecuador to merge 12 districts that have pair-wise joined over time to form modern parishes. Then, instead of matching those 128 locations uniquely by name, I reconstruct the colonial territory by merging back areas that have split over time. For example, instead of matching colonial Yaruqui (Quito) only to the modern parish of Yaruqui, I match it to both Yaruqui and Tababela, given that Tababela was part of Yaruqui until 1962. Moreover, in the colonial tax records, the location of Tababela appears as a place (*añejo*) of Yaruqui.

I take as a reference mainly all modern highland parishes to delimit what would have been the colonial territory where both Spanish had a major influence and *concertaje* existed[5]. I then match them to their corresponding colonial parish. I do this for the different political divisions of each census from 1950 to 2010. This matching procedure has three advantages. First, it makes the results comparable over time as it fixes the political unit. Second, it reduces bias concerns because fast-growing locations are more likely to split and form new parishes. Finally, it creates larger geographical units to capture better satellite data resolution.

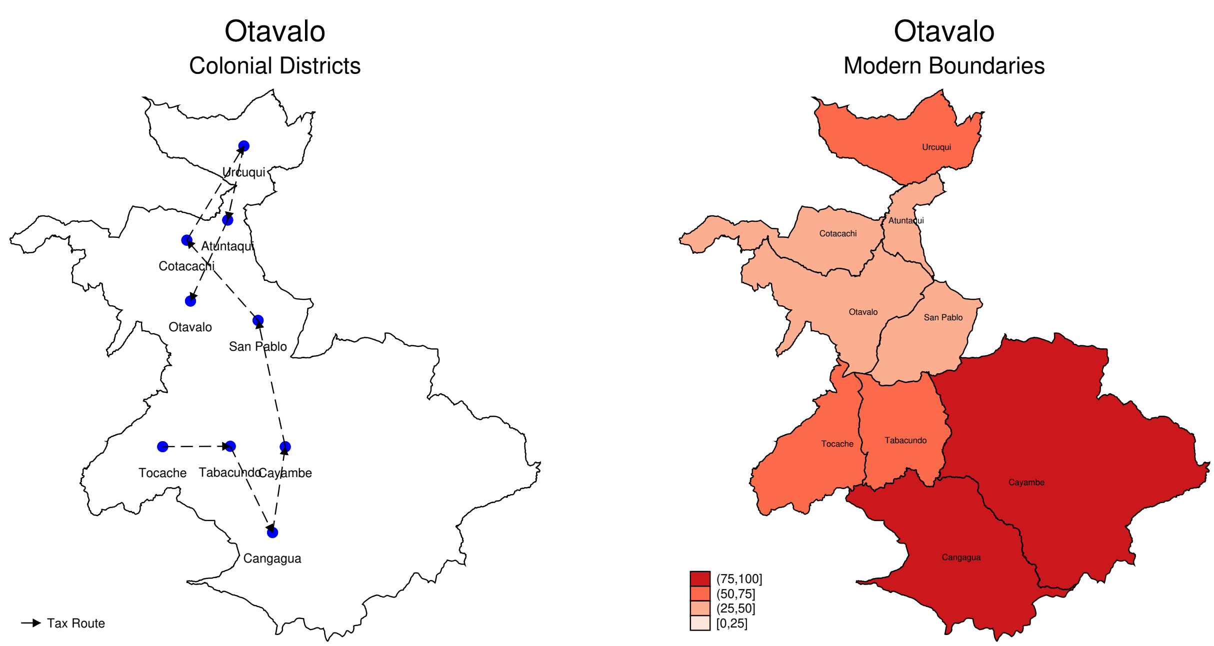
I use mainly three sources to match locations: a) the colonial tax records, which most of the time also include the name of places that later on split; b) the work of Delaunay et al. (1985), which describes how parishes have split from 1950-1982, and c) the official parishes' websites, which sometimes describe their territory history. Whenever the match of some location is unclear, I use proximity as a criterion. The left maps of Figure A.3 provide the location of the 128 towns and the tax route during colonial times, according to the order in which they appear in the records. The maps on the right show the intensity of *concertaje* in each district, considering the likely colonial territory constructed as described above.

[5]For instance, the colonial province of Quito (now Pichincha) excludes the current *cantones* San Miguel de los Bancos, Pedro Vicente Maldonado, and Puerto Quito, that were referred as *Yumbos*,(hot valleys) since they lost economic relevance after 1700 due to its population decline (Tyrer, 1988, p. 12)

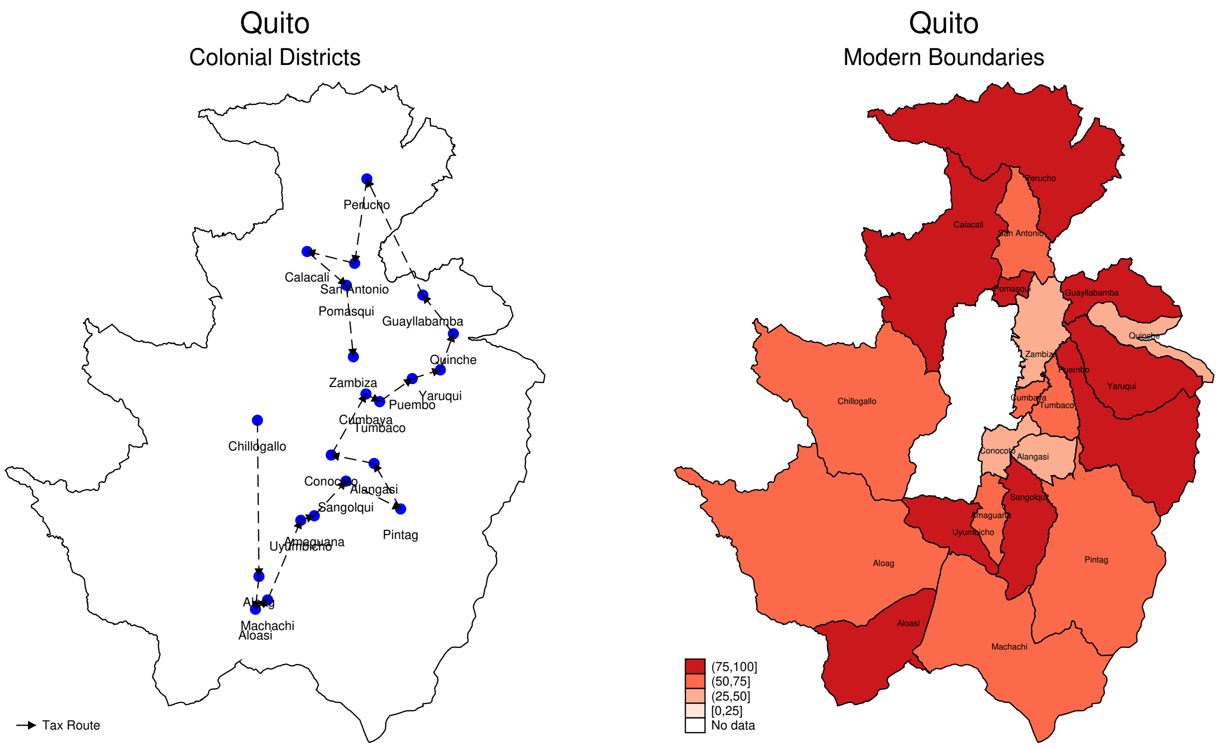
Figure A.3: Colonial Districts and Modern Boundaries



