

The Costs of Patronage: Evidence from the British Empire*

Guo Xu*

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Abstract

I study how patronage affects the promotion and performance of senior bureaucrats within a global organization: the British Empire. I combine newly digitized personnel and public finance data from the colonial administration 1854-1966 to study the inner workings of a bureaucracy that controlled close to a fifth of the earth's land mass at its peak. Exploiting the ministerial turnover in London as a source of within-governor variation in social connections, I find that governors are more likely to be promoted to higher salaried colonies when connected to their superior during the period of patronage. At the same time, they provide more tax exemptions, generate less revenue, invest less and are less likely to be recognized for their service. The promotion and performance gaps disappear after the abolition of patronage appointments. Exploiting a fixed allocation rule to predict the appointment of connected governors unrelated to colony characteristics, colonies administered for longer periods by connected governors during the period of patronage exhibit lower fiscal capacity today. Exposure to connected governors after the removal of patronage has no long-run impact.

JEL codes: O1, M51, D73

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*London School of Economics, Department of Economics and STICERD, Houghton Street, London WC2A 2AE, United Kingdom (e-mail: g.xu2@lse.ac.uk)

1 Introduction

State capacity is fundamental to development and growth. Bureaucrats are a key element of state capacity: they embody the human capital of the state and are critical for the delivery of public services and the implementation of policies. Understanding how to select and allocate bureaucrats is central for improving organizational performance.

Throughout history, patronage has been the dominant method for the appointment to public office (Grindle, 2012).¹ From chiefdoms to royal courts, patronage played a key role in the allocation of positions. Discretionary appointments of bureaucrats remain widespread even in developed countries today. In the U.S. alone, more than 8,000 senior federal positions are still allocated “at the pleasure of the President”.² Discretionary appointments are also pervasive outside the public sector. The appointment of CEOs or board members based on family ties and social networks, for example, is common practice (Bertrand, 2009).

In theory, the impact of patronage on organizational performance is ambiguous. Discretion over appointments can improve selection if principals hold private information over appointees or if loyalty limits agency problems. Patronage, however, can also be detrimental for organizational performance if principals exercise discretion to bias the allocation of positions in favor of socially connected subordinates (Aghion and Tirole, 1997; Prendergast and Topel, 1996). Despite the importance of patronage in shaping the allocation of bureaucrats, empirical evidence on how patronage affects performance has remained scarce.

This paper studies how patronage affected the allocation and performance of socially connected senior bureaucrats within a public organization that spanned the globe: the Colonial Office of the British Empire. At its peak, the Colonial Office administered close to a fifth of the world’s land mass through its colonial governors. These governors were leaders of the colonies and appointed at the discretion of their political minister, the Secretary of State for the Colonies. I digitized over 3,000 volumes of historical personnel and public finance reports to construct a unique individual-level dataset covering the universe of 456 colonial governors across 70 colonies from the birth of the Colonial Office in 1854 to its dissolution in 1966. This is the first time these historical sources have been assembled into a single dataset.

My setting provides two sources of variation to identify the impact of patronage. First, the turnover of Secretaries of State induced by the electoral cycle in London generated shocks in social connections among serving governors. These within-governor shocks enable me to examine how changes in connections affected the allocation and performance of the same governor, thus holding constant time-invariant unobserved characteristics. Second, the long study period captures variation in the extent of discretion the Secretary of State could exercise in allo-

¹“Patronage” refers to the discretionary appointment of individuals to governmental or political positions (Webster’s II New College Dictionary 1995). The term is derived from the Latin word “patronus”, the protector or advocate. Only in recent decades has there been a shift towards a negative connotation (Lyttle and Orgel, 1981).

²This count is derived from the list published after each Presidential election in the “United States Government Policy and Supporting Positions”, commonly known as the “Plum Book” (GAO-13-299R, March 1 2013).

cating governorships. In the early period (1854-1929), governors were exclusively appointed at the discretion of the Secretary of State. After 1930, the Warren Fisher Reform placed the appointment of governors under the oversight of an independent civil service appointment board. Hailed as the “Magna Carta of the Colonial Office”, this civil service reform limited the extent to which discretionary appointments could be made (Kirk-Greene, 2000; Banton, 2008). Combining both sources of variation allows me to study the impact of social connections on promotions and performance before and after the removal of patronage.

To measure social connections, I leverage genealogical and biographical data to construct predetermined proxies of connectedness between the governors and Secretaries of State that is defined by shared ancestry, membership of groups like the aristocracy or the attendance of the same elite school or university. To measure performance, I exploit the fact that governors were sufficiently important to control policies that could credibly affect measurable aggregate outcomes. As the “man on the spot”, governors wielded substantial executive and legislative power. Under the revenue imperative - whereby colonies had to “pay their way” by raising funds for public service provision - revenue generation was a central measure of performance and state capacity (Jeffries, 1938; Besley and Persson, 2009). Building on the literature on leaders and CEOs, the focus on colonial governors allows me to map top bureaucrats to aggregate economic outcomes (Bertrand and Schoar, 2003; Jones and Olken, 2005).

Guided by a model of career concerns and job allocation, my empirical analysis yields four sets of results. First, exploiting within-governor variation in connections to the Secretary of State induced by the ministerial turnover in London, I find that the same governor receives 10% higher salaries during the period of patronage. As wages are fixed across positions, this increase is driven by the promotion to higher salaried governorships. These governorships are also in larger colonies that lie closer to London, suggesting that the salary difference reflects the assignment to more desirable jobs. The preferential promotion of connected governors disappears after the removal of patronage in the Warren Fisher reform 1930.

Second, exploiting governor-colony variation in connections to the Secretary of State, the same governor generates 4% less annual revenue in the same position when connected during the period of patronage. This decline is driven by lower customs revenue and coincides with lower investments. I use hand-coded data on colonial tax laws to show that connected governors provide more trade tax exemptions. Text mining of newspapers and UK parliamentary debates provides evidence consistent with lower performance. Governors when connected are more likely to be associated with higher levels of reported social unrest, more likely to be mentioned with negative sentiments in the UK parliamentary debates and less likely to receive public awards. All performance differences disappear after the abolition of patronage.

Third, favoring connected governors induces the Secretary of State to screen less on ability and misallocate talent. The removal of patronage increases the match quality, as measured by a stronger positive association between colony and governor fixed effects. To provide causal

evidence for selection effects, I exploit the six year rotation rule to instrument for connected appointments and estimate the effect of connections *across* positions. Consistent with capturing a selection effect beyond the incentive effect, one additional year under a connected appointment decreases revenue growth over the appointment by 13% points. Again, this negative performance difference is attenuated after the abolition of patronage.

In the last part of the paper, I relate cross-sectional variation in the exposure of colonies to connected governors in the colonial period to differences in fiscal capacity today. I exploit the same six year rotation rule to instrument the cumulative number of connected appointments. I find that one additional year under a connected governor in the patronage period (1854-1929) decreases the tax to GDP ratio in 2010 by 0.7% points. Exposure to connected governors *after* the abolition of patronage, however, has no negative effect. Consistent with the interpretation that connected governors adversely affected fiscal capacity, the decline in tax/GDP is driven by a lower share of trade taxes and associated with a longer time needed to comply with tax and trade regulations, more complex modern trade tax structures and a higher degree of misreporting in the modern customs system. As colonies under connected governors received more trade tax exemptions, these long-run effects are suggestive of policy persistence.

The study of the organization of the state is rapidly expanding as state capacity is increasingly seen as a key driver of economic performance (Besley and Persson, 2009; Finan et al., 2015). My paper contributes to this growing literature by studying a global bureaucracy - the British Empire - and particularly how the method of appointment of their leaders can affect colony-level performance. Combining the unique dataset with theory allows me to study how patronage and performance are linked. My paper differs from the existing literature as I focus on civil service leaders that have bearing on macroeconomic fiscal outcomes (Ashraf et al., 2014; Dal Bo et al., 2013; Deserranno, 2016).³ In contrast to Jia et al. (2015) and Iyer and Mani (2012), the abolition of patronage also enables me to study the impact of social connections under two different allocation regimes.⁴ Finally, my long-run results contribute to further un-bundling institutions by tracing out a specific institutional channel through which patronage shaped contemporary state capacity (Acemoglu et al., 2001, 2005; Acemoglu and Robinson, 2006; Besley and Persson, 2010; Rauch and Evans, 2000). The identification of long-run effects hinges critically on the organizational features of the Colonial Office before and after the removal of patronage, thus tightly linking the personnel-level with aggregate long-run outcomes. Taken together, my results underpin a long tradition of intellectual thought that views the transition away from a patronage-based system of administration to a rule-based civil service as the emergence of the modern state (Northcote and Trevelyan, 1854; Weber, 1922).⁵

³More broadly, I also add to the growing literature on the incentives and selection of public servants (Khan et al., 2015; Vanden-Eynde et al., 2016; Persson and Zhuravskaya, 2016; Rasul and Rogger, 2016; Dal Bo et al., 2016).

⁴In contrast to the role of connections in firms (Fisman, 2001; Bandiera et al., 2009, 2010; Kramarz and Thesmar, 2013; Burks et al., 2015), less is known about connections in public organizations. These organizations, characterized by low exit rates and the absence of performance pay, differ from firms in substantive ways (Dewatripont et al., 1999).

⁵The transition from discretionary appointments to a rule-based system of appointment is seen as the birth of

The remainder of this paper is organized as follows. Section 2 introduces the historical background, the data and the measurement of social connections. Section 3 discusses a conceptual framework that guides the empirical analysis. The results are presented in four blocks. Section 4 presents evidence on how social connections affect the allocation of governors. Section 5 and 6 present evidence on the performance of governors within and across appointments. Section 7 discusses the long-run estimates. Section 8 concludes.

2 Empirical context and data

2.1 Background and natural experiment

The organization under study is the British Colonial Office. The Colonial Office was founded 1854⁶ and tasked with administering overseas possessions.⁷ At the peak of British colonialism, this bureaucracy spanned the globe, covering nearly a fifth of the world's land mass (Figure 1). Studying how the appointment of colonial leaders shaped the performance of this global bureaucracy is central to understanding modern differences in economic performance.

Two institutional settings of the Colonial Office provide variation that enable me to study the impact of patronage on the allocation and performance of socially connected bureaucrats. The first source of variation is the ministerial turnover. The Colonial Office was headed by the Secretary of State for the Colonies. A political position, the Secretary of State was appointed by the monarch on advice of the Prime Minister. Changes in the Secretary of State are driven by two margins: Cabinet reshuffles at the discretion of the Prime Minister, and changes of Prime Ministers through elections. The average duration of a Secretary of State appointment between 1854-1966 is around 3 years. The temporal changes in Secretaries of State with varying connectedness to the serving governors is the first source of variation I exploit.

The second source of variation is the change in the appointment regime. The Secretary of State enjoyed discretion over the appointment of governors who were tasked with administering their assigned colonies between 1854-1930:⁸ throughout the paper, I refer to this period of discretionary appointment as *patronage*. 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 appointments were progres-

the modern state: from the introduction of the Chinese imperial examinations in the 1st Century to the British Northcote-Trevelyan reform and the US Pendleton Act in the 19th, the removal has been a cornerstone of many civil service reforms (World Bank, 1997, 2008).

⁶From 1800-1853, the Colonial Office was merged with the War and Colonial Department. In 1907, the Dominions Division was created to oversee the relations with the self-governing territories of Canada, Australia, New Zealand, South Africa, Newfoundland and the Irish Free State. In 1966, the Colonial Office merged with the Foreign Office.

⁷British possessions in modern day India, Bangladesh, Burma and Pakistan were administered from the India Office. My analysis excludes these colonies as they are not under the control of the Secretary of State for the Colonies.

⁸The title of the administrator of a colony is typically the governor, or governor, commander-in-chief, captain-general, governor-in-chief or governor-general. For expositional simplicity, I refer to all those as *governor*.

sively eliminated from the domestic Crown Civil Services and *de jure* replaced by competitive examinations following the seminal Northcote-Trevelyan Report of 1854, the “principle of patronage steadfastly continued until 1930” for senior appointments (Kirk-Greene, 2000). Only after 1930 were patronage appointments of governors replaced by a formal system of open recruitment. Named after the civil servant Warren Fisher, the abolition of patronage appointments for governors has been hailed as the “Magna Carta of the Colonial Service”.

