The Colonial Origins of Fiscal Capacity: Evidence from Patronage Governors*

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Abstract

We combine historical personnel data from the British colonial administration with modern public finance data to study the impact of colonial governors on fiscal capacity. Exploiting rule-based variation in the allocation of governors connected to their superior at the time of appointment, we find that modern-day countries exposed to more patronage governors exhibit lower fiscal capacity today. These negative effects are persistent over time and driven by indirect taxes that patronage governors disproportionately controlled in the colonial period. The results thus provide evidence for a public finance channel through which the effects of patronage appointments extend beyond decolonization.
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1 Introduction

Fiscal capacity is a key determinant of economic development (Besley and Persson, 2009). The ability of states to tax and provide public services is seen as a pre-requisite for the emergence of the modern economy. Despite its importance, we know relatively little about the colonial origins of fiscal capacity. While a long tradition of work focuses on explaining historical investments in fiscal capacity for today's developed (European) countries over time (North and Weingast, 1989; Tilly, 1990), there is less empirical evidence on what explains fiscal capacity in modern day countries that emerged from decolonization.

This paper studies the colonial origins of fiscal capacity by asking whether the exposure to colonial governors under a specific system of appointment - namely patronage - had any long-term impacts on the ability of modern-day countries to raise taxes. The study of fiscal capacity through the lens of colonialism is interesting for several reasons. First, colonialism was a defining episode in history - most developing countries today experienced a period of colonization. Second, by nature of colonialism, policy choices were often externally imposed, thus providing a plausibly exogenous source of variation in historical public investments. Finally, the sheer size of Empires allows a focus on single colonizers. At its peak, the British Empire, for example, spanned 20% of the Earth's landmass. The focus on a single organization enables us to hold constant cross-colonizer differences in institutions and colonization strategies.

We study the exposure of former British colonies to patronage governors during the colonial period 1854-1966. Patronage governors are senior bureaucrats who held social ties to their superior, the Secretary of State for the Colonies, at time of their appointment. These patronage connections have shown to be an important determinant for the assignment of governors to colonies. Most importantly, favored protégées have been documented to raise less indirect taxes and invest less in the revenue generation capacity of their assigned territories (Xu, 2018). As governors wielded power over their assigned colonies, especially in terms of public finance, these patronage governors may have had lasting impacts beyond the colonial period.

The main difficulty in estimating such long-run relationships is that governors are not randomly allocated across colonies. In the context of the British Empire, patronage governors

were historically allocated to higher salaried and larger colonies (Jones, 2013; Xu, 2018). If these differences across colonies persist, it is difficult to empirically disentangle the impact of patronage governors from selection. In order to overcome this empirical challenge, we introduce a novel microfounded instrument, making use of a six-year fixed term rule that was in place during the colonial period to instrument for the cumulative exposure to patronage governors. Specifically, we exploit the interaction between the staggered opening of vacancies and the governors' social ties to the Secretary of State for the Colonies at the time of appointment to compute the share of patronage governors who face a higher probability of being transferred to another colony. Given the rule-based variation, we argue and provide evidence that (i) longitudinal variation in the share of available patronage governors a year before the vacancy is filled predicts the probability of actually receiving a patronage governor, and (ii) this variation is uncorrelated with colony-level characteristics.

Our central outcome measure is the tax-to-GDP ratio. This is the central measure in the fiscal capacity literature (Besley and Persson, 2009). Exploiting the exogenous source of variation in the assignment of patronage governors to colonies, we find that an increase in the share of patronage governors in the colonial period by 1% point decreases the tax/GDP ratio in 2015 by 1.6% points. The variation in exposure to patronage governors explains around 8% of the overall cross-sectional variation in the modern tax/GDP ratio. This negative relationship is persistent over time between 1980-2015. The observed decline in modern tax/GDP is driven by a lower share of indirect taxes, in particular trade taxes. This is consistent with historical evidence that connected governors adversely affected fiscal capacity through trade taxes and exemptions. The results thus uncover a public finance channel through which historical patronage governors impact fiscal capacity beyond decolonization.

The limited number of modern day countries that emerged from the British Empire following decolonization imposes data constraints on the analysis. We use weak instrument robust inference, re-sampling and randomization inference to confirm the statistical significance of the main finding. We also conduct a range of additional checks to confirm that the results are not driven by selection. The point estimates are robust to the inclusion of a wide range of candidate confounders. In addition, a bounding exercise suggests that selection on unobservables

needs to be implausibly large to "explain away" the main results.

By providing evidence for persistence, these results suggest that the short-run costs of patronage shown in Xu (2018) also extend to the long-run.[1] The findings thus link two strands of the literature on long-run development. First, we add to the literature on the long-run determinants of fiscal capacity (Besley and Persson, 2009; Huillery, 2009). Much empirical work in this area has focused on studying the impacts of fiscal capacity and focused on today's developed countries (Aidt and Jensen, 2009; Dincecco and Prado, 2012; Dincecco and Katz, 2016). Our study provides evidence on how differences in fiscal capacity emerge and persist in the long-run, covering former colonies across all continents.[2] Second, we contribute to the literature on the persistence of colonial legacies. Most work has focused on the role of colonial institutions (Acemoglu et al., 2001; La Porta et al., 2008; Dell, 2010; Dell and Olken, 2017; Anderson, 2018). We complement this literature by highlighting the role of colonial leaders and their means of appointment. Using variation from the rotation rule - a common feature of bureaucracies - we link the literature on colonial legacies to the personnel economics of the Empire, speaking to the administrative determinants of development (Greif, 2007).

2 Context and data

2.1 Patronage in the colonial administration

This study focuses on the territories administered by the British Colonial Office. The Colonial Office was founded in 1854 and tasked with administering overseas possessions.[3] At the peak of British colonialism, this bureaucracy spanned the globe, covering nearly a fifth of the world's land mass (Figure A1). The administration of the colonies was handled by the colonial

[1] Xu (2018) uses within-governor and colony variation in social ties to the Secretary of State for the Colonies to estimate the short-run costs of patronage. We depart from this empirical strategy by leveraging an instrumental variable strategy to estimate the long-run effect of patronage *across* colonies.

[2] The negative long-run impact of a decline in trade taxes on fiscal capacity also resonates with historical panel data evidence that identifies a large fiscal cost of trade liberalization (Cage and Gadenne, 2018).

[3] This description follows Xu (2018). Studying patronage in the Colonial Office, the analysis excludes all territories not under control of the Secretary of State for the Colonies. These comprise territories administered by the India Office (modern-day India, Bangladesh, Burma and Pakistan) as well as territories whose oversight were devolved. See Appendix Table B1 for the full list.

governor. The governor was appointed by the Secretary of State for the Colonies to serve for a fixed period of 6 years, and would directly report to the Secretary of State. With their duties codified in the *Colonial Rules and Regulations*, governors were bureaucrats in the classic sense. Their powers were formally delineated under the "general powers of an officer appointed to conduct colonial government." At the same time, however, they effectively acted, as famously noted by governor Frederick Lugard, as the "man on the spot." Despite the subordinate position, governors enjoyed substantial discretion in their administration of the colonies. Governors in the most unchecked colonies exercised all executive powers and could enact laws directly by proclamation. With colonies spread across the globe, "the distance between the centre and the periphery required a policy of trust" (Banton, 2008). In effect, high monitoring costs rendered "any attempt to conduct the details of the administration from this country [UK] [...] absolutely impracticable." The autonomy of governors created different policies and practices across the dependencies (Jeffries, 1938). If these policies are persistent, exposure to patronage governors may thus have impacts that extend beyond the colonial period.

Governorships were primarily awarded based on social connections, or patronage. The Secretary of State enjoyed substantial discretion over the appointment of governors tasked with administering their assigned colonies. Governorships were explicitly held to be "proper objects for the exercise of patronage by the Secretary of State for the Colonies," and this practice lasted well into the 20th century (Jeffries, 1938).

While patronage often gave rise to favoritism, there are also potential benefits of patronage. Loyalty was a key aspect (Grindle, 2012): as governors were often sent out to administer far-away places, a central concern to the Crown was its ability to keep administrators in check. Patronage appointments directly tied bureaucrats to politicians, thus making governors easier to monitor and control (Laidlaw, 2005; Bourne, 1986). In addition, appointing governors through family ties and alumni networks could also improve screening and create reputational concerns, thus alleviating the role of imperfect information (Blakeley, 1972).

2.2 Data and descriptive statistics

We use an individual-level personnel dataset of the Colonial Office to construct our instrumental variable. The main source of data used to construct the governor-level panel is the Colonial Office List. These lists contain data on postings, backgrounds, and salaries of governors. The files have been compiled by the Colonial Office to document changes in the administrative structure and personnel of each colony under the control of the Colonial Office between 1860 and 1966. We digitized the entire set of Colonial Office Lists to match governors at any given point in time to the appointed colony and the corresponding salary. For the period before these lists were available, we derive the same information from the Civil Establishment List of the Colonial Blue Books, annual statistics for the possessions of the Crown.

