Office-Selling, Corruption, and Long-Term Development in Peru*

Jenny Guardado†
jgr45@georgetown.edu
Georgetown University

Current Version: July 2016

Abstract

The paper uses a unique hand-collected dataset of the prices at which the Spanish Crown sold colonial provincial governorships in 17th-18th century Peru to examine the impact of the quality of government officials on long-run development. Combining provincial characteristics with exogenous variation in appointment criteria due to the timing of European wars, I first show that provinces with greater extraction potential tended to fetch higher prices and attract "lower-quality" buyers. In the long-run, these high-priced provinces have lower household consumption, schooling, and public good provision. The type of rulers of these provinces likely exacerbated political conflict and undermined inter-generational cultural assimilation in the population.

---

*I am grateful to Scott Abramson, Alberto Diaz-Cayeros, Oeindrila Dube, Sanford Gordon, Jens Heinmueller, Noel Johnson, Mark Koyama, Steven Pennings, Adam Przeworski, Pablo Querubin, David Stasavage, Leonard Wantchekon, Tianyang Xi, and seminar participants at 2016 Ridge-LACEA PEG Workshop, UNC Chapel Hill, Georgetown, UChicago, Stanford, George Mason, Princeton, IR/PS UCSD, Emory, ITAM, and the Harris School of Public Policy, as well as to conference participants of the 2014 Empirical Political Economy Network Conference, 2014 International Economics Association, 2013 Northeast University Development Consortium (NEUDC), 2013 American Political Science Association Conference, 2013 International Society for New Institutional Economics, 2013 Alexander Hamilton Conference, 2013 Midwest Political Science Association Conference, 10th Midwest International Economic Development Conference, and NYU Dissertation Seminar for valuable comments on this paper. All remaining errors are my own.

†Corresponding Author. Georgetown University 3700 O ST NW Room ICC 484 Ph. 202-603-7945.

1 Introduction

The three-hundred-year Spanish rule of the Americas was carried out, to a large extent, by provincial governors who had huge discretion over the lives of their indigenous subjects. While some of these individuals were recognized as competent and honest, historians regard others as little more than "tyrants or thieves" (Solorzano y Pereira cited by Moreno 1977: 71). Many of these governors bought their posts from the Spanish crown during times of fiscal crisis. In this paper, I use a hand-collected dataset of these prices to distinguish the "tyrants or thieves" from other governors, and investigate their long-run impact on economic development within Peru. I find that provinces with worse colonial governors experience economic disadvantage even hundreds of years after the governors left office, largely because they created a persistent culture of political violence, mistrust, and low incentives to culturally assimilate. While other papers have discussed how historical institutions explain contemporary underdevelopment through property rights or public goods,[1] how the selection and quality of public officials determine those outcomes has been relatively overlooked.

Specifically, I take advantage of a unique market for colonial offices in 17th-18th century Spain to estimate how the returns from extraction shape the incentives of low-quality or "extractive" types to seek office. Between 1674 and 1751 the Spanish Crown routinely sold provincial offices[2] in the colonial government of Peru and elsewhere[3] as a way to alleviate fiscal crises due to involvement in costly European wars. Since greater fiscal need pressured the Crown to trade-off individuals' qualities in exchange for revenue, colonial officials were appointed based on their willingness and ability to pay and less so on their qualifications or experience.

Using office prices, I first show that low-quality governors actively sought to rule Peruvian provinces with more opportunities for profit (negative selection). Following a difference-in-difference strategy, I compare the increase in prices as the Crown's relaxed its selection criteria — during fiscal crises caused by European wars — in provinces with greater potential for profit vis-a-vis others. In the absence of individuals seeking office for private gain, there should be no substantial

[1] See Engerman and Sokoloff 1997; Acemoglu et. al. 2001, 2002; Banerjee and Iyer 2005; Acemoglu et. al. 2012; Bruhn and Gallego 2012; Dell 2010; among others.

[2] Interchangeably, I will use governor, governorship, and provincial office, as they all refer to the same position ("corregidores").

[3] The Crown sold local government positions across all its territories in the Americas and Asia (the Philippines) as well as other treasury, military, and ecclesiastical posts.

difference in the willingness to pay. Yet, estimates show that an additional year at war leads to a 2% increase in the prices of provinces with greater potential to profit from dealings with the indigenous population (known as *repartimiento*) versus others. This results translates into twice the yearly wage of a military captain in the Spanish army at the time[4]. The finding is not driven by common-year or fixed-provincial traits. There is also no evidence that changes in office prices are reflecting career benefits, altruism, or prestige-seeking, among others.

Rather, these results are consistent with what a number of historians described as the advent of "poor quality" colonial officials due to office-selling (Ramos 1985; Lohman-Villena 1957; Moreno 1977: 71). Indeed, additional findings show that provinces with greater opportunities for *repartimiento* dealings were disproportionately bought by individuals of lower social status, particularly during war times. Considering that in 18th century Spain appointing individuals of high social status was a mechanism to enforce better behavior (Allen 2005), these results are consistent with negative selection.

After using office prices and individual traits to identify negative selection, I then examine how these rulers affect long-run development within Peru. The main empirical challenge is that factors rendering certain offices more valuable to "tyrants or thieves" may also affect long-run economic outcomes in ways that have nothing to do with the quality of government officials. To distinguish between these effects, I focus on the *within-province* difference between office prices at times of low oversight (during wars) relative to prices during periods of high oversight (during peace). By including both measures, I rely only on price deviations driven by hard-to-anticipate factors (such as European wars), which would affect the Crown's selection criteria but are plausibly orthogonal to the economic fundamentals of Peruvian provinces. I also limit the analysis to the set of neighboring districts in different provinces, which had different governors but were otherwise similar.

Results from this approach show that a 30% increase in the average office price sold during wartime leads to a 10% reduction in the average household consumption in the district today. Because I control for the prices paid during peace times (a time of higher oversight), this estimate accounts for time-invariant characteristics and perceived attractiveness of the province. Higher prices also reduce schooling outcomes and public good provision, and are robust to different samples and specifications. Since there are no preexisting differences in demographics,

[4]Approximately 500 pesos.

taxation, and administrative capacity prior to office-selling, these are not likely to be driving the differences. Moreover, the economic gap is already visible by 1827 — immediately after Peru gained independence from Spain – suggesting the importance of colonial rather than post-colonial factors.

Exploitation by low-quality governors often led to rebellions by the indigenous population. These rebellions were usually brutally put down, creating further resentment and a cycle of perpetual conflict. Using detailed data on local rebellions for 18th century Peru, I show that provinces with higher prices experienced a higher likelihood of spontaneous uprisings against colonial governors. This pattern was particularly accentuated during the office-selling period and is still visible in recent times (1980s): districts with higher prices also exhibited greater support for anti-government Maoist guerrillas (Shining Path). Given that frequent political violence discourages investment in physical and human capital, and reduces the effectiveness of government, it is unsurprising that these high-priced provinces exhibit lower levels of consumption, schooling and public good provision today.

Exploitation and political violence also exerted a long-run effect on culture and trust, which a recent literature has argued has important effects on economic development (e.g. Nunn and Wantchekon 2011). The most direct effect of exploitation and violence was thus a reduction in trust of public institutions such as the electoral system and the judiciary, potentially undermining the efficient functioning of government.

More generally, persistent discrimination led the indigenous population to limit their interactions with the outside world by refusing to culturally assimilate and adopt Spanish (Diaz-Cayeros 2011). Yet, such a lack of assimilation may be economically costly in the long-run due to the loss in potential gains from interactions with the majority (and richer) group (Lazear 1999), leading to worse economic conditions. Low cultural assimilation in higher priced provinces was already visible in the 1780s, strengthened in the 19th century, and is still present in household surveys today.

To the best of my knowledge this is the first study to use office prices to examine negative selection into government positions. Existing studies have clearly laid out the problem of political selection (Besley 2005) and the returns to public office (Lenz and Lim 2009; Querubin and Snyder 2013; Bhavnani 2012; Eggers and Hainmueller 2009), but have not examined the long-term consequences of "bad" rulers.[5]

[5]Jones and Olken (2005) focus on short-run growth.

More importantly, this paper shows that the type of local rulers can be a key driver of the persistent effects of colonial institutions. A number of studies document the legacy of certain extractive institutions for long-run development (Engerman and Sokoloff 1997; Acemoglu et.al. 2001, 2002; Banerjee and Iyer 2005; Bruhn and Gallego 2012; Dell 2010). Yet, these studies have not examined how the long-term effect of these extractive institutions can be driven by attracting low-quality types into office in the first place.[6] In other words, low-quality individuals self-selecting into certain positions partially explains why institutions affect current development across sub-national regions in Peru.

Finally, these results establish a direct link between contemporary local conflict, a lack of cultural assimilation and trust, and extractive activities in the 18th century. While previous studies document how armed conflict is able to persist across generations (Fearon and Laitin 2013; Besley and Reynal-Querol 2014), only few have examined why these patterns emerge (or not) in the first place (Jha 2013).