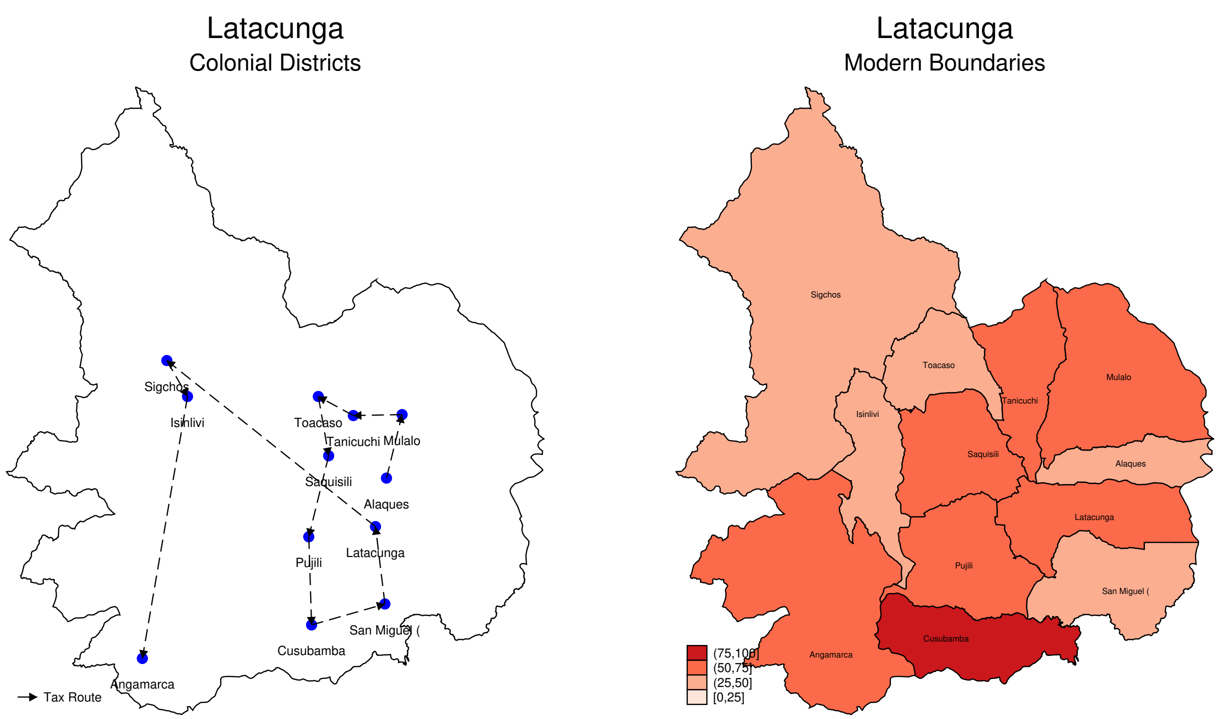
(a) Ibarra



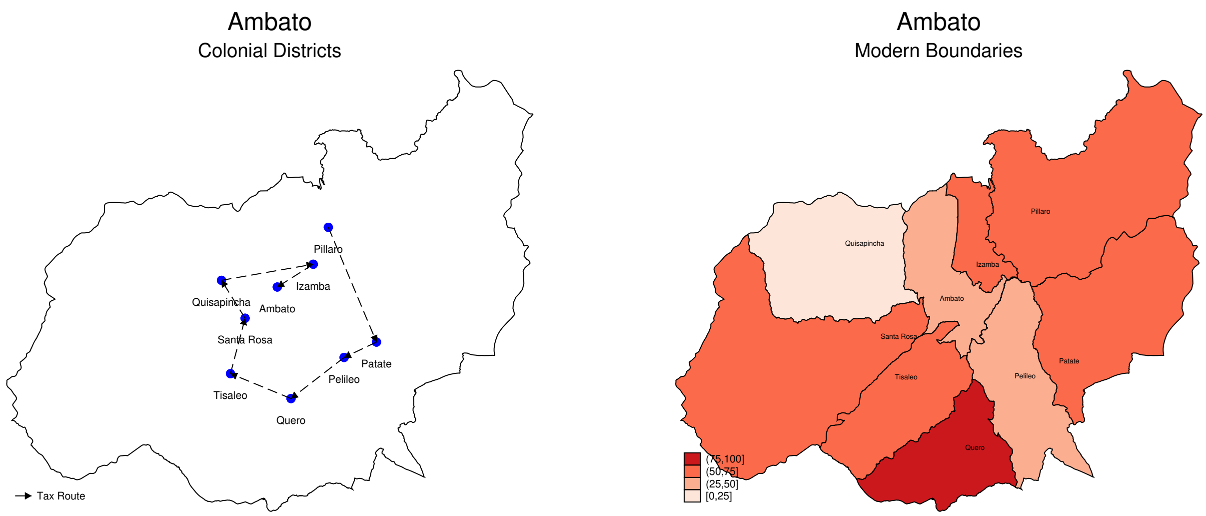
(b) Otavalo



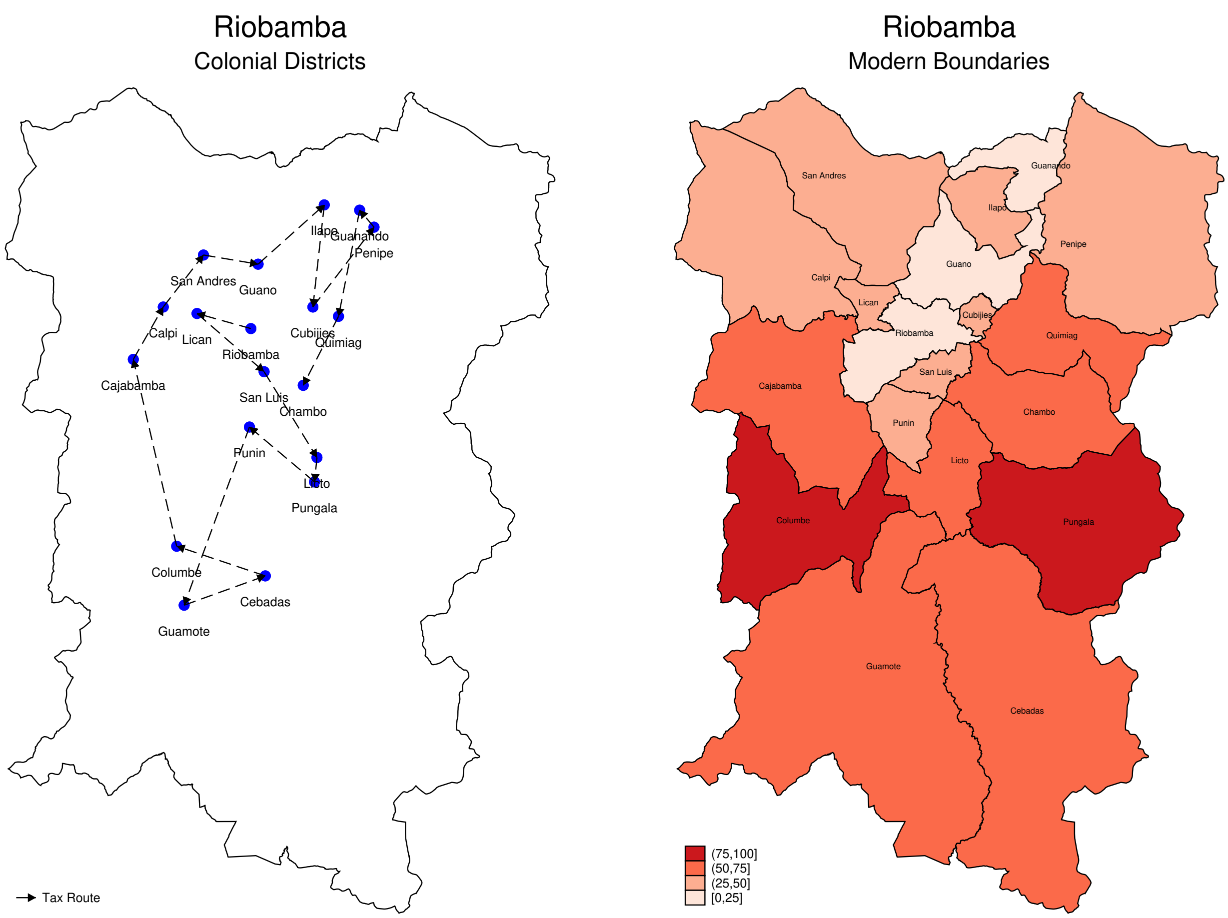
(c) Quito



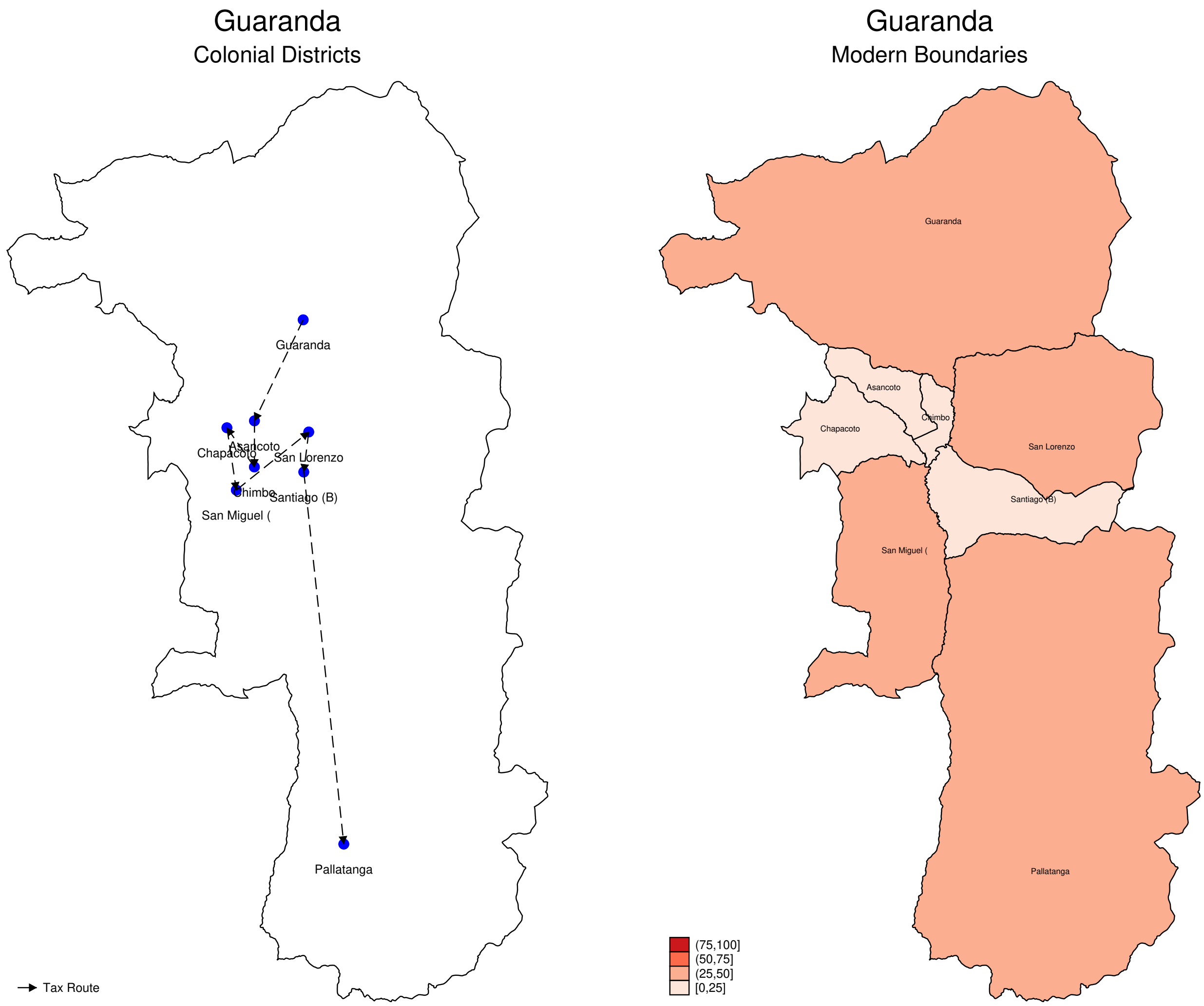
(d) Latacunga



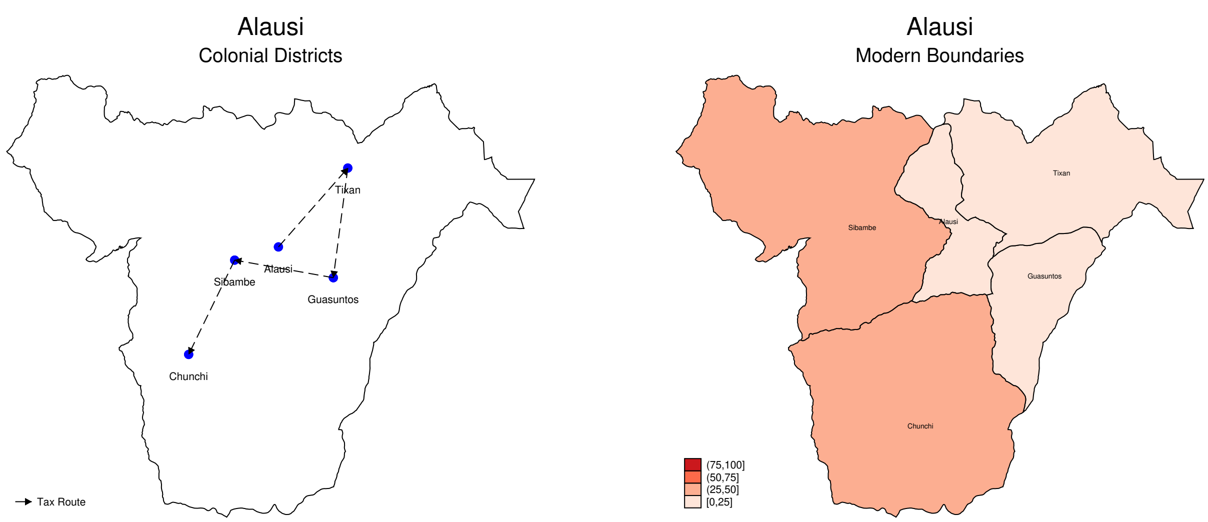
(e) Ambato



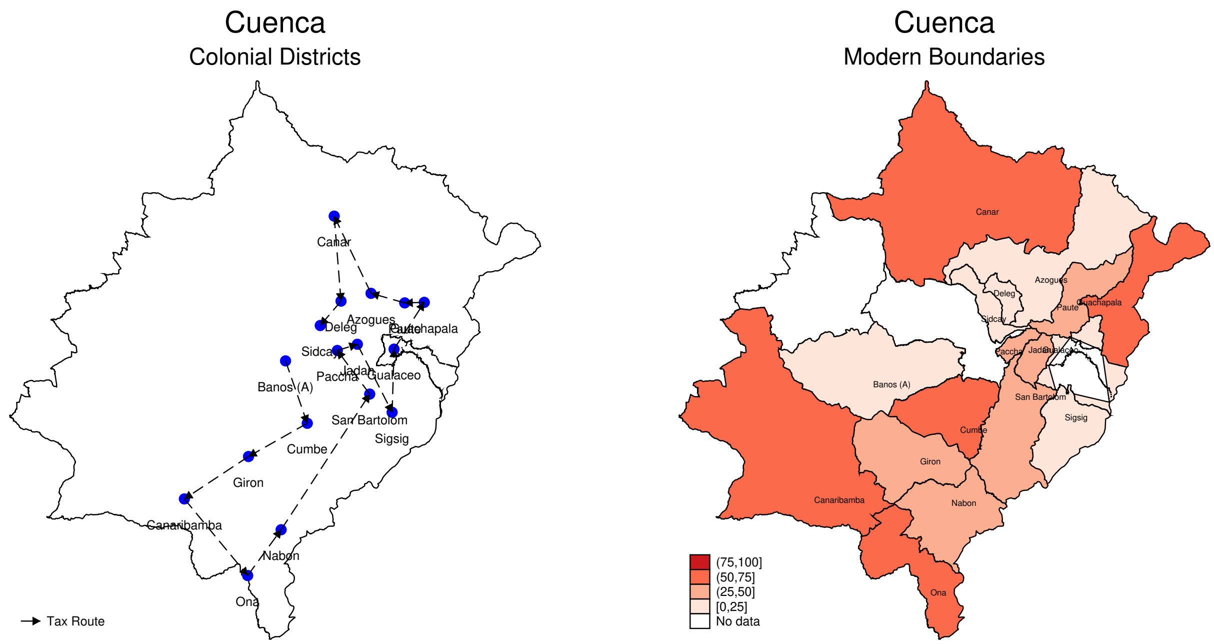
(f) Riobamba



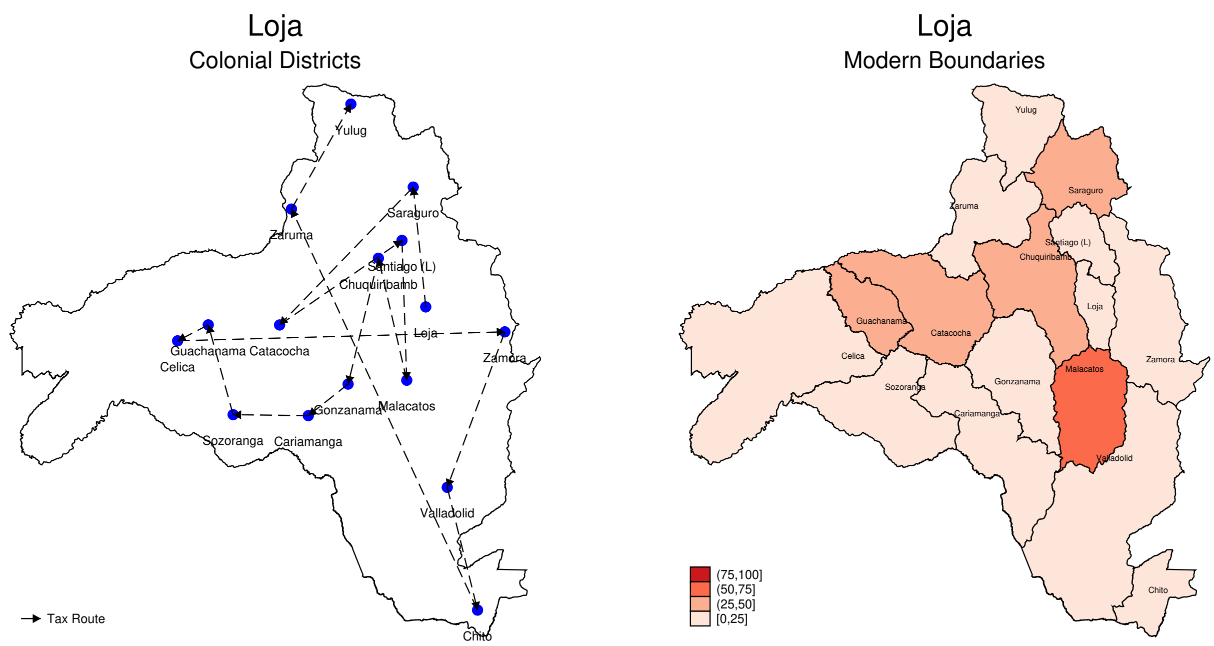
(g) Guaranda



(h) Alausi



(i) Cuenca



(j) Loja

A.3 Colonial and Early Republican Data

I digitized all the following data from the sources described below.