Measuring patronage. We follow the standard approach in the literature and measure patronage using social ties (Fisman, 2001; Jia et al., 2015). We use pre-determined measures of connections between subordinate governors and their superior Secretary of States for the Colonies. We use the measure constructed in Xu (2018) that defines connectedness as a composite of three dimensions of homophily:

1. The appointed governor and Secretary of State share common ancestry
2. Both hold hereditary aristocratic titles (e.g. Baron, Duke)
3. Both either went to Oxford, Cambridge or Eton

Since we draw upon an existing measure of connectedness, we only briefly sketch its construction and refer to Xu (2018) for a more detailed description. The measure of connectedness is derived from genealogical and biographical data. To construct a measure of common ancestry, governors and Secretary of States are matched in the genealogical dataset "The Peerage" based on their full name and birth date. The shortest distance by pre-determined family ties is then computed for each of the governor and Secretary of State pair, and a pair is defined to have shared ancestry if the degree of separation is sufficiently low. Biographical data from the UK Who's Who and the Oxford Dictionary of National Biography is used to identify whether both individuals belong to the Peerage (a system of hereditary aristocrat titles), or attended the same elite university and schools. By definition, a governor is connected to the Secretary of State if both share any of the common traits.

A governor is thus defined as a patronage governor if he either shares common ancestry, peerage or an alumni network with the Secretary of State at time t of appointment to colony s. The overall exposure to patronage governors throughout the colonial period is then derived by computing the share of patronage appointments. Table 1 provides summary statistics for the overall share of patronage governors. On average, the share of patronage governors a British colony is exposed to is 6.8% in the colonial period 1854-1966.[4] There is substantial variation across colonies: the interquartile range is 5.2%.

Linking historical and modern public finance data. Relating historical colony-level variation in the exposure to patronage governors (C_s) to contemporary outcomes (y_{st}) also requires a mapping of colonial territories into modern countries and subnational units. This is straightforward for the majority of colonies which can be directly mapped into modern countries. For Australia and Canada, historical colonies can be mapped into subnational provinces. We drop two territories that cannot be mapped into modern regions, as well as the set of small island territories that still remain dependencies of the UK.[5]

We link the historical data to modern public finance data drawn from several sources. First and foremost, we undertook a targeted data collection process to construct national and subnational tax/GDP ratios. The main source of data for the tax/GDP ratios is from the International Centre for Tax and Development (ICTD). The dataset provides consolidated and harmonized public finance data from a wide range of sources including IMF Country Reports, OECD Tax Statistics, the IMF's Government Finance Statistics (GFS), and governmental sources (Prichard, 2016). To complement this dataset with subnational measures for the historical colonies that map into modern territories, we use data from the Canadian (CAMSIM) and Australian statistical office (Government Finance Statistics). When computing the province-level tax/GDP measures, we focus on state-own revenues, excluding transfers and taxes that are raised at the federal level. The temporal coverage of this data ranges from 1980 to 2015.

[4]Since the paper defines patronage ties exclusively through exogenous family connections, the measure of patronage governors is likely to be conservative. This number is thus likely to form a lower bound.

[5]Heligoland is part of Germany, Ionian Islands part of Greece. The 7 colonies that are still part of Britain are small entities: Bermuda, Cayman Islands, Falkland Islands, Gibraltar, Montserrat, St. Helena, British Virgin Islands. Appendix B1 provides a summary of the mapping process.

3 Empirical setting and research design

For the modern outcome y_{st} of the country or the subnational province corresponding to the historical colony s in year t, we estimate the following model:

$$y_{st} = \beta \times C_s + \gamma' x_s + \mu_{R(s)} + \tau_t + \varepsilon_s \quad (1)$$

where $C_s = \Sigma_t c_{I(s,t),t}/N_s$ denotes the share of patronage governors in the colonial period and $i = I(s,t)$ is the link function that returns the governor i serving in colony s at time t. The vector x_s comprises pre-determined colony-level controls and $\mu_{R(s)}$ are continent fixed effects, where $r = R(s)$ denotes the continent the colony was located in. Since we pool data across the years 1980-2015 to increase precision, we include year fixed effects to flexibly partial out secular trends. With the identifying source of variation across colonies, we cluster the standard errors at the colony-level s.

3.1 Empirical strategy and colonial allocation rule

The main empirical challenge when estimating equation (1) is that patronage governors are not randomly allocated across colonies. Table 2 shows this by reporting the bivariate elasticity between colony-level characteristics and the actual exposure to patronage governors (Column 3). Higher shares of patronage governors are significantly more likely to be found in higher salaried colonies, and less likely to be found in tropical colonies or other regions. Indeed, a significance test rejects the null hypothesis that all elasticities are jointly zero at the 1% level. If these historical cross-colony differences persist, any observed correlation between the modern-day outcome and the share of patronage governors C_s is likely to be spurious. Higher salaried colonies, for example, may have higher fiscal capacity to begin with. If these initial differences persist, the estimated relationship between historical patronage and fiscal capacity today will be upward biased. Similarly, if higher salaried colonies experienced a reversal of fortune (Acemoglu et al., 2002), OLS might be downward biased. The estimation of long-run effects requires an instrument that predicts the likelihood of a colony to receive

patronage governors, but that is otherwise unrelated to colony-level characteristics.

We exploit the rule-based rotation of governors to construct a microfounded instrument to estimate long-run effects of exposure to patronage governors. The proposed instrument exploits two institutional features that generate variation to meet both the relevance and exclusion conditions. The first source of variation stems from the allocation rule that predicts the pool of candidates who are more likely to be transferred to a vacant colony: by the colonial regulations, the length of a governorship is limited to six years. While there exists non-compliance, the plurality of governorships (25%) indeed end in the sixth year (Figure A2).

The second source of variation stems from the turnover of Secretaries of State which generates cross-sectional variation in the connections to serving governors. The interaction of both sources of variation results in temporal variation in the number of patronage governors who are likely to be moved to a newly vacant colony. The share of patronage governors with at least 6 years of tenure is hence an instrument for a patronage appointment in the subsequent year. The share of patronage governors with at least six years of tenure in t is defined as:

$$p_t = \left(\frac{\Sigma_i \mathbf{1}[T_{it} \geq 6] \times c_{it}}{\Sigma_i \mathbf{1}[T_{it} \geq 6]} \right) \tag{2}$$

where $c_{it} = 1$ if the governor is connected to the Secretary of State for the Colonies. T_{it} denotes the years of tenure for governor i in year t. Governors with at least 6 years of tenure are defined as "available" governors due to their higher probability of transfer to new vacancies.

To provide some intuition, consider following example. In 1858, none of the available governors were connected to their superior (Figure A3). In the subsequent year, 1859, after a new Secretary of State for the Colonies took over, 43% of the available governors in service were connected. This implies that a colony that fell vacant in 1859 due to the six year term limit will then be much more likely to receive a patronage governor than a colony that opened up a year earlier. *Which* colony opens up, however, depends on the staggered opening of the colonies, thus providing plausibly exogenous variation in the propensity of a colony to be filled with a patronage governor that is unrelated to the characteristics of the colony.[6]

[6]The staggered opening of colonies arises from the combination of two factors: (i) differences in the initial year of colonization, and (ii) differences in non-compliance with the six year term limit. This non-compliance is caused

Consider another example: Philip Edmond Wodehouse was appointed governor of Cape Colony in 1862, after having completed his previous stint in British Guiana. In the year before the Cape Colony was filled, Woodhouse was one of six governors connected to the Secretary of State who had to be moved. In 1861, there were a total of 15 governor that needed to be moved, making the share of connected governors that served beyond the term limit as high as 40%. Had the Cape Colony vacancy opened up three years earlier, there would have been no available connected governors, and the likelihood of the Cape Colony to receive a connected governor thus lower.

Predicting patronage governors: To first show the relevance of the instrument in predicting individual appointments in the colonial period, we estimate following equation,

$$C_{st} = \beta \times p_{t-1} + \gamma' x_{st} + \theta_s + \varepsilon_{st} \quad (3)$$

where $C_{st} = 1$ if the governor appointed to colony s at time t was connected to the Secretary of State (and hence a patronage governor) and 0 otherwise. p_{t-1} is the share of patronage governors with at least 6 years of tenure in the year prior to the appointment. The vector x_{st} comprises controls. In the baseline specification, this includes a time trend and decade fixed effects. Since colonies experience many appointments, it is also possible to include colony fixed effects θ_s. Finally, ε_{st} captures the error term, which we cluster at the year level. This is the same level at which the instrument - the share of available patronage governors - varies.

Table 3, Panel A relates variation in the share of patronage governors available for reshuffle the year before the appointment to the propensity of a colony to actually receive a patronage governor. The probability of a governorship to be filled by a patronage governor is 35% points higher if all available governors in the previous year were connected vis-a-vis when all available governors were unconnected (Column 1). Compared to the mean of the dependent variable, this magnitude is large. Since the identifying variation of the instrument is longitudinal, a natural concern is that the variation is correlated with long-term trends. Given the idiosyncratic nature of the variation (Figure A3), however, the inclusion of the annual trend

by idiosyncratic factors such as early death, or delays in appointments. Most importantly, Table B2 shows that this non-compliance is uncorrelated with our instrument.

and decade fixed effects does not substantially change the estimated coefficients (Column 2).

For the share of available patronage governors to be a valid instrument for patronage appointments also requires the instrument to be uncorrelated with characteristics of the appointed colonies. Since colonies are filled with governors multiple times throughout the colonial period, it is possible to test this assumption by assessing the coefficient stability in response to the inclusion of colony fixed effects. Consistent with the share of available patronage governors being unrelated to fixed colony characteristics, the inclusion of colony fixed effects does not substantially move the point estimates (Column 3). Finally, to provide a placebo test, the last column includes leads and lags in the variation of the share of available patronage governors. In line with the intuition of the instrument, it is only the variation in the share of available patronage governors the year prior to the opening that drives the first-stage. Leads and lags in the variation do not predict patronage appointments (Column 4).