In this principal agent setting, the relationship between a colony and London was centered around the Secretary of State and the subordinate governor. As an appointed representative of the Crown for a fixed period of 6 years, a governor 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”.⁹ 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.¹⁰ 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 cost rendered “any attempt to conduct the details of the administration from this country [UK] [...] absolutely impracticable.” The autonomy of the governor created widely different policies and practices across the dependencies.¹¹ The delegation of power from London to the colonies enabled governors to develop “real” authority.

This tension between devolving real authority to the governor to promote initiative and the loss of control for the Secretary of State reflects the classic delegation problem (Aghion and Tirole, 1997). Not only did governors balance the demands of the local elites against the directives from the Secretary of State; governors would often also seek to maximize their own rents from the public office (Gardner, 2012). As Banton (2008) summarizes, “in distant Crown Colonies the Home Government can only supervise - they cannot judge except on the governor’s local information. Their original act is sending a good governor, and their check is dismissing him”. With the appointment and dismissal subject to the discretion of the Secretary of State, however, patronage is likely to have a large impact on the selection and incentives of the governors.

⁹The main duties were (Regulations of 1862): (i) Control over public finance (III.16), (ii) Legislate (I.23) (iii) Confer civil service appointments in colonies (III.20) (iv) Security (III. 26), (v) Grant pardon (III.5) and approve marriages (I.18). Overall, the aim was to “direct [...] attention to [...] the Aboriginal advancement in civilisation” (III. 25).

¹⁰The governor was also not responsible for the defense of the colonies as this was an Imperial responsibility.

¹¹ “[The Secretary of State] necessarily relies mainly upon the governor to lay before him all the necessary information and considerations [...] To overrule the considered and maintained advice of a governor is a thing which no Secretary of State would do lightly.” (Jeffries, 1938).

2.2 Data sources and digitization

I undertook a large-scale data digitization exercise to construct an individual-level personnel dataset of the Colonial Office. My analysis combines newly digitized data from four sources: the Colonial Office List 1860-1966, the Colonial Blue Books 1821-1949, biographical data from DeBrett's and the UK Who-is-Who, and genealogical data from the online database The Peerage. The sample period is 1854-1966, tracing the entire period of the Colonial Office from its establishment to dissolution. The Appendix Section B provides a detailed documentation.

Colonial Lists. The first source of data on the postings, backgrounds and salaries of governors is derived from the Colonial Office Lists. These files have been systematically compiled by the Colonial Office to document changes in the administrative structure and personnel of each colony under the British Empire from 1860-1966. I digitized the entire set of Colonial Office Lists. This allows me 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, I derive the same information from the Blue Books (see below).

Blue Books. The main source of colonial statistics is drawn from the Colonial Blue Books 1821-1949. The Blue Books were annually compiled administrative statistics providing detailed information about public finance (revenue and expenditures), demographics (population size, births and deaths), trade and socio-economic statistics such as education (e.g. number of schools) and prices. The key advantage of the Blue Books is the comparability across colonies and time. Statistics from the Blue Books were collected through standardized forms, which governors were required to submit on an annual basis (See Figure A1). I conducted extensive archival work and digitized the *full set* of 3,905 volumes from holdings at the UK National Archives, the Commonwealth Library and the library of the Royal Commonwealth Society to construct comparable economic series across colonies and time. For the later periods, I use colony-specific statistical yearbooks to extend the series up to the dissolution of the colonies around 1966. The final dataset contains 70 colonies (See Appendix Table B1 for list).

Genealogical data. I obtained biographical information about the Secretaries of State and governors from the DeBrett's database and the UK Who-is-Who. For governors that were not listed in these data sources, information was drawn from the Colonial Lists and secondary sources. Finally, I draw upon genealogical data to create a comprehensive family network of the British elite. I use family tree data from The Peerage (www.thepeerage.com). The data provides a genealogical survey of the peerage of Britain as well as the royal families of Europe, including the family trees of the British elite. This enables me to create a measure of connectedness between the Secretary of State and his subordinate governors. The construction of the measure of connectedness is described in Section 2.3.

[Table 1 here]

Table 1 reports descriptive statistics for a wide set of governor and colony-level characteristics. About 9% of the governors are aristocrats and members of the peerage (Panel A).¹² The vast majority of governors (84%) have served as civil servants before their first governorship. 44% of governors pursued a military career before first serving as a governor. 9% of governors have held political positions prior to joining the Colonial Office. 18% (15%) of the governors have graduated from Oxford (Cambridge). Governors are senior: the average age at entry is 49 years. In terms of colony-level characteristics (Panel B), average public revenue and expenditure have been increasing over time. Trade taxes comprise nearly half of all revenue across the entire sample period. Governor salaries have been increasing over time. There is substantial cross-sectional variation in salaries. 76% of this variation is explained by differences in colony size, as measured by total revenue and population (Appendix Table B6).

2.3 Measuring connectedness

A valid measure of social ties between the Secretary of State and the governors is central to this study. This measure must meet two criteria. First, the measure must capture objective ties. This is a challenge as social connections are difficult to directly observe. Second, the measure social ties must address the issue of endogenous network formation (Manski, 1993). If high performing governors are more likely to be both promoted and establish social ties with their superior, the resulting estimates would mistakenly attribute differential ability to the effect of connectedness in explaining promotion patterns. To meet both criteria, I combine several pre-determined measures to proxy for unobserved social ties: shared ancestry, membership in the aristocracy, and having attended the same elite school and university. These are group traits that historians have shown to be important predictors of patronage networks in the 19th century British colonial service (Kirk-Greene, 2000; Laidlaw, 2005).¹³

Shared ancestry. I use exogenous family networks to proxy for unobserved social ties. By measuring connectedness through relatedness by blood,¹⁴ I derive a network measure that is both predetermined and objectively measurable using family trees. The use of family networks as a measure of connectedness is particularly suitable in my context. As a large share of Secretaries of State and governors originate from the British elite, their ancestry is well documented in existing genealogical datasets. Furthermore, the role of family ties in securing jobs has been well documented in the literature (Laidlaw, 2005).

The main source of genealogical data comes from the online database The Peerage, which maps the ancestry of over a million individuals across Europe's elite. I first extract the data to

¹²Peerage is defined as encompassing the hereditary titles of Duke, Marquess, Earl, Viscount and Baron.

¹³Networks were consciously employed, "overtly as 'connections' or more obliquely through the recognition of shared politics, professional camaraderie, or the obligations of friendship and family." (Laidlaw (2005), p. 14)

¹⁴I exclude relatedness through marriage that occurred after entry into the colonial service.

create a large dataset of dyadic relationships. I then restrict the relationships to blood-relations and then identify the 456 governors and 37 Secretaries of State by matching them against their full name and date of birth. 94% of the Secretaries of State and 34% of the governors are reliably matched in the genealogical data. Missing individuals are not connected.¹⁵ Since I am exploiting within-governor variation, this assumption does not introduce selectivity issues. For the remaining individuals, I apply Dijkstra's shortest path algorithm¹⁶ to calculate the degrees of separation between any governor and his superior Secretary of State. I define a Secretary of State and governor to have shared ancestors if the governor and Secretary of State are connected in the family tree and if the degree of separation is sufficiently close. To obtain sufficient variation in shared ancestry, I use the cut-off of 16 degrees of separation, which corresponds to 25% of the governors sharing ancestry with their superior in the sample.¹⁷ The cut-off was chosen to maximize the switcher sample (Appendix Figure A4). The results, however, do not critically hinge on this choice of the cut-off. The data appendix contains a detailed documentation of the construction and validation procedure.

Membership in aristocracy and common schooling. I complement the measure of shared ancestry with three additional measures of connectedness. First, I define "both aristocrats" to be a dummy that is 1 if both the governor and Secretary of State are members of the British peerage, holding hereditary aristocrat titles (e.g. Baron, Duke). Second, I construct a dummy "Both Eton" that is 1 if the governor and Secretary of State both attended Eton, an elite school nearly half of the Secretaries of State attended. Finally, I construct a dummy "Both Oxbridge" that is 1 if both the governor and Secretary of State either attended Oxford or Cambridge.¹⁸

These proxies of social ties do not go without objections. In terms of shared ancestry, being connected per se, especially if with a large degree of separation, need not always imply the presence of social ties. Indeed, neither the intensity nor the direction of the actual social tie between two relatives is observed. Similarly, belonging to the aristocracy does not imply that two individuals have necessarily established social ties. All these measure of connectedness are, in effect, instruments for social ties that are not directly observed. For the purpose of the identification strategy and the interpretation of my reduced form estimates, I only require that two connected individuals are more likely to share social ties with each other than two

¹⁵The two Secretaries of State that could not be matched (George Hall, Arthur Jones) are Labour party politicians who tend to come from non elite backgrounds. The missing governors are also less likely to be aristocrats. Since the family trees of Secretaries of State are fully mapped out, I assume that governors who are not within the family trees of these secretaries are unconnected. This can also be interpreted as having an infinite degree of separation.

¹⁶The computation is implemented using Matlab's *graphshortestpath* package. For details refer to Dijkstra (1959).

¹⁷To put this in perspective, I drew 1,000 random pairs from the full Peerage dataset and find that only 10% of those are closer than 16 degrees of separation. As the database already covers a highly elite group of individuals, the actual distance between two randomly drawn individuals in the broader UK population is likely to be even larger.

¹⁸Oxford and Cambridge are pooled as there are the number of switchers for "both Oxford" is too small. The remaining schools and universities are too dispersed to allow for accurate coding. There is not enough statistical power to break down Oxford and Cambridge attendance by specific colleges as membership is almost entirely concentrated in Christ Church (Oxford) and Trinity (Cambridge).

unconnected individuals. Although the actual social ties are never observed, all four measures of connectedness are, consistent with the assumption, highly positively correlated (Appendix Table B5). In my later analysis, I combine all measures into a single measure of connectedness.

3 Conceptual framework

I introduce a model of job allocation and career concerns for two purposes. First, to make precise the distinction between patronage and social connections. Second, to guide the interpretation of the empirical analysis by delineating two margins through which patronage impacts performance: (i) by affecting the allocation of governors and (ii) their effort on the job. I derive three predictions that guide the empirical analysis in Sections 4, 5 and 6.

3.1 Technology, preferences and incentives

For parsimony and without loss of generality, I consider the setting with two colonies and two governors. One colony is large ($s = 1$), and the other colony is small ($s = 0$). One governor is connected ($c = 1$) and the other is unconnected ($c = 0$). The Crown's objective is to maximize colonial revenue less wage bill across both colonies. The objective function is $W = y_{c1} + y_{c0} - w_1 - w_0$, where y_{cs} is the revenue generated by the governor with connectedness c in the colony with size $s = \{1, 0\}$. The salary of the governor in colony s is denoted $w_s > 0$. As is common in bureaucracies, salaries w_s are fixed across positions. Matching the empirical setting, salaries are increasing in the colony's size so that $w_1 > w_0$. The revenue of governor c in colony s is given by $y_{cs} = \kappa\theta_s m_s + e_c$, where e_c denotes the privately observed effort, θ_s the observed ability of the governor, and m_s the size of the colony.¹⁹ I assume that the higher ability governor generates higher revenues in the large colony, so that $m_1 > m_0 > 0$. This is due to their greater span of control (Lucas, 1978). The parameter $\kappa > 0$ measures the strength of the complementarity between ability and colony size.

The Secretary of State's utility depends on a fixed salary f , the welfare of the organization W and the salary of the subordinate governors $w_{S(c)}$,

$$U^S = f + gW + \sigma_1 w_{S(1)} + \sigma_0 w_{S(0)} \quad (1)$$

where $s = S(c)$ returns the colony size of the governor with connectedness $c = \{0, 1\}$.