The remainder of the paper is organized as follows: section 1 provides historical background. Section 2 describes the data. Section 3 presents the evidence on negative selection. Section 4 examines its long-run implications. Section 5 explores the potential channels of persistence and Section 6 concludes.

2 Background - Sale of Public Offices

The sale of public offices was a common practice in settings as diverse as France, England, the Ottoman Empire, and China (Swart 1980). In the case of Spain, the sale of colonial governorships started under the reign of Charles II and entailed the appointment to serve[7] in a given office in exchange for money. Although office-selling by the Spanish Crown was used in the early 16th century to allocate minor posts and nobility titles, it only became an important mechanism to select colonial governors in the late 17th century. That is, prior to 1674, appointments to governorships in the colonies were granted only on the basis of scholarly or professional merits, lifetime service to the King, nobility status, or military service.

By 1674, however, the dire financial situation of the Crown led to the sale of local political posts – governorships. The decision was framed as an emergency (and temporary) measure to face "pressing needs" (*urgencias presentes*) caused

[6] Acemoglu et. al. 2001 do imply that officials willing to go to high settler mortality areas would be most likely extractive. This paper complements this view.

[7] In these cases ownership of the office is not transferred to the purchaser in perpetuity.

by wars. Although the Crown was aware of the negative consequences of office-selling, the policy remained in place due to a combination of persistent weakness and direct benefits accruing to members of the court involved in it (Sanz-Tapia 1998). For example, once Philip V took office, he revoked all appointments sold in the last ten years by his predecessor – Charles II – to improve local governance in the colonies. Philip's intentions, however, proved short-lived, since as early as 1705 – during the war of the Spanish succession – Philip himself began to sell governorships citing the prospect of potential military defeat in Europe and subsequent disintegration of the Spanish Empire (Burkholder and Chandler 1977). The practice would only end definitely in 1751, during an Empire-wide effort to modernize government, known as the Bourbon Reforms.

2.1 Provincial Governors (*Corregidores*)

Among all positions sold, that of provincial governor or *corregidor* was quite highly demanded (Sanz Tapia 2009: 89). Despite the fact that provincial governorships were not particularly prestigious, there were no guarantees of further appointments after the five-year term in office, and the task of governing the often unruly indigenous population of Peru could cost officials their lives. Moreover, wages paid to officials were a rather small, stagnant, and insufficient amount for the task at hand and the risks involved (Moreno 1977).[8] It is telling that when the Crown tried to improve the performance of governors by the late 18th century, the main proposals centered around increasing wages and creating a career-based system so that "good" governors could be promoted to future posts (Moreno 1977: 604). These proposals never materialized.

Two main avenues existed when purchasing a position. First, the soon-to-be-vacated position was publicized, both in the Americas and the royal court in Madrid, via public criers and edicts. Interested candidates would send sealed letters with their qualifications and the price offered for the position. The monarch – together with the Royal Chamber – would then assess the bids and merits of candidates and usually choose the highest bidder. The second, and by far the most frequently taken avenue was the purchase of positions years in advance of potentially taking office (*futuras*), in which individuals directly (or via intermediaries) approached the Crown and offered a price for the position. In these cases,

[8] Wages were also subject to a one-time tax of half a salary (*media anata*), and the whole amount was susceptible to be retained by the monarch for "emergency" reasons. See Table A.1 in the Appendix for the list of provincial wages during office-selling.

the Crown decided whether or not to accept the offer, yet it was well known that interested candidates could greatly improve the likelihood of securing the appointment by offering prices well above that of other contenders or above the prices previously paid for the position.

Although the Crown generally tried to select individuals of proven quality — originally from Spain, of high social status, or military background — it was often willing to undermine its own standards in exchange for money. For instance, a minority of individuals living in the colonies were able to buy positions despite strong concerns[9] about their performance (Sanz Tapia 2009: 89). Similarly, although governors were barred from ruling the same province more than once – as a way to prevent corruption – the Crown was ready to make exceptions if rewarded accordingly. Other regulations established that governorships were not to be transferred to a third-party. However, for around one-third of the original price paid, the Crown agreed to such exchanges. As a result, governor titles became little more than a commodity: appointments were purchased a number of years in advance and often changed hands in between; it was then possible for title-bearers to bequest it or use it as a dowry for whomever married their daughters or nieces. Not surprisingly, individuals serving in the colonies would be more likely to be driven by profit (Moreno 1977: 71; Sanz Tapia 2009; Lohman Villena 1957).

2.2 Rent-Extraction

The main attraction for occupying office was that governors served as tax collectors and "justice administrators", thus exerting considerable leverage among the population to engage in extractive activities. The most common of these activities – outlawed at the time of office-selling – was *repartimiento,* or the forced sales of merchandise. This practice entailed the forced distribution of goods and credit to the local population at inflated prices, which kept them in heavy and permanent debt.[10] Under this arrangement, *corregidores* allied with merchants in Lima, or in Seville, to obtain goods and resell them at inflated prices, under coercion, or without the proper consent of locals (Andrien 1984: 13). Debt repayment was ensured by the fact that the *corregidor* was not only the executive, but also the judicial authority in his province (Moreno 1977; Baskes 2000); debtors could be flogged,

[9]It was believed that appointing residents from the *audiencia* they would rule would lead to conflicts of interest.

[10]There is a debate as to whether *repartimiento* was a forced activity or not (Baskes 2000), yet, it still entailed monopolistic practices and steep markups on prices for goods and credit.

jailed, or have their goods confiscated. The rents obtained via *repartimiento* are estimated to be at least twice as large as those obtained from forced labor (*mita*) and head taxes (*tributo*) from the Indian population (Golte 1980).

Rumors about potential profits from *repartimiento* guided bids for a particular province.[11] For example, while the province of Ica was considered "first-class" or "high utility" due to the ease in collecting proceeds from illegal trade activities, the province of Cercado (near the capital) was described as yielding "poor returns" (Cebrian 1977: 78). Not surprisingly, Ica commanded an average price of twice that of Cercado. Hence, factors such as circulating currency, taxable markets, and the ease in collecting payments were important in determining the extractive value of certain offices versus others and the governor's willingness to pay.

Furthermore, governors were known to overtax the population when collecting the yearly head tax (*tributo*) that every indigenous person owed to the Spanish Crown. Historians document how governors often extorted amounts above the permitted legal amount from those exempted (e.g. mestizos), and how they even forced families to pay for those who were absent due to death or migration. Moreover, funds from local taxes that were earmarked for hospitals or other public works were often used for the governors' own purposes (Andrien 1984: 13-14).

Finally, governors also benefited from mobilizing labor in their province for a profit. For instance, they would "lease" workers to neighboring haciendas in exchange for 2/3 of the wage owed to the workers (Andrien 1984). In addition, governors serving in provinces obliged to provide forced labor to the mines (*mita*) would be able to delay implementation of forced labor regulations in exchange for greater gains from *repartimiento* (Mukherjee 2008). They might even allow individuals to forego the *mita* altogether in exchange for a fee. Using these potential sources of profit as a basis, I analyze in Section 4 how they would influence the prospective officials' willingness to pay.

3 Data

The data used in this paper comes from several sources. The time series for the prices of governor positions was coded from primary sources located at the Spanish Colonial Archives (*Archivo General de Indias*). I collected the prices of governorships from 1674 until 1751, when the last sale was made. Governor titles –

[11] Given the size of the investment at the time, it is not surprising that buyers sought information about their purchase.

which contain details on the appointment to office (including price) – also provide information about the personal characteristics of the purchaser, such as its social status. An example of the title of local governor can be found in Figure A.1 and A.2 of the Appendix. All prices account for inflation, using changes in the price of silver (base year = 1674) provided by Arroyo-Abad et. al. (2005).

Figure 1: Current districts and Office Prices

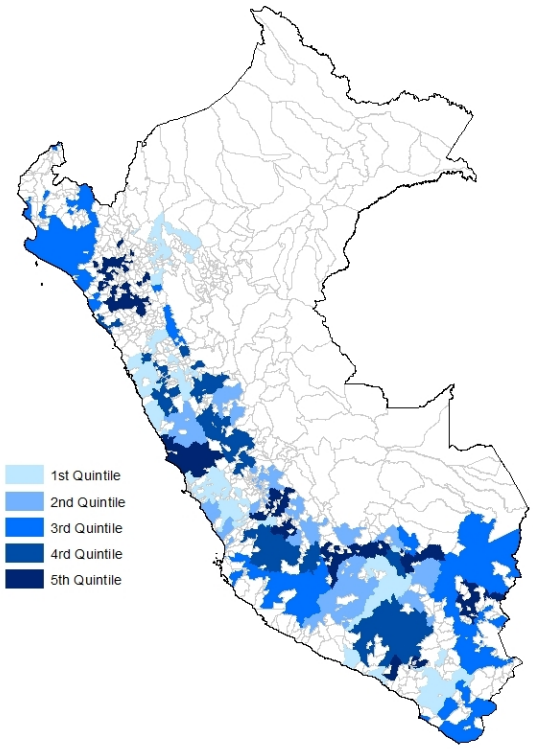

Figure 1 provides a visual representation of the spatial distribution of office prices for contemporary districts in Peru. Note that provinces in the highest price quartiles are not clustered geographically and often border low / middle priced provinces.