A potential worry may arise in presence of strategic non-compliance with the six year term rule. Indeed, the six year fixed term for governorships is not perfectly enforced (Figure A2). While the official duration was six years, unexpected events such as governor illness, death, or external conditions have often led to the early or late termination of appointments.[7] There might thus be a concern that early or late termination of previous appointments is correlated with the share of patronage governors and characteristics of colonies. Appendix Table B2 tests for this possibility by relating the share of available patronage governors to a dummy that is 1 if the appointment ended regularly in the sixth year. We find no evidence that the share of available patronage governor is systematically correlated with the propensity of colonies to open up. There is also no difference by the desirability of the colony, as measured by governor salary or revenue size. The first-stage is not mechanical: the Secretary of State may override the transfer rule and appoint a connected governor who has not completed the term. Similarly, the Secretary of State may decide to choose from outside the pool of available, serving governors by appointing a new governor. It is exactly this endogenous source of variation in

[7]Unfortunately, there is no systematic mortality data that has been compiled for colonial governors. Anecdotally, however, there is certainly evidence that governors suffered from tropical diseases: Edward Rushworth died of yellow fever in Jamaica. William Edward Maxwell died of blackwater fever in the Gold Coast. Similarly, Robert William Keate died just ten days into his governorship aged 59.

the appointment of governors that is purged using the instrument.

3.2 Aggregation and cross-colony first-stage

The cross-sectional long-run instrument is implemented by aggregating the identifying source of variation from the appointment level st to the colony-level s. This allows us to instrument the endogenous share of patronage governors in the colonial period with the share of patronage governors between 1854-1966 as predicted by the instrument. For each colony, we calculate the predicted share of patronage governors as,

$$P_s = \left(\Sigma_t p_{t-1} \times \mathbf{1}[T_{I(s,t),t} = 1]\right)/N_s \tag{4}$$

where p_{t-1} is the proportion of patronage governors among all governors available for transfers a year before the position in colony s is filled. The indicator $\mathbf{1}[T_{I(s,t)t} = 1]$ counts the total number of appointments, where $T_{I(s,t)t} = 1$ denotes the first year of the appointment, and N_s is the total over the period 1854-1966. Instead of weighting each appointment with the actual dummy of a patronage governor, the colony-level instrument P_s is the number of appointments weighted by the share of patronage governors available the year before the appointment p_{t-1}, and finally divided by the overall years. The predicted share of patronage governors thus captures the intensity of exposure across colonies.[8]

Balance test: Table 2 provides a balancing test by comparing the characteristics of colonies with a large share of predicted patronage governors to places with a low share of patronage governors. Column 4 reports the elasticity of a bivariate regression of colony characteristics on the predicted share of patronage governors. Both the predicted share and actual shares are standardized with a mean 0 and standard deviation 1 to ensure the comparability of the coefficients across Column 3-4.[9] In contrast to the same elasticities reported for the actual exposure to patronage governors, there is no statistically significant association between colony characteristics and variation in the instrument. Indeed, we cannot reject that all coefficients

[8]The results are also robust when using the predicted number of patronage appointments. We choose the share specification in order to compare our estimates with those found in the literature (See Section 4).

[9]See Table 1 for descriptive statistics on the instrument.

are jointly zero. Based on these observables, variation in the predicted share of patronage governors is as good as randomly assigned.

For the instrument to be valid, we also require the predicted exposure to patronage governors to be associated with the actual exposure to patronage governors. The cross-colony first-stage regression that relates past exposure to patronage governors to the exposure predicted by the six year allocation rule is given by:

$$C_s \quad = \quad \beta \times P_s + \gamma' x_s + \mu_{R(s)} + \varepsilon_s \tag{5}$$

where $C_s = \Sigma_t c_{I(s,t),t}/N_s$ denotes the share of patronage governors in the pre-Independence period and $i = I(s,t)$ is the link function that returns the governor i serving in colony s at time t. P_s is the predicted share of patronage appointments as described in equation (4). The vector x_s comprises pre-determined colony-level controls. The basic controls include continent fixed effects $\mu_{R(s)}$, where $r = R(s)$ denotes the continent the historical colony is located in, geographical controls for the area within the tropics and a dummy for landlockedness. We also control for the overall years colonies were under British control. In extensions, we include colonial controls such as the initial (log) governor salary and the initial (log) revenue size of the colony. Given the high level of aggregation, the level of treatment coincides with the unit of observation, and we compute heteroskedasticity robust standard errors.

Table 3, Panel B demonstrates the relevance of the instrument in predicting aggregate cross-colony variation in the share of patronage governors. The instrument is significantly associated with the endogenous variables: an increase in the predicted share of patronage governors in the colonial period 1854-1966 increases the actual share of patronage governors by 1.3% points (Column 1). Given the exogenous nature of the identifying variation and mirroring the pattern observed on the appointment level, the inclusion of additional controls leaves the point estimates nearly unchanged. In Column 2, we include the basic controls, which comprise the overall duration under British control and pre-determined geographical characteristics (continent fixed effects, landlockedness, area under tropics). While the estimated magnitude declines somewhat, we cannot statistically reject the equality of the coefficients. In

Column 3, we control for (log) governor salary and the initial (log) revenue size of the colony. These are characteristics that have shown to be important for the allocation of governorship (Xu, 2018). Once again, the coefficients remain comparable. Column 4 includes both set of control variables. The first-stage elasticity remains virtually unchanged. Finally, Figure 1 plots the partial correlation between the share of patronage governors and the predicted share of patronage appointments 1854-1966. Despite the limited sample size, the figure shows a clear positive relationship between the endogenous variable and the instrument which is not driven by outliers, thus prodiving visual evidence for the presence of a first-stage relationship.

4 Long-run persistence

4.1 Effects on fiscal capacity

We now estimate regression (1) and relate variation in exposure to patronage governors in the colonial period to a central measure of fiscal capacity: the tax/GDP ratio.

[Table 4]

The results are reported in Table 4 and provide evidence for a negative impact of patronage governors on post-independence fiscal capacity. Since the identifying variation is cross-sectional, we first estimate a single cross-section based on the latest available tax/GDP ratios in 2015. This is the simplest specification, and we subsequently also present the pooled results based on 1980-2015. All specifications include the basic controls.

Column 1 shows the OLS estimate. While negative in sign, we find no statistically significant association between the overall exposure to patronage governors in the colonial period and modern-day fiscal capacity. Columns 2-3 report the reduced form effects using the predicted share of patronage governors with different sets of controls.[10] The reduced form effect is significantly negative: an increase in the share of patronage governors as predicted using the allocation rule by 1% point reduces the tax/GDP ratio by 1.6% points (Column 2). In Column

[10]To focus on the main coefficient of interest, we do not report the point estimates of any control variables. For completeness, these can be found in Appendix Table B5.

3, we include the colonial controls, which comprise the (log) initial salary paid to governors and the (log) initial revenue size of the colony. These variables capture the lucrativeness of a colony, and thus may drive both historical appointment decisions and long-run differences in fiscal capacity. Given the idiosyncratic variation of the instrument we exploit for identification, the inclusion of the controls leaves the point estimate almost unchanged and, if anything, increases the magnitude of the estimate. Finally, in order to gauge the magnitude of the effect, Columns 4-5 report the 2SLS estimates. Consistent with the preferential allocation of patronage governors, the OLS estimate (Column 1) is upward biased: the negative IV estimate is about 5.5 times smaller. Given the limited sample size, the first-stage for the patronage period is weak, with an F-statistic of 4. We address this by computing weak-instrument robust confidence sets (Chernozhukov and Hansen, 2008) and continue to use the reduced form as the preferred specification.

The magnitude is about three times larger than the (OLS) estimate of exposure to external wars (Besley and Persson, 2009).[11] While this appears large, the estimate is substantially smaller than the effects of other candidate determinants. For example, Besley and Persson (2009) report that countries with Scandinavian legal origin have 29% points higher tax/GDP ratios today. Overall, variation in the predicted exposure to patronage governors explain only 8% of the cross-sectional variation in tax/GDP observed in 2015. By comparison, variation in the share of area under tropics explain 40% of the cross-sectional variation in tax/GDP.

In Panel B of Table 4, we report results using the pooled data for 1980-2015. While the identifying variation remains cross-sectional, the pooled data allows us to obtain greater precision and study how persistent the estimated effects are. In Panel B, Column 1, we estimate the average impact between 1980-2015 and confirm the negative relationship visually in Figure 2. The magnitude is now half of the size in 2015. When estimating the impact by 5 year bins (Figure 3), the plot reveals a gradual tapering off over time until the 2000s, and a reversal afterwards. Overall, however, all coefficients are negative and the trend is, if anything, declining.[12]

[11] Similarly, Dincecco and Prado (2012) find that 1 additional casualty per square km between 1816-1913 (mean casualty 0.10, SD 0.26) is associated with 0.13% point higher tax/GDP today.

[12] The result is also not driven by composition changes but persists when constraining the sample to a balanced panel of countries (Appendix Figure A4). When testing for persistence by interacting the exposure to patronage governors with a linear time trend, the interaction is negative though statistically insignificant.