Patronage enters as the parameter $g \geq 0$. This captures the quality of governance and determines the Secretary of State's degree of alignment with the organization's objective. Under patronage ($g = 0$), the Secretary of State has full discretion in the allocation of governors. A civil service reform that limits the extent of patronage, then, constitutes an upward shift in

¹⁹Allowing the return to effort to vary by colony size does not affect results (See Appendix Section A.1). I assume constant returns to effort to simplify the exposition.

the parameter g . Clearly, this specification abstracts from the potential upside of patronage as $g > 0$ is always weakly decreasing the organizational welfare. This however comes at no empirical loss as my setting uncovers large costs of patronage.²⁰

Social connections enter as the Secretary of State's private preference. Similar to Prendergast and Topel (1996) and Bandiera et al. (2009), the Secretary of State's utility depends on the salary of his subordinate governors. The parameter σ_c captures the Secretary of State's preference for the governor with connectedness c . The Secretary of State has a greater preference for the connected governor so that $\sigma_1 > 0$ and $\sigma_0 = 0$.

In the absence of performance pay, career incentives induce the governor to exert effort. The total benefit of governor c when allocated to colony s is,

$$U^G(e_c) = w_s + \beta V(y_s(e_c), c, g) - \frac{1}{2\theta_c} e_c^2 \quad (2)$$

In addition to a fixed salary w_s , the governor also enjoys a promotion prospect with utility of $V(y_s(e_c), c, g)$. This continuation value depends on the observed revenue performance, social connections and patronage. It can be interpreted as a future promotion, or the progression into a more distinguished job outside the organization. Specifically, the promotion prospect is linearly increasing in the observed revenue performance and connectedness to the superior, so that $V_y(g) > 0$, $V_{yy}(g) = 0$ and $V(y, 1, g) > V(y, 0, g)$. Those with better performance and enjoying social connections have greater chances of securing a profitable job in the future. These returns, however, will depend on the prevailing institutional environment, as captured in the patronage parameter g . Finally, $\beta \in [0, 1]$ is the discount rate and governor c faces a disutility of effort of $\frac{1}{2\theta_c} e_c^2$, which is decreasing with governor ability.

3.2 Effort choice and allocation decision

The governor chooses effort levels $e_c \geq 0$ to maximize utility as shown in equation (2). The first order condition yields governor c 's optimal effort,

$$e_c^* = \theta_c \beta V_y(c, g) \quad (3)$$

Governor effort is increasing in ability θ_c and in the degree to which higher revenue performance translates into better promotion prospects $V_y(c, g)$. Taking ability and effort choices of the governors as given, the Secretary of State chooses the optimal allocation to maximize his own utility. The Secretary of State will allocate the connected governor to the large colony if,

$$g \left(y_{11}(e_1^*) + y_{00}(e_0^*) \right) + \sigma_1 w_1 > g \left(y_{01}(e_0^*) + y_{10}(e_1^*) \right) + \sigma_1 w_0 \quad (4)$$

²⁰Drugov (2015) introduces a trade-off between the disincentivizing effect of patronage and the higher return to effort associated with winning the patronage position.

3.3 Results: Allocation, performance and misallocation

Result 1: Allocation. *In the presence of patronage, the connected governor is more likely to be allocated to the large and higher salaried colony than the unconnected governor.*

The connected governor is allocated to the large colony if,

$$\frac{\sigma_1}{g} \geq -\left(\theta_1 - \theta_0\right) \frac{m_1 - m_0}{w_1 - w_0} \kappa \quad (5)$$

The Secretary of State trades off the private gain from assigning the connected governor to the large and higher salaried colony against the potential loss in revenue associated with misallocating the governor. As patronage increases ($g \rightarrow 0$), the private gain more likely outweighs the potential revenue loss. This implies that connected governors are screened less on ability. There is no trade-off when the connected governor is more able than the unconnected governor. As patronage is curtailed ($g \rightarrow \infty$), the assignment is increasingly merit-based.

Equation (5) bears two empirical implications. First, to disentangle differential ability $\theta_1 - \theta_0$ from the extent to which favoritism is feasible, I estimate the promotion gap between the connected and unconnected governor of same ability using the within-governor identification in Section 4. Second, the extent to which the connected governor with same ability is allocated to the large colony depends on the interaction between social connections and patronage (σ_1/g). This motivates the double-differences strategy where I study the impact of patronage on socially connected governors before and after the removal of patronage.

Result 2: Performance. *Under patronage, connected governors exert more (less) effort if social connections and performance are complements (substitutes) for promotions.*

Comparing revenue performance within governor and colony identifies the effort effect,

$$\text{Incentive effect} = \Delta y^*|_{\theta,s} = \Delta e^*|_{\theta,s} = \theta\beta \left(V_y(1, g) - V_y(0, g) \right) \quad (6)$$

Under the prevailing extent of patronage g , the impact of social connections on revenue performance is ambiguous. If connections and performance are complements in the governor's promotion prospect $V_y(1, g) > V_y(0, g)$. The connected governor then exerts more effort than the unconnected governor. The perhaps most prominent example for this is the case of loyalty, where the connected governor's promotion prospect also depends on how well the Secretary of State performs.²¹ The connected governor, however, exerts less effort than the unconnected governor if connections and performance are substitutes. In the extreme case, the connected governor's promotion prospect does not depend on performance so that $V_y(1, g) = 0$, whereas the unconnected governor needs to exert effort to gain promotions $V_y(0, g) > 0$.

²¹The promotion prospect, for example, could depend on the governor's own revenue performance and the Secretary of State's utility, $V(y_{cs}, c, g) = \alpha y_{cs} + \sigma_c U^S$.

The difference in revenue generation while holding constant ability and colony identifies the incentive effect. I estimate the performance gap using the within-position identification in Section 5. Furthermore, estimating equation (6) before and after the removal of patronage in a double-differences also provides a test for whether social connections and performance are substitutes or complements for promotions under two different allocation regimes.

Result 3: Misallocation. *The removal of patronage improves the match quality between governor and colony. The impact of patronage extends beyond the incentive effect by inducing misallocation.*

The observed revenue difference between a connected and unconnected governor holding the colony constant is,

$$\text{Incentive + Selection effect} = \Delta y^*|_s = \kappa m_s(\theta_1 - \theta_0) + \beta \left(\theta_1 V_y(1, g) - \theta_0 V_y(0, g) \right) \quad (7)$$

This comparison captures both selection and incentive effects. The first term is the selection effect. In presence of complementarities between governor ability and colony size $\kappa > 0$, governor and colonies are assortatively matched to maximize revenue. Patronage lowers the connected governor's threshold for the allocation to the large colony (5). This increases the range in which the ability of the connected governor is lower, increasing the likelihood of misallocation. The second term is the incentive effect, which is similar to (6) but now allows ability to vary across governors since higher ability governors exert more effort. The sign of the incentive effect will now depend on the difference in governor ability and the complementarity or substitutability between social connections and performance (Result 2).

I estimate the combined incentive and selection effect in Section 6 by exploiting a six year rotation rule to predict connected appointments holding constant fixed colony-level characteristics. This allows me to estimate the effect of appointing a connected governor across positions. Again, interacting connected appointments with the removal of patronage in a double-difference allows me to test whether the combined effect varies by the degree of patronage.

4 Salaries, Promotions and Connectedness

Under patronage, connected governors are more likely to be allocated to higher salaried governorships (Result 1). To test this prediction, I first estimate the reduced form effect of social connections on the salary and allocation of serving governors. I then combine the shocks in connections with the removal of patronage. The resulting double-differences then identifies the extent to which patronage affected the pay and allocation of connected governors.

4.1 Salary premium of social connections

I first estimate the reduced form effect of social connections on governor remuneration. For governor i in colony s at time t , I estimate following specification:

$$\log w_{ist} = \beta \times c_{it} + \theta_i + x'_{it}\gamma + \tau_t + \varepsilon_{ist} \quad (8)$$

where w_{ist} is the governor's salary and the dummy $c_{it} = \{0, 1\}$ denotes the connectedness to the Secretary of State in office. The connectedness between the governor and his superior is measured by the shared ancestry, the membership in the British aristocracy, or having attended the same elite secondary school (Eton) or university (Oxford/Cambridge).

The turnover of Secretaries of State in London generates variation in social connections to serving governors. To exploit this source of variation, I introduce governor fixed effects θ_i . These absorb all unobserved governor-specific heterogeneity that are correlated with connectedness, for example that higher ability governors receive higher salaries and are more likely to be connected. The identification is therefore driven by governors who change their connections during their career. Table B2 provides balancing statistics for these "switchers".²²

Around 21% of the 456 governors experience a change in connections over their career, corresponding to 28% of the full sample in the governor-year panel. Governors are as likely to be connected early on in their careers and appointments as later. There is also no statistically discernible difference in the likelihood of transfer to another governorship and retirement from the Colonial Office, though the combined measure suggests that governors are, if anything, less likely to either transfer or retire when connected. Throughout the subsequent analysis, I include the remaining governors to remove noise and to obtain more precise estimates.

With the governor fixed effects holding constant time-invariant confounders, the remaining identification threat is that "within-governor" shocks in connections are correlated with other time-varying governor-specific characteristics. As Table B2 shows, however, this variation is uncorrelated with time-varying individual-specific observables. While concerns over unobserved time-varying governor-specific characteristics may still remain, introspection does not suggest obvious candidates. The reason is that the measure of connectedness is pre-determined and driven by the temporal turnover of Secretaries of State which, in turn, generates cross-sectional variation in connectedness to *all* serving governors. So although the unobserved lobbying activities of an exceptionally powerful governor may, for example, induce the appointment of a connected Secretary paying higher salaries, the entry of the new Secretary will generate shocks to connections to all other serving governors. This implies that lobbying as an omitted variable will only pose a threat if all governors who became connected at a given time engaged in lobbying. This case, however, is captured by the inclusion of year

²²In terms of descriptive statistics, the "switchers" are between those who are always connected and never connected (Appendix Table B7).

fixed effects τ_t that absorbs unobserved temporal shocks common to all serving governors. The ministerial turnover occurs through elections unrelated to colony outcomes.²³

Nonetheless, I include x_{it} as a vector of time-varying characteristics: these comprise the total number of colonies served and a full set of dummies for each year of tenure in the current governorship. Finally ε_{ist} is the error, which is clustered at the governor-secretary level, corresponding to the level of the identifying source of variation.²⁴

[Table 2 here]

The results are presented in Table 2 and suggest that connected governors receive substantially higher salaries. Column 1 to 4 include each separate measure of connectedness, showing that the same governor, at times connected to the Secretary of State, receives higher salaries based on all four measures. In terms of the point estimate, the salary premium is largest when both are members of the British aristocracy and comparable for the shared ancestry and having attended the same elite school and university. These four measures of connectedness are positively correlated, suggesting that connected individuals are more likely to share similar biographies and socio-economic backgrounds.²⁵ when including all four measures of social connectedness (Column 5), the point estimates are smaller and noisier. Given the noisiness of the estimates, however, I cannot statistically reject the equality of all point estimates. To increase the power, Column 6 combines all measures into a single measure of connectedness that is 1 if the governor and Secretary of State are connected based on at least one of the four dimensions.²⁶ The combined estimate shows a salary premium of 9.8%.²⁷

While the within-governor analysis alleviates concerns over unobserved fixed governor-specific confounders, these estimates are invariably conditional on governors not exiting from the Colonial Office. Since the main focus of this paper is to understand how social connections shape the allocation of jobs *within* the organization, exit in this context implies a salary of zero. Given the seniority of the governors (the median age at exit is 58), almost all governors retire after their last governorship. The estimate of the premium I obtain from only comparing the salaries of those who did not exit the organization will hence constitute a lower bound.²⁸

²³The only predictor of turnover are elections (Appendix Table B4). The results are robust to using only variation in connections induced by elections (Appendix Table C2, Column 2).

²⁴The results are robust to alternative clustering strategies, such as two-way clustering on the governor and Secretary of State level or clustering on the dyadic governor-secretary level *and* the year level as multiple governors are connected to the same secretary at any point in time (Cameron and Miller, 2014). See Appendix Table C6.

²⁵See Appendix Table B5 for the correlation matrix for all measures.

²⁶The results are robust when dropping one of the four dimensions in turn (Appendix Table C1).