To account for the Crown's fiscal needs and reduced oversight in the colonies, I use warfare in Europe involving Spain as a source of exogenous variation in prices. Hence, $WarLength_t$ is coded as the duration of years in a given war, under the assumption that longer wars tend to drain the treasury more. The Nine Years' War, for example, lasted from 1688 until 1697; hence, 1688 is coded as one, 1689 as a two, and so on, and the first year of peace is coded as a 0. Wars in the period under study are described in the Appendix.

Given the lack of time-varying information on rent-availability from taxable markets, I use different types of data. First, I use the 1751 official decree establishing the legal *repartimiento* quotas that colonial governors had to follow when

trading with the local population (Moreno 1977). Because these quotas were based on the estimates volume of consumption of goods commonly traded by the *corregidor* prior to being legalized in 1754 (Lohman Villena 1957: 115), they are likely to capture differences in the possibilities to engage in this activity. In fact, I cross-check whether these quotas actually reflect these activities by examining the letters sent by every priest of the Cusco region between 1689 and 1690 denouncing this practice. Figure A.3 and discussion in the Appendix show these *repartimiento* quotas are a good proxy of these activities.

Second, I use agro-climatic data by the UN's Food and Agricultural Organization (FAO 2012), which provides an index of agricultural suitability based on climate, soil, and slope of the terrain for agricultural activities. This measure was coded for 2002, yet its components are slow-changing and not a likely source of measurement error in my estimates. I also cross-check that the agricultural suitability variable actually predicts taxable markets by constructing an indicator of provinces identified as commercial hubs for agricultural goods such as wine, salt, textiles, coca, or flour production based on O'Phelan (1988: 61).

Another source of potential rents is information about whether or not the province was assigned in 1573 to participate in the mining *mita*, the system of forced labor. Under this system, indigenous communities had to send 1/7 of their adult male population to work in the Potosi and Huancavelica mines (O'Phelan 1988). Forced labor shaped the type of colonial institutions at the time (Arias and Girod 2014; Dell 2010). In addition, given that it is possible that the prices paid for provincial offices reflect their governors' ability to profit not from forced sales, but from their access to natural resources, I coded provinces in which the production of gold or silver was their main economic activity (Golte 1980: Mapa 11).

To match colonial and current districts, I use the geographic accounts by Cosme Bueno (1951 [1783]), who wrote a detailed description of each province and its colonial districts. Out of around 1850 current districts today, I am able to match 842 with their colonial counterpart. I only include districts considered part of the province in the 18th century, to make sure all districts had similar levels of colonial presence at the time. Figure A.4 the Appendix shows roughly how these former colonial provinces match current districts.

Using the 2007 census results provided by INEI, I measure district-level public good provision and household characteristics. In addition, I collect geographic indicators per district of their latitude, longitude, distance to Lima, and elevation (in meters over sea level), all obtained from the Ministry of Education. Finally, to

look at household consumption, individual political attitudes and ethnic composition, I use the 2013 national household census (ENAHO), which includes a sample of around 42,000 individuals clustered in 450 districts with presence in each of all the 47 colonial provinces.

Because differences in development could be reflecting differences prior to the office-selling episode, I also collected taxation, budgetary, and demographic data of districts within colonial provinces (*reducciones*) dating from the first census conducted in Peru ordered by Viceroy Toledo between 1571 and 1573 (Miranda 1873). These measures provide a baseline against which to compare subsequent economic changes in Peru's *corregimientos* (provinces) plausibly driven by colonial governors.

The proposed channel of persistence is the heightened political conflict prevailing in provinces since colonial times. Therefore, I use province-level data based on historical accounts that describe the number and types of rebellions occurring throughout the 18th century (O'Phelan 1980; Golte 1980). These sources distinguish whether the rebellion is against a provincial governor (*corregidor*) or against other actors (e.g. priests). I also use more contemporary data on violence collected by the Peruvian Truth and Reconciliation Commission (CVR), which recorded individual level data on not only the number and type of human rights violations, but also each violation's perpetrator (government forces, guerrilla, or paramilitary groups) during the Peruvian Civil War (1980–2000).

Additional details of other variables used throughout the analysis are in the Appendix. Descriptive statistics are included in Table A.2 of the Appendix.

4 Do Higher Prices for Governorships Imply More Extraction?

Using the prices paid for office, I compare the differential increase in prices paid for positions with greater rents available versus others at times in which "low-quality types" are more likely to successfully bid for a position. In the absence of negative selection there should be no substantial differences in the within-province and year willingness to pay.

To identify the *cross-sectional* distribution of profit opportunities I rely on the *repartimiento* quotas assigned to each province in 1751 as well as the levels of agricultural suitability of each province. In terms of *time-variation* in the likelihood of "low-quality" types to successfully bid for a position, I exploit plausibly exoge-

nous changes in the fiscal needs of the Spanish Crown driven by European wars. European wars represented a huge fiscal burden to the Spanish treasury, making it more likely to trade-off "quality" for revenue. Figure A.5 in the Appendix shows how war times are associated with spikes in the Crown's expenditures. More importantly, the onset and length of wars is largely driven by geopolitical calculations in Europe and can therefore be considered exogenous to Peruvian provincial traits. Finally, focusing on wars also reduces the concern that the Crown may strategically sell offices when demand is higher: sales are driven by emergency needs rather than by other considerations.[12]

The empirical strategy thus follows a *difference-in-differences* approach and interacts the cross-sectional variation in profit potential from office with plausibly exogenous time-variation in the likelihood of "low-quality" types to successfully bid for a position. By comparing *when* and *where* office prices exhibit greater increases relative to their own average level, it accounts for competing explanations that either do not vary greatly within a province over time (e.g. prestige, location) or would not be different for positions with greater access to rents precisely at times of greater scrutiny or not (e.g. altruism, career benefits). I estimate the following:

$$Log(Price_{ijt}) = \alpha_{ij} + \gamma_t + \beta(RentAvailable_{ij} \times War_t) + \mathbf{X}_{ijt} + \mathbf{W}_{jt} + \epsilon_{ijt} \quad (1)$$

Where $Log(Price_{ijt})$ represents the price paid for province i in bishop region j in year t; α_{ij} and γ_t captures provincial and year fixed effects, respectively. $\mathbf{X}_{ijt}$ includes time-varying controls such as rebellions; $\mathbf{W}_{jt}$ captures yearly trends across seven different bishop regions. $RentAvailable_{ij}$, are different measures of access to rents (forced sales or *repartimiento* quotas, mining, presence of taxable income, or forced labor *mita*) and $WarLength_t$ is either the duration of war in a given year, or the simple presence of international conflict War_t.

[12]For instance, the Crown may be deciding to sell offices when prices are higher. By focusing on war events, this type of behavior is less likely.

4.1 Results

Estimates from Table 1 show that greater potential for profit lead to differentially higher prices than for provinces without those traits, particularly when revenue is an important selection criteria. Specifically, the coefficient of 0.019 in column (1) Panel A, suggests that for provinces with a high *repartimiento* quota (above the median) – a proxy for the ability to trade illegally with the local population – an additional year of war leads to an increase in the prices of these provinces of around 2%. This represents approximately 500 pesos or two times the yearly wage of a military captain in the Spanish army, evaluated at the average duration of war and mean office prices. Results comparing periods of war versus peace (Panel B) are similar, yet, slightly less precisely estimated.

Table 1: Office Prices and *Repartimiento*: OLS.

	(1)	(2)	(3)
DV: Log Prices (pesos)			
Panel A - War Length			
$WarLength \times HighRepartimiento$	0.019***	0.019***	0.016**
	(0.0068)	(0.0069)	(0.0067)
Block Bootstrap 90% CI	[0.005, 0.032]	[0.005, 0.033]	[0.001, 0.029]
R-squared	0.835	0.835	0.816
Panel B - War Indicator			
$War \times HighRepartimiento$	0.17**	0.17**	0.15*
	(0.081)	(0.081)	(0.078)
Block Bootstrap 90% CI	[0.006, 0.34]	[0.004,0.34]	[0.011, 0.287]
R-squared	0.834	0.834	0.828
Observations	431	431	479
Number of Provinces	43	43	48
Rebellion indicator	No	Yes	Yes
Provinces Bolivia	No	No	Yes
Province and Year FE	Yes	Yes	Yes

Robust standard errors in parentheses. Block bootstrapped 90% CI from 500 draws in brackets for coefficient on $WarLength \times HighRepartimiento$ and $War \times HighRepartimiento$ as specified. All specifications include time-trends for individual bishop regions. Bolivia provinces are five provinces ruled by the Audiencia of Charcas at the time (not Lima) but currently part of Peru. *** p<0.01, ** p<0.05, * p<0.1

One concern with these estimates is that they do not account for serial correlation in office prices within a province. Because the number of clusters is not large enough relative to the vector of fixed effects, time effects, and differential trends, it is not possible to credibly estimate standard errors clustered at the province level

(which would be preferable). Instead, I follow Bertrand et. al. (2004) and block bootstrap the t-statistic by randomly drawing groups (equivalent to the number of provinces) with replacement. Using the bootstrapped t-statistic I construct a confidence interval for each of the baseline specifications and report it across all tables.