The ICTD country-level data also allows us to break down the taxes into sources. A key finding in Xu (2018) is that governors with social ties to their superior generate fewer indirect taxes and provide more exemptions at the customs. Consistent with this negative impact on the colonial period, Columns 2-5 of Panel B suggest that the negative impact is primarily driven by the decline in indirect taxes, which comprise trade taxes and taxes on goods and services (Columns 2-3). When breaking the indirect taxes further into goods & services and trade taxes, we find that the negative impacts are driven by declines in trade taxes. This is consistent with the evidence from the colonial period (Xu, 2018), thus pointing to a public finance channel through which colonial governors might affect fiscal capacity in the long-run.[13]

4.1.1 Robustness

A major empirical caveat is the validity of the exclusion restriction. Although Section 3.1 provided evidence that variation in the instrument is uncorrelated with fixed colony-level characteristics in the colonial period, concerns might still remain over whether the aggregate cross-colony variation in the predicted share of patronage governors is correlated with unobserved cross-colony differences, even after conditioning on continent fixed effects and the colonial and geographic controls. We thus investigate the extent of selection on observables by controlling for a set of candidate confounders. For example, an important strand of the literature identifies ethnic fractionalization to be a key long-term determinant of development (Alesina et al., 2003). Another strand of the literature argues that colonial institutions brought by the settlers might have had long-run impacts (Acemoglu et al, 2001; Spolaore and Wacziarg, 2009). Finally, a body of literature has documented the link between external conflicts and the need to invest in fiscal capacity (Tilly, 1990; Dincecco and Prado, 2012). If these characteristics are also correlated with the predicted exposure to patronage governors, our estimates would be biased. In Appendix Table B6, we show that the inclusion of ethnic fractionalization, genetic distance to the UK, settler mortality and conflict leaves our point estimates largely unchanged. We also show that the inclusion of state antiquity, agricultural suitability and ruggedness does

[13]To explore whether the persistence operates through a narrow channel or also affects a wide range of broader economic outcomes, Table B3 reports the impact of exposure to patronage governors on (log) GDP per capita (Feenstra et al., 2015). We find no evidence for wider impacts.

not affect our conclusions (Appendix Table B7).[14]

Concerns over unobserved correlates still remain. To assess the extent to which unobserved correlates might still bias our estimates, we conduct a robustness check following Altonji et al. (2005) and Oster (2017). Specifically, we ask how large the amount of selection on unobservables relative to observables would need to be to "explain away" the negative coefficient in Panel B, Column 1. Under conservative assumptions,[15] we find that the amount of selection on unobservables needs to be unreasonably large when using conventional thresholds in the literature (Oster, 2017). For the reduced form result (Column 5), selection on unobservables would need to be 1.5 times larger than selection on observables. With the standard cut-off ratio of equal selection between unobservables and observables, our estimate is robust.

Another obvious caveat in this setting is the sample size, which is constrained by the number of now-independent former British colonies for which we have tax/GDP data. The small sample size creates two related concerns: first, as discussed, the relatively low F-statistic may raise concerns of weak instruments, which would bias the IV estimates towards OLS. Since the OLS is upward biased, the presence of weak instruments would then provide us lower bounds of the impact of patronage governors. To econometrically address the presence of weak instruments, we follow Chernozhukov and Hansen (2008) and also report results using fully weak instrument robust inference procedures. In Table 4, we report the 90% confidence sets. The confidence set lies in the negative range, thus further assuaging concerns of weak instruments. The second, related concern is small-sample inference. We use standard re-sampling methods to compute bootstrap standard errors, which we report in Table B4. As Table B4 shows, our main results also remain significant when reporting bootstrap standard errors.

Finally, we conduct placebo tests by regressing tax/GDP in 2015 on random allocations of the predicted share of patronage governors. Figure A5 shows the corresponding distribution of estimated coefficients based on 1,000 random permutations. As Figure A5 shows, the bulk of the estimated coefficients lie centered around zero. In contrast, the actual coefficient of -1.62, marked with a solid vertical line, is at the lower end of the distribution. Only 1 of the 1,000

[14] We also tested for heterogeneity in the effect of patronage governors. We find no evidence that the effect varies with the level of democracy, both at the time of independence and in the year 2017.

[15] We implement the bounding exercise using *psacalc* in Stata. We conservatively assume an maximum $\bar{R}^2 = 1$.

placebo coefficients is smaller than the actual estimate. The combined results thus suggest that our results are, despite the data limitations, very robust.

4.1.2 Heterogeneity - Civil Service Reform

We provide a last cut that exploits heterogeneity in early vs. late exposure to patronage governors. As Kirk-Greene (2000) discusses, the practice of patronage appointments was substantially more prevalent before 1930. After 1930, a civil service reform (The Warren Fisher Reform) limited the amount of discretion the Secretary of State could exercise in the appointment of governors. As Xu (2018) shows using panel data in the historical period, the reform indeed reduced favoritism and the negative fiscal performance between connected and unconnected governors. In this case, we expect the negative effect to be driven by patronage governors in the pre-reform period. If early choices of governors have more persistent impacts, we also expect the effect of early exposure to patronage governors to be larger.

We thus compute the instrument separately for the pre-1930 and post-1930 period, and report the results in an augmented cross-sectional regression in Table 5. Consistent with the interpretation that patronage had a larger impact in the early period, the negative impacts are driven by the exposure to patronage governors in the early period (Column 1-2). The coefficients move little when including both instruments, in line with the idiosyncratic, rule-induced variation used to construct the instrument (Column 3). In the remaining columns 4-5, we confirm the heterogeneity using two alternative measures based on the pooled country-level data. Once again, the results remain comparable.

4.1.3 Channels of persistence

There are two channels that could give rise to the negative impact on trade tax revenues: (i) lower tariffs, holding enforcement constant and (ii) weaker enforcement (and thus greater evasion), holding tariffs constant.

While it is difficult to disentangle both channels, Table 6 provides suggestive evidence that the colonial legacy of patronage is driven by weaker enforcement of existing policies. To show this, we first collect data on the effectively applied weighted average tariff from the World

Bank's WITS dataset. Since the WITS data is not available for the entire sample period 1980-2015, we constrain the sample to 1993-2015. As the comparison between Column 1 and 2 show for trade taxes, the results using both sample periods are similar.

Table 6, Column 3 relates the effective tariff rate to the exposure to patronage governors in the colonial period. If the colonial legacy of patronage makes it more likely that officials today provide exemptions from trade taxes, I expect a statistically significant negative effect. While the point estimate is indeed negative, the magnitude is economically and statistically insignificant. In a mediation analysis, Column 4 augments the baseline specification (Column 2) by holding constant difference in tariff rates. If tariff rates were a mediator, we expect the inclusion of the control to substantially reduce the negative effect. The estimate however remains nearly identical to the baseline estimate of Column 2.

If exposure to patronage governors does not lower trade revenue through reduced tariff rates, evasion is more likely to be the alternative explanation. We thus conclude the analysis by providing suggestive evidence for weaker enforcement at the customs. To test this, we follow Fisman and Wei (2004) by constructing a measure of "missing imports" from the UK. This approach uses the fact that a transaction is recorded twice: once in the UK as an export to the destination country; and another time at the destination country as an import from the UK. While discrepancies in the reported values can reflect measurement error, Fisman and Wei (2004) argue that a systematic association is more likely to reflect evasion. We follow Fisman and Wei (2004) by computing the sum of the absolute deviation of the import gap at the six-digit level across all product categories.

As Column 5 shows, the measurement error in reported import values is larger for countries exposed to a large number of patronage governors. Most importantly, Column 6 repeats the mediation analysis by extending the baseline specification (Column 2) to hold constant variation in the measure of misreporting. Consistent with the interpretation as evasion, the amount of misreporting at the customs is negatively related to trade revenue. Most importantly, the inclusion of the control more than halves the impact of patronage governors, now rendering the estimate insignificant. The results thus suggest that the lower trade taxes today are more likely to stem from persistent administrative shortcomings.

5 Conclusion

A long tradition of economic thought views the ability of states to raise taxes as a critical prerequisite for a well-functioning state. How fiscal capacity emerges in the first place, however, still remains an open question. This paper contributes to answering the question by studying the colonial origins of fiscal capacity among former colonies of the British Empire.

The results provide evidence that colonies that were exposed to a large share of patronage governors exhibit lower fiscal capacity today. This negative relationship is persistent over time and driven by the lower capacity to generate indirect tax revenue. Consistent with the negative impact of patronage governors on the generation of indirect trade taxes during the colonial period (Xu, 2018), the results thus provide evidence for a public finance channel through which colonial legacies may extend beyond independence. More broadly, the empirical findings resonate with an emerging empirical literature that identifies patronage as a key impediment to state effectiveness. The long-run results caution that patronage appointments - no matter how fleeting - can have substantial persistent effects.

Given the ubiquity of patronage appointments in history, an intriguing question is whether the results would extend beyond the British Empire. While the vast differences across Empires and colonizers prevent us from drawing strong conclusions, we hypothesize that the power given to appointed governors to enact policies at discretion was a key factor that gave rise to the observed negative effects. Exploring the colonial origins of fiscal capacity for other European colonizers with different administrative styles might provide a fertile testing ground.

A challenge in long-run studies is to pinpoint the exact mechanism through which persistence operates. For example, it remains an open question why the decline in indirect taxes is not offset by an increase in direct taxes (Besley and Persson, 2013). While the results provide suggestive evidence for weak enforcement as a channel of persistence, we view the discussion on mechanisms certainly as not definite. Future work using microdata, for example, might enable analyses on the tax legislation or tariff-level. This would enable us to trace out the pattern of persistence from the colonial period to decolonization, and up to the modern day.