A.3.1 Population (1590-1600)

Using several sources, I compute the average population *circa* 1600. Data from 1590 and 1591 comes from Tyrer (1988), who reports disaggregated information using as sources *"La relacion de Zaruma"* and *"El Censo de Morales Figueroa"*. Data for 1598 comes from Albuja Mateus (1998). I match this data with other records by comparing names of towns, although keep in mind that this process is noisier than the one for the late colonial period. This is due to two reasons. First, some records report aggregated information for numerous towns; second, some towns did not exist by 1600. Hence, I have early population data for fewer locations.

A.3.2 Population (1784)

Population data for the late colonial period come from Salmoral (1994), which reports the most trustable census records *circa* 1784. Data for the 5 districts of Alausi and that of Sidcay are missing, so instead, I use data from the 1765 and 1778 census, reported by Poloni (2006).

The census does not report information on some towns as they were joined to others. That is the case of El Angel (Puntal), Cangagua (Cayambe), San Lorenzo (Santiago), Guamote (Cebadas), Nabon (Ona), Cumbe (Paccha), Guachapala (Paute), and Jadan and Sigsig (Gualaceo). In those cases, I split the data using corresponding taxpayers' weights. For instance, the total indigenous population of Cayambe in the census is 5215. In the tax records, the tributary population of Cangagua, relative to both Cayambe and Cangagua, is, on average, 44%. So, I assume the indigenous population of the latter was 2295 (0.44*5215).

A.3.3 Tributarios (1780-1801)

I obtained data for indigenous taxpayers from the Archivo General de Indias de Sevilla (AGI) in Spain[6]. This data summarizes the tax records from which *concertaje* information was collected. However, it does not explicitly separate the number of *conciertos* and free workers. As mentioned earlier, the aggregate information is virtually the same in the cases where one can observe both the detailed and summarized tax records. This information is mainly used as a reference for the imputation procedure described in Section A.1.2. Table A.2 shows the specific source of the data. Within the Archive, they belong to the Section *Gobierno*, First Division *Audiencia* de Quito, Series *Contaduria: Cuentas*.

[6] I am grateful to Katiushka Pacheco, who initially requested the reprographic service of the data, and to Sofia Granizo, who collected both this information, as well that of tithes, from the AGI.

Table A.2: Summary of Tributary Population Data

Year	Source	Location	Tax Administration
1779-1786	AGI	ES.41091.AGI/23.12.18.26.22//QUITO,435	Quito
1787-1802	AGI	ES.41091.AGI/23.12.18.26.23//QUITO,436	Quito
1779-1801	AGI	ES.41091.AGI/23.12.18.27.3//QUITO,447	Ambato
1782-1800	AGI	ES.41091.AGI/23.12.18.24//QUITO,448	Alausi
1779-1790	AGI	ES.41091.AGI/23.12.18.35.9//QUITO,460	Cuenca
1791-1801	AGI	ES.41091.AGI/23//QUITO,461	Cuenca
1783-1799	AGI	ES.41091.AGI/23.12.18.28.3//QUITO,492	Guaranda
1780-1781	AGI	ES.41091.AGI/23//QUITO,494	Ibarra
1753-1799	AGI	ES.41091.AGI/23.12.18.37.1//QUITO,500	Loja
1780-1801	AGI	ES.41091.AGI/23.12.18.31.1//QUITO,504	Latacunga
1780-1802	AGI	ES.41091.AGI/23.12.18.32.1//QUITO,506	Otavalo
1780-1792	AGI	ES.41091.AGI/23.12.18.33.3//QUITO,537	Riobamba

Notes: This Table summarizes the indigenous tributary population data. Additional details are discussed in Appendix A.3.3.

A.3.4 Tithe (1780-1830)

I collected tithe data from both the Archivo Nacional de Historia (ANH) in Ecuador and the Archivo General de Indias de Sevilla (AGI) in Spain. Table A.3 shows the sources I use. The data is not homogenous because some records are drafts, others include the bidding process, and some are just summaries. But, generally, they are consistent and show relatively stable patterns. However, as mentioned in the main text, I first averaged the raw data by decade to reduce concerns regarding its fluctuations before merging it with *concertaje* data.

For example, tithe data corresponding to 1804 is the average of the years 1802,1804-1807. Since *concertaje* data for each Tax Administration do not overlap by decade (except for Ibarra (1817-1818)), the mapping is unique. This procedure also helps reduce inflationary/deflationary movements, if any existed. Unfortunately, price data is scattered throughout the ANH, so no consistent price series exists (Andrien, 1995, p. 92).

For the case of Loja, since I have *concertaje* data for 1784 but no tithe data for the same period, I use the corresponding information for the 1790s. I made the same adjustment for the 1780 data of Quito, Ibarra, and Otavalo because several towns are aggregated. Similar to the procedure described for the population data, I split the tithe data in two for the towns of Puntal, Guamote, Santiago, and Riobamba, which come jointly with the data of El Angel, Cebadas, San Lorenzo, and San Luis, respectively.

A.3.5 Land value (1909)

Land value for 1909 is reported in the *Guía Comercial Agrícola e Industrial de la República del Ecuador (1909)* (Compañía Guía del Ecuador (1909)). The book includes information, by district, of the total land value reported in the official cadastral records.

Table A.3: Summary of Tithe Data

Year	Source	Location	Tax Administration							
			Quito	Ibarra & Otavalo	Latacunga	Ambato	Riobamba	Guaranda	Cuenca	Loja
1781	ANH	Serie diezmos, caja 3 exp. 07	X							
1782	ANH	Serie diezmos, caja 3 exp. 07	X		X					
1783	ANH	Serie diezmos, caja 3 exp. 07	X							
1784	ANH	Serie diezmos, caja 3 exp. 07	X	X						
1785	ANH	Serie diezmos, caja 3 exp. 07	X							
1786	ANH	Serie diezmos, caja 3 exp. 07	X							
1791	AGI	ES.41091.AGI/23//SANTAFE,971	X	X	X	X	X	X	X	X
1792	AGI	ES.41091.AGI/23//SANTAFE,971	X	X	X	X	X	X	X	X
1795	AGI	ES.41091.AGI/23//SANTAFE,971	X	X	X	X	X	X	X	X
1797	ANH	Serie diezmos, caja 9 exp. 09	X	X	X	X				
1799	AGI	ES.41091.AGI/23//SANTAFE,971	X	X	X	X		X	X	X
1800	ANH	Serie diezmos, caja 9 exp. 19	X	X	X					
1802	ANH	Serie diezmos, caja 10, exp. 02	X	X	X	X	X	X		
1804	AGI	Serie tributarios, caja 27, exp.16	X	X	X	X	X	X	X	X
1805	ANH	Pres. de Quito, caja 256, lib. 635	X	X	X	X	X	X		
1806	ANH	Serie diezmos, caja 10, exp. 13	X	X	X	X	X	X		
1807	ANH	Serie diezmos, caja 10, exp. 16	X	X	X	X	X	X		
1814	ANH	Serie diezmos, caja 12, exp. 07	X	X	X	X	X	X		
1819	ANH	Serie diezmos, caja 13, exp. 27	X	X	X	X	X	X		
1820	ANH	Serie diezmos, caja 14, exp. 09	X	X	X	X	X	X		
1821	ANH	Serie diezmos, caja 14, exp. 25	X	X	X	X	X			
1822	ANH	Serie diezmos, caja 15, exp. 05	X	X	X					
1823	ANH	Serie diezmos, caja 15, exp. 07	X	X	X	X				
1824	ANH	Serie diezmos, caja 15, exp. 09	X	X	X					
1825	ANH	Serie diezmos, caja 15, exp. 11	X	X	X	X	X			
1826	ANH	Serie diezmos, caja 15, exp. 17	X	X	X	X				
1827	ANH	Pres. de Quito, caja 256, lib. 635	X	X	X	X	X			
1830	ANH	Serie diezmos, caja 16, exp. 09	X	X	X	X	X			

Notes: This Table summarizes the tithe data. Additional details are discussed in Appendix A.3.4.

A.4 Historical Surnames

The names of individuals in the historical tax records were digitized into a spreadsheet with the assistance of historians familiar with this type of documentation[7]. Since these records belong to the late colonial period (1780-1830), a time by which all individuals had already incorporated the tradition of baptism, each of the listed workers reported a surname. The digitized records correspond to the best quality data, as described in Table A.1[8]. As far as possible, I respect the original form of each name and transcribe them exactly as written in "old" Spanish. Nonetheless, I update certain surnames that are no longer written in a certain fashion. For instance, the historical surname "Caxamarca" can be found nowadays only as "Cajamarca". To distinguish these types of cases and to minimize transcription errors, I use the surname application of the National Institute of Statistics and Census of Ecuador (INEC), which shows the historical incidence of each surname[9]. However, given the arbitrariness in which some names were written, I posteriorly apply a cleaning algorithm that standardizes surnames, which is described in Appendix A.13. Finally, for the sake of consistency, the cleaning process of this individualized dataset follows the same guidelines as those described in Appendix A.1.1, while the geographical matching is the one of Appendix A.2.

Table A.4 displays some basic surname statistics in the historical records. The main observation is the low number of surnames compared to the current distribution (see Table A.5). However, as mentioned in the main text, this small number of surnames matches many individuals nowadays.

Table A.4: Summary Surnames Historical Records

Province	Mean (Concierto)	Individuals	Number of Surnames	Individuals per Surname
Azuay	.28	5219	836	6.24
Canar	.27	2983	552	5.4
Carchi	.39	816	334	2.44
Cotopaxi	.33	6628	1011	6.56
Chimborazo	.46	8746	1168	7.49
Imbabura	.41	5563	676	8.23
Loja	.25	2685	550	4.88
Pichincha	.63	10226	1535	6.66
Tungurahua	.39	3312	697	4.75

Notes: This Table summarizes the surname distribution in the historical tax records. The sample and treatment of the data are described in Appendix A.4. Surnames correspond to their standardized version after using the algorithm described in Appendix A.13.

[7] I am particularly grateful to Katiushka Pacheco for her help with this task.

[8] Ambato (18023), Cuenca (1792), Ibarra (1783), Latacunga (1828), Loja (1785), Otavalo (1820), Quito (1784), Riobamba (1812). They cover all the Ecuadorian Highland region, except the current province of Bolivar.

[9] In this application, one can search for any surname and see how many individuals were registered in the Civil Registry with that surname in each year.
`http://aplicaciones3.ecuadorencifras.gob.ec/VDATOS2-war/paginas/vrad/nom_ape.xhtml`

A.5 Geographic Information

All geographical variables were processed using the ArcGIS software and projected to the system of coordinates UTM 17S. For the case of rasters from the GAEZ-FAO project[10], given their relatively low resolution, I resample them when projecting using the option *"BILINEAR"* for continuous variables and *"NEAREST"* for discrete ones. For all raster data, I compute the average of each variable within a district using as reference the reconstructed colonial boundaries described in Appendix A.2. A GIS map with district administrative boundaries (2010) was obtained from INEC.

A.5.1 Potential Yields

Data for potential yields was obtained from GAEZ-FAO. I selected highland maize, potatoes, and wheat crops and their respective agro-climatically attainable yields under rain-fed water supply and low and medium inputs. Figure A.4 shows the relative productivity differences in maize versus potatoes (rain-fed water and low inputs) for the Highlands.

A.5.2 Overall Agricultural Suitability

The GAEZ-FAO project also includes an index of rainfed soil and terrain agricultural suitability for both low and high inputs. The index classifies each resolution bin into 8 categories, ranging from 1 (very high) to 8 (unsuitable). I use the information corresponding to low inputs to compute the overall agricultural suitability metric.

A.5.3 Temperature and Precipitation

Mean annual temperature and total annual precipitation were obtained from GAEZ-FAO.