Historical accounts also mention that desirable positions for extraction were those overseeing taxable markets and surplus-producing activities (Golte 1980; Moreno 1977). Therefore, in Table A.3 of the Appendix I include a measure of agricultural suitability, to capture the relative attractiveness of those provinces versus others. Indeed, results from Columns (1) through (3) show that the presence of greater agricultural suitability increased the willingness to pay for office, even after accounting for the role of *repartimiento* quotas, thus in-line with historical accounts showing the importance of taxable markets of agricultural products.

In contrast, when I include measures of forced labor regulations (*mita*) and/or mining, most of the variation in prices is still driven by the *repartimiento* quota and agricultural suitability variables (Tables A.4 and A.5 of the Appendix). This suggests, therefore, that the willingness to pay for office is not driven by the presence of mining or the provision of forced labor (*mita*) per se, but by the ways that these activities might make trading with the local population (*repartimiento*) more profitable. For instance, the presence of mining in the province or nearby provided circulating currency for economic transactions, which in turn increased the attractiveness of *repartimiento*, since payments would be in hard currency rather than in kind. Moreover, *corregidores* were geographically and legally constrained to directly benefit (but not indirectly) from mining.[13]

Robustness Checks. Different robustness checks allow me to assess the sensitivity of these findings. The first concern is that of selective sales or left-censoring; that is, that the Crown is only selling positions that will command a higher price in the market (e.g. high *repartimiento* provinces). Although this type of strategic behavior would only downward bias my estimates, in Table A.6 of the Appendix I show that there are no differences in the likelihood of selling offices in provinces with high *repartimiento* quotas during lengthy wars (Panel A) or war

[13]The main role of governors was that of tax collection from the population with no direct contact with mining proceeds. Other colonial officials, such as assayers, officials overseeing forced labor, and mint traders, among others, had much more direct contact with bullion exports.

periods more generally (Panel B).

A second concern is that the *repartimiento* measure is instead capturing other dimensions of the provinces unrelated to extractive traits, such as population size or its composition (e.g. indigenous presence). Therefore, in Table A.7 of the Online Appendix, I include measures of overall population size (Panel A) or the number which are of indigenous origin (Panel B). Results indicate that coefficients on forced sales and agricultural suitability are similar to the baseline (Table 1), while direct measures of population size or the presence of the indigenous population are not statistically different from zero. This suggests that most of the effect on prices is driven by the returns from this activity due to economic reasons rather than mechanically reflecting indigenous presence.

Third, I examine whether the results are robust to analyzing only those conflicts in which Spain was theoretically less susceptible to manipulate the onset and length of wars. Specifically, "succession" wars, where the timing is driven by the sudden death of a monarch or by military alliances dragging in other countries.[14] Results shown in Table A.8 suggest that the findings are not driven by Spain entering wars financed by office sales in Peru.

Fourth, since *repartimiento* quotas date from 1751, towards the end of the office-selling period, in theory they could be endogenous to the intensity of prior extraction via *repartimiento*. Yet, given that buyers made efforts to know with precision the potential returns to their "investment" (offices), this type of measurement error will only run against finding any relationship between the willingness to pay for office and their established quota.

Finally, given the main effect is an interaction term, it is susceptible to potential outliers. Therefore, I estimate the model from Column (2) in Table (2) – preferred specification – while leaving one province out at the time (out of 48) to assess the sensitivity of the results to changes in the sample. As shown in Figure A.6 of the Appendix, the T-statistic is always well-above conventional levels of statistical significance.

4.2 Mechanism: Negative Selection of Colonial Officials?

If indeed "worse" colonial officials are entering office via sales, there should be noticeable differences among those purchasing positions with greater opportunities

[14]These are the War of the Spanish Succession (1704–1714) and the War of Jenkins' Ear (1738–1740), which overlaps with the War of the Austrian Succession (1740–1748).

for extraction at times of war versus others. Although measuring "quality" in this context is no easy task, I provide evidence based on the social status of purchasers: whether they belong to the nobility (Duke, Marquis, Count, etc.), to a nobility order (Knight of the Santiago or Calatrava order), or have pursued a career in the army (captain, sergeant, among others).

In 18th century Spain, social status was a sign (if sometimes imperfect) of socioeconomic status; social capital and connections to the royal court in Madrid; as well as of lineage and a family reputation.[15] In fact, individuals of high-social status were considered the best suited to serve as *corregidores* in the colonies (Lohman Villena 1957: 100). Such a preference is optimal in a context of poor monitoring since it allows the monarch to "police behavior" by choosing individuals with higher reputation costs in the form of social capital (Allen 2005: 161). Being found in contempt of the King could ostracize members across generations thus reducing social capital and economic opportunities.

In Table 2 I examine whether provinces with high *repartimiento* are significantly less likely to be purchased by those with nobility and military titles during war times compared to peace ones. Specifically, column (1) of Panel B shows that at times of war, high *repartimiento* provinces were 24% less likely to be bought by individuals with any nobility background. Estimates for those with a military career (column 3) or for lengthier wars (Panel A) are in the expected direction, but less precisely estimated.

[15] It was quite common for individuals to be rewarded for services to the Crown or military accomplishments done by relatives (e.g. parents, uncles, grandparents, etc.)

Table 2: Office-Selling and Social Status of *Corregidores*: OLS

	(1)	(2)	(3)
DV:	**Any Title?**	**Nobility?**	**Military?**
Panel A: War Length			
$WarLength \times HighRepartimiento$	-0.018*	-0.016**	-0.0056
	(0.0095)	(0.0074)	(0.0081)
Block Bootstrap 90% CI	[-0.03, 0.001]	[-0.03,0.00]	[-0.02, 0.00]
R-squared	0.231	0.233	0.226
Panel B: War Onset			
$WarOnset \times HighRepartimiento$	-0.24**	-0.21**	-0.10
	(0.11)	(0.086)	(0.095)
Block Bootstrap 90% CI	[-0.44,-0.03]	[-0.38,-0.04]	[-0.28, 0.08]
R-squared	0.234	0.236	0.227
Observations	433	433	433
Number of provinces	43	43	43
Rebellion indicator	No	Yes	Yes
Provinces Bolivia	No	No	Yes

Robust standard errors in parentheses. Block bootstrapped 90% CI from 500 draws in brackets for the coefficients on $WarLength \times HighRepartimiento$ or $WarOnset \times HighRepartimiento$, as specified. All specifications include time-trends for individual bishop regions. Bolivia provinces are five provinces ruled by the Audiencia of Charcas at the time (not Lima) but currently part of Peru. *** p<0.01, ** p<0.05, * p<0.1

These results are in-line with historical accounts describing how office-selling allowed the appointment of individuals "without preparation or vocation" to serve in the colonies (Sanz Tapia 2009:43). Similarly, Lohman Villena (1957: 130) argues that individuals with "merit," but who were economically constrained, were being displaced by those who were able to pay but did not always fulfill the desired traits of "competence and probity".[16] Even at the time, known Spanish jurists thought that office-selling brought about "tyrants or thieves" to the colonies (Solorzano y Pereira cited by Moreno 1977: 71).[17]

Furthermore, the presence of low "quality" *corregidores* coincides with the exponential increase of *repartimiento* activities in Peru. According to Andean historians, *repartimiento* – already known in the 16th century – only became widespread by the end of the 17th century, precisely when office-selling started (O'Phelan 1988: 90; Lohman Villena 1957: 130). In fact, a secret report to

[16] Hollyer (2009) formally shows that when offices are sold, credit-constrained but high quality officials may be left out of office due to their inability to pay.

[17] One possibility is that at times of war the pool of "quality" individuals shrinks. Yet, this possibility would be entirely consistent with the results presented here.

King Ferdinand VI ("Secret News of the Americas") unequivocally links office-selling with the pernicious behavior of *corregidores* regarding forced sales (Ramos 1985: 174-175). This is not surprising considering that selling offices also relaxed potential punishment from wrongdoing by purchasers. Since offices were sold in Madrid, the post-tenure performance assessment (*juicio de residencia*) had to also be conducted there (Lohman Villena 1957: 468). Because this was the only avenue to prosecute *corregidores* for wrongdoing, it became significantly harder for those affected (the local indigenous population in Peru) to ever be able to voice their concerns.

4.2.1 Alternative Explanations

In this section, I offer evidence against four alternative explanations based on (1) career concerns, (2) altruism, (3) tax theft as opposed to tax extraction, and (4) local economic booms.