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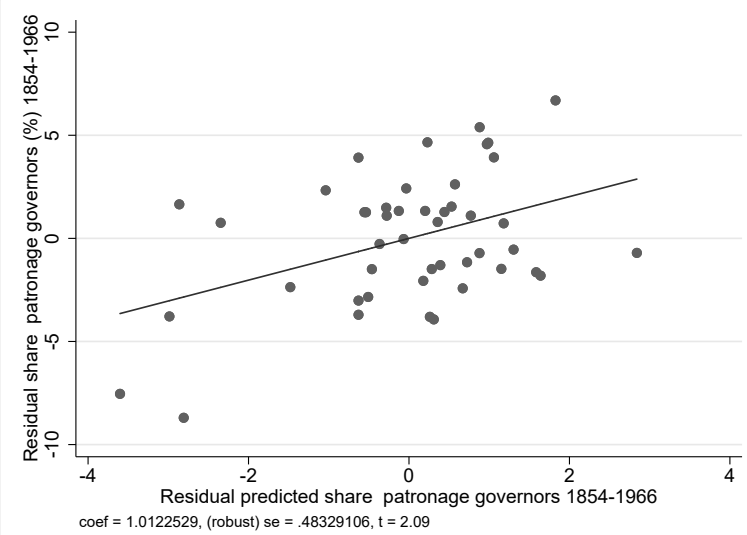
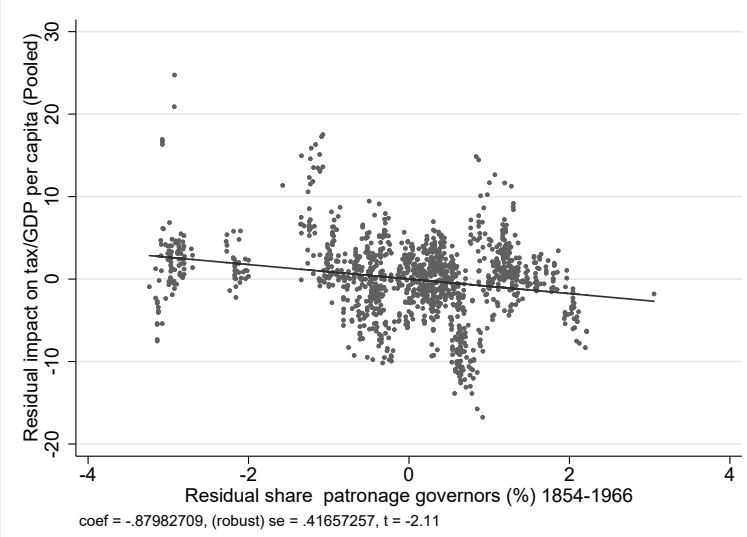
6 Figures and Tables

Figure 1: Share connected governors and predicted share - First-stage



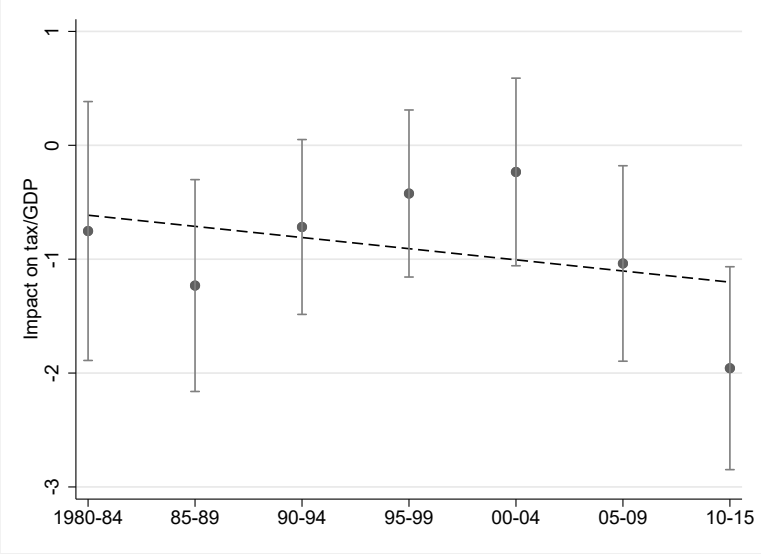
Notes: Partial correlation between the share of patronage governors and the predicted share of patronage governors 1854-1966, controlling for the total duration under British control, (log) initial governorship salary and (log) initial revenue size (in GBP), the share of land area within tropics, landlockedness and absorbing continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania). Robust standard errors.

Figure 2: Patronage appointments and fiscal capacity - Partial correlation



Notes: Reporting the partial correlation between the predicted share of patronage governors 1854-1966 and tax/GDP 1980-2015, conditioning on the continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania), the number of years under British control, the (log) initial salary and revenue (in GBP), the area in the tropics and landlockedness.

Figure 3: The impact of patronage appointments on fiscal capacity over time



Notes: Reporting the impact of the share of patronage governors in the colonial period 1854-1966 on tax/GDP, by 5-year bins. Standard errors are clustered at the colony-level, showing 90% confidence intervals.

Table 1: Descriptive statistics of treatment and outcomes

	(1) Mean	(2) SD	(3) IQR	(4) Obs
Share patronage governors 1854-1966	6.767	4.090	5.191	46
Predicted share of patronage governors	5.053	1.402	1.424	46
Subnational Tax/GDP 2015 (%)	19.374	7.455	9.205	46
Country-level Tax/GDP (%) 1980-2015	18.893	7.090	9.03	1,442
Country-level direct Tax/GDP (%) 1980-2015	10.191	7.014	14.166	1,315
Country-level indirect Tax/GDP (%) 1980-2015	10.879	4.722	4.557	1,352

Notes: Descriptive statistics for treatment and key outcome variables: Share of patronage governors is the share of colonial governors who were connected at time of appointment. Predicted share of patronage governors is the share of patronage governors as predicted using the allocation rule (See Section 3.1). Subnational Tax/GDP 2015 (%) is the tax/GDP ratio in 2015 or the latest year which varies on the subnational level for provinces of Australia and Canada that comprised separate colonies in the colonial period. Tax/GDP (%) is the share of tax/GDP between 1980-2015. Direct Tax/GDP (%) is the share of direct taxes (sum of taxes on income profits and capital gains, taxes on payroll, workforce and property tax, excluding social contributions and resource revenue) over GDP, and Indirect Tax/GDP (%) is the share of indirect taxes (sum of taxes on goods and services, taxes on international trade and other taxes) over GDP.

Table 2: Correlates of country-level exposure and balance test

	(1)	(2)	(3)	(4)
			β: % patronage governors (standardized)	
	Mean	SD	Actual	Predicted
(Log) initial governor salary	7.605	0.827	0.191*	-0.035
			(0.104)	(0.126)
(Log) initial revenue	10.587	1.729	0.170	-0.234
			(0.202)	(0.213)
Area tropics	0.567	0.443	-0.204***	-0.077
			(0.053)	(0.071)
Landlocked	0.108	0.314	-0.064	-0.052
			(0.040)	(0.047)
Africa	0.348	0.481	-0.143**	-0.095
			(0.057)	(0.079)
Asia	0.065	0.249	0.015	0.004
			(0.026)	(0.011)
Europe	0.043	0.206	0.024	0.029
			(0.020)	(0.023)
Latin America & Carribbean	0.239	0.431	-0.120**	-0.017
			(0.051)	(0.058)
Northern America	0.109	0.314	0.060	0.026
			(0.068)	(0.058)
Oceania	0.196	0.401	0.164**	0.053
			(0.069)	(0.070)
Duration under British control	108.43	62.52	0.261	9.344
			(7.747)	(6.900)
Joint test all coeff = 0 (p-value)			0.000	0.798
Observations	46		46	46

Notes: Descriptive statistics for colony-level characteristics and the correlation with the actual share of patronage governors and the instrument. The table reports the mean (Col. 1), SD (Col. 2) as well as the elasticities of regressing the variable on the share of (actual) patronage governors (Col. 3), as well as the share of (predicted) patronage governors 1854-1966 (Col. 4). Both shares are standardized with mean 0 and SD 1 to ensure comparability of the coefficients. In Panel B, (log) initial salary is the (log) of the first governor salary observed in the colony. (log) initial revenue is the (log) of the initial public revenue in the colony (in GBP). Area tropics is the share of the landmass in the tropics. Landlocked is a dummy that is 1 if the colony has no access to the sea. Africa, Asia, Latin America & Carribbean are region dummies that denote whether the colony is located in either of the regions. The joint test reports the p-value of the joint hypothesis that all partial elasticities estimated are 0. Reporting robust standard errors.