²⁷An alternative interpretation of the results is that the shock in connections does not only reflect changes in the dyadic connection to the direct superior but to the entire cabinet. To provide evidence against this interpretation, Appendix Table C2, Column 1 runs a horse-race between the connectedness to the Secretary of State for the Colonies and the Prime Minister. The results show that the salary premium is only driven by the connectedness to the direct superior. The premium for connections does not vary by the party in office (Column 4).

²⁸See Appendix Figure A5 for the survival curve for remaining in the colony. Note that there is also no association between the overall length of service and the share of connectedness in the switcher sample.

The large increase in salaries for connected governors is striking as salaries within bureaucracies are typically fixed across positions. Table 3 sheds light on the drivers of the observed salary increase by exploring two channels: increasing the salary for connected governors in the same colony or by transferring connected governors to higher paid colonies.

[Table 3 here]

Column 1 reports the salary premium based on the combined measure of connections (the same as in Table 2, Column 6). To first test whether the observed increase by 9.8% is driven by increasing the salary for the same position, I repeat the exercise by holding constant the position using colony fixed effects (Column 2). The result suggests that the increase is not driven by the intensive margin, and the salary premium for connections within the same colony is near zero. Consistent with the rigidity of the salary structure within bureaucracies, the finding suggests that the salary increase is driven by transferring connected governors to higher paid governorships. As larger colonies pay more (See Appendix Table B6), this implies the disproportionate promotion of connected governors to larger colonies.

I provide evidence for this in Columns 3 to 5, where the dependent variables are time-invariant colony characteristics. The results suggest that connected governors are indeed more likely to be promoted to larger colonies (Column 3). In line with a career based civil service, both the salary and the assigned colony are increasing with experience, as captured by the number of colonies served. Evaluating the coefficients, the premium of connections corresponds to almost a half of the gain from serving in one additional colony (Column 1). Connected governors therefore receive higher salaries by being fast-tracked in their careers. The reallocation channel through which Secretaries of States increase their connected subordinates' salary stands in stark contrast to the private sector, where discretionary salary hikes within the same position are common (Kramarz and Thesmar, 2013). Discretion in promotions could hence undermine the ability of fixed wage schedules to limit favoritism.

Although all governors exercise comparable administrative tasks across different colonies, one concern for the interpretation is that differences in salaries may reflect compensating differentials (Dal Bo et al., 2013). While expenses in the colonies were typically covered by the Crown, thus alleviating concerns over differences in local price levels, salary differences could still arise due to amenity differences across colonies: governors are then, for example, compensated with a higher salary for serving in colonies with a greater disease burden or further away from London. In Columns 4 and 5, I test if the higher paid and larger colonies are also more likely to be in tropical regions or further away from London. The results show that this is not the case, providing evidence against compensating differentials. Higher paid governorships thus are more likely to indeed reflect more desirable jobs.²⁹

²⁹If anything, colonies with higher settler mortality pay lower wages. Given the incomplete data on settler mortality, however, I only report the cross-colony correlations in Appendix Table B6.

4.2 The removal of patronage - Warren Fisher Reform 1930

The results demonstrate the centrality of social connections in shaping the allocation of governors during a period in which securing senior positions through connections was the norm. Although the practice of patronage appointment was gradually eliminated from the domestic civil service following the seminal Northcote-Trevelyan report of 1854, civil service reforms within the Colonial Office had lagged behind. While competitive examinations were introduced for the lower-tier colonial administrative service as early as the 1850s, the right to appoint senior governors by patronage remained a legal privilege until the reform of 1930.

Implementing the Warren Fisher report “On the System of Appointment in the Colonial Office and Colonial Services” published in the same year, the Colonial Office saw sweeping changes in the system of appointment. As the report noted, the “system is open to criticism first and foremost as being at any rate in theory, a system of patronage”, where the “[Secretary of State] has the sole power, through his private secretary, over the selection of candidates.”³⁰ The report hence recommended that the “existing arrangement should be replaced by a system of recruitment at once more authoritative and more independent”.

More specifically, the reform replaced the role of the private secretary of appointments, who acted under the direct control of the governor, with the *Colonial Service Appointments Board*. This board consisted of a Chairman and two members nominated by the independent UK civil service commission. Although the final selection was submitted to the Secretary of State on whose authority appointments would ultimately be made, the board imposed considerable constraints on the extent of discretion by overseeing the machinery of recruitment and appointments. The Warren Fisher Reform, therefore, replaced the “century-old patronage system by a public process of application and interview under the auspices of an independent and formal selection board” (Kirk-Greene, 2000). The reform led to the creation of a personnel department by separating the recruitment functions from the direct influence of the Secretary of State. In effect, these reforms led to the professionalization of the colonial bureaucracy. Hailed as the “Magna Carta of the Colonial Service”, the 1930 reform was a defining moment of the Colonial Office (Kirk-Greene, 2000).³¹

The reform provides a natural experiment to study the extent to which the removal of patronage appointments limited favoritism. I test for a differential effect of social connections after the reform by estimating the difference-in-differences:

$$\log w_{ist} = \beta_0 \times c_{it} + \beta_1 \times c_{it} \times \mathbf{1}[t \geq 1930] + x'_{it}\gamma + \theta_i + \tau_t + \varepsilon_{ist} \quad (9)$$

where w_{ist} is the wage and $c_{it} = \{0, 1\}$ is the dummy for connectedness. This specification

³⁰Warren Fisher Committee Report on System of Recruitment (1930, CAOG 13/317), page 21

³¹Interestingly, Kirk-Greene (2000) also mentions the lack of scalability as a reason for the abolition of patronage. He writes: “With the increase in demand for colonial administrators in Britain’s new and sizeable African acquisitions, patronage in its sense of family favouritism and personal protégés would be insufficient to find enough staff”.

now allows the gap between the connected and unconnected governor to vary before and after the reform. Since the Warren Fisher reform formally abolished patronage, I expect the promotion gap to be smaller after the reform. The remaining variables are defined as before, with the only difference that the vector x_{it} now also allows for the impact of a large set of observable characteristics to vary after the reform. This mitigates concerns that the reform also had impacts on dimensions other than social connections. These time-interacted characteristics include the number of colonies served, as well as the previous career background of the governor (civil servant, military, politician).

[Table 4 and Figure 2 here]

The results in Table 4 show that the promotion gap disappears after the 1930 reform. While connected governors receive 12.7% higher salaries before 1930, the salary gap after the reform is statistically indistinguishable from zero (Column 2). This is an important result as the introduction of a formalized appointment board changes the allocation and promotion patterns of governorships: the preferential treatment of connected governors, as evidenced in the positive salary difference, disappears after the reform limited the extent of discretion the Secretary of State could exercise. This suggests that the Warren Fisher reform was effective in reducing the impact of social connections in shaping the allocation of public leadership positions.

The results are robust to more flexible controls. The remaining columns allow the impact of social connections to trend linearly (Column 3) and vary by a host of individual characteristics (Column 4) to account for trends or shifts in the composition of governors. The main concern is that the Warren Fisher reform also affected other characteristics correlated with connections. To alleviate this, I interact all individual background measures with the post 1930 dummy. The results are nearly identical. Finally, Figure 2 provides visual evidence by plotting the salary gap for social connections over time. The gap is estimated using an augmented version of (9) where the effect of social connections is allowed to vary by five year bins. The figure shows that the point estimate for the salary gap is positive in the pre-reform period. After 1930, however, the point estimates are close to zero, consistent with the weaker impact of social connections in determining the salaries and positions of governors after the abolition of patronage.

5 Governor and colony performance

The interpretation of the salary premium hinges on the performance of connected governors. If connected governors perform better than unconnected governors, social connections need not be detrimental to organizational performance. Under patronage, I expect connected governors to perform worse (better) if connections and performance are substitutes (complements) for promotions (Result 2). I test the performance prediction in this section.

I focus on revenue generation as the central measure of performance. Revenue generation

was a key performance measure for the Colonial Office and the governors exercised direct control over colony public finances.³² Under the “revenue imperative”, colonies were expected to balance budgets: “the colonies were expected to pay their way [...] If they were prosperous, they were free to go ahead with whatever [...] developments the local authority wished” (Jeffries, 1956).³³ The size of the budget therefore is a direct measure of state capacity.

To identify the reduced form impact of social connections on the performance measure y_{ist} of governor i in colony s at year t , I first estimate:³⁴

$$y_{ist} = \beta \times c_{it} + \gamma' x_{it} + \nu_{is} + \tau_t + \varepsilon_{ist} \quad (10)$$

where $c_{it} = \{0, 1\}$ is the dummy for connectedness. The governor-colony fixed effects ν_{is} limit the variation to “within-appointment” shocks in connections. This alleviates concerns over governor-colony specific match heterogeneity that may be correlated with connections, for example that higher ability governors perform better in larger colonies. As appointments are fixed for six years, I compare the performance of the same governor already allocated to a colony when connected and unconnected, holding constant the selection margin. Interpreted through my model, these within-position performance differences reflect incentive effects (Result 2). Table B3 reports balancing statistics for the within-appointment switcher sample.

The switcher sample is now more stringent. Only 15% of all 729 appointments experience a shock in connections, corresponding to 20% of governors. Table B3 shows balance on all time-varying characteristics: governors are as likely to experience a shock earlier on in their appointment as later on. The probability of exit does not significantly vary by connectedness. Finally, the inclusion of year fixed effects τ_t absorbs shocks common to all colonies. The errors ε_{ist} are clustered at the governor-secretary level.³⁵ As before, I estimate the regression using the full sample to obtain more precise estimates.

[Table 5 here]

Table 5 reports the key result. Under patronage, governors perform worse when connected to their superior.³⁶ The same governor in the same colony generates 4% lower annual revenue in years connected compared to years unconnected to the Secretary of State (Panel A, Column

³²As the Colonial Rules and Regulations state, all the “monies to be expended for public services are issued under his [the governor’s] warrant”. (Colonial Rules and Regulations 1862, III. 17.)

³³By the 19th century, the administration of the colonies had become a financial burden to London. The revenue imperative, whereby administrators were “tasked with raising sufficient revenue locally to pay for the local costs of colonial governance [...] and the construction of public works”, was a direct response (Gardner, 2012).

³⁴The results are also robust when using revenue growth instead of levels (Appendix Table C3).

³⁵Again, the results are robust to alternative clustering strategies. See Appendix Table C6).

³⁶This result stands in contrast to Jia et al. (2015) and Jia (2014) who document that connections to the Chinese Central Committee induce higher performance. In theory, the sign depends on whether connections and effort are complements or substitutes. Appendix Table B8 replicates the main result of Jia et al. (2015) and shows that connections and effort are substitutes and not complements in the Colonial Office, reconciling the different findings.

1).³⁷ Consistent with the mitigating effect of the Warren Fisher reform on the salary gap, the negative performance gap vanishes after the abolition of patronage in 1930 (Column 2). Patronage hence impacts the revenue performance of colonies run by connected governors, suggesting that the incentives of leaders can affect macroeconomic outcomes.

The remaining columns provide the breakdown of the aggregate revenue to shed light on the nature of the observed fiscal reduction. For data quality reasons, this analysis is confined to a subsample: changing accounting standards often prevented the construction of comparable time-series. The main results, however, also apply to this subsample, thus alleviating concerns of sample selection. I break down revenue by external and internal sources: external sources comprise trade/customs taxes, while internal sources are primarily licenses and direct taxation (e.g. land revenue, hut/income taxes). Trade taxes are collected at entry points (e.g. a customs house at ports), whereby the collection of internal revenue is more decentralized. The decrease in revenue generation is primarily driven by a reduction in customs revenue, which make up the bulk of the colonial revenue (Table 1). The point estimate for internal revenue is negative but insignificant (Columns 3 to 4).

Turning to the expenditure side (Table 5, Panel B), the lower revenue generation coincides with a decline in overall expenditure for connected governors, though the point estimate is statistically insignificant (Column 5). Once broken down by reform period, however, the expenditure gap is statistically significant (Column 6). This suggests that the negative gap is once again driven by the patronage period. The decline in public spending can be interpreted in two ways: first since colonies were self-financed under the Crown's "revenue imperative", the decrease in revenue will necessarily translate into a decrease in public spending. Second, since spending public funds requires active effort, lower expenditures can also be interpreted as a measure of performance. To that end, I also disaggregate expenditures to study spending for revenue collection services and public works (Panel B, Columns 7 to 8). Observing differential spending on revenue collection and infrastructure investments may provide further evidence for the underlying mechanism that drives the decrease in revenue generation. As most of the expenditures are determined by the size of the bureaucracy fixed by the Crown, I focus on "extraordinary" spendings over which governors have purchase. The decrease in public investments in revenue collection is substantial: the point estimate suggests a significant decrease by about 8.9%. For public works, there is a significant decrease by 10.7%. Connected governors, hence, decrease their revenue generation. Faced with a smaller budget, this translates disproportionately into lower spendings for revenue services and public works.