First, it is possible that governing certain provinces might bring about *career benefits* within the royal bureaucracy that would justify paying disproportionate prices for becoming an official. Yet this is unlikely for three reasons: first, as shown in Table A.9 of the Appendix, the most prestigious positions (bishop seats) were not particularly valued during war times. Instead, buyers paid hefty sums to govern backwater provinces – provided they have high *repartimiento* – despite wages being small and risks high. Second, since office-selling was an emergency and temporary measure, purchasers were not entitled to any future appointment after the five-year term, and were not considered members of the royal bureaucracy. Once office-selling stopped altogether, the Crown quickly filled vacant positions with individuals whom the Crown considered "trustworthy" (Moreno 1977). Finally, analyzing the career paths of all those serving throughout the 18th century in the level of government above that of *corregidor* (Lohman Villena 1974) – which is being minister of the *Audiencia* in Lima – shows that only 2 out of 147 ministers had ever served as a *corregidor* of a Peruvian province.

Second, an alternative possibility is that these individuals are altruistic and authentically want to help the Crown during difficult times (e.g. fiscal crises). Yet, altruist types would not be paying disproportionately more for positions in provinces with greater profit potential.

A third possibility is that officials may seek these positions not to extract from the population, but to cheat the Crown from owed taxes. This is unlikely, given that the Crown mandated all governors to a pay a bond or name a creditor

who would cover the head taxes that the Crown expected (*fianza*). Taxes were specified in advance according to the previous census, set in the contract, and settled even before entering office. Moreover, since the time to assume office was often uncertain, revenue growth at the time of purchasing office may be an unreliable reference point. Nonetheless, in Table A.10 of the Appendix, I show that revenue collected throughout the period was *not* differentially higher in high *repartimiento* provinces at times of war which could justify paying higher prices.

A final concern is that individuals may be timing their purchases to coincide with economic booms in the region. Yet, because the specific moment *corregidores* entered office is uncertain, this is unlikely.[18] Rather, bids were driven by known provincial characteristics and expected profitability such that price increases during war captures changes in the selection process and not fixed-traits of the province or hard to anticipate events. In addition, an analysis of different indicators of the Peruvian economy at the time (inflation indexes, prices of key staples, silver production volume, and silver sent to Spain) show little relationship to the onset or length of European wars in Spain.[19]

5 The Long-Run Impact of Negative Selection

If "low-quality" officials self-select into positions offering higher returns to extractive activities, how does their rule impact the long-term development outcomes of these provinces? A priori, the answer is far from obvious. On the one hand, the experience of exploitation by local governors during office-selling may have had no impact on economic fundamentals or in the processes of agglomeration and increasing returns to colonial settlements (Maloney and Valencia 2015). On the other, rent-extraction by colonial officials may have led to profound and persistent institutional and governance disparities among provinces in Peru, leading to markedly different development paths. In this section, I explore how differences in the prices paid for office — as a proxy for these disparities — relate to sub-national economic patterns across Peru.

The main empirical challenge to identify the effect of "bad" colonial officials is that traits that render certain offices more attractive to rule for certain "types" may also affect long-run development outcomes in ways that have nothing to do

[18] For instance, the title specified that an individual would access office after individual X, who himself would go after another person, thus making the time of access to office rather uncertain.

[19] Results available upon request.

with the performance of government officials. For instance, provinces with greater agricultural suitability may be poorer today, not because they were governed by "worse" colonial officials, but because of changes in the returns to agricultural activities in the last century. Therefore, it is crucial to separate the effect of these traits on long-run development from their influence in the process of political selection and subsequent governance.

To do so, I focus on the variation in provincial office prices stemming from deviations in their own provincial average at times of high versus low screening by the Crown. That is, I examine how office prices paid during war affect long-term development outcomes today, while controlling for the price at which the province was sold during peace times. By relying only on the *within-province* variation in prices between war and peace years, I can better account for slow-changing unobservable provincial traits, which may impact development today, but would not be different during wars versus peace times. As shown in the previous section, factors largely orthogonal to economic fundamentals in Peru (e.g. European wars) increased the Crown's need for revenue, making it more likely for "low-quality types" to successfully purchase offices in the Americas during wars.

The baseline equation for the regression specifications is given by:

$$y_{ip} = \alpha + \beta_1 \cdot \log(PriceWar_p) + \beta_2 \cdot \log(PricePeace_p) + \mathbf{z}_{ip} + e_{ip}$$

Where y_{ip} represents contemporary development outcomes in district i and province p. The sample comprises 842 current districts belonging to 48 colonial provinces. $Log(PriceWar_p)$ is either the average, maximum, or last price paid for province p during war periods between 1674 and 1751. The coefficient of interest is β_1, which if positive, shows that higher average prices during 18th century European wars leads to greater disparities in development outcomes while controlling for either the average, minimum, or first price it drew during peace – $\log(PricePeace_p)$. $\mathbf{Z}_{ip}$ accounts for district level pre-treatment controls (elevation, agricultural suitability index - slope, soil, and climate - latitude, longitude, and distance to the capital). All prices are normalized by total population size in 1754 to account for scale effects. Finally, I cluster the standard errors at the level of the province to account for arbitrary correlation across districts that belong to the same province.

Table 3 – Baseline: Office Prices and Contemporary Development Outcomes

	(1)	(2)	(3)	(4)
Panel A: Controlling for *Minimum* Price Peace				
	Household-Level		*District-Level*	
DV	Log (Household Consumption)	Years of Education	Indoor Toilet?	Dirt Floor?
$AvgPrice_{war}$	-0.245***	-0.758***	-5.921***	14.469***
	(0.065)	(0.254)	(2.123)	(3.878)
Observations	50,407	47,191	842	842
Clusters	47	47	46	46
	Panel B: Controlling for *First* Price Peace			
$AvgPrice_{war}$	-0.218***	-0.624**	-5.415**	13.037***
	(0.061)	(0.241)	(2.171)	(3.753)
Observations	50,407	47,191	842	842
Clusters	47	47	46	46

Robust standard errors clustered at the province level in parentheses. All specifications include: elevation, distance to Lima, latitude, longitude and agricultural constraints (soil, climate, slope). Household specifications include: number of adults, infants, and kids in the household, age and gender. Prices are normalized by population size in 1754. *** p<0.01, ** p<0.05, * p<0.1

In Table 3, I report the estimates from Equation 2. Across a number of development measures, it is clear that higher prices paid during wars are associated with worse household or district outcomes while controlling for the minimum (Panel A) or first price (Panel B) it drew during peace. In particular, estimates from column (1) suggest that a 10% increase in the average price paid during war leads to a 2.3% reduction in household consumption. The same is true for years of education (column 2), and district-level share of households with indoor toilets (column 3) or dirt floors (column 4). Additional results show that the higher prices paid during war also lead to a reduction in electricity, water provision, and other educational indicators. The latter is not surprising given the strong correlation across development outcomes.[20]

Robustness. Table A.11 of the Online Appendix shows that these results are not driven by the measure of office prices used. Panel A documents a negative

[20]Results available upon request.

and statistically significant effect of the maximum prices paid during wars on contemporary measures of household consumption and education while controlling for the minimum price paid during peace. Since purchasers had an incentive to pay the least amount possible, the maximum price paid may be closer to the true valuation of offices than the average, although potentially more susceptible to outliers. Results do not change if one controls for either the first price paid during peace (Panel B) or if one compares the last price paid at war times with the first price paid during peace (Panel C).

Furthermore, Table A.12 of the Appendix presents the estimates after removing all variation in prices driven by fixed provincial traits and common yearly events.[21] Because such residuals have mean zero by construction, I focus on the largest (maximum) deviation in office prices during sales. Results invariably show that the highest proportional deviations from the within province average, the worse household consumption and schooling is today, even after controlling for different prices during peace. Moreover, results after bootstrapping the standard errors suggest that these estimates are not artificially small.

Additional results in Table A.23 examine the robustness of these findings to including other provincial traits, such as measures of forced labor (*mita*), mining, presence of bishop seats, provincial wages, and the size of the indigenous population in 1754. If anything, the inclusion of these controls leads to larger coefficient estimates than the baseline. For instance, Panel A shows that the effect after accounting for measures of forced labor (*mita*) remains very similar to baseline estimates. The reason may lie in the fact that forced labor regulations were ubiquitous across Peru leaving ample room for sub-national differences in colonial governance. All the results remain the same if instead I control for the first price paid during peace times (Table A.14).

Timing of Gap. While the results above quantify the contemporary gap in development outcomes, they do not show when the divergence started. Evidence from the first economic census collected after independence (1827) exhibits a sharp difference in the contribution to national income, total income, and population (rough proxies for GDP) for provinces with higher prices paid in the 18th century. Although this data is only available at the province-level and is calculated using the estimated income per capita per ethnicity at the time (indigenous, Spanish, or mestizo), it still provides some indication of the development paths of these

[21] Specifically, I collect the residuals from a regression: $Log(Price_{it}) = \alpha_i + \gamma_t + e_{it}$, where α_i and γ_t represent provincial and year effects, respectively.

provinces at the start of independent life. As shown in Column (3) Panel A of Table A.15 of the Appendix, an increase of 10% in the average price paid for the province during war times in the 18th century is associated with a decrease of 3.6% in the gross provincial income by 1827.