Table 3: Predicting patronage appointments - First-stage

Panel A: Appointment-level	(1)	(2)	(3)	(4)
	Patronage governor (C_{st})			
Mean of dep. var	0.277	0.277	0.277	0.277
Lag prob. connected governor (p_{t-2})				0.049
				(0.123)
Prob. connected governor (p_{t-1})	0.346***	0.417***	0.369***	0.362***
	(0.104)	(0.123)	(0.127)	(0.130)
Lead prob. connected governor (p_t)				0.024
				(0.138)
Linear trend	-	Y	Y	Y
Decade FEs	-	Y	Y	Y
Colony FEs	-	-	Y	Y
Observations	947	947	947	947
Panel B: Cross-colony level	(1)	(2)	(3)	(4)
	Share patronage governor (C_s)			
Mean of dep. var	6.768	6.768	6.768	6.768
Predicted share patronage governors (P_s)	1.367***	0.980**	1.381***	1.012**
	(0.407)	(0.482)	(0.403)	(0.483)
Basic controls	-	Y	-	Y
Colonial controls	-	-	Y	Y
Observations	46	46	46	46

Notes: First-stage for the appointment level st (Panel A) and the cross-colony level s (Panel B). In Panel A, the dependent variable patronage governor is a dummy that is one if the governor was connected to the Secretary of State for the Colonies at time of appointment. Prob. of connected governor is the share of governors that are connected and beyond their fifth year (and hence available for reshuffle) the year prior to the appointment, see equation (2). Column 4 includes one period leads and lags. In Panel B, the dependent variable is the actual share of patronage governors by colony over the entire colonial period 1854-1966. Predicted share of patronage governors is calculated based on the share of available connected governors the year before each appointment. Basic controls comprise continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania), the share of land area that lies in the tropics, landlockedness and the overall duration under British control. Colonial controls comprise the (log) initial governor salary and (log) initial revenue (in GBP). Standard errors are clustered at the year level for Panel A. Panel B computes robust standard errors.

Table 4: Exposure to patronage governors and modern-day fiscal capacity

Panel A: Tax/GDP in 2015	(1)	(2)	(3)	(4)	(5)
	Tax revenue/GDP (%)				
Mean of dep. var	19.37	19.37	19.37	19.37	19.37
Share patronage governors (C_s)	-0.314			-1.700*	-1.651*
	(0.229)			(0.958)	(0.934)
Predicted share patronage governors (P_s)		-1.598***	-1.627***		
		(0.527)	(0.525)		
Estimation	OLS	Reduced form		2SLS	
Basic controls	Y	Y	Y	Y	Y
Colonial controls	-	-	Y	Y	Y
Kleibergen-Paap F-statistic	-	-	-	3.910	4.042
Weak IV A-R 90% confidence set	-	-	-	[-3.1;-0.3]	[-2.9;-0.3]
Data source	ICTD + Provincial tax/GDP: latest 2015				
Observations	46	46	46	46	46
Panel B: 1980-2015, by source	(1)	(2)	(3)	(4)	(5)
	Tax revenue	Direct taxes	Indirect taxes	Goods & services	Trade taxes
Mean of dep. var	18.89	10.19	10.88	6.799	3.932
Predicted share patronage governors (P_s)	-0.824**	0.015	-1.042**	-0.065	-1.017**
	(0.371)	(0.268)	(0.396)	(0.160)	(0.422)
Estimation	Reduced form				
Basic controls	Y	Y	Y	Y	Y
Colonial controls	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y
Data source	Country-level ICTD: 1980-2015				
Observations	1,442	1,315	1,352	1,344	1,342

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Predicted share of patronage governors is the share of patronage governors the country/province was administered by between 1854-1966. In Panel A, the dependent variable is the tax/GDP ratio in 2015 or the latest year. In Panel B, the dependent variables are pooled for 1980-2015 and broken down by direct (e.g. income and property taxes) and indirect taxes (e.g. taxes on goods and services, trade taxes). The actual share of patronage governors between 1854-1966 is instrumented by the predicted share of patronage governors calculated based on the share of available governors connected to the Secretary of State the year before the appointment. Basic controls comprise continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania), the share of land area that lies in the tropics, landlockedness and the overall duration under British control. Colonial controls comprise the (log) initial governor salary and (log) initial revenue (in GBP). The weak IV robust 90% confidence set is computed as described in Chernozhukov and Hansen (2008). Panel A reports robust standard errors in parentheses. Panel B reports standard errors clustered at the colony-level.

Table 5: Exposure to patronage governors and modern-day fiscal capacity, pre/post 1930

	(1)	(2)	(3)	(4)	(5)
	Tax/GDP (%)			Tax rev.	Trade taxes
Mean of dep. var	19.37	19.37	19.37	18.89	3.932
Predicted share patronage gov's 1854-1929	-1.260***		-1.301***	-0.707*	-0.640*
	(0.406)		(0.399)	(0.401)	(0.332)
Predicted share patronage gov's 1930-1966		-0.403	-0.479	-0.147	0.066
		(0.359)	(0.344)	(0.226)	(0.277)
Estimation	Reduced form				
Basic controls	Y	Y	Y	Y	Y
Colonial controls	Y	Y	Y	Y	Y
Year FEs				Y	Y
Sample	Cross-section			Pooled	
Observations	46	46	46	1,442	1,342

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Predicted share of patronage governors 1854-1929 is the share of patronage governors the country/province was administered by between 1854-1929. Predicted share of patronage governors 1930-1966 is the share of patronage governors the country/province was administered by between 1930-1966. In Columns 1-3, the dependent variable is the tax/GDP ratio in 2015 or the latest year. In Columns 4-5, the dependent variables are pooled for 1980-2015. The dependent variable is the total revenue (direct+indirect taxes) as a share of GDP (Column 4) and the share of trade taxes over GDP (Column 5). Basic controls comprise continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania), the share of land area that lies in the tropics, landlockedness and the overall duration under British control. Colonial controls comprise the (log) initial governor salary and (log) initial revenue (in GBP). Columns 1-3 compute robust standard errors for the cross-section. For the panel (Columns 4-5), the standard errors are clustered at the colony-level.

Table 6: Patronage governors and channels: tariff rates and evasion

	(1)	(2)	(3)	(4)	(5)	(6)
	Trade taxes 1980-2015		Effective tariff	Trade taxes	Missing imports	Trade taxes
Mean of dep. var	3.932	3.049	7.381	3.049	19.64	3.049
Predicted share patronage governors	-1.017**	-0.920**	-0.160	-0.899**	0.315*	-0.303
	(0.422)	(0.449)	(0.302)	(0.427)	(0.166)	(0.218)
Effectively applied tariff				0.131**		
				(0.056)		
Missing imports						-1.984***
						(0.270)
Estimation			Reduced form			
Basic controls	Y	Y	Y	Y	Y	Y
Colonial controls	Y	Y	Y	Y	Y	Y
Observations	1,342	664	664	664	664	664

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Predicted share of patronage governors is the share of patronage governors the country/province was administered by between 1854-1966. In Columns 1-2 the dependent variable is the share of trade taxes. Column 1 is the full sample, while Column 2 is the constrained sample for which we have tariff data. In Column 3, the dependent variable is the effectively applied weighted average tariff (WITS) between 1993-2015. In Column 5, the dependent variable Missing imports is the (log) total absolute discrepancy between import values reported at the importing country and the export values reported at the exporting country for country-UK pairs for 1993-2015 (see Fisman and Wei 2004). Basic controls comprise continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania), the share of land area that lies in the tropics, landlockedness and the overall duration under British control. Colonial controls comprise the (log) initial governor salary and (log) initial revenue (in GBP). Standard errors clustered at the state-level.

Appendix - For online publication

Table B1: British colonies and territories in sample

Colony	Start	Indep.	Modern territory
Antigua	1816	1981	Part of Antigua & Barbuda
Bahamas	1718	1973	Bahamas
Barbados	1663	1931	Barbados
Basutoland	1884	1966	Lesotho
Bechuanaland	1891	1966	Botswana
British Columbia	1858	1931	Province of Canada
British Guiana	1831	1966	Guyana
British Honduras	1854	1981	Honduras
Cape of Good Hope	1803	1931	Part of South Africa
Ceylon	1802	1948	Sri Lanka
Cyprus	1878	1961	Cyprus
Dominica	1784	1978	Dominica
Fiji	1874	1970	Fiji
Gambia	1843	1965	Gambia
Gold Coast	1843	1957	Ghana
Grenada	1833	1974	Grenada
Hong Kong	1843	1997	Hong Kong (SAR, PR China)
Jamaica	1670	1962	Jamaica
Kenya	1920	1963	Kenya
Malta	1813	1964	Malta
Mauritius	1810	1968	Mauritius
New Brunswick	1784	1931	Province of Canada
New South Wales	1788	1931	State of Australia
New Zealand	1841	1931	New Zealand
Newfoundland	1824	1931	Province of Canada

Nigeria	1914	1960	Nigeria
Northern Rhodesia	1924	1964	Zambia
Nova Scotia	1784	1931	Province of Canada
Nyasaland	1891	1964	Malawi
Prince Edward Island	1769	1931	Province of Canada
Queensland	1859	1931	State of Australia
Seychelles	1814	1976	Seychelles
Sierra Leone	1808	1961	Sierra Leone
Solomon Islands	1893	1978	Solomon Islands
South Australia	1836	1931	State of Australia
Southern Rhodesia	1923	1980	Zimbabwe
St. Lucia	1816	1979	St. Lucia
St. Vincent	1802	1931	St. Vincent & Grenadines
Straits Settlements	1867	1957	Malaysia
Swaziland	1902	1968	Swaziland
Tanganyika	1920	1961	Tanzania
Tasmania	1855	1931	State of Australia
Trinidad & Tobago	1899	1962	Trinidad & Tobago
Uganda	1893	1962	Uganda
Victoria	1855	1931	State of Australia
Western Australia	1832	1931	State of Australia

Table B2: Predicting non-compliance with six year rotation rule - Appointment-level

	(1)	(2)	(3)
	Regular six-term appointment		
Mean of dep. var	0.248	0.248	0.248
Prob. connected governor (p_{t-1})	-0.058	-0.057	-0.064
	(0.109)	(0.110)	(0.109)
Prob. connected governor (p_{t-1}) × Log initial governor salary		0.066	
		(0.100)	
Prob. connected governor (p_{t-1}) × Log initial revenue size			0.036
			(0.064)
Linear trend	Y	Y	Y
Decade FEs	Y	Y	Y
Colony FEs	Y	Y	Y
Observations	947	947	947

Notes: First-stage for the appointment level. Dependent variable is a dummy that is 1 if the previous governor term ended regularly in the sixth year. Prob. of connected governor is the share of governors that are connected and beyond the six year term limit (and hence available for reshuffle) the year prior to the appointment, see equation (2). (Log) initial governor salary and (log) initial revenue size are the initial salaries paid to governors or the initial revenue size of the colony (in GBP). Standard errors in parentheses, clustered at the year-level.