A.5.4 Elevation

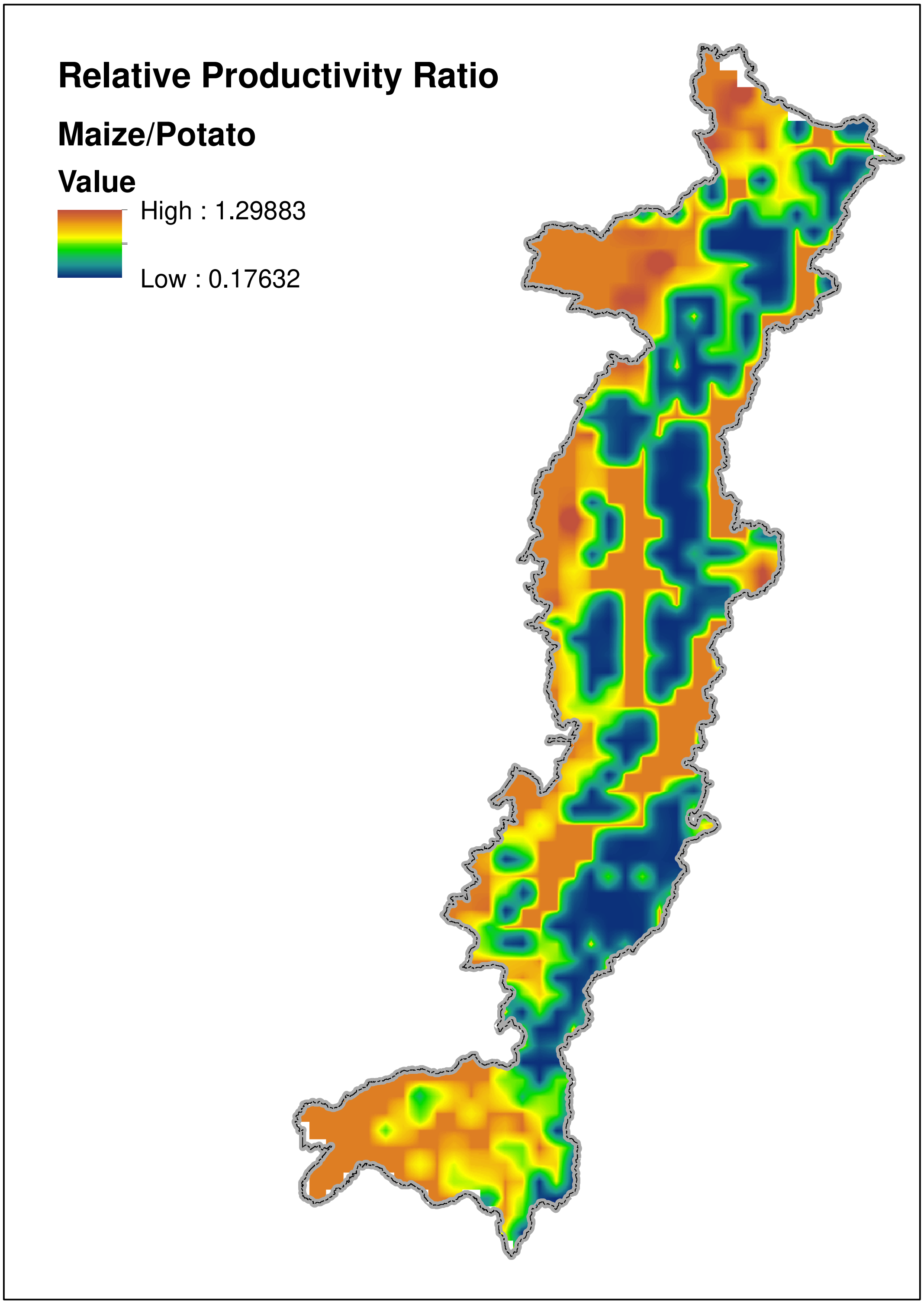
Elevation data are from the Shuttle Radar Topography Mission (SRTM). Details about it can be read in Dell (2010). I use this data to compute each district's average elevation and slope. To accurately calculate both, water bodies were removed. A GIS file of the waterbodies of Ecuador was obtained from DIVA-GIS (2016).

A.5.5 Coordinates

I obtain a map of each district's point location (coordinates) from the Military Institute of Ecuador. I also overlay this map with the SRTM one to get each district's altitude.

[10]GAEZ v3.0: https://www.gaez.iiasa.ac.at

Figure A.4: Relative productivity differences in maize and potatoes



Notes: Ratio of productivity in highland maize (in tonnes/ha) relative to productivity in potatoes (in tonnes/ha).
Source: Author's calculations based on data from the GAEZ-FAO project.

A.6 Agricultural Census

A.6.1 Agricultural Census 1974

This data is only available in books, so I digitized them. The unit of observation used by the census is the Agricultural Unit of Production (Unidad de Producción Agropecuaria (UPA) in Spanish), which is defined as any unit of land devoted partially or totally to agricultural production, regardless of size, location, type of ownership or legal status.

Land Gini. The Gini coefficient of land inequality is computed using data about the number and size of holdings. The size of each holding is classified as follows: (1) less than 0 hectares, (2) 0.01 to 0.5 hectares, (3) 0.5-1 hectares, (4) 1-2 hectares, (5) 2-3 hectares, (6) 3-4 hectares, (7) 4-5 hectares, (8) 5-10 hectares, (9) 10-20 hectares, (10) 10-20 hectares, (11) 20-50 hectares, (12) 50-100 hectares, (13) 100-500 hectares, (14) 500-1000 hectares, (15) 1000-2500 hectares, (16) more than 2500 hectares. Each group includes the total number of holdings and its average size. So, I calculate the Gini coefficient following the same procedure as Nunn (2008).

Distance to farm. The census also includes information regarding the distance of a UPA to a road that is accessible during the whole year ("Distancia a la via transitable todo el año"). The categories are less than 1 km, between 1 and 5 km, 5 and 10 km, and above 10 km. I compute the fraction of production units further than 5km by adding the last two categories and dividing by the total number of farms in a district.

A.6.2 Agricultural Census 2000

Similar to the 1974 Census, the unit of observation is the UPA. However, due to the sample design of this Census, statistics by parish are not necessarily representative. The microdata of this census can be obtained from INEC.

Land Gini. I compute the Land Gini by considering the total size of each UPA in a district. Weights provided in the database are used to calculate the statistics.

A.7 Population and Household Census (1950-1982)

Census data before 1990 are not digitized, so I transcribed the corresponding information for 1950, 1962, 1974, and 1982 from the physical books. In particular, I use the province volumes of each census, which contain desegregated data by parish. Data for Quisapincha and the Oriental districts of Zamora, Chito, and Valladolid in 1962 is not reported. Data for Alausi and Sibambe in 1982 is not reported. Additional details are provided below.

A.7.1 Illiteracy

Illiteracy (1950). Population of 10 years and above that can't read or write (Table 21).

Illiteracy (1962). Population of 6 years and above that can't read or write (Table 2).

Illiteracy (1974). Population of 10 years and above that can't read or write (Table 21).

Illiteracy (1982). Population of 10 years and above that can't read or write (Table 27).

A.7.2 Enrollment

Population from 6 to 14 years old that assist to regular education institutes.

Enrollment (1962). Table 3.

Enrollment (1974). Table 23.

Enrollment (1982). Table 29.

A.7.3 Years of School

Years of School (1962). Education of the population 6 years old and above (Table 2). Classification categories [4]: illiterate, early primary (1-3), late primary (4-6) and secondary and above. I assume each category represents 0, 2, 5, and 12 years, respectively.

Years of School (1974). Education of the population 6 years old and above (Table 22). Classification categories [5]: illiterate, early primary (1-3), late primary (4-6), secondary, and superior. I assume each category represents 0, 2, 5,12, and 16 years respectively.

Years of School (1982). Education of the population 6 years old and above (Table 28). Classification categories [5]: illiterate, early primary (1-3), late primary (4-6), secondary, and superior. I assume each category represents 0, 2, 5, 12, and 16 years respectively.

A.7.4 Population and Migration

To compute net migration rates, I follow the methodology of Delaunay (1987). First, I compute annual population growth rates based on intercensal data reported by Delaunay et al. (1985). Then, I subtract natural population growth rates (reported by Delaunay (1987) and computed at a province level) to obtain net migration rates.

A.8 Contemporary outcomes

Data for contemporary outcomes come from the population census of 1990, 2001, and 2010, obtained from the *Sistema Integrado de Indicadores Sociales del Ecuador* (SIISE (2011)) and *Sistema Nacional de Información* (SNI (2011)). Data for Lican in 2001 is not reported.

A.8.1 Extreme Poverty

The poverty definition I use is that of Unsatisfied Basic Needs. To be considered extremely poor under that definition, a household should satisfy two of the following criteria,

1. Hous has a dirt floor or is made of precarious building materials
2. Lack of running water, sewers, or toilets
3. Number of people per bedroom is greater than 3
4. Children between 7 and 11 years old do not attend school
5. Head of household has more than three dependents and less than fourth-grade education.

(SIISE; Section: *Desigualdad y Pobreza*; Subsection: *Pobreza*; Variable: Extrema pobreza por necesidades básicas insatisfechas (NBI).).

A.8.2 Illiteracy

Illiteracy is reported relative to the population of 15 years and above that can't read or write (SIISE; Section: *Educación*; Subsection: *Educación de la poblacion*; Variable: Analfabetismo.).

A.8.3 Years of School

Is the average years of school (approved) for the population above 24 years (SIISE; Section: *Educación*; Subsection: *Cobertura y acceso a la Educación*; Variable: Escolaridad.).

A.8.4 Share of workers in different occupations

The 1990 census divides the working population into four large industries: 1) Agriculture (Agricultura, Silvicultura, Caza y Pesca); Manufacture (Manufactura); Wholesale and Retail (Comercio al por Mayor y Menor); Public Sector (Administración Pública y Defensa). The working population is defined as the number of individuals above 10 years that, during the census week, declared a) worked at least one hour or b) had a job but did not work in the corresponding industry. (SNI; Section: *Economía*; Variable: Población ocupada en [SECTOR].)

A.9 Other Contemporary Outcomes

A.9.1 Consumption Poverty and Inequality (2014)

Molina et al. (2015) employ Elbers et al. (2003) methodology and combine the *Encuesta de Condiciones de Vida* (2014), a household welfare survey, with the 2010 census, to compute both consumption poverty and inequality at a parish level. I digitized the reported estimates.

A.9.2 Stunting (1990)

I digitized data from the 1990 height national census reported by Freire and Bacallao (1992). The census was performed on children from 6 to 9 years old and reports the number of children with retard in height (<-2 standard deviations), as well as the total number of children by parish. So I compute by locality the percent of children with height retardment.

A.9.3 Mortality (1990)

Both infant and maternal mortality are computed using micro-data from the Anuarios de Estadísticas Vitales[11]. Both rates consider the regular living parish of the mother instead of the place where the disease occurred and are calculated relative to the number of live births. As the latter is sometimes reported with a delay, I use 1990-2001 data to compute actual 1990 births using posteriorly updated information. Infant mortality is defined as the number of children dead before age 1, relative to 1000 live births. Maternal mortality is defined as the number of deaths related to birth causes (IS codes 380, 381) relative to 100.000 live births.

A.10 Satellite Lights

I obtain night light intensity from National Geophysical Data Center (2010). The data is reported as a six-bit digital number (DN) between 0 (no light) and 63 for every 30 arc-second output pixel. I use geospatial software to compute average intensity by district.

A.11 Road Density

Road density is defined as the total length of roads (considering elevation changes) divided by a district's surface area. The total length is computed using a GIS road network map of Ecuador available from the Military Geographic Institute of Ecuador. Roads are classified by type as National (3), Local (4), Primary (14), or Secondary (15), and by material: paved (1), not paved (2), temporary (3) or other (4). I omitted the national category when computing the total length. Surface data come from administrative boundaries described in Appendix A.5.

[11] Data can be retrieved from https://aplicaciones3.ecuadorencifras.gob.ec/BIINEC-war/index.xhtml

A.12 Administrative records

A.12.1 Surnames and Individual Chraracteristics

The Ecuadorian Internal Revenue Service (Servicio de Rentas Internas (SRI)) provided me with the complete list of taxpayers for the years 2007-2016. This list comes from a web page of public access, in which one can search the amount of income taxes to be paid for any individual in Ecuador[12]. However, given that one can only retrieve information individually from this service, I submitted a formal request to SRI to obtain the entire taxpayer register.

The dataset provided comprises only three variables. First, a single string that includes the full name of an individual; second, the citizen's ID; and third, the amount of income taxes to be paid. To recover an individual's surname, I split the string that contains its full name and then separate names from surnames. Ecuadorians, like most Spanish-speaking people, usually have two surnames. The first is associated with the father, and the second with the mother. It is also common for people to have two names. Nonetheless, there are cases in which an individual only has one surname or only one name. I consider all the possible cases and develop a procedure to separate names from surnames. Posteriorly, I infer the gender of a person, given their name, programmatically[13]. Aside from gender, the only other individual characteristic I can recover is the province of birth, which is included in the first two digits of a citizen's ID.

Table A.5 shows some summary statistics of the contemporary tax records for the eligible sample. As can be seen, there are important variations in labor income by place of birth (see below for income definition). In addition, one can notice the distribution of surnames is more skewed than the one reported in the historical records.

Table A.5: Summary Surnames Historical Records

Province	Mean (Income)	Individuals	Number of Surnames	Individuals per Surname
Azuay	6351.13	111230	2859	38.91
Canar	5945.12	32784	1666	19.68
Carchi	6871.31	39791	1504	26.46
Cotopaxi	5977.4	87830	2495	35.2
Chimborazo	6603.98	92263	2718	33.95
Imbabura	5975.8	90738	2439	37.2
Loja	6606.27	95365	1741	54.78
Pichincha	7680.77	625648	10785	58.01
Tungurahua	6717.93	94494	2796	33.8

Notes: This Table summarizes the distribution of surnames in the current tax records. The sample includes only males. Surnames correspond to their standardized version after using the algorithm described in Appendix A.13.