One threat to the interpretation of the results is selective exit. In the absence of a perfect compliance with the fixed six year term limit, the results could be spuriously driven by selective attrition: if connected governors, for example, are more likely to be kept in their ap-

³⁷The results are robust when using per capita total revenue. But since the population data is interpolated between decadal Census years, my preferred specification is to examine the total revenue.

pointment when subsequent revenue performance is low, the negative results may be driven by the fact that unconnected governors never stay in the colony when revenue grows. Three pieces of evidence, however, suggest that selective noncompliance with the six year term is unlikely to be a major concern: first, as Table B3 shows, the probability of exit does not significantly vary with connectedness.³⁸ Second, entering connected and “switching out” has a symmetric effect to entering unconnected and “switching in” (Appendix Table C4). Finally, I conduct a bounding exercise to rule out that the results are driven by connected governors more likely to remain when subsequent revenue growth is low (Appendix Table C5).

5.1 Tax policy and exemptions

The revenue decline by 4% is striking. Indeed, there are many channels through which connected governors may have impacted fiscal performance: connected governors could have exerted lower effort in monitoring, thereby increasing tax evasion. Similarly, connected governors could have also engaged in more corruption by diverting revenue. Given the covert nature of such activities, however, it is inherently difficult to test specific channels.

To provide evidence for one observable channel, I examine whether the reduction is driven by actual changes in policies. Raising taxes in the colonies required legitimacy, and “rebellion by [local] taxpayers was a constant worry which shaped tax policy” (Gardner, 2012).³⁹ Governors were hence forced to balance pressure from urban elites against the directives of the Secretary of State to whom they were ultimately accountable to. Connected governors could have acted against the interest of the Crown by succumbing more easily to local political pressure or by extracting private rents from providing tax exemptions.

In order to test this, I extracted information on legislation from the National Archive’s catalogue and the Blue Books. By the colonial regulations, governors were required to report changes in legislation made through ordinances and proclamations to the Colonial Office. These changes were communicated in two ways: through direct correspondence with the Secretary of State, and by reporting the full set of ordinances and proclamations in the Blue Book. I code both the direct correspondence catalogued in the National Archives into different types of legislation as well as the content of specific laws.⁴⁰

Given data constraints, extracting and reading the full set of correspondence and legislation lies beyond the scope of this paper. To reduce the data intensity, I therefore constrain the historical sample to the switcher sample (Table B2). This is the sample of governors that experience a switch in connectedness while serving in the same colony and hence drive the identification of the main results (Section 5). Dropping the large part of the full sample, while

³⁸There is also no statistically significant difference in the survival curves for governors when connected and unconnected (Appendix Figure A6).

³⁹For an account of other conflicts between the colonies and London, see Francis (1992).

⁴⁰See the Data Appendix for a more detailed description of the procedure.

not impeding the identification strategy, however, comes at a cost of noisier estimates. The results of this subsample are therefore more likely to be attenuated.

[Table 6 here]

The results are summarized in Table 6. The regressions are based on the same within-appointment identification used to estimate performance effects in previous sub-section. In Column 1, the dependent variable is the total number of ordinances as computed based on the National Archive's catalogue extract. Consistent with the proposed institutional mechanism, I find that connected governors are more likely to issue ordinances than unconnected governors. As before, the effect is driven the patronage period. The remaining columns break down the total number of ordinances by type. The results show that the increase in legislation is primarily driven by more ordinances in tax and customs, which primarily comprise legislation on import tariffs and duties. This is consistent with customs driving the decline in colonial revenue (Section 5). As before, the removal of patronage mitigates the gap.

One concern is that the number of legislation on trade tax laws does not allow me to infer to the exact policies that were implemented. More legislation need not be detrimental but instead indicate a more active governor. To address this interpretational issue, I read and hand-coded 405 years worth of laws. Given data constraints, I focus primarily on an easily measurable policy change, namely the introduction of import tariff exemptions.⁴¹ There are several reasons why this is particularly suitable. First of all, tariff exemptions are more systematically recorded and unambiguously reduce trade revenue. Identifying changes in exemptions is hence substantially easier than computing the average tariff rates for all goods. Customs laws are also more harmonized than tax laws, making it easier to compare policies across colonies. In addition, import customs revenue is economically significant as it makes up more than 50% of the revenue throughout the study's sample period. Finally, import exemptions have been documented to be one of the contested margins of colonial tax policy, as perhaps most famously known in the Boston Tea Party rebellions against the Townshend import duties.⁴²

The result is summarized in Column 4, where the dependent variable is a dummy that is 1 if the governor introduced a customs exemption in a given year and zero otherwise. Connected governors are more likely to legislate import exemptions during the period of patronage but not thereafter. Finally, connected governors have no statistically discernible bearing on other legislation, such social programs encompassing education, health and welfare (Column 5) or public works (Column 6), consistent with the fiscal channel uncovered.

⁴¹See Appendix Figure A2 for an example of such laws.

⁴²Despite all efforts, the sample is smaller as it was not possible to obtain the tax legislation for all years. The results documented using the full sample, however, also apply to the smaller sample, alleviating concerns over selectivity.

5.2 Additional performance measures

Revenue generation may not be an adequate measure of governor performance. Lower revenue generation, for example, could indicate that connected governors are less extractive. Under multitasking, connected governors may have also directed their efforts to other dimensions of performance which revenue does not capture. My analysis does not take a stance on whether revenue generation is detrimental for the colonies. The focus instead lies on the principal-agent relationship between governors and their superior. Since revenue generation was one of the declared duties of the governors, deviations from this objective can be interpreted as lower performance.

To alleviate remaining concerns over the interpretation of the revenue measure, I corroborate the findings using additional performance proxies. In particular, I use newspaper reports of social unrests, sentiment analysis of parliamentary debates and individual-level public awards to proxy for performance. To keep the exposition concise, I only briefly describe the outcome measures and refer to the Appendix Section B.3 for a detailed description.

I examine social unrest as an additional colony-level outcome. Uneven taxation of the natives and dismal colony conditions have been associated with unrest, with the infamous Sierra Leonian Hut Tax riots of 1898 and the Jamaican Morant Bay rebellion 1865 as prominent examples. I use the reports of riots in UK newspapers to proxy for social unrest. This has several advantages. First, it enables the measurement of conflict in a consistent way as colonial conflict data is largely absent. Second, while reported unrests may not capture all unrests in the colony, the visibility of colony conditions in London explicitly captures the principal-agent relationship: bad news about a colony in the domestic press is likely to reflect poorly on the Secretary of State. Following the same logic, I text mine parliamentary debates in London. As Secretaries of State were themselves accountable to the parliament, observing a large number of discussions over a given colony and its associated sentiment can be seen as an alternative performance measure. For this purpose, I extracted all parliamentary debates between 1855-1966 during which a given colony was mentioned. For each of the mentions, I compute the sentiment using standard text mining procedures.⁴³ The algorithm assigns sentiment scores to text passages, where a negative score indicates a more negative sentiment. Finally, for awards as an individual-level performance measure, I focus on the highest awards, the Knight Grand Cross for the Order of St. Michaels and the Order of Bath (GCMG/GCB). These awards were introduced by the Crown in 1818 as part of an honours system to recognize the outstanding performance of public servants in the colonies. The recommendation is made by the Secretary of State, but the final approval is made by the Crown.

[Table 7 here]

⁴³The sentiment analysis is implemented using R's *qdap_polarity*. See Appendix for a detailed description.

Table 7 summarizes the results using alternative measures of performance. To be consistent, I use the same double-differences specification as in (10). In column 1, the dependent variable is a dummy that is 1 if a social unrest was reported in the UK newspapers. The estimate suggests that colonies of connected governors are 3.8% points more likely to have social unrest reported during the period of patronage. As before, this gap vanishes after the removal of patronage.⁴⁴ Columns 2 to 3 report evidence from parliamentary debates. On average, connected governors see their colony mentioned more than unconnected governors in a given year, though the estimate is not statistically significant (Column 2). The associated sentiment, however, is significantly less likely to be positive (Column 3). Consistent with previous results, this negative sentiment gap vanishes after the removal of patronage. Finally, the dependent variable in Column 4 is a dummy that is 1 if the governor received the Knight Grand Cross (GCMG/GCB), the highest award. The estimate suggests that connected governors are 3.1% points less likely to receive the highest recognition. Compared to the mean of the dependent variable (2%), the decrease is economically large. Taken together, the evidence suggests that connected governors indeed perform worse on a wide range of margins.

6 Misallocation and selection effects

The estimated effects in previous section, by nature of the within-appointment variation, are conditional on governors who were not moved to another colony. While this alleviates concerns over unobserved governor heterogeneity, these effects hold constant the selection margin and identify only within-governor changes, which, interpreted through the lens of the conceptual framework, only capture incentive effects.

Patronage, however, also affects the selection of governors (Result 3). In presence of complementarities between ability and colony size, high ability governors should be allocated to the largest colonies. Under patronage, however, favoritism may induce misallocation as the Secretary of State screens less on ability. To motivate the test for selection effects, I first provide descriptive evidence consistent with selection. Figures 3 and 4 plot the relationship between colony and governor fixed effects for the pre- and post-patronage period. The estimates are based on (log) revenue as the dependent variable and a variant of the specification (10), where I now separately estimate colony and governor fixed effects.⁴⁵ While there is no association between governor and colony fixed effects in the patronage period (Figure 3), the association is significant and positive for post-patronage appointments (Figure 4).⁴⁶ The fig-

⁴⁴While this appears at odds with the tax exemptions granted, reductions on trade taxes benefited the local elites. Social unrests, however, are sparked by the broader, native population (Gardner, 2012).

⁴⁵The results are comparable using (log) expenditure as the alternative outcome variable. Specifically, I estimate following augmented model: $y_{ist} = \beta \times c_{it} + \gamma' x_{it} + \theta_i + \mu_s + \tau_t + \delta_s \times t + \varepsilon_{ist}$, where θ_i is the governor fixed effect, μ_s the colony fixed effect and $\delta_s \times t$ captures colony-specific trends.

⁴⁶The relationship remains significant for the post-patronage period when using bootstrapped standard errors.

ure also shows the shift towards higher governor fixed effects. Seen through the lens of the model, this increase in the matching assortativeness suggests an improvement in the overall matching efficiency.⁴⁷

[Figure 3 and 4 here]

While consistent with selection, the patterns remain descriptive. For the remainder of this section, I therefore proceed to estimate the causal impact of appointing a connected governor. This will capture both the incentive and selection effects of social connections (Result 3).

6.1 Effect of appointing a connected governor

Let the index st denote the appointment in colony s at time t . Let $y_{st+n_{st}}$ be the revenue or expenditure level at the end of the appointment, where n_{st} is the duration of the appointment. By the colonial regulations, this duration is fixed for six years. The fiscal growth over the appointment period is then estimated using two-stage least-squares:

$$g_y = \beta \times C_{st} + \gamma' x_{st} + \mu_s + \varepsilon_{st} \quad (11)$$

where $g_y = \log(y_{st+n_{st}}/y_{st})$ and $C_{st} = \sum_t^{t+n_{st}} c_{I(s,t)st}$ is the number of connected years in the appointment. $i = I(s, t)$ is a function that returns the governor i serving in colony s at time t .

The main challenge in estimating the effect of appointing a connected governor is that connected governors are not randomly allocated. Indeed, previous results suggest that connected governors are more likely to be allocated to higher salaried governorships which also tend to be in larger colonies. To estimate the causal effect of social connections on the extensive margin, I therefore require an instrument that predicts the likelihood of a given colony to receive a connected governor, but that is otherwise unrelated to colony-level characteristics.