Table B3: Robustness: GDP per capita and patronage governors

	(1)	(2)
	(log) GDP p.c. 1980-2015	
Mean of dep. var	9.124	9.124
Predicted share patronage governors	0.067	0.086
	(0.080)	(0.076)
Estimation	OLS - Reduced form	
Basic controls	Y	Y
Year FEs	Y	Y
Colonial controls		Y
Observations	1,452	1,452

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Predicted share of patronage governors is the predicted share of governors who were appointed while connected to the Secretary of State for the Colonies. The dependent variables (log) GDP per capita are from the Penn World Table 9.0, 1980-2015. Basic controls comprise continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania), the share of land area that lies in the tropics, landlockedness and the overall duration under British control. Colonial controls comprise the (log) initial governor salary and (log) initial revenue (in GBP). Standard errors are clustered at the colony-level.

Table B4: Robustness: Tax/GDP and patronage governors - Standard errors

	(1)	(2)
	Tax/GDP (%)	
Mean of dep. var	19.37	19.37
Predicted share patronage governors	-1.626***	-0.682***
- Robust SEs	(0.525)	(0.114)
- Cluster SEs		(0.416)
- Bootstrap SEs	(0.471)	(0.663)
- CGM Wildboot SEs [p-value]		[0.07]
- Twoway colony+year		(0.412)
Estimation	OLS - Reduced form	
Basic controls	Y	Y
Colonial controls	Y	Y
Geographic controls	Y	Y
Year FEs	-	Y
Sample	2015	1980-2015
Observations	46	1,308

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Predicted share of patronage governors is the predicted share of governors who were appointed while connected to the Secretary of State for the Colonies. The dependent variable is the tax/GDP ratio in 2015 (Col. 1) or for the period 1980-2015 (Col. 2). Basic controls comprise continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania), the share of land area that lies in the tropics, landlockedness and the overall duration under British control. Colonial controls comprise the (log) initial governor salary and (log) initial revenue (in GBP). Reporting (i) robust standard errors in parentheses (ii) standard errors clustered at the colony-level, (iii) computing bootstrap SEs, clustered at the colony-level. (iv) CGM Wild bootstrap SEs clustered at the colony-level are reported in p-values in brackets. (v) Two-way standard errors, clustered at the colony and year level.

Table B5: Robustness: Tax/GDP and patronage governors - controls

	(1)	(2)	(3)	(4)	(5)
	Modern tax/GDP ratio in 2015				
Mean of dep. var	19.37	19.37	19.37	19.37	19.37
Share patronage governors (C_s)	-0.314			-1.700*	-1.651*
	(0.229)			(0.958)	(0.934)
Pred. share patronage gov's (P_s)		-1.598***	-1.627***		
		(0.527)	(0.525)		
Log initial governor salary			-1.380		0.748
			(1.044)		(2.660)
Log initial revenue size			0.256		0.093
			(0.561)		(1.219)
Landlocked	-2.467	-2.520	-2.687	-3.588	-3.689
	(3.942)	(3.147)	(3.289)	(4.844)	(4.789)
Area in tropics	-20.153***	-19.635***	-19.957***	-22.465***	-22.332***
	(5.376)	(3.906)	(3.967)	(5.001)	(5.291)
Duration under British control	0.027*	0.031**	0.031	0.048	0.050
	(0.014)	(0.013)	(0.012)	(0.020)	(0.020)
Estimation	OLS	Reduced form		2SLS	
Continent fixed effecs	Y	Y	Y	Y	Y
Data source	ICTD + Provincial tax/GDP: latest 2015				
Observations	46	46	46	46	46

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Predicted share of patronage governors is the predicted share of governors who were appointed while connected to the Secretary of State for the Colonies. The dependent variable is the tax/GDP ratio 1980-2015. All the specifications include as basic controls continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania). Standard errors in parentheses, clustered at the colony-level.

Table B6: Robustness: Tax/GDP and patronage governors - controls (2)

	(1)	(2)	(3)	(4)
	Tax/GDP ratio 1980-2015			
Mean of dep. var	17.93	17.93	17.13	17.93
Predicted share patronage governors	-0.634*	-1.066**	-1.153**	-0.849**
	(0.340)	(0.439)	(0.513)	(0.406)
Ethnic fractionalization	-4.357			
	(3.747)			
Genetic distance to UK		-0.848**		
		(0.413)		
Settler mortality			-1.784**	
			(0.757)	
Nubmer of reported unrests				-0.102
				(0.467)
Estimation	OLS - Reduced form			
Basic controls	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
Colonial controls	Y	Y	Y	Y
Geographical controls	Y	Y	Y	Y
Observations	1,308	1,308	881	1,308

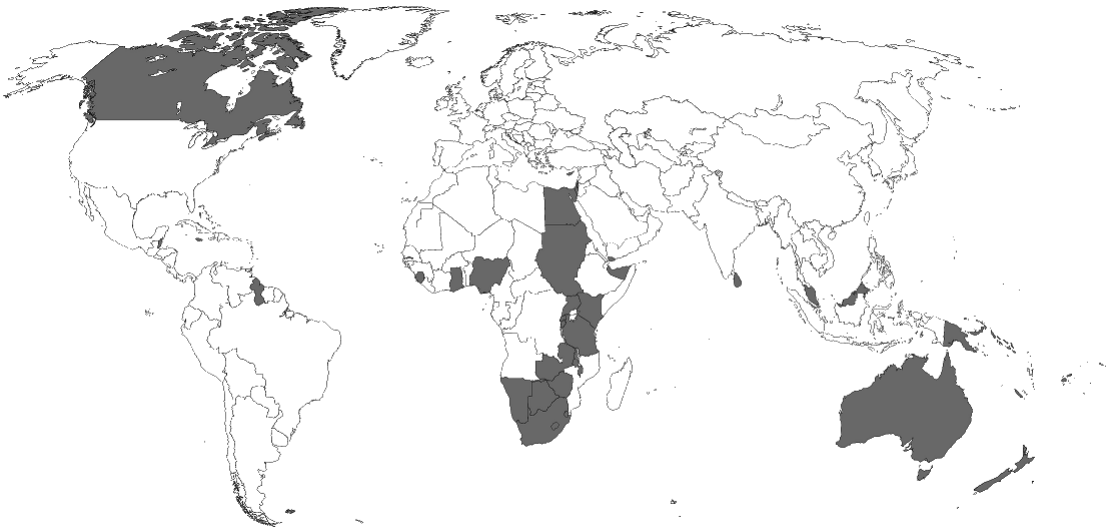
Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Predicted share of patronage governors is the predicted share of governors who were appointed while connected to the Secretary of State for the Colonies. The dependent variable is the tax/GDP ratio 1980-2015. All the specifications include as basic controls continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania) as well as the years of British colonization, the (log) initial governor salary of the historical colony, the (log) initial revenue size of the colony (in GBP), the share of the region/state within the tropis and a dummy for landlockedness as controls. Ethnic fractionalization is computed as Alesina et al. (2003). Genetic distance to the UK is computed as Spolaore and Wazciarg (2009). Settler mortality is from Acemoglu et al. (2001). No. of unrests is the reported number of unrests computed in Xu (2018). Standard errors in parentheses, clustered at the colony-level.

Table B7: Robustness: Tax/GDP and patronage governors - controls (3)

	(1)	(2)	(3)	(4)
	Tax revenue/GDP (%)			
Mean of dep. var	19.21	19.21	19.21	19.21
Predicted share patronage governors	-1.564***	-1.210**	-1.238**	-1.129**
	(0.433)	(0.542)	(0.500)	(0.500)
State antiquity index	-4.847***			
	(0.934)			
Share of fertile soil		-0.022		
		(0.040)		
Share of desert			0.227	
			(0.249)	
Terrain ruggedness index				0.010
				(0.033)
Estimation	Reduced form			
Basic controls	Y	Y	Y	Y
Colonial controls	Y	Y	Y	Y
Observations	1,158	1,158	1,158	1,158

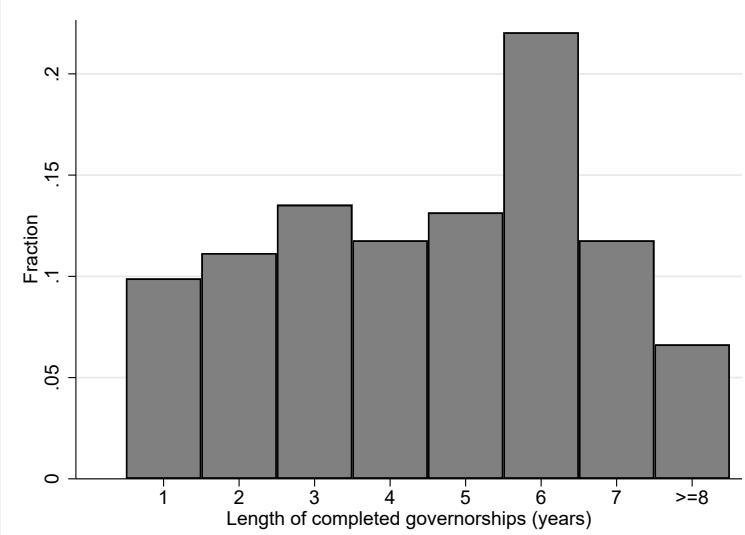
Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Predicted share of patronage governors is the share of patronage governors the country/province was administered by between 1854-1966. The dependent variable is the tax/GDP ratio in 2015 or the latest year. The predicted share of patronage governors calculated based on the share of available governors connected to the Secretary of State the year before the appointment. Additional controls shown are derived from Putterman (2012) and Nunn and Puga (2012): State antiquity index is an index measuring the antiquity of the state constructed by Louis Putterman ("Statehist") standardized to have a mean 0 and standard deviation 1. Share of fertile soil (%) is the share of fertile soil. Terrain ruggedness index is the measure derived from Riley et al. (1999). Basic controls comprise continent fixed effects (Africa, Europe, North America, Latin America, Asia, Australia and Oceania), the share of land area that lies in the tropics, landlockedness and the overall duration under British control. Colonial controls comprise the (log) initial governor salary and (log) initial revenue (in GBP). Robust standard errors in parentheses.