[12] `https://declaraciones.sri.gob.ec`. Information is available on the webpage from 2001, but conversations with tax officials suggested the data quality from 2001 to 2006 is not as good as afterward.

[13] Specifically, I use both Gender and GuessGender packages from R.

A.12.2 Tax Records

Individuals report income taxes in Ecuador through two different forms. The first one, called Form 107, corresponds to labor income for employees. In this case, employers fill out and submit income tax declarations for their employees, so the latter only provide information about their personal expenses. Instead, individuals with income from other sources (including self-employment, rents, dividends, and capital income) fill out Form 102. Individuals with income from dependent employment and other sources must consolidate all the information in the 102 Form. Since employers are more likely to report their employees' incomes truthfully, given the tax deductions in their declarations, I only use Form 107 data.

Since Form 107 is the simplest one and has remained almost the same over time, it is straightforward to homologate it across years. I define labor income as the total sum of taxable income from the main employer during a year, omitting deductions[14]. I further built a matched employer-employee dataset to recover workers' firm characteristics. To define a worker's location or industry, I use the mode of the whole period for which I have data.

A.13 Surname Standardization

I apply a simple standardization procedure to current and historical surnames to associate similar surnames with small variations. First, I remove any H if the surname starts with that letter unless accompanied by a U. Then, I replace every Z and V with S and B, respectively. I also replace certain syllables that sound similar. Finally, I change the end letters in cases with familiar sounds. The following Table summarizes this procedure,

Table A.6: Standardization Procedure

(1) Start with			(2) General			(3) Ends in	
From	To	Except	From	To	Except		
H		HU	Z	S		M	N
			V	B		A	O
			CE	SE		E	I
			CI	SI		U	O
			GE	JE		Y	I
			EY	EI		G	
			RR	R			
			LL	L			
			HUA	GUA	CHUA		
			AHUI	AGUI			
			GXS	X			
			GC	X			
			GS	X			

Notes: This Table describes the rules of the standardization procedure applied to surnames.

[14]The sum of fields 301, 303, 311, 331, and 401 in the 2016 form.

B Additional Results

B.1 Historical Background

B.1.1 Persistence

Persistence of the institution during colonial times can be noticed in Table B.1, which shows the differences between attached-to-*hacienda* and free indigenous workers for two regions, Cayambe and Otavalo, in a similar exercise as Valarezo (2002). As Valarezo notices, it is remarkable that even two centuries later, the percentages of indigenous workers attached to *haciendas* had barely changed in each zone. While the Otavalo region only had one-third of its workers as *conciertos*, Cayambe had almost 75 percent of them under that condition.

Table B.1: Free and attached indigenous workers for two regions

	Cayambe		Otavalo	
Year	Free (%)	Attached to *hacienda* (%)	Free (%)	Attached to *hacienda* (%)
1645/1685	20.97	79.03	67.07	32.93
1804	27.63	72.37	61.41	38.59
1820	27.49	72.51	66.49	33.51
1830	29.33	70.67	71.54	28.46

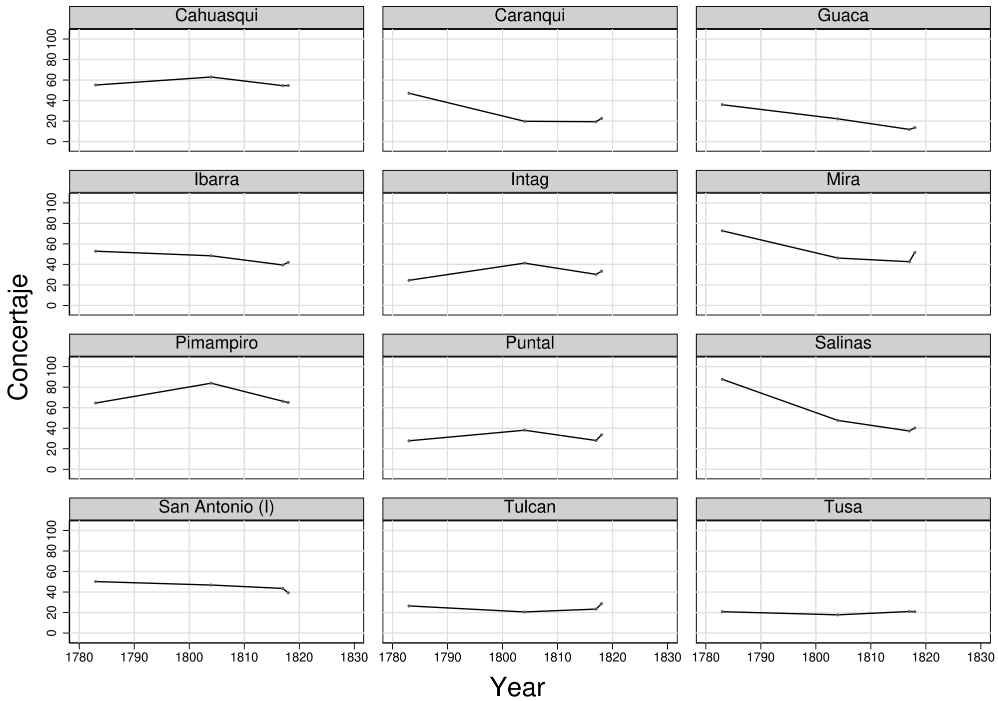
Notes: Otavalo region includes the colonial districts of San Pablo, Atuntaqui, Cotacachi, and Otavalo, while Cayambe those of Tabacundo, Tocachi, Cayambe, and Cangagua. Attached to *haciendas* is the percentage of workers living in *haciendas*. Data for 1645/1685 is from Valarezo (2002). See Appendix A.1 for a detailed explanation of the source of the rest of the data.

I confirm the same pattern using the collected data. For the colonial administrations of Ibarra and Otavalo, which have data for the longest period, Figure B.1 shows the evolution of *concertaje* from 1785 to 1830. Except for a few districts, the fraction of workers attached to *haciendas* is relatively stable[15]. Moreover, for those towns where *concertaje* rates are observed for at least two periods, Figure B.2 shows a strong correlation between *concertaje* in t with its immediate predecessor, in $t-1$.

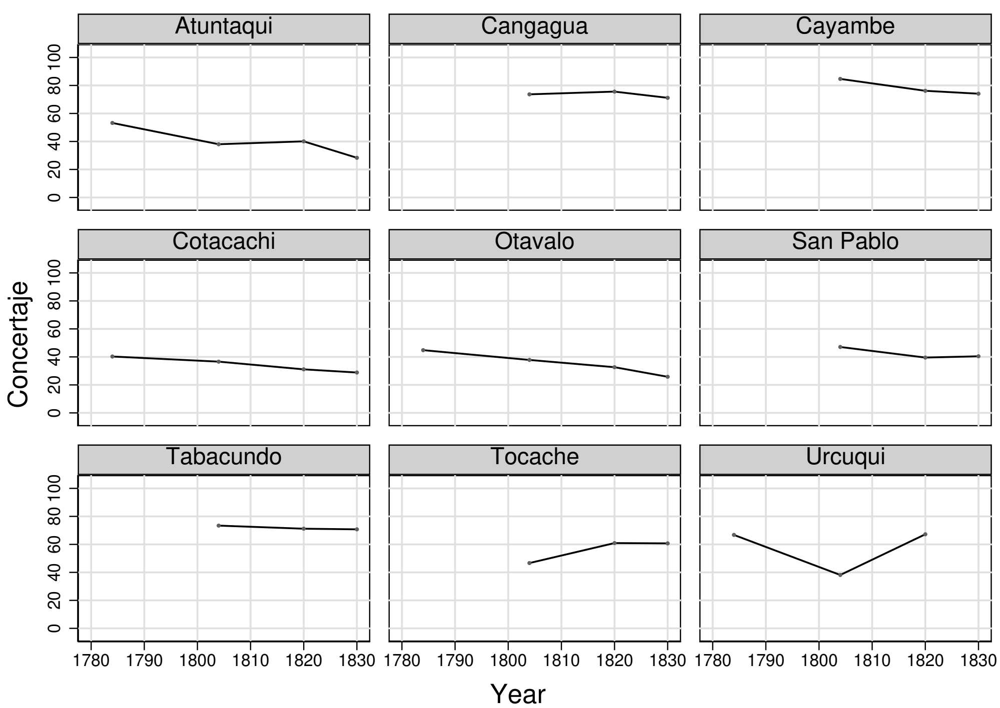
As mentioned in the text, aside from supplying a stable labor force, one of the reasons for this persistence is that indebted workers ended up being an asset to the property. Thus, if affordable, landlords had strong incentives to preserve them, as freeing them would have supposed a loss in their wealth. Although I have no information regarding the value of *haciendas* during colonial times, I collected data from the land registry in 1909. This data reports the total value of rural properties in

[15] Mira and Salinas had a large number of slaves in 1782, which might substituted *conciertos*.

Figure B.1: *Concertaje* rates for different years.



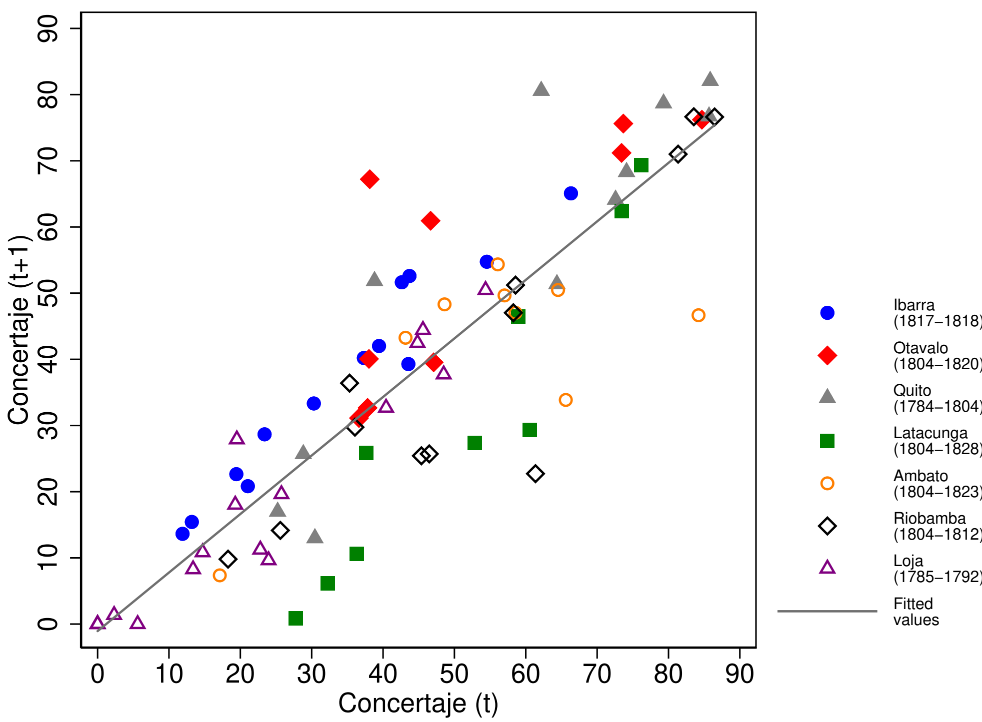
(a) Ibarra



(b) Otavalo

Notes: See Appendix A.1 for a detailed explanation of the source of the *concertaje* data. The town of Puntal includes information on both Puntal and El Angel.

Figure B.2: Correlation between *concertaje* in period t and *concertaje* in period $t + 1$.

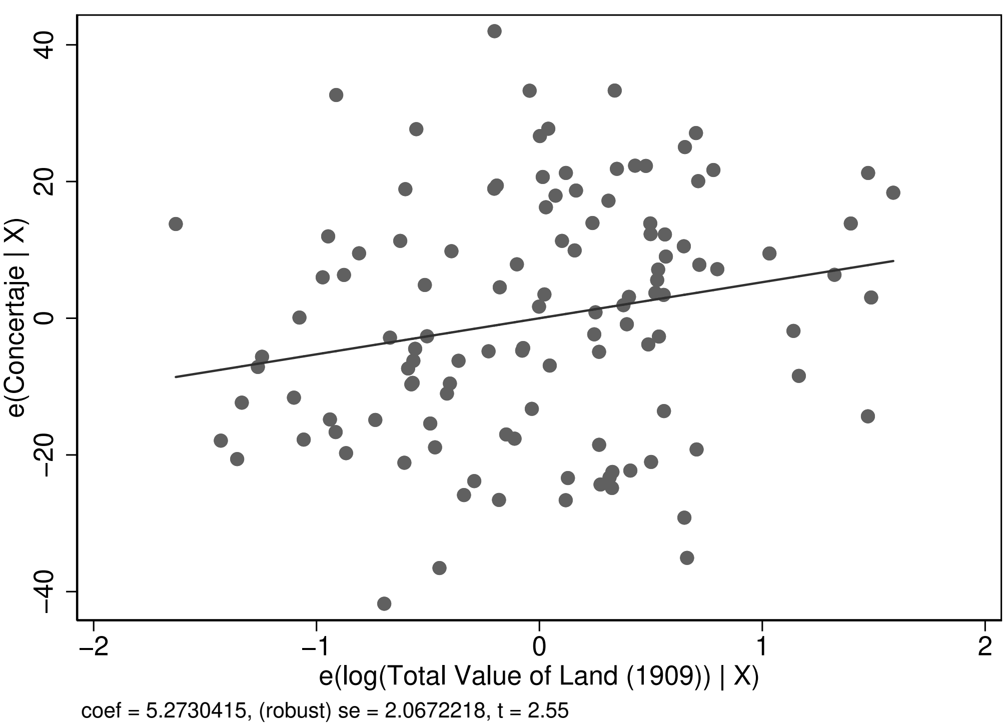


Notes: See Appendix A.1 for a detailed explanation of the source of the *concertaje* data. For 1804, only class quality data 1 and 2, as defined in Table A.1, is included.

each district[16]. Figure B.3 shows that in 1909, almost 90 years after the end of the colonial regime, the total value of land in those districts that had previously higher rates of *concertaje* was also higher.