5.1 Neighboring Districts

Despite these robustness checks and controls, it is possible that the results may reflect omitted variable bias from factors determining both prices in the 18th century and development outcomes today. To alleviate this concern, I exploit the fact that the colonial governor's "type" changed discretely with the provincial border: on one side, all districts of a province were subject to a particular governor, while on the other, all districts were ruled by a different one. Yet, border districts share a number of observable and unobservable characteristics (e.g. geographic features, disease environment, markets).

Table 4 below estimates the baseline regression on the sample of bordering districts. Results show that higher paid governorships during 18th century wars negatively impact different measures of household consumption, education, and household characteristics. Specifically, estimates from Column 1 in Panel A suggest that an increase of 10% in the prices paid during war times leads, on average, to a 3% reduction in household consumption. This result is encouraging, given that Peruvian districts are relatively small. Moreover, the estimated coefficients for household consumption and education are slightly larger from the ones obtained at the province level, suggesting that omitted variable bias might be a small concern when using household level data. In contrast, variables using district-level aggregates are slightly smaller than the baseline estimates.

Table 4 – Office Prices and Development Outcomes Across Neighboring Districts

	(1)	(2)	(3)	(4)
Panel A: Controlling for *Minimum* Price Peace				
	Household-Level		*District-Level*	
DV	Log(Household Consumption)	Years of Education	Indoor Toilet?	Dirt Floors?
$AvgPrice_{war}$	-0.317***	-1.005***	-3.903**	11.933***
	(0.075)	(0.301)	(1.934)	(3.648)
	Panel B: Controlling for *First* Price Peace			
$AvgPrice_{war}$	-0.298***	-0.948***	-3.711*	10.056***
	(0.072)	(0.289)	(1.940)	(3.498)
Observations	20,207	18,958	417	417
Clusters	46	46	46	46

Robust standard errors clustered at the province level in parentheses. All specifications include: elevation, distance to Lima, latitude, longitude and agricultural constraints (soil, climate, slope). Household specifications include: number of adults, infants, and kids in the household, age and gender. Prices are normalized by population size in 1754. *** p<0.01, ** p<0.05, * p<0.1

Spill-overs, Sorting, and Pre-existing Differences. A crucial assumption behind this strategy is that there are no spill-overs from the rent-extracting activities of one governor affecting the districts of the neighboring province. While unlikely – there are very few records of inter-governor conflicts – this would only bias against finding any difference among districts.

A second concern is that of individual migration, or the idea that individuals might flee highly extractive areas and resettle in less extractive ones. During colonial times, the ability of individuals to relocate was limited. The market of land for the indigenous population is practically nonexistent, and relocating to a different *ayllu* or community imposed a cost on that community to share an already small plot of land. Moreover, as shown in Table A.16 of the Online Appendix, the available provincial level measures of internal migration from 1754 show no disproportionate number of migrants or *forasteros* outside high-price provinces.

Finally, a crucial assumption for this strategy is that border districts are similar in important characteristics such as geography and not driving the differences in prices observed. Therefore, Table A.17 to A.20 of the Appendix shows a series of regressions of geographic characteristics and pre-treatment outcomes on office

prices. Results show that, among neighboring districts, prices paid for offices during wars in the 18th century are not related to (1) geographic traits such as elevation, agricultural suitability, distance to Lima, latitude, and longitude (Table A.17), although there is a borderline significant coefficient on elevation in one specification. Similarly, prices paid during wars are unrelated to (2) measures of district population size, taxation rates, or size of the taxable population in 1572 (Table A.18); (3) they are also not related to the share of the budget allocated to governors (*corregidores*), indigenous tax-collectors, or *encomenderos*[22] in 1572 (Table A.19); finally, the composition of the population appears similar across border districts, with just a slightly higher share of young boys (who do not pay taxes) in districts belonging to provinces with higher prices paid during wars (Table A.20 of the Appendix). Overall, these findings support the idea that border districts are indeed similar on average in the earliest available data.

Results from the 1572 census also alleviate concerns that provincial borders may be drawn endogenously to economic characteristics of the districts. The census reflects the first measure of taxation and administrative budgets after the Spanish relocated most of the indigenous population into *reducciones*, the basis for colonial provinces. The purpose of the relocation was to concentrate the population and facilitate taxation by Spanish authorities. Yet the Crown, worried about the demographic collapse of the population, repeatedly ordered that the relocation should be to areas with access to water and lands suitable for cultivation and pasture (Malaga 1975: 11). Because these initial *reducciones* do not exhibit pre-existing geographic, taxation, or budgeting differences, it is unlikely that their subsequent assignment to certain provincial jurisdictions versus others is driven by the presence of (dis)advantageous pre-conditions.

One noteworthy aspect of Tables A.17 to A.20 is that the number of observations dramatically drops to a fifth of the original sample when examining taxation and budgeting as captured in the 1572 census. Therefore, an important robustness check is to examine whether baseline results remain the same even if the sample is restricted to only those districts already in place by 1572. As shown in Table A.21 of the Online Appendix, baseline estimates are similar, if not larger, when the sample is restricted to districts by 1572.

Put together, the statistical results among neighboring districts are consistent with the idea that provinces more likely to suffer from "bad" rule have relatively

[22]Encomiendas were rights to the use of certain land, as well as the indigenous population associated with it, that were granted typically to the first *conquistadores* in Peru and their descendants.

worse development outcomes today. In the next section, I explore potential channels of persistence for why this is the case.

5.2 Channels of Persistence: Political Conflict and Cultural Assimilation

For "low-quality" individuals between 1687 and 1754 to have a long-lasting impact on development outcomes, they must have led to persistent changes in slow-moving factors, which hindered the economic prospects of these regions relative to others. Since a temporal change in extraction is unlikely to have altered fixed geographic characteristics, I focus on how the activities of colonial governors could have fostered two different forms of "resistance" to outside rule with long-term development consequences. First, violent resistance in the form of spontaneous uprisings; and second, a lack of cultural assimilation as a response to explicit discrimination policies and low perceived returns to adopt the "Spanish-mestizo" culture.

5.2.1 Political Conflict

To determine the impact of "low-quality" governors on violent forms of resistance, I use detailed data on local rebellions for 18th century Peru. Uprisings were common during colonial rule, caused by what was perceived as "excesses" and lack of proper justice administration from the governor. For instance, governors were known to rely on violent mechanisms (or the threat of these mechanisms) to arbitrarily impose excessive taxation, coerce labor, and collect undue debts (Andrien 1984; Moreno Cebrian 1977; Golte 1980; O'Phelan 1988). O'Phelan (1988) provides a list and brief description of all known rebellions between 1708 and 1780 in Peru. She describes how in 1726, in Ayabata (Carabaya) the indigenous population stoned and killed the governor Don Luis Cerro. In Cajamarca, both the indigenous and mestizo population stoned the local governor to rescue a prisoner. In 1731, a revolt occurred against the governor Don Juan Bautista Fandino because he had unilaterally increased forced labor requirements (*mita*). That same year, in Cajamarca, there was an uprising against the governor because he had included mestizos (normally exempt) in his forced sales or *repartimientos*. And so on. Yet, rebellions were not only against governors: in Tarma, a two-year uprising (1721–1723) began against the owner of the local textile factory (*obraje*) Don Joseph de Sale for the factory's arguably abusive treatment.

Table 5 shows the estimates of the effect of higher prices on the number of anti-governor rebellions in the 18th century as collected by O'Phelan (1988) and Golte

(1980). Estimates show that provinces with differentially higher prices were more likely to experience spontaneous rebellions against governors during the office-selling period (column 2) and afterwards (column 3). However, the effect of prices on other rebellions is positive, yet less precisely estimated (column 1 of Panel B). Specifically, column (3) suggests that a 10% increase in the average price per capita paid during wartimes is associated with a 0.5% increase in the number of anti-governor rebellions per capita. Effects are stronger if conditioning only on those provinces that ever experienced rebellion, suggesting that higher office prices can better explain the intensity of rebellion rather than its onset (Table A.22 of the Appendix).

Table 5: Office Prices and Number of Uprisings

	(1)	(2)	(3)
Target and Period	Any 1708-1754	Anti-Governor 1708-1754	Anti-Governor 1708-1780
	Panel A: Controlling for *Minimum* Price		
$AvgPrice_{war}$	0.030**	0.035**	0.053***
	(0.013)	(0.013)	(0.016)
R2	0.157	0.214	0.272
	Panel B: Controlling for *First* Price		
$AvgPrice_{war}$	0.017	0.025**	0.036**
	(0.012)	(0.013)	(0.016)
R2	0.126	0.189	0.220
Observations	842	842	842
Clusters	46	46	46

Robust standard errors clustered at the province level in parentheses. All specifications include: elevation, distance to Lima, latitude, longitude and agricultural constraints (soil, climate, slope). Prices are normalized by population size in 1754. Dependent variables normalized and weighted by population size in 1754. *** p<0.01, ** p<0.05, * p<0.1

One concern with these estimates is whether prices are correlated with the presence of anti-governor rebellions. Certainly, it is quite likely that buyers would discount the probability of facing an uprising when bidding for office, thus leading to lower prices paid for these provinces. Yet, this would only downward bias my estimates, reducing the potential association between higher prices paid for office and spontaneous uprisings.