Figure A1: Territories administered by the Colonial Office - 1905



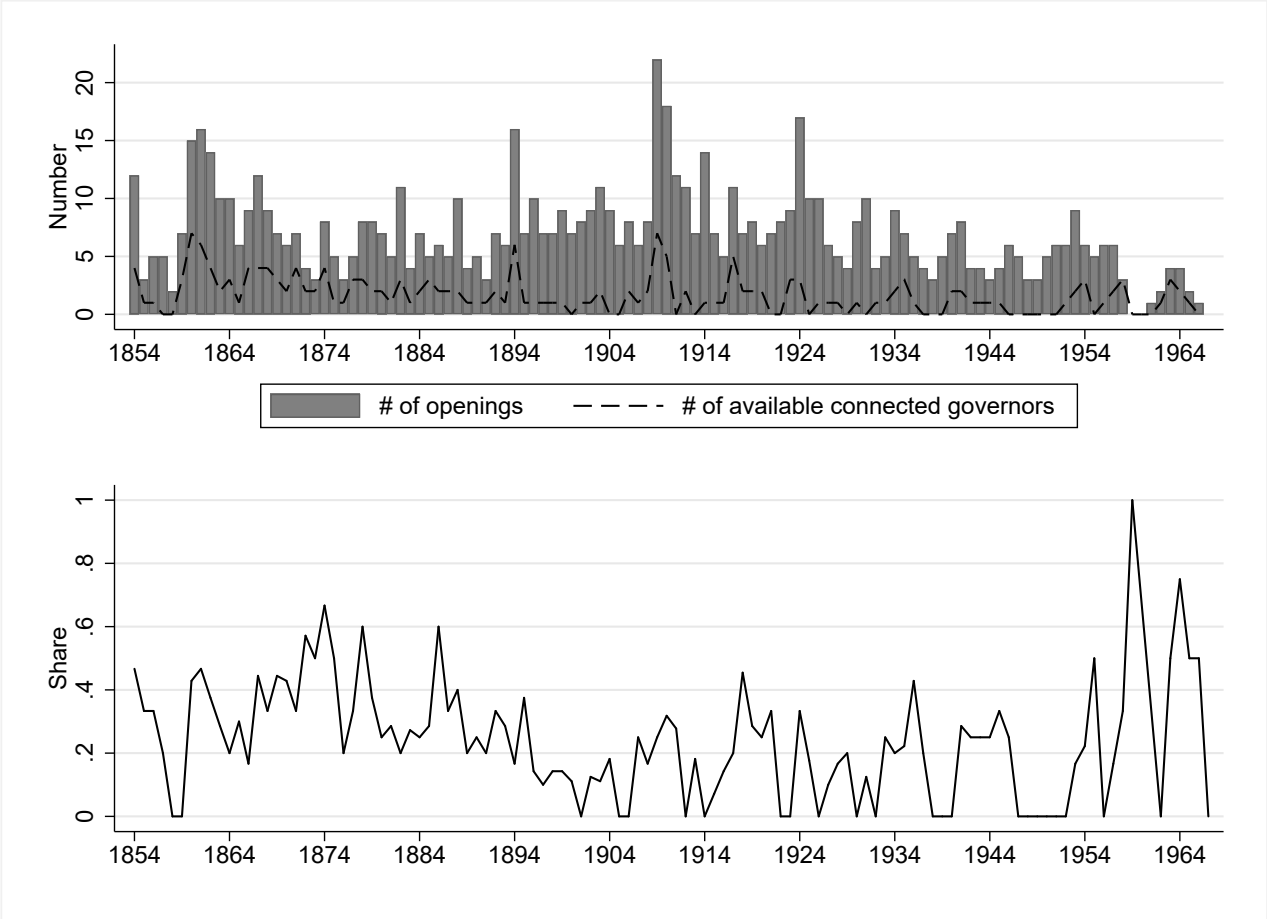
Notes: British territories administed by the Colonial Office in 1905.

Figure A2: Distribution of tenure length for completed governorships



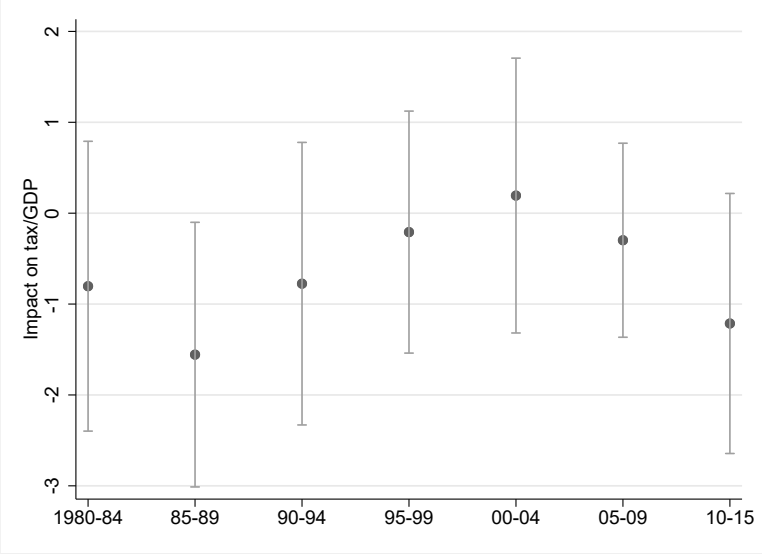
Notes: Distribution of tenure length for all completed governorships between 1854-1966. The statutory term limit is six years.

Figure A3: Share of available connected governors - breaking down sources of variation



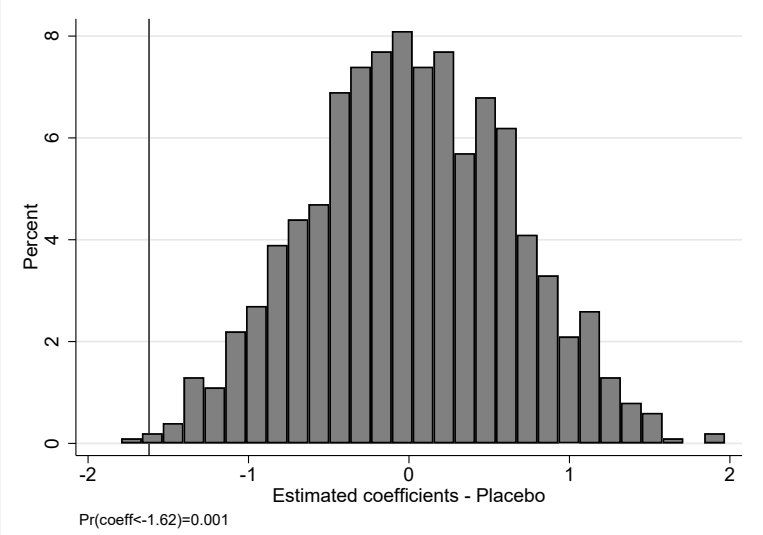
Notes: Showing the sources of variation for the instrument described in equation (2). Top figure shows the number of governorships that need to be filled (i.e. are beyond the statutory six year term limit, $\Sigma_i \mathbf{1}[T_{it} \geq 6]$) and the number of available connected governors (i.e. those who are connected and beyond the statutory six year term limit, $\Sigma_i \mathbf{1}[T_{it} \geq 6] \times c_{it}$). Bottom figure shows the share of available connected governors (i.e. the number of available connected governors divided by the total number of serving governors, $p_{it} = (\Sigma_i \mathbf{1}[T_{it} \geq 6] \times c_{it}) \,/\, (\Sigma_i \mathbf{1}[T_{it} \geq 6])$).

Figure A4: The impact of patronage appointments on fiscal capacity over time - Balanced sample



Notes: Reporting the impact of the predicted share of patronage appointments in the colonial period 1854-1966 on tax/GDP by 5-year bins for the subset of 25 territories with complete data between 1980-2015. Standard errors are clustered at the colony-level, showing 90% confidence intervals.

Figure A5: Randomization inference - Distribution of placebo treatment effects



Notes: Distribution of placebo treatment effects based on random allocation of patronage officers across the colonies. Solid line marks the actual estimated reduced form treatment effect.