Data limitations do not allow us to observe the institution's evolution after Ecuador's independence. Still, the system persisted until 1964 when the agrarian reform abolished the *huasipungo* system, a variation of *concertaje*. One way to see the long-run persistence of *concertaje* is by comparing its presence during colonial times with information about the *huasipungo* system 150 years later. Figure B.4 shows the correlation between the distribution of *concierto* and *huasipungo* workers across several modern provinces. As can be seen, the provinces with the greater concentration of *conciertos circa* 1804 continued to show the larger presence of *huasipungo* workers in 1959.

Figure B.3: Partial correlation between Log(Total Value of Land (1909)) and *Concertaje* (*circa* 1800)



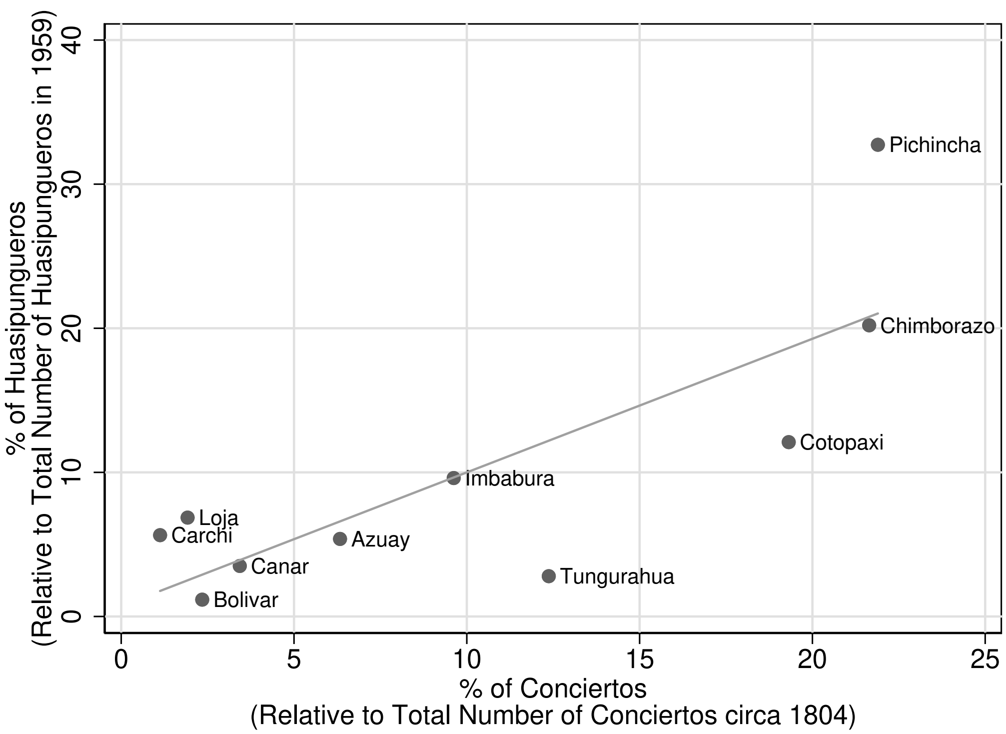
Notes: The regression controls for colonial province fixed effects. Standard errors are robust. Data about land's value comes from land tax records reported in Compañía Guía del Ecuador (1909) and described in Appendix A.3). See Appendix A.1 for a detailed explanation of the source of *concertaje* data.

B.1.2 Instrument and Initial Colonial Conditions

Differences in crop productivity may have created differences in population, leading to differences in economic organization. In that sense, panels (a) and (b) in Figure B.5 show both the relation between indigenous tributary population and tax per capita around 1600, respectively, with maize to potato

[16]Unfortunately, disaggregated information or simply the number of taxed rural properties is not shown.

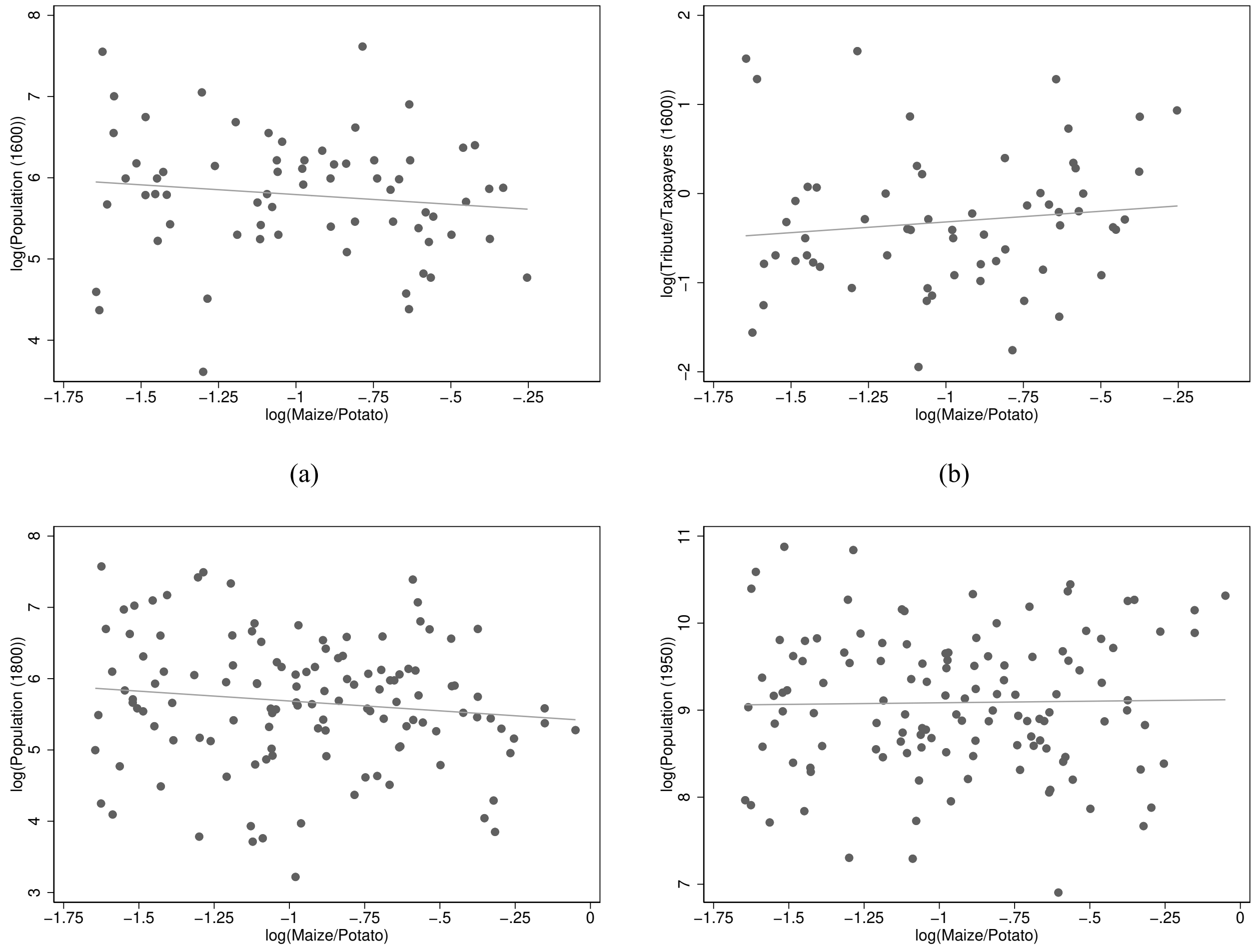
Figure B.4: Relation between *Concertaje* and *Huasipungo* system.



Notes: Data about conciertos *circa* 1804 is from Oberem (1981b), except for the provinces of Azuay and Canar, that comes from the historical tax records of the colonial province of Cuenca, described in Appendix A.1. Data about huasupingueros in 1959 comes from Barsky (1984) (p.73). The match between historical and modern provinces has been done following the criteria explained in Appendix A.2.

productivity. As can be noticed, there is no clear relation between the relative productivity of crops and the initial colonial conditions. Thus, although this result should be taken with caution since early data for the population is of low quality (Ortiz de la Tabla Ducasse (1980)), it nonetheless indicates the initial conditions of the region. Moreover, panels (c) and (d) confirm that the instrument is not correlated with population differences either at the end of colonial times (1800) or in 1950.

Figure B.5: Relation between instrument and population



(a) (b) (c) (d)

Notes: See Appendix A.3 for a detailed explanation of the source of the population data. Maize to Potato is computed as explained in Equation (3) in the main text.

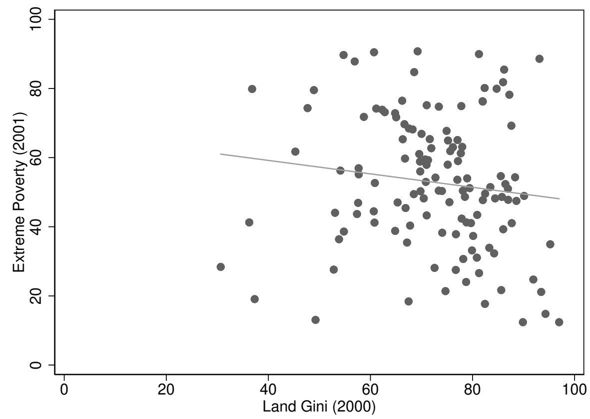
B.2 Mechanisms

B.2.1 Relation between inequality and economic development

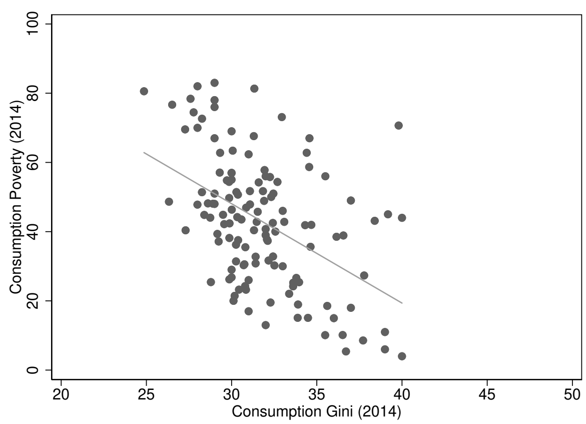
Figure B.6 shows the correlations between inequality and economic development. As seen, except for consumption Gini, land inequality is not correlated with economic development.

Figure B.6: Relation between inequality and economic development

(a) Land Gini (1974)



(b) Land Gini (2000)



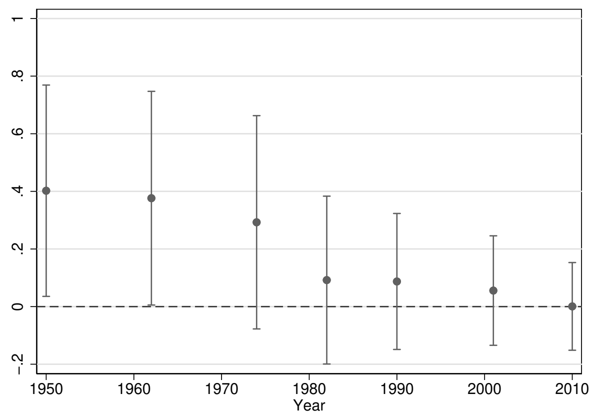
(c) Consumption Gini (2014)

Notes: See Appendix A.6, A.8 and A.9 for a detailed explanation of the sources and definitions of the data.

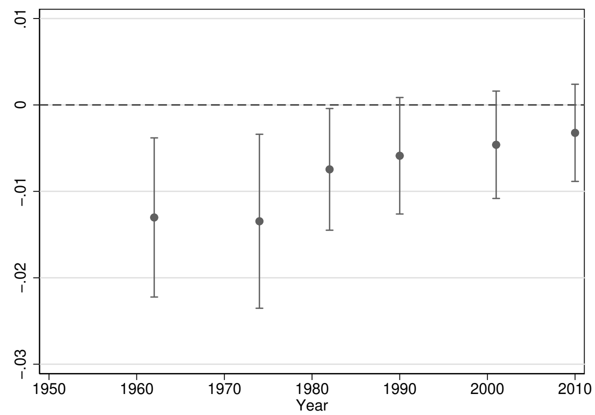
B.2.2 Effect on Human Capital for Different Periods

Similar to Figure 4 in the main text, Figure B.7 shows how the effect of *concertaje* on different measures of human capital has reduced over time.