Although these rebellions were relatively small and highly localized, their presence signaled a general discontent with certain colonial officials. Furthermore, recurrent rebellions would also lower the returns from office for prospective officials[23]: ruling these districts becomes relatively "risky" such that only those with "extractive abilities" would be willing to govern these provinces, furthering the cycle of poor governance and revolt.

Shining Path. Against this background, the rise of an extremist Maoist organization called *Sendero Luminoso,* or Shining Path, in these districts two hundred years later appears less surprising. Based on the charisma of its leader (Abimael Guzman), the long-term work of its members in the Peruvian countryside, and the longstanding support to Marxist ideologies (McClintock 1984), Shining Path cells managed to infiltrate numerous Indigenous communities that provided shelter, sustenance, and recruits. While "fiercely ideological," the movement capitalized on economic discontent, poor service delivery, and lack of government relief to these impoverished areas since immemorial times. As described by Weinstein (2007: 84) "*Sendero luminoso* generated interest and support by pointing to the government's economic and political failures." Thus making it likely, then, that those places with relative worse local governance from colonial times, as measured by office prices, could be more attracted to Shining Path's core message.

In Table 6 below, I examine the effect of office prices in the 18th century on support for Shining Path among the sample of neighboring districts. To measure support, I use both the violent incidents committed by Shining Path members, but I also take advantage of the fact that villages which *Sendero* had infiltrated routinely conducted "popular trials" in which hated individuals among the population (thieves, adulterers) and government representatives denounced by locals would be put publicly to death. By examining the identity of the victims, it is possible to better capture support for the insurgency and not opposition to it. In addition, I focus exclusively on violent episodes in the early stages (1980–1983) of the conflict, just before Shining Path launched its "Great Leap" to expand operations beyond its strongholds in 1984 (Weinstein 2007: 86; CVR 2004). It is these areas that constituted the initial "base" of Shining Path's territory and were plausible more supportive to its ideas.

Estimates from Table 6 show that districts belonging to provinces for which governors paid higher office prices during the 18th century exhibit higher levels of

[23]This logic is similar to the one presented by Caselli and Morelli (2004) in a democratic context.

insurgent violence during initial phases of the conflict and had a disproportionate number of victims belonging to local government officials. Because only a handful of districts ever experienced violence, I (1) use count models to account for the distribution of the number of attacks, and (2) limit the sample to only those districts that ever experienced any attack and weight by population size in 1990[24] (Table A.23 of the Appendix). In either case, I find that higher prices increase the intensity of violence, particularly against local authorities. Specifically, an increase of 1 log points (approximately one standard deviation) in the average price paid in the 18th century is associated with an increase in the rate of conflict events of 3.9, 4.3, and 4.6 for overall attacks, attacks by Shining Path, and attacks against local authorities, respectively. Effects are stronger when conditioning only on those provinces that ever experienced violence, again suggesting that high office prices in the 18th century can better explain the intensity of conflict rather than its onset.

Table 6: Office Prices and Shining Path Attacks: Border Districts

	(1)	(2)	(3)
	Negative Binomial Estimates		
	Panel A: Controlling for *Minimum* Price		
	All Attacks	Guerrilla Attacks	Authority Victim
$AvgPrice_{war}$	1.368**	1.468**	1.536*
	(0.634)	(0.605)	(0.788)
	Panel B: Controlling for *First* Price		
$AvgPrice_{war}$	1.378***	1.410***	1.661**
	(0.528)	(0.514)	(0.760)
Observations	417	417	417
Clusters	46	46	46

Robust standard errors clustered at the province level in parentheses. All specifications include: elevation, distance to Lima, latitude, longitude and agricultural constraints (soil, climate, slope). Prices are normalized by population size in 1754. *** p<0.01, ** p<0.05, * p<0.1

Violence during the Peruvian Civil War shares with anti-colonial rebellions the fact that it was particularly targeted against local political figures: estimates from column (3) support the idea that political authorities (local party leader, mayor,

[24]There is no reliable measure for district-level population in 1980.

council member) were in greater danger of being attacked in districts with higher prices paid for office in the 18th century.

Political Attitudes. If indeed the presence of anti-colonial rebellions and the appeal of the Shining Path movement are the product of recurrent poor governance due to negative selection, we should observe less favorable attitudes towards the government among citizens from high priced provinces compared to low priced ones. Unfortunately, the most comprehensive household survey (ENAHO) only asks questions on political attitudes for a sub-sample of respondents, thus greatly limiting the number of observations available when looking at border districts. Therefore, I focus on province-level results and interpret these estimates with caution. Table A.24 shows how, across different measures of trust in general (column 1) and of particular aspects of politics such as local governments (column 2), electoral institutions (column 3), judicial and law enforcement bodies (column 4), and even the media (column 5), there is a significantly lower degree of trust among citizens belonging to provinces with higher prices paid for office in the 18th century. This provides some evidence that places plausibly ruled by "bad" colonial rulers have more conflictive politics today and lower trust in existing institutions.

5.2.2 Cultural Integration

A second form of resistance to "bad" rulers was to limit interactions by refusing to culturally assimilate or adopt the dominant language (see Diaz-Cayeros 2011 for the case of Mexico). Yet, failure to culturally assimilate may be economically costly to minorities, since this failure deprives these groups from the gains from trade with the majority or mainstream group (Lazear 1999). This backlash against the dominant culture and loss of economic opportunities is quite plausible in the case of Peru: the presence of colonial exploitation targeted against the indigenous group, combined with strong geographical barriers (e.g. Andes), and the preservation (until recently) of traditional patterns of land-holding, may all have reduced the incentives to culturally assimilate into the "Spanish" and "Mestizo" culture, leading to relatively worse economic outcomes over time.

To examine this possibility, Table 7 traces the composition of the population until today in the sample of neighboring districts. Specifically, I examine the share of Indigenous, Mestizo, and Spanish or "white" in the population of these districts from 1780 until 2013. During colonial times, the population was greatly limited in their settlement choices (even Spanish-born individuals were confined to live

in *corregimientos de espanoles*), but after independence (1824), individuals were relatively free to move and arbitrage any differences.

Indeed, Table 7 shows a positive (but not significant) difference in 1780 in the share of Indigenous population among neighboring districts. Yet by 1876, there is a differentially larger share of Indigenous population in districts from provinces with higher office prices in the 18th century. Such a gap is even stronger today (2013) when using measures of self-reported native language and identification. Conversely, these districts also exhibit a systematically lower share of Spanish settlers in border districts from high price provinces compared to lower priced ones (Table A.25).

Table 7: Office Prices and Ethnic Composition: Border Districts

	(1)	(2)	(3)	(4)
Year	1780	1876	2013	2013
	Panel A: Controlling for *Minimum* Price			
DV:	% Indig 1780	% Indig 1876	% Quechua 2013	ID Indig peasant
$AvgPrice_{war}$	0.074	0.079*	0.146**	0.105***
	(0.050)	(0.043)	(0.067)	(0.032)
Observations	184	246	18,958	4,912
Clusters	42	46	46	46
	Panel B: Controlling for *First* Price			
DV:	% Indig 1780	% Indig 1876	% Quechua 2013	ID Indig peasant
$AvgPrice_{war}$	0.068	0.084**	0.114	0.088**
	(0.045)	(0.039)	(0.072)	(0.035)
Observations	184	246	18,958	4,912
Clusters	42	46	46	46

Robust standard errors clustered at the province level in parentheses. All specifications include: elevation, distance to Lima, latitude, longitude and agricultural constraints (soil, climate, slope). Household specifications include: number of adults, infants, and kids in the household, age and gender. Prices are normalized by population size in 1754. *** p<0.01, ** p<0.05, * p<0.1

These results suggest that in post-independence Peru – with relatively more freedom of movement – initial differences in the quality of governance and in the economic returns to cultural assimilation could have been exacerbated by the migration and settlement choices of individuals in the 19th century. Individuals chose to settle in places with better institutions, where they were better able to interact socially and economically with the local population. Indeed, additional

results[25] show a lower share of current immigrants in places with higher office prices in the 18th century.

6 Conclusion

This paper uses the prices paid for colonial offices between 1674 and 1751 to study how selling offices plausibly led to a more extractive ruling class, which negatively impacted long-term development within Peru.