I construct an instrument that exploits two sources of variation to meet both the relevance and exclusion condition. The first source of variation I exploit 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 no more than six years. As Figure 5 shows, the majority of the governorships indeed end in the sixth year.

[Figure 5 here]

The second source of variation stems, once again, from the turnover of Secretaries of State which generates cross-sectional variation in the connectedness of serving governors. The interaction of both sources of variation results in temporal variation in the number of connected

⁴⁷Similar to Card et al. (2013), the key assumption required for this interpretation is that match-specific effects remain constant across the pre- and post-patronage period.

governors who are likely to be moved to a vacant colony. The share of connected governors with at least 6 years of tenure in $t - 1$ is an instrument for a connected appointment in t ,

$$p_t = \frac{\sum_i \mathbf{1}[T_{it} \geq 6] \times c_{it}}{\sum_i \mathbf{1}[T_{it} \geq 6]} \quad (12)$$

where T_{it} denotes the years of tenure for governor i in year t . I refer to those governors with at least 6 years of tenure ($T_{it} \geq 6$) as “available” governors.

Appendix Figure A3 shows the variation which, given the interaction of two distinct sources of variation, appears idiosyncratic. The figure also illustrates the intuition behind the instrument. While half of the available governors were connected in 1855, for example, none of the available governors were connected a year later. A colony that falls vacant in 1856 due to the six year term limit is then much more likely to receive a connected governor than a colony that opens up a year later. The first-stage for (11) then is,

$$C_{st} = \alpha \times p_{t-1} + \eta' x_{st} + \nu_s + \epsilon_{st} \quad (13)$$

where the number of connected years in the appointment is instrumented by the share of available connected governors the year before the appointment p_{t-1} . This instrument is valid for following reasons: a colony is more likely to receive a connected governor if the pool of available connected governors at time of vacancy is larger.⁴⁸ Which exact colony falls vacant at a given point in time, however, depends on the six year tenure limit. Here, introspection does not easily suggest why characteristics of the vacant colony should be correlated with the number of available connected governors, which depends both on the six year transfer rule and the connections to the serving Secretary of State.

With colony fixed effects μ_s and ν_s absorbing time-invariant cross-colony differences, the identifying variation of the instrument p_{t-1} is temporal and driven by the share of connected governors who are available for transfers. To ensure that the results are not driven by growth picking up over time while the share of connected appointments declines, I include a linear time trend in the control vector x_{st} .⁴⁹ The vector also comprises the (log) initial governor salary of the appointment and spell length fixed effects.⁵⁰ Perhaps most importantly, I include previous spell duration dummies to control for whether the previous appointment ended regularly. This alleviates concerns that appointments were systematically terminated early (or later) when many connected governors had to be reshuffled.⁵¹ Finally, the errors are clustered by year and colony. The year level corresponds to the identifying source of variation, and the

⁴⁸Monotonicity is satisfied as long as a higher share of connected governors with at least 6 years of tenure does not reduce the likelihood of a given colony to receive a connected appointment.

⁴⁹The results are also robust to quadratic trends and decade fixed effects, though the first-stage is weaker.

⁵⁰The length of the spell is uncorrelated with the instrument. The inclusion serves to obtain more precise estimates.

⁵¹Early or late termination of previous appointments is uncorrelated with the share of available connected governors (Appendix Table C8).

colony clustering accounts for serial correlation within a colony over time.

[Table 8 here]

To demonstrate the relevance of the instrument, Column 1 of Table 8 reports the first stage to confirm that the share of available connected governors in the year prior to the appointment predicts the probability of a colony to receive a connected governor. Indeed, governorships are more likely to be filled by a connected governor if the share of available connected governors in the year before the appointment was high. The probability of a governorship to be filled by a connected governor is 21% points higher if all available governors in the previous year were connected vis-a-vis when all available governors were unconnected. This implies that the instrument also predicts the number of connected years (Column 2). The inclusion of colony fixed effects does not substantially move the point estimates (Column 3), consistent with the share of available connected governors being unrelated to fixed colony characteristics of the vacancy. Finally, in line with the intuition of the instrument, it is only the variation in the share of available connected governors the year prior to the opening that drives the first-stage. Leads and lags in the variation do not predict connected appointments (Column 4)⁵²

The first-stage is not mechanic: 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 the appointment of governors that is purged using the instrument. The complier population hence constitutes those serving connected governors who are transferred in accord to the colonial regulations.

[Table 9]

The reduced form and IV estimates are summarized in Table 9 and suggest the presence of negative selection effects. As expected, the OLS is upward biased: while OLS shows no difference in the revenue growth between a connected and unconnected appointment, as measured by the connected years (Column 1), the reduced form estimate in Column 2 suggests that a higher probability of being allocated a connected governor is associated with significantly lower revenue growth over the entire appointment. As before, the inclusion of colony fixed effects leaves the point estimate nearly unchanged (Column 3). In order to facilitate the comparison of the estimated magnitudes with those recovered from the intensive margin in previous section, Column 4 reports the instrumental variable estimate. The point estimate, insignificant due to the weak first stage, suggests that an additional year under a connected governor is associated with a 13% point lower revenue growth over the appointment.

Mirroring the analysis in previous section, Column 5 tests if the negative impact of connected appointments is mitigated after the abolition of patronage. Given the weak first stage,

⁵²The results are similar when using two period leads and lags but this substantially reduces the number of observations (Appendix Table C7).

I only report the reduced form estimates. As before, the negative impact of social connections is mitigated after 1930. The last column repeats the same estimation for public expenditures. The corresponding spending declines as well, consistent with the estimates based on within-position variation in connections. Once again, the negative impact is mitigated after the removal of patronage. The fiscal performance results using two different identification strategies are consistent: connected governors perform worse both within and across appointments.

7 Long-run persistence

The large short-run costs of patronage motivate the question whether patronage in the colonial period has had any scarring effects. Indeed, there are numerous channels through which events in the colonial period may translate into long-term differences in state capacity. The lack of historical investments in fiscal capacity mechanically affects the ability of states to raise taxes and provide public goods (Besley and Persson, 2010; Besley et al., 2013; Guardado, 2016). The differential policies of connected governors could have also led to institutional lock-ins (North and Weingast, 1989; Acemoglu et al., 2001; Robinson et al., 2005). While pinpointing each mechanism is clearly beyond the scope of a single paper, I document one channel through which historical patronage can have long-run effects.

Specifically, I focus on an institutional lock-in induced by historical changes in tax and customs policies. The reason is twofold: first, evidence from the historical period showed that the reduction in revenue was driven by lower customs revenue (Section 5). Second, the reduction in customs revenue coincided with a larger number of legislation on trade taxes, especially exemptions (Section 5.1). These historical policies in the patronage period may not only have a short-run bearing on revenue performance but also a persistent impact on the ability to raise taxes: legislation, once in place, is likely to persist (Morris and Coate, 1999).

Relating historical colony-level variation in connected governors to contemporary outcomes requires a mapping of colonial territories into modern regions. 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. I omit two dependencies that cannot be mapped into modern regions,⁵³ as well as the set of territories that still remain dependencies of the UK. These cases all constitute small islands.⁵⁴ The Appendix provides a detailed summary of the mapping process. When using country-level data, I impute the same value for all subnational provinces, implicitly assuming that within-country differences have been equalized. For the main result on tax/GDP, however, I compute the corresponding subnational values from statistical yearbooks of the modern countries.

⁵³Heligoland is a tiny island that is now part of Germany, Ionian Islands are a group of isles now part of Greece.

⁵⁴The 7 colonies that are still part of Britain are small entities: Bermuda, Cayman Islands, Falkland Islands, Gibraltar, Montserrat, St. Helena, British Virgin Islands.

With the mapping completed, the long-run effect can be estimated using a cross-sectional regression. For the modern country or subnational province coinciding with the historical colony s , the estimation of contemporary outcome y_s is:

$$y_s = \beta \times C_s + \gamma' x_s + \mu_{R(s)} + \varepsilon_s \quad (14)$$

where $C_s = \sum_t c_{I(s,t),t}$ denotes the number of historical connected appointments and $i = I(s, t)$ is the link function that returns the governor i serving in colony s at time t .

The empirical challenge in estimating long-run effects in this setting is that connected governors are - as before - systematically allocated to higher salaried and larger colonies. If historical cross-colony differences persist, any observed correlation is likely to be spurious: higher salaried colonies, for example, may have been richer to begin with. If these initial income differences persist, the estimated relationship between historical connectedness and income differences today will be upward biased. The estimation of long-run effects therefore demands an instrument that predicts the likelihood of a given colony to receive connected governors, but that is otherwise unrelated to historical colony-level characteristics.

I use the same instrument from previous section to estimate the long-run effects of social connections. The long-run instrument is implemented by aggregating the identifying source of variation from the appointment level st to the colony-level s . This allows me to instrument the endogenous number of connected years in the colonial period with the expected number of connected appointments as predicted by the instrument. For each colony, I calculate the expected number of connected appointments P_s calculated for the same time period,

$$P_s = \sum_t p_{t-1} \times \mathbf{1}[T_{I(s,t),t} = 1] \quad (15)$$

where p_{t-1} is the proportion of connected 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. Instead of weighting each appointment with the actual dummy of connectedness, the colony-level instrument P_s is the number of appointments weighted by the share of connected governors available the year before the appointment p_{t-1} . I compute the connected years and the instrument separately for the pre- (1854-1930) and post-patronage period (1930-1966).

The relevance of the instrument has been shown in the estimation of historical selection effects (Section 6.1). The exclusion restriction is that the historical shares of connected governors with at least 6 years of tenure and who were thus available at time of a governor vacancy are unrelated to modern outcomes other than by affecting the historical propensity of connected governors to be appointed. The first stage then is,

$$C_s = \alpha \times P_s + \delta' x_s + \nu_{R(s)} + \epsilon_s \quad (16)$$

where the cumulative number of connected years is instrumented by the expected number of connected appointments P_s constructed as described in equation (15). The vector x_s comprises a set of pre-determined colony-level controls. Since countries that were longer under British control are mechanically more likely to have received more connected appointments, I first and foremost control for the years under British rule. In addition, I include colony-level characteristics such as the initial (log) governor salaries and the share of land area within the tropics. Finally, $\mu_{R(s)}$ and $\nu_{R(s)}$ are region fixed effects, where $r = R(s)$ denotes the region (Africa, Europe, North America, Latin America, Asia and Oceania) the historical colony is located in. Given the high level of aggregation, the level of treatment coincides with the unit of observation, and I compute heteroskedasticity robust standard errors.

7.1 Effects on subnational fiscal capacity

With the instrument at hand, I relate variation in exposure to connected governors before and after the removal of patronage to a summary measure of fiscal capacity: the tax/GDP ratio. This is the central measure in a large body of literature on fiscal capacity. It is also highly correlated with GDP per capita across countries (Besley and Persson, 2009). Appendix Table B9 provides the summary statistics for the modern outcomes and explanatory variables.

The results are reported in Table 10 and provide evidence for a negative impact of connected governors on post-independence fiscal capacity. As expected, the OLS estimate is upward biased. While the endogenous number of connected appointments is not significantly correlated with the tax/GDP ratio in 2010 (Column 1), instrumenting the endogenous regressor yields a statistically significant and negative elasticity: one additional year under a connected governor in the colonial patronage period decreases tax/GDP in 2010 by 0.7% points (Column 2). The first-stage for the patronage period is strong.⁵⁵

[Table 10 here]

Most importantly, the historical variation in connectedness after the abolition of patronage 1931-1966 has no impact on modern tax capacity. In Column 3, I separately instrument variation in connected governors for the post-patronage period using the corresponding instrument. While exposure of connected governors in the patronage period remains associated with negative long-run fiscal capacity outcomes (Column 2), there is no impact after the removal of patronage (Column 3).⁵⁶ Consistent with the historical evidence, the impact of connections is only detrimental under patronage. The flat relationship in Figure 7 stands in stark contrast to the strong negative relationship from the patronage period (Figure 6).

⁵⁵The first-stage is reported in Appendix Table B10.

⁵⁶The first-stage of the instrument for 1931-1966 is weaker due to two factors: (i) a shorter time period post-patronage and (ii) less variation in connected governors, which in itself may result from the removal of patronage.