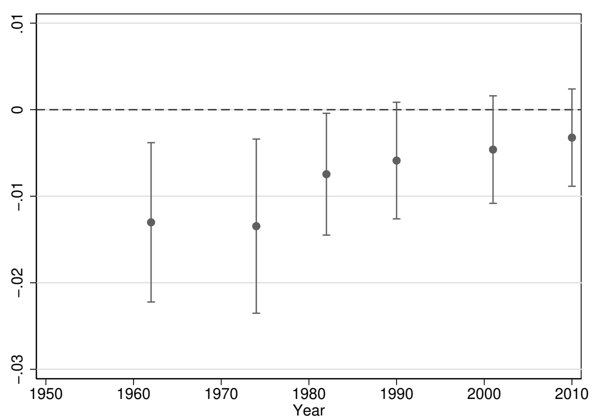
Figure B.7: *Concertaje's* effect on Human Capital for different years



(a) Illiteracy (IV)



(b) Years of School (IV)



(c) Enrollment (IV)

Notes: regressions as columns (1) and (2) in Table 4 in the main text, respectively. Confidence interval: 95% (robust standard errors). See Appendix A.8 for definitions and sources of years of school and illiteracy data for different periods.

B.2.3 Other Channels

Health One mechanism by which *concertaje* could have affected long-run development is by deteriorating the health of *hacienda* workers. In fact, it has been documented that both the harsh and unsanitary labor conditions *conciertos* faced affected their health (Oberem, 1981a, p. 324). This takes particular relevance given that health affectations like trauma may have intergenerational implications (Costa et al. (2018)), which could partially explain the persistent effects of the institution. Moreover, health directly affects children's human capital formation and worker's productivity (Thomas and Frankenberg (2002)). To test this channel, and due to the limited information, I collected data from different sources to have measures that proxy local health conditions. I describe the origins and treatment of the data in Appendix A.9. Table B.2 presents the results.

First, column (1) shows the effect of *concertaje* on the percentage of stunting children in 1990. As can be seen, although the IV coefficient is positive, it is not statistically significant. Since children's malnutrition is typically reflected in low height and weight, it seems *concertaje* did not directly affect it. Instead, in columns (2) and (3), I estimate the effect of *concertaje* on children and maternal mortality, respectively. Once again, the IV estimates are not statistically significant. These results suggest that health may not be a channel of persistence, although more and better data is needed to assess this conclusion.

Table B.2: Mechanisms: Health

	Percent of Stunting Children (1990)	Infant Mortality (1990)	Maternal Mortality (1990)
	(1)	(2)	(3)
Panel A: 2SLS			
Concertaje	.1015	-.0137	2.2362
	(.1526)	(.3476)	(1.7981)
	[.2023]	[.2084]	[1.9013]
Panel B: First Stage			
Instrument	101.8123	101.8123	101.8123
	(26.4564)***	(26.4564)***	(26.4564)***
F Statistic (Robust)	14.8095	14.8095	14.8095
Panel C: OLS			
Concertaje	-.0261	-.0532	2.1232
	(.0599)	(.1333)	(.8541)**
	[.0772]	[.1088]	[.6486]***
Geo. Controls	Yes	Yes	Yes
Observations	125	125	125
Mean Dep. Var.	56.4941	43.8369	115.3613

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include elevation, latitude longitude, slope, total annual precipitation, mean annual temperature, and average agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Migration Another way *concertaje* could have generated persistent effects is by influencing migration patterns. On one side, the harsh labor conditions in *haciendas* indeed motivated *conciertos* to escape. Although difficult to do so, it is likely that when specific coercive mechanisms were abolished (like debtor's prison), some workers immigrated out of the highlands[17]. So, similar to what was observed in the United States during the so-called "Great Migration"[18], it is likely that former *conciertos* abandoned their regions once legally allowed to. As immigrants may select on attributes like health or ability (Halliday and Kimmitt (2008); Hendricks and Schoellman (2017)), this situation could have implied the departure of productive workers.

Conversely, limited migration via imposed restrictions could also have important implications for labor productivity and welfare (Tombe and Zhu (2019)). In that sense, the nature of labor coercion demands limited worker mobility, which altogether, with the close links of indigenous workers to their *hacienda's* communities, could have disincentivized migration (Mörner (1978)). In either case, migration (or lack of) likely affected regional development.

To test this channel, one would like to have historical migration rates across districts. Such data, to the best of my knowledge, does not exist. So I follow the same procedure as Delaunay et al. (1985) and impute annual net migration rates across census periods by computing a district's population growth rates and subtracting the corresponding provincial population growth rate. Implicitly, it is assumed that the provincial growth rate captures the "natural" population growth rate, so deviations from it at a district level are attributed to migration. I provide additional details on how to compute these rates in Appendix A.7.4. Due to data availability, one can only impute these rates since 1950. However, the main limitation of this procedure is that it is prone to cofound both fertility and migration patterns and does not allow differentiation from out-migration. Table B.3 shows the results.

As can be seen, in all columns, both OLS and IV estimates show a positive, although barely statistically significant, effect of *concertaje* on net migration. Note also that average net migration rates were negative during these periods, showing out-migration from the Highland. In terms of the magnitude, the estimates suggest that a 10 pp increase in *concertaje* rates during colonial times increased average net migration rates between 0.27 (1962-1950) to 0.64 (1982-1974) percentage points (almost 1/5 of an SD). Interestingly, the effect during the period that coincided with most of the agrarian reform (1962-1974) was not statistically significant. Instead, the period posterior to it (1982-1974) shows the largest effect, perhaps related to the more flexible immigration conditions once the *huasipungo* system was abolished. Despite data caveats, this suggests that the migration channel may have only started to operate recently.

These results are somehow in line with those of Dell (2010), who evidences that during colonial times, migration was higher in *mita* districts, although using a limited sample of locations. More precisely are the results of Carpio and Guerrero (2021), who show that *mita* districts nowadays have fewer surnames, a proxy for long-term migration and depopulation. However, they argue that the

[17] Anecdotal evidence is the novel "A la Costa" (To the Coast), of Luis A. Martinez, which describes how the more attractive economic conditions of the Coast (due to the cacao boom), motivated workers from the highlands (not exclusively indigenous) to migrate there looking for a more prosperous future.

[18] See Collins (2021) for a recent literature review on the topic.

Table B.3: Mechanisms: Migration

	Migration (1962-1950)	Migration (1974-1962)	Migration (1982-1974)
	(1)	(2)	(3)
Panel A: 2SLS			
Concertaje	.0269	.0284	.0635
	(.0184)	(.0174)	(.0208)***
	[.0159]*	[.0174]	[.0245]***
Panel B: First Stage			
Instrument	101.8123	101.8123	101.8123
	(26.4564)***	(26.4564)***	(26.4564)***
F Statistic (Robust)	14.8095	14.8095	14.8095
Panel C: OLS			
Concertaje	.007	.01	.0205
	(.0073)	(.0055)*	(.007)***
	[.007]	[.0052]*	[.0069]***
Geo. Controls	Yes	Yes	Yes
Observations	125	125	125
Mean Dep. Var.	-1.7693	-1.3353	-1.474

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include, in logarithms, (center) altitude, latitude longitude; (average) elevation, slope, total annual precipitation, mean annual temperature, and agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

demographic effect of the mining *mita* was more likely to have occurred during colonial times. In the case of Ecuador, in contrast, there does not seem to be a regional relation between migration and *concertaje*, at least before the agricultural reform. Although, as shown in the main text, the pseudo-descendants of *conciertos* seem less prone to migrate. Unfortunately, the lack of better data does not allow me to study this mechanism in more extensive detail.

B.2.4 Sectoral composition (individual effects)

Table B.4: Intergenerational Mobility: Second Stage (2007-2016)

	Primary Sector	Secondary Sector	Terciary Sector
	(1)	(2)	(3)
Concierto	.0002**	.00043***	-.00062***
	(.00008)	(.0001)	(.00014)
Adjusted R-squared	.0296	.0076	.0087
Province FE	Yes	Yes	Yes
Observations	762799	762799	762799
N Surnames	2379	2379	2379
Mean Dep. Var.	.0864	.2628	.6508

Notes: Clustered standard errors by surname in parentheses. The sample includes only males. Surnames correspond to their phonetic algorithm version described in Appendix B.4.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Since current employee's tax records can be matched with those of their employers, it is possible to know in which industry an individual works. Section A.12.2 explains the treatment of the data. So similar to the procedure to compute the effect on individual migration, I estimate the impact of descending of a *concierto* on the probability of working in the primary, secondary or tertiary sector[19]. Table B.4 shows the results.

Consistent with the regional estimates, the individual ones also show an effect of the institution on sectoral composition. However, these estimates show a positive, rather than a negative, effect on working in the secondary sector, which also includes manufacturing industries. In this case, an increase in 10 percentage points (pp) in a surname's *concertaje* rate increments the probability of working in the primary and secondary sector by 0.2 and 0.4 pp, respectively and decreases the likelihood of working in the tertiary industry by 0.6 pp.

B.3 Robustness Checks (regional effects)

In this section, I present several results with alternative decisions to those shown in the main text to test the robustness of the estimates. They include different variations of the instrument and the estimation procedure.

[19] I define the sectors according to their ISIC codes. Primary (11), Secondary (12,13), Services (22, 33, 34, 45, 46, 58.)

B.3.1 Alternative variations of the instrument

Table B.5 shows the effect of *concertaje* on extreme poverty in 1990, but using alternative measures of the proposed instrument. Column (1) shows the baseline results when the instrument is the relative productivity of maize to potatoes, using predicted yields for rain-fed water and low inputs. In Column (2), the instrument is also the relative productivity of both crops, although it uses intermediate inputs to predict yields. As can be seen, the IV estimate of *concertaje* remains quite similar, although the instrument's strength is somehow smaller.

The same occurs when I use other crops. Columns (3) and (4), equivalent to (1) and (2), respectively, show the effect when the instrument is the relative productivity of maize to wheat. Wheat, similar to potatoes, is a crop with lower labor requirements than maize but has almost the same caloric content. However, it is not a crop native to the Americas, which might have some influence on local food preferences. Nevertheless, in both cases, the effect of *concertaje* lies close to the baseline but with a reduced predictive power of the instrument.

Finally, column (5) shows the estimates when the baseline instrument is used directly, as opposed to employing the two-step IV method (Wooldridge, 2010, p. 939). In conclusion, neither the choice of crops or inputs nor the method used significantly alter the baseline estimates.

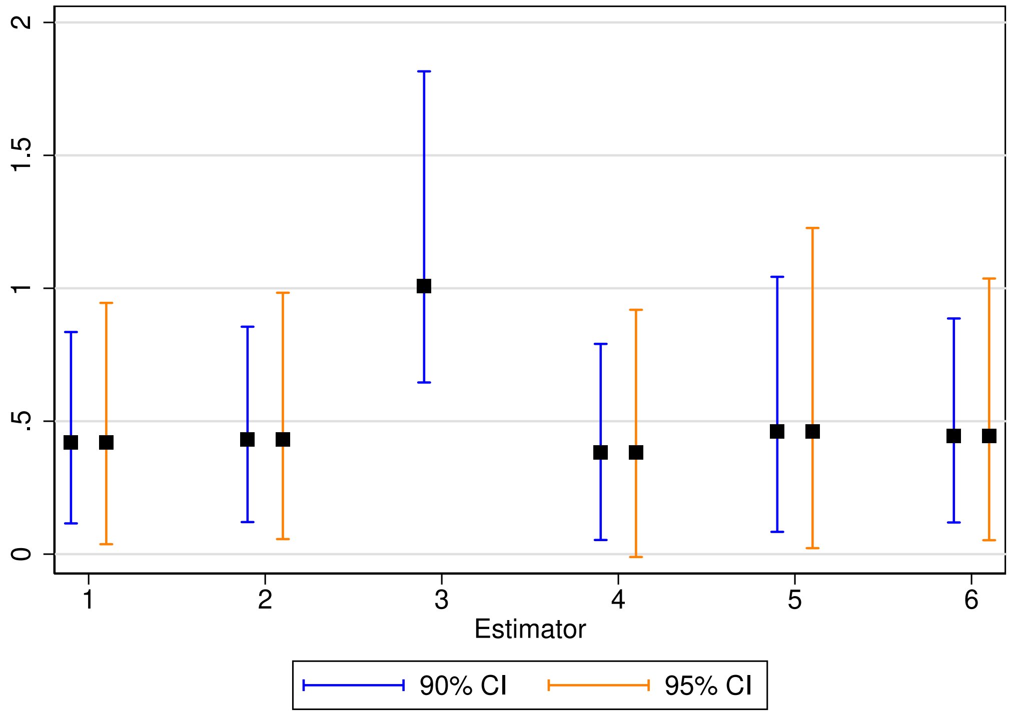
Table B.5: Robustness Checks

	Instrument				Method
	Maize/Potato (Low)	Maize/Potato (Intermediate)	Maize/Wheat (Low)	Maize/Wheat (Intermediate)	Standard IV
	(1)	(2)	(3)	(4)	(5)
Panel A: 2SLS					
Concertaje	.4207	.3446	.3582	.3095	.4317
	(.1976)**	(.1923)*	(.2171)*	(.213)	(.1996)**
	[.1903]**	[.186]*	[.2131]*	[.2047]	[.1985]**
Panel B: First Stage					
Instrument	101.8123	101.3887	101.861	101.7278	21.0406
	(26.4564)***	(26.5104)***	(29.0686)***	(28.718)***	(5.5293)***
F Statistic (Robust)	14.8095	14.6266	12.2791	12.5479	14.4801
Panel C: OLS					
Concertaje	.0798	.0798	.0798	.0798	.0798
	(.0729)	(.0729)	(.0729)	(.0729)	(.0729)
	[.0812]	[.0812]	[.0812]	[.0812]	[.0812]
Geo. Controls	Yes	Yes	Yes	Yes	Yes
Observations	125	125	125	125	125
Mean Dep. Var.	64.9141	64.9141	64.9141	64.9141	64.9141

Notes: Robust standard errors are in parentheses. Conley standard errors are in brackets. Geographic controls include, in logarithms, (center) altitude, latitude longitude; (average) elevation, slope, total annual precipitation, mean annual temperature, and agricultural suitability.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

B.3.2 Anderson-Rubin Confidence Intervals

Figure B.8: Anderson-Rubin Confidence Intervals for different specifications.