I first show that officials were more likely to pay more and be of "lower quality" when purchasing positions with greater opportunities to profit, precisely at times in which "worse" types are more likely to succeed in their bids. This finding is incompatible with officials driven by altruism, prestige, or career-benefits, among others. Rather, these results are consistent with more extractive individuals willing to pay higher prices to secure access to office for private gain – or negative selection. In fact, the long-term impact of their rule was overall detrimental: positions coveted by "bad" types during colonial times are systematically worse off in terms of household consumption and public goods provision today.

One of the reasons the effect of worse colonial governors persisted over time is that their rule warped the relationship between citizens and authorities. The presence of exploitative practices led to contentious politics characterized by political violence, first, against the colonizing power (anti-colonial rebellions) and later, against democratically elected governments. Even today, individuals living in provinces with highly valued offices are more likely to mistrust state institutions. Such perceived antagonism to political office could have lowered the payoffs for "good" types to seek office, furthering the cycle of negative selection and poor governance. Similarly, the presence of economic extraction may have entrenched ethnic identities as a way to limit interactions with the outside world, making it difficult to culturally assimilate or adopt the common language. Such a lack of assimilation may have deprived minority groups from the gains from trade with the majority or mainstream group.

[25] Results available upon request.

Bibliography

Acemoglu, D., S. Johnson, and J. A. Robinson. 2001. "The Colonial Origins of Comparative Development: An Empirical Investigation" American Economic Review, 91: 13691401.

_____. 2002. "Reversal of Fortune: Geography and Institutions in the Making of the Modern World Income Distribution" Quarterly Journal of Economics, 117: 1231-1294.

Arias, Luz and Desha M. Girod. 2014. "Indigenous Origins of Colonial Institutions" Quarterly Journal of Political Science 9(3):371-406.

Allen, Douglas. W. 2005. Purchase, patronage, and professions: incentives and the evolution of public office in pre-modern Britain. Journal of Institutional and Theoretical Economics JITE, 161(1), 57-79.

Andrien, Kenneth J.1984. "Corruption, inefficiency, and imperial decline in the seventeenth-century viceroyalty of Peru" The Americas 41(1): 1-20.

Arroyo Abad, L. et.al. 2012." Between conquest and independence: Real wages and demographic change in Spanish America, 15301820" Explorations in Economic History 49(2): 149-166.

Banerjee, Abhijit and Lakshmi, Iyer 2005. History, Institutions and Economic Performance: The Legacy of Colonial Land Tenure Systems in India. American Economic Review 95(4): 1190-1213.

Baskes, Jeremy. 2000. "Indians, merchants, and markets: a reinterpretation of the 'Repartimiento' and Spanish-Indian economic relations in colonial Oaxaca, 1750-1821" Palo Alto: Stanford University Press.

Bertrand, M. et. al. 2004. "How Much Should We Trust Differences-In-Differences Estimates?" The Quarterly Journal of Economics 119 (1): 249-275.

Besley, T. 2005. "Political selection" The Journal of Economic Perspectives, 19(3): 43-60.

Besley, T., & Reynal-Querol, M. 2014. "The legacy of historical conflict: Evidence from Africa" American Political Science Review 108(02): 319-336.

Bhavnani, Rikhil R. 2012, Using Asset Disclosures to Study Politicians Rents: An Application to India, Working Paper.

Bruhn, M., & Gallego, F. A. 2012. "Good, bad, and ugly colonial activities: do they matter for economic development?" Review of Economics and Statistics 94(2):433-461.

Bueno, Cosme. 1951. "Geografia del Peru Virreinal del Siglo XVIII" Daniel Valcarcel (ed.) University of Texas.

Burkholder, M. A. and D.S. Chandler. 1977. "From Impotence to Authority: the Spanish Crown and the American Audiencias, 1687 - 1808" Columbia: University of Missouri Press.

Caselli, F., & Morelli, M. 2004. "Bad politicians" Journal of Public Economics, 88(3):759-782.

Comision de la Verdad y Reconciliacion (CVR). 2004. "Informe Final" *http* : *//www.cverdad.org.pe/ifinal/index.php* (March 3, 2015)

Dell, Melissa. 2010 "The Persistent Effects of Perus Mining Mita" Econometrica 78(6): 1863-1903.

Diaz-Cayeros, A.. 2011. "Indian Identity, Poverty and Colonial Development in Mexico" Unpublished manuscript, Stanford University.

Eggers, Andrew and Jens Hainmueller. 2009, MPs for Sale? Estimating Returns to Office in Post-War British Politics, American Political Science Review, 103(4): 513-533.

Engerman, Stanley L., and Kenneth L. Sokoloff. 1997. "Factor Endowments, Institutions, and Differential Paths of Growth among New World Economies: A View from Economic Historians of the United States." In Stephen Haber, ed., How Latin America Fell Behind: Essays on the Economic Histories of Brazil and Mexico, 18001914. Palo Alto: Stanford University Press.

FAO. 2012. FAOSTAT Online Database. Available at http://faostat.fao.org/

Fearon, J. D., and Laitin, D. D. 2013. "How persistent is armed conflict?" Stanford University, Unpublished Manuscript.

Golte, Jurgen. 1980. "Repartos y Rebelions: Tupac Amaru y las contradicciones de la economia colonial" (Lima: Instituto de Estudios Peruanos).

Hollyer, J. R. 2009. Patronage or Merit? The Choice of Bureaucratic Appointment Mechanisms. Mimeo.

Jha, Saumitra. 2013. Trade, institutions, and ethnic tolerance: Evidence from South Asia. American political Science review, 107(04), 806-832.

Jones, Benjamin F. and Benjamin A. Olken. 2005. Do Leaders Matter? National Leadership and Growth Since World War II. The Quarterly Journal of Economics 120 (3): 835-864.

Lazear, Edward. 1999. Language and culture. Journal of Political Economy, 107(6), S95-S126.

Lenz, G.S. and Lim, K.. 2009. The Returns to Office: Public Service Requires No Financial Sacrifice for US Representatives. Unpublished manuscript.

Lohmann-Villena, Guillermo. 1957. El Corregidor de Indios en el Peru bajo los Austrias. Madrid: Ediciones Cultura Hispanica.

_____. 1974. Los Ministros de la Audiencia de Lima En El Reinado de los Borbones. Sevilla: Escuela de Estudios Hispano-Americanos.

Malaga, Alejandro. 1975. "Las Reducciones En El Virreinato Del Peru (1532-1580)". Revista De Historia De America, no. 80. Pan American Institute of Geography and History: 942.

Maloney, W., and F. Valencia. 2015. "The persistence of (Subnational) Fortune" Economic Journal Forthcoming.

McClintock, Cynthia. 1984. Why peasants rebel: The case of Peru's Sendero Luminoso. World Politics37(1): 48-84.

Miranda, C. 1873. Tasa de la visita general de Francisco Toledo. Lima, Peru: Universidad Nacional de San Marcos, Direccin Universitaria de Biblioteca y Publicaciones.

Moreno Cebrian, Alfredo. 1977. El corregidor de Indios y la economa peruana del siglo XVIII (los repartos forzosos de mercancias) (Madrid: Consejo Superior de Investigaciones Cientficas, Instituto "G. Fernndez Oviedo,")

Mukherjee, A. 2008. La Negociacion de un Compromiso: la mita de las minas de plata de San Agustin de Huantajaya, Tarapaca, Peru (1756-1766). Bulletin

de l'Institut Franais d' Etudes Andines, 37(1), 217-225.

Nunn, N., & Wantchekon, L. 2011. The Slave Trade and the Origins of Mistrust in Africa. American Economic Review, 101(7), 3221-52.

O'Phelan, Scarlett. 1988. "Un siglo de rebeliones anticoloniales: Peru y Bolivia 1700-1783" (Cuzco: Centro de Estudios Rurales Andinos "Bartolome de las Casas").

Querubin, P. James Snyder, Jr. 2013."The Control of Politicians in Normal Times and Times of Crisis: Wealth Accumulation by US Congressmen, 18501880." Quarterly Journal of political science, 8(4), 409-50.

Ramos Gomez, L.J., 1985. Las "Noticias Secretas" de America de Jorge Juan y Antonio de Ulloa (1735-1745). Madrid, Consejo Superior de Investigaciones Cientficas, No. 2.

Sanz Tapia, Angel. 2009. "Corrupcion o necesidad?: la venta de cargos de gobierno americanos bajo Carlos II (1674-1700) (Vol. 13). Editorial CSIC-CSIC Press.

_____ 1998. "Aproximacion al beneficio de cargos politicos americanos en la primera mitad del siglo XVIII" Revista Complutense de Historia de America 24: 147-176.

Swart, Koenraad Wolter. 1980. Sale of offices in the seventeenth century. (HES: Utrecht)

Vollmer, G. 1967. Bevlkerungspolitik und Bevlkerungsstruktur im Vizeknigreich Peru zu Ende der Kolonialzeit (1741-1821). Gehlen.

Weinstein, Jeremy M. 2006. Inside rebellion: The politics of insurgent violence. Cambridge Mass.: Cambridge University Press.