Column 4 reports the combined instrumental variable estimates. Due to the inclusion of both instruments, the first-stage is substantially weaker.⁵⁷ Reassuringly, however, the point estimates remain nearly identical. To remain consistent, I proceed with reporting the combined IV estimates. All results are robust in reduced form (see Appendix Table B11).

To see how persistent the effects are, Columns 5 to 7 constrain the sample to a balanced panel for which I have regional data in 1990, 2000 and 2010. The point estimates suggest that the negative persistence strengthens over time.

[Figures 6 and 7 here]

The main results are robust: as the partial correlation in Figure 6 shows, the results are not driven by outliers.⁵⁸ Perhaps more striking, the relationship is even negative when confining the sample to only modern provinces of Australia and Canada (Figure 8). This is the sample for which the effect should be weakest as national policies are likely to have equalized any historical regional differences. Finally, the results are robust to dropping the “Neoeuropes” and to the inclusion of additional colony-level controls, like landlockedness, ethnic fractionalization and genetic distance to the UK (Appendix Table C9).⁵⁹ While the sample size is clearly limited by the number of colonies administered by the Colonial Office, the effect is found among a homogeneous group exposed to the same colonizer.

To gauge the magnitude, moving the number of connected years from the 1st quartile to the 3rd quartile corresponds to 18.5 connected years (Appendix Table B9). With the elasticity of 0.7, this implies an increase in tax/GDP by 13% points, corresponding to moving from the tax/GDP ratio of Kenya (15%) to New Zealand (28%). While the estimated magnitudes appear large, it is important to caution that this elasticity is derived from subnational regions and countries with British colonial legacy. Results derived from this sample may therefore not correspond to those uncovered from average cross-country relationships.⁶⁰ Variation in connections during the period of patronage explain about 7% of the cross-sectional variation in tax/GDP in 2010.⁶¹ In comparison, variation in the area under tropics explain 31%.

[Table 11 here]

⁵⁷As Appendix Table B10 shows, each instrument is only relevant for the corresponding endogenous variable. Given the matrix form of the F -test statistic, the inclusion of both instruments will lower the power of the test.

⁵⁸When removing Lesotho, the elasticity is -0.525***. Due to small sample size I include Lesotho throughout.

⁵⁹The results are also robust when excluding the sample of small islands or modern tax havens.

⁶⁰In the sample, for this no significant association between tax/GDP and GDP per capita in 2010. There is also no statistically significant impact on GDP per capita in 2010 (Appendix Table B12).

⁶¹In comparison, Besley and Persson (2009) find that the impact of external wars, a key driver of fiscal capacity (Tilly, 1990), is of a larger magnitude, with one additional year of external war between state formation and 1975 increasing the average tax/GDP ratio between 1975-2000 by 0.7% points.⁶² Comparing across 103 countries, they also find that countries with Scandinavian legal origin have 29% points higher tax/GDP ratios today. Dincecco and Prado (2012) find that 1 additional casualty per square km between 1816-1913 (mean casualty 0.10, standard deviation 0.26) is associated with 0.13% point higher tax/GDP today.

Table 11, Panel A breaks down country-level revenue by using data from the International Centre for Tax and Development (ICTD). The ICTD provides harmonized data on government revenue that is aimed at addressing concerns over incomparable fiscal data (Prichard, 2016). The harmonized ICTD data allows me not to only probe deeper into the sources of revenue, but also validate the results using an independent country-level dataset, where I impute the country value for all subnational units. The results show that the decline in tax/GDP is primarily driven by trade taxes. Column 1 confirms the main result by showing that more years under a connected governor decreases tax/GDP ratio today. The decline is not driven by non-tax revenue, which comprises natural resource revenue (Column 2). Columns 3 to 6 provide cuts along direct and indirect taxes. The negative impact is only driven by the reduction in indirect taxes. While the impact on goods and service tax is negative, only the reduction trade revenue is significant. This is consistent with the disproportionate reduction of customs revenue and the increased provision of exemptions in the colonial period. Again, connectedness in the post-patronage period has no bearing except on non-tax revenue.

7.2 Effects on customs and quality of tax systems

To provide evidence consistent with policy persistence, I now examine whether connectedness affected customs and tax policy. These are the two margins that were relevant in the historical period (Section 5). Table 11, Panel B relates the exposure to connected governors before and after the removal of patronage to measures policy outcomes. Consistent with the large number of exemptions and the negative impact on trade revenue, colonies administered longer under connected governors during the period of patronage have lower average tariff rates (Column 7).⁶³ While a decrease in indirect taxes and trade barriers per se may not be detrimental, there is evidence that the modern trade tax systems are less effective. Using WTO data on tariffs, I find that modern countries that were longer administered by connected governors during patronage are more likely to have customs systems with more tariff lines (Column 8). These countries also experience more misreporting at the customs (Column 9), as measured by the discrepancy in the reported values of imports on the 6-digit level from the UK (Fisman and Wei, 2004).⁶⁴ Countries exposed to more connected governors are also more likely to report longer time needed to clear customs and comply with tax regulations (Columns 10-11). Finally, exposure to connected governors is also associated with lower trade volumes, as measured by the share of trade over GDP. Consistent with all previous results, the exposure to connected governors after the removal of patronage has no long-run impact (Column 12).

[Table 11 here]

⁶³See Appendix Figure A7 for the corresponding first-stage.

⁶⁴Let X_{is} denote the value of exports of 6-digit level class of good i to country s reported in UK and Z_{is} the corresponding imports reported in country s . Misreporting is the sum of mean absolute deviations, $\log(\sum_i^N N^{-1} |X_{is} - Z_{is}|)$.

Overall, the evidence from multiple independent datasets is consistent with the disproportionate reduction of customs revenue for connected governors in the colonial period and the higher number of exemptions granted. The higher number of trade taxes and exemptions legislated in the colonial period coincides with more misreporting, consistent with Fisman and Wei (2004) and Sequeira and Djankov (2014), who document that more complex customs systems create more ambiguity and scope for corruption and misclassification.⁶⁵ Taken together, the evidence along several cuts is consistent with the evidence from the historical period, lending credence to policy persistence as a plausible channel for the long-run effects.

8 Conclusion

For much of human history, bureaucrats have been selected and allocated based on discretionary appointments. It was only through the seminal thinking of Weber (1922) and landmark contributions like Northcote-Trevelyan (1854) and Warren Fisher (1930) that this practice has been curtailed and modern professional bureaucracies developed.⁶⁶ Despite numerous civil service reforms, the use of patronage in appointing civil servants remains widespread today. Whether or not discretionary appointments undermine government effectiveness and state capacity, however, remains an open question and theory is ambiguous about this issue.

My paper contributes to answering this question. I undertook a large-scale digitization of colonial records to construct a unique dataset that matches personnel records with public finance data of all British territories administered by the Colonial Office from its birth in 1854 to its dissolution in 1966. Two sources of variation are critical for my analysis. The first source of variation stems from observing how connected governors and colonies are linked to the Secretary of State in London. The second source of variation is the Warren Fisher reform of 1930 which removed the full discretion of the Secretary of State to appoint governors. Combining changes in connections to the Secretary of State with the introduction of the Warren Fisher reform enables me to study if differences in the promotion and performance of socially connected bureaucrats vary with the extent of discretionary appointments.

My data and empirical setup is particularly relevant as governors were administrative leaders of the colonies. I am hence able to examine whether or not patronage had costs by affecting the revenue performance of these territories, both during the colonial period and beyond decolonization. Being able to observe both connectedness during the period of patronage and after the Warren Fisher reform provides a unique opportunity to study how patronage affects economic performance in the long-run. This paper therefore goes beyond the existing body of literature that focuses on lower level bureaucrats and front-line providers who are unlikely

⁶⁵Consistent with a narrow fiscal channel, there are no impacts on other measures of institutional quality, such as the quality of legal and judicial institutions or the quality of land administration (Table Appendix B12).

⁶⁶As Max Weber succinctly conjectured in his seminal work, "bureaucracy develops more perfectly the more it succeeds in eliminating all personal elements that escape calculation" (Weber, 1922).

to have discernible individual effects on macroeconomic outcomes.

Four key findings emerge from my analysis. First, I find that governors when connected to the Secretary of State enjoy higher salaries through the promotion to higher paid and larger colonies. This salary premium only appears in the period before the discretionary power of the Secretary of State in appointing governors was curtailed. Second, even when examining the same governor in the same position, I find that the colony's revenue performance declines in years during which the governor is connected to the Secretary of State. This is strongly consistent with the interpretation that patronage exerts a negative effect on the performance of socially connected governors. Consistent with previous result, the negative fiscal performance gap disappears after the removal of patronage. Third, exploiting the fact that governors are transferred after their sixth year, I also shed light on selection effects by comparing the performance across appointments. Consistent with the interpretation that the Secretary of State is screening less on ability when allocating governors, the revenue performance of connected appointments is lower during the period of patronage. Finally, by linking historical datasets with contemporary data in countries and subnational provinces corresponding to the historical colonies, I am able to show that regions exposed longer to connected governors still exhibit lower fiscal capacity today. Interestingly, and in line with the other results, this only holds for connected years in the patronage period.

Taken together, these results provide compelling evidence that there are large costs of patronage, both for the British Empire but also for the independent countries that emerged from the Empire following decolonization. This paper therefore has implications for bureaucracies around the world who still rely on patronage as a means of allocating public office. The key conclusion hence is that incremental reforms aimed at curtailing discretion in the appointment of bureaucrats might often improve government effectiveness and economic performance.

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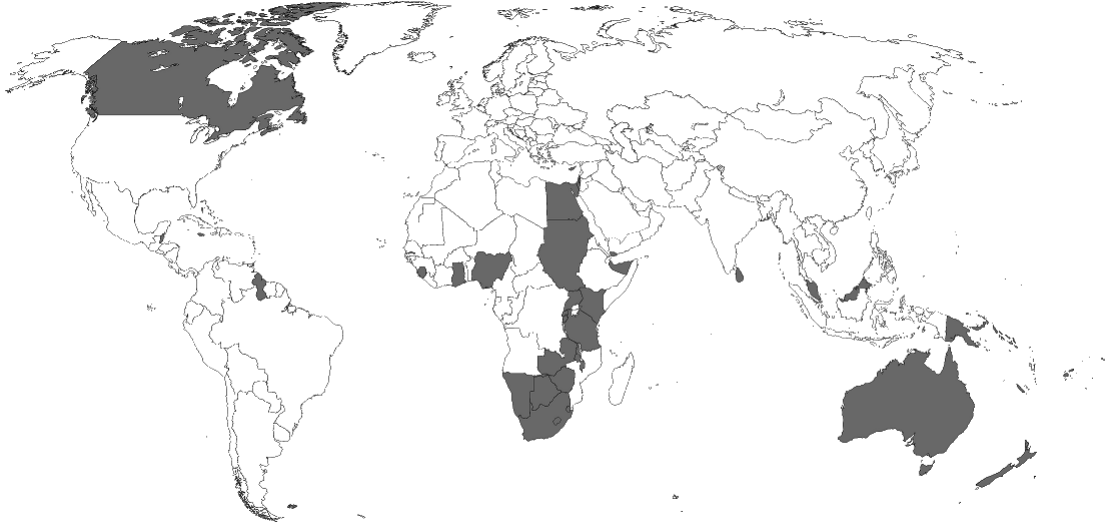
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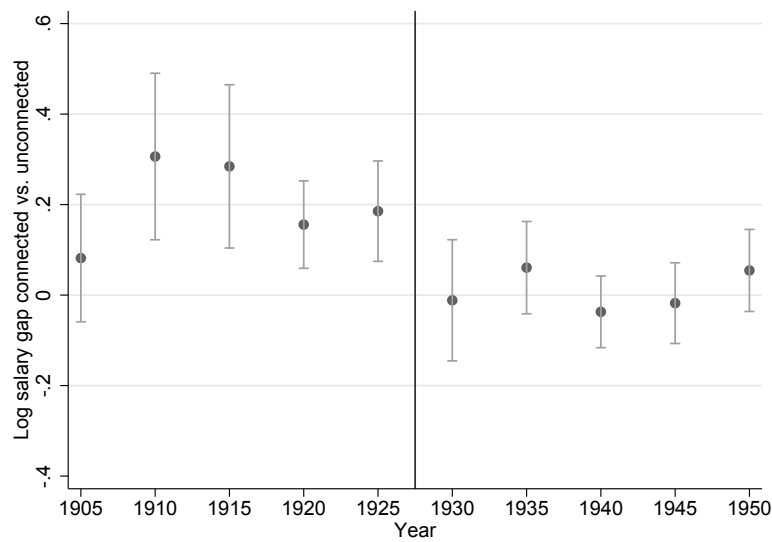
9 Figures