Notes: Each estimator corresponds to a column in Table 4 in the main text.

B.3.3 Summary Statistics

Table B.6: Summary Statistics

	Mean	S.D	p10	p90
	(1)	(2)	(3)	(4)
Concertaje (%) [1800]	44.82	22.54	15.46	76.48
Log(Maize/Potato)	-0.95	0.40	-1.52	-0.38
Agricultural Suitability (index)	4.06	1.43	2.11	5.85
Altitude (m.)	2606.93	422.99	2072.00	3084.00
Slope (m.)	7.03	2.28	4.01	10.07
Total annual precipitation (mm)	960.60	205.53	668.71	1204.61
Mean annual temperature (oC)	13.05	2.60	10.30	15.64
Extreme poverty (%) [1990]	64.91	16.86	41.64	83.63
Night Light Intensity [1992]	3.76	6.29	0.00	9.99
Illiteracy (%) [1962]	44.38	16.07	26.56	68.40
Years of school [1962]	2.08	0.81	1.11	3.15
Primary enrollment (%) [1962]	51.16	15.21	28.67	68.55
Workers in agriculture (%) [1990]	54.73	25.20	16.16	82.51
Workers in manufacture (%) [1990]	10.53	9.78	2.31	22.49
Workers in wholesale and retail (%) [1990]	5.54	3.98	1.73	11.65
Land Gini (%) [1974]	75.57	14.87	56.73	91.32
Land Gini (%) [2000]	72.94	12.91	56.91	87.22
Consumption Gini (%) [2014]	31.85	3.10	28.61	36.56
Total Road Density [2010]	461.25	256.17	181.13	840.52
Farms further than 5km to main road (%) [1974]	19.20	20.97	0.11	53.93
Workers in public Sector (%) [1990]	8.00	5.69	2.52	16.19

Notes: See the main text for a definition of each variable. Appendices A.1-A.8 provide further detailed explanation of the data sources.

B.4 Robustness Checks (individual effects)

Aside from treating surnames in how they are spelled, I also rewrite them using a Spanish-phonetic algorithm proposed by Amón et al. (2012). I employ this algorithm after applying the cleaning procedure described in Appendix A.13. Tables B.7 and B.8 show the results, which are directly comparable to those of the first and second stage in the main text, respectively. First, in Table B.7, one can see that phonetic surnames continue to explain an important variation of the *concierto* status (statistically significant), although in a smaller magnitude.

Table B.7: Intergenerational Mobility (First Stage): Concierto Status (1780-1830)

	Base Model	Weight Inverse Surnames' Frequency (Colony)	Province Rare Surnames (Colony)	Weight Inverse Concertaje's SD (Colony)	Weight Local Surname Specificity (Colony)
	(1)	(2)	(3)	(4)	(5)
F Statistic	2.9466	7.6112	2.232	3.7609	6.0677
Adjusted R-squared	.1254	.2159	.17	.1558	.2078
Province FE	Yes	Yes	Yes	Yes	Yes
Observations	45818	45818	11510	44910	45818
N Surnames	1512	1512	1385	1461	1512
Mean Dep. Var.	.4233	.4233	.4172	.4239	.4233

Notes: Robust standard errors are in parentheses. Surnames correspond to their phonetic algorithm version described in Appendix B.4.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table B.8 shows the estimates corresponding to the second stage instead. In this case, the effect of descending from coerced workers is also statistically significant, although quantitatively, it shows smaller effects. For instance, the base model implies that an increase of 10 percentage points in a surname's *concertaje* rate reduces labor income to descendants by 12%, 6 percentage points smaller than the baseline estimates. The rest of the columns show the estimates of the same robustness checks discussed in the main text. As can be seen, they all are similar to those reported in the main text but in a smaller magnitude. The fact that the phonetic algorithm associates surnames more liberally can explain these differences.

Table B.8: Intergenerational Mobility: Second Stage (2007-2016)

	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)
Concierto	-.00118**	-.00086**	-.00112**	-.00126**	-.00122**
	(.00053)	(.00042)	(.00048)	(.00057)	(.00053)
Adjusted R-squared	.0068	.0033	.0039	.0072	.0144
Province FE	Yes	Yes	Yes	Yes	Yes
Observations	994303	994303	426666	916233	994303
N Surnames	1313	1313	1222	1261	1313
Mean Dep. Var.	8.2344	8.2344	8.3103	8.2229	8.2344

Notes: Clustered standard errors by surname in parentheses. The sample includes only males. Surnames correspond to their phonetic algorithm version described in Appendix B.4.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

C Derivation of Reduced-Form Estimates

C.1 Individual Estimates (OLS)

Recall the model is composed of the following three equations,

$$y_{id',2} = \alpha_{id',2} + \gamma_2 w_{\overline{d},1} + \beta_2 y_{id,1} + \epsilon_{id',2} \tag{C.1}$$

$$y_{id,1} = \alpha_{id,1} + \gamma_1 w_{\overline{d},0} + \beta_1 c_{id,0} + \epsilon_{id,1} \tag{C.2}$$

$$c_{id,0} = \alpha_{id,0} + \gamma_0 z_d + \epsilon_{id,0} \tag{C.3}$$

The OLS estimate of equation (2) in the paper is

$$\mathcal{B}_2^{OLS} = \frac{cov(y_{id',2}, c_{id,0})}{var(c_{id,0})} \tag{C.4}$$

By substituting (C.1) in (C.4), we get

$$\begin{aligned}
\mathcal{B}_2^{OLS} &= \frac{cov(\alpha_{id',2} + \gamma_2 w_{\overline{d},1} + \beta_2 y_{id,1} + \epsilon_{id',2}, c_{id,0})}{var(c_{id,0})} \\
&= \frac{cov(\alpha_{id',2}, c_{id,0})}{var(c_{id,0})} + \gamma_2 \frac{cov(w_{\overline{d},1}, c_{id,0})}{var(c_{id,0})} + \beta_2 \frac{cov(y_{id,1}, c_{id,0})}{var(c_{id,0})} + \frac{cov(\epsilon_{id',2}, c_{id,0})}{var(c_{id,0})} \\
&= \frac{cov(\alpha_{id',2}, c_{id,0})}{var(c_{id,0})} + \gamma_2 \frac{cov(w_{\overline{d},1}, c_{id,0})}{var(c_{id,0})} + \beta_2 \frac{cov(y_{id,1}, c_{id,0})}{var(c_{id,0})}
\end{aligned} \tag{C.5}$$

Where the last line comes from the *i.i.d* assumption of the error term. Now, since $\alpha_{id',t} = \rho\alpha_{id',t-1}$, then $\alpha_{id',2} = \rho^2\alpha_{id',0}$. Similarly, recall $w_{\overline{d},1} = \lambda w_{\overline{d},0}$. Hence, substituting these expressions, in addition to (C.2), in equation (C.5) leads to,

$$\mathcal{B}_2^{OLS} = \rho^2 \frac{cov(\alpha_{id',0}, c_{id,0})}{var(c_{id,0})} + \gamma_2 \lambda \frac{cov(w_{\overline{d},0}, c_{id,0})}{var(c_{id,0})} + \beta_2 \frac{cov(\alpha_{id,1} + \gamma_1 w_{\overline{d},0} + \beta_1 c_{id,0} + \epsilon_{id,1}, c_{id,0})}{var(c_{id,0})} \tag{C.6}$$

Once again, since the error is *i.i.d*, $\alpha_{id,1} = \rho\alpha_{id,0}$, and $w_{\overline{d},0} = \delta c_{\overline{d},0}$, we have,

$$\mathcal{B}_2^{OLS} = (\rho^2 + \beta_2\rho)\frac{cov(\alpha_{id',0}, c_{id,0})}{var(c_{id,0})} + (\gamma_2\lambda + \beta_2\gamma_1)\sigma\delta + \beta_1\beta_2 \tag{C.7}$$

where $\sigma = \frac{cov(c_{\overline{d},0}, c_{id,0})}{var(c_{id,0})} = \frac{var(c_{\overline{d},0})}{var(c_{id,0})}$ is the between-region proportion of the population variance (Güell et al. (2018), Borjas (1992)).

C.2 Regional Estimates (IV)

$$y_{\tilde{d},2} = a_{\tilde{d},2} + g_2 w_{\overline{d},1} + b_2 y_{\overline{d},1} + e_{\tilde{d},2} \tag{C.8}$$

$$y_{\overline{d},1} = a_{\overline{d},1} + g_1 w_{\overline{d},0} + b_1 c_{\overline{d},0} + e_{\overline{d},1} \tag{C.9}$$

$$c_{\overline{d},0} = a_{\overline{d},0} + g_0 z_{d,0} + e_{\overline{d},0} \tag{C.10}$$

The IV estimate of equation (6) in the paper is

$$\mathbf{B}_2^{IV} = \frac{cov(y_{\tilde{d},2}, z_d)}{cov(z_d, c_{\overline{d},0})} \tag{C.11}$$

By substituting (C.81) in (C.11), we get

$$\begin{aligned}
\mathbf{B}_2^{IV} &= \frac{cov(a_{\tilde{d},2} + g_2 w_{\overline{d},1} + b_2 y_{\overline{d},1} + e_{\tilde{d},2}, z_{d,0})}{cov(z_d, c_{\overline{d},0})} \\
&= \frac{cov(a_{\tilde{d},2}, z_d)}{cov(z_d, c_{\overline{d},0})} + g_2 \frac{cov(w_{\overline{d},1}, z_d)}{cov(z_d, c_{\overline{d},0})} + b_2 \frac{cov(y_{\overline{d},1}, z_d)}{cov(z_d, c_{\overline{d},0})} + \frac{cov(\epsilon_{\tilde{d},2}, z_d)}{cov(z_d, c_{\overline{d},0})} \\
&= \frac{cov(a_{\tilde{d},2}, z_d)}{cov(z_d, c_{\overline{d},0})} + g_2 \frac{cov(w_{\overline{d},1}, z_d)}{cov(z_d, c_{\overline{d},0})} + b_2 \frac{cov(y_{\overline{d},1}, z_d)}{cov(z_d, c_{\overline{d},0})}
\end{aligned} \tag{C.12}$$

Where the last line comes from the *i.i.d* assumption of the error term. Now, since $a_{\tilde{d},t} = \rho a_{\tilde{d},t-1}$, then $a_{\tilde{d},2} = \rho^2 a_{\tilde{d},0}$. Similarly, recall $w_{\overline{d},1} = \lambda w_{\overline{d},0}$. Hence, substituting these expressions, in addition to (C.9), in equation (C.12) leads to,

$$\mathbf{B}_2^{IV} = \rho^2 \frac{cov(a_{\tilde{d},0}, z_d)}{cov(z_d, c_{\overline{d},0})} + g_2 \lambda \frac{cov(w_{\overline{d},0}, z_d)}{cov(z_d, c_{\overline{d},0})} + b_2 \frac{cov(a_{\overline{d},1} + g_1 w_{\overline{d},0} + b_1 c_{\overline{d},0} + e_{\overline{d},1}, z_d)}{cov(z_d, c_{\overline{d},0})} \tag{C.13}$$

Now, since $w_{\overline{d},0} = \delta c_{\overline{d},0}$, in addition to the orthogonality condition of the instrument ($cov(a_{\tilde{d},0}, z_d) = 0$), and the fact the error is *i.i.d*, we have,

$$\mathbf{B}_2^{IV} = (g_2 \lambda + g_1 b_2)\delta + b_1 b_2 \tag{C.14}$$

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