Figure 1: Territories administered by the Colonial Office - 1905



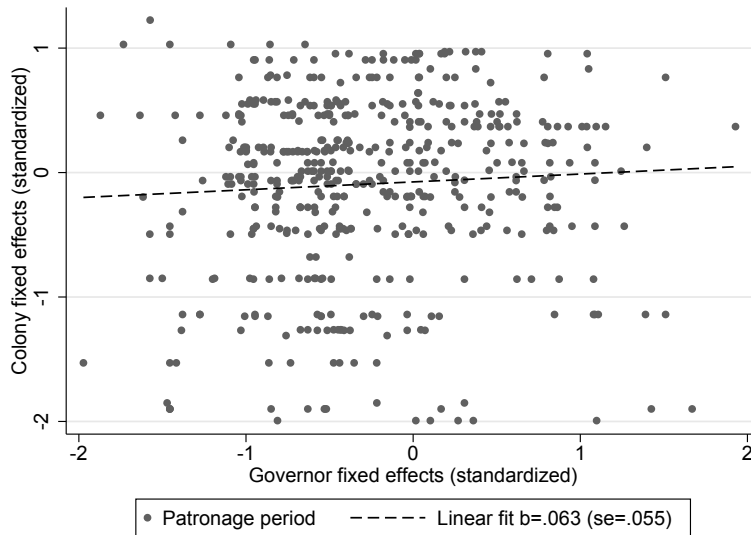
Notes: British territories administered by the Colonial Office in 1905.

Figure 2: Salary gap and the removal of patronage (Warren Fisher Reform 1930)



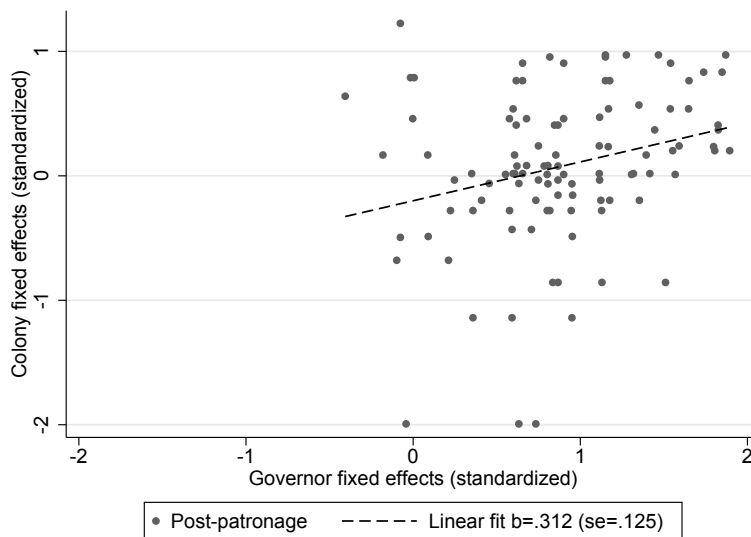
Notes: Difference in (log) salaries for connected and unconnected governors around the Warren Fisher Reform 1930 (solid vertical line). The salary gaps are estimated with an extension of specification (10), where connect- edness is allowed to vary by five year bins.

Figure 3: Matching assortativeness before the removal of patronage



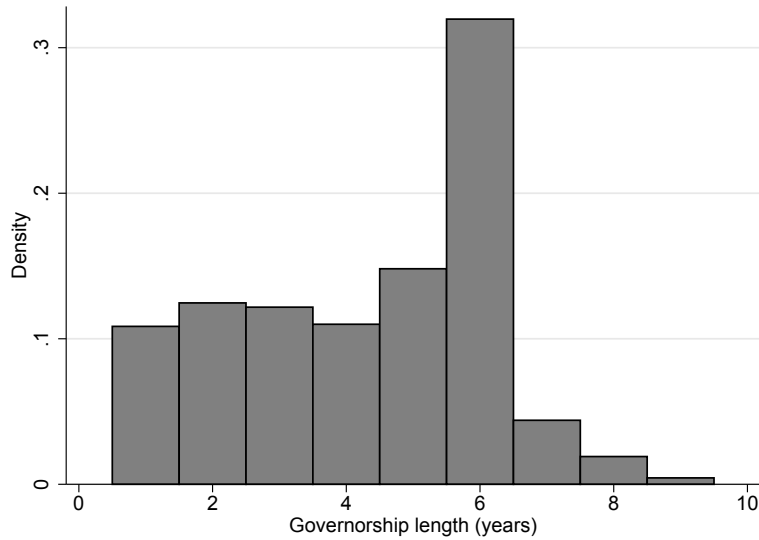
Notes: The relationship between colony and governor fixed effects for the patronage period (1854-1929). Reporting the estimated slope and corresponding robust standard errors.

Figure 4: Matching assortativeness after the removal of patronage



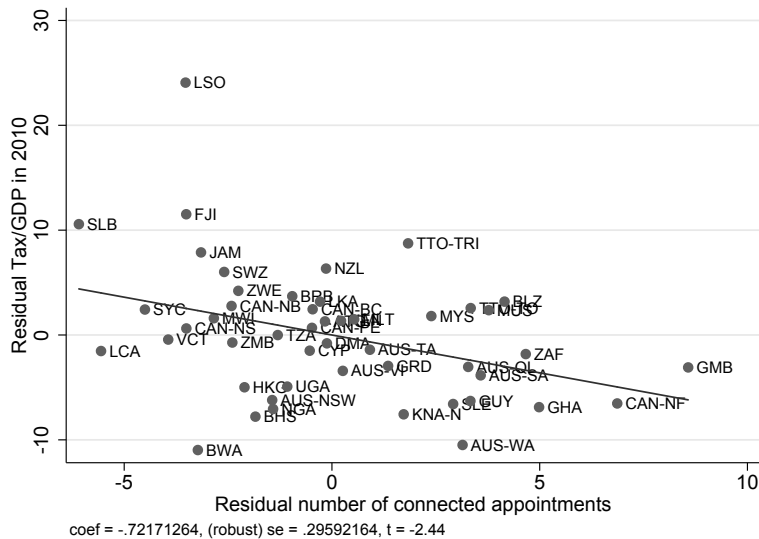
Notes: The relationship between colony and governor fixed effects for the post-patronage period (1930-1966). Reporting the estimated slope and corresponding robust standard errors.

Figure 5: Distribution of tenure length for completed governorships



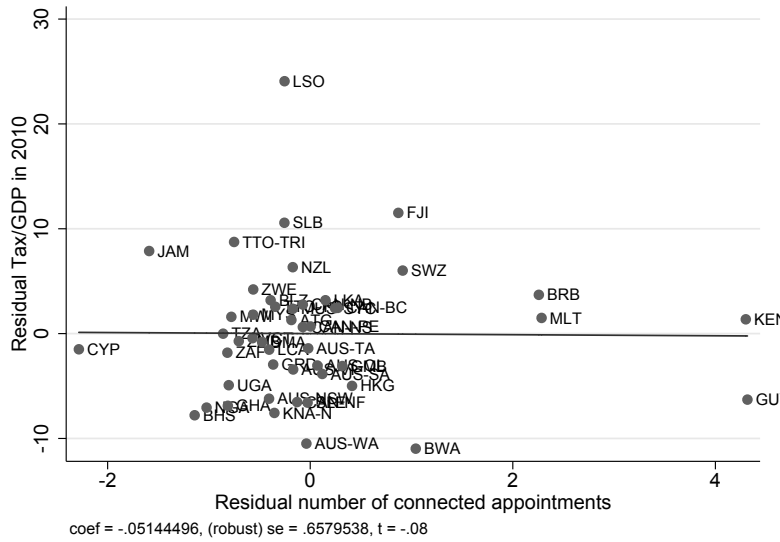
Notes: Distribution of tenure length for completed governorships between 1854-1966. The statutory term limit is six years.

Figure 6: Current Tax/GDP and connected appointments under patronage



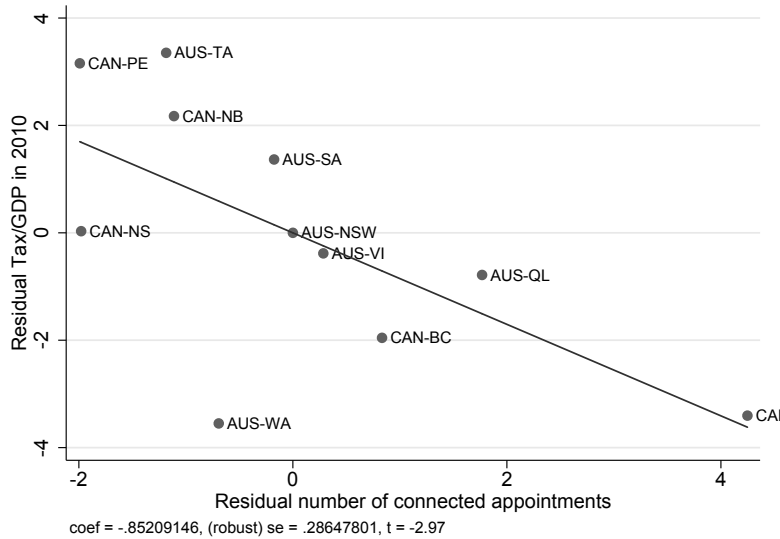
Notes: Second stage (IV) partial correlation between the regional Tax/GDP ratio in 2010 and the number of connected appointments 1854-1930. Controlling for the years under British rule, (log) initial governorship salary, the share of land area within tropics and absorbing continent fixed effects. Robust standard errors.

Figure 7: Modern Tax/GDP and connected appointments in the post-patronage period



Notes: Second stage (IV) partial correlation between the regional Tax/GDP ratio in 2010 and the number of connected appointments 1930-1966. Controlling for the years under British rule, (log) initial governorship salary, the share of land area within tropics and absorbing continent fixed effects. Robust standard errors.

Figure 8: Regional Tax/GDP and connected appointments (Within Canada and Australia)



Notes: Second stage (IV) partial correlation between the regional Tax/GDP ratio in 2010 and the number of connected appointments 1854-1930. Controlling for the years under British rule, (log) initial governorship salary, the share of land area within tropics and absorbing country dummy (Australia/Canada). Robust standard errors.

10 Tables

Table 1: Descriptive characteristics of governors and British colonies

Panel A:	(1)	(2)	(3)	(4)	(5)	(6)
Governor characteristics	Pooled years		By year			
	Mean	SD	1860	1900	1930	1960
Peerage	0.085	0.280	0.047	0.157	0.027	0.000
Civil servant	0.843	0.363	0.809	0.921	0.810	1.000
Military	0.439	0.496	0.416	0.424	0.333	0.200
Politician	0.087	0.283	0.166	0.131	0.027	0.000
Eton	0.109	0.312	0.125	0.068	0.068	0.111
Oxford	0.178	0.383	0.136	0.151	0.303	0.100
Cambridge	0.150	0.358	0.103	0.171	0.242	0.600
Age at entry	48.652	8.990	41.600	46.078	50.800	48.900
Observations	456 (330)		42 (22)	38 (29)	37 (29)	10 (9)
Panel B:	(7)	(8)	(9)	(10)	(11)	(12)
Colony characteristics	Pooled years		By year			
	Mean	SD	1860	1900	1930	1960
(log) Total revenue	12.309	2.185	10.850	12.638	13.135	15.961
- Share customs revenue	0.470	0.206	0.550	0.467	0.431	0.575
(log) Total expenditure	12.333	2.166	10.879	12.551	13.236	15.964
(log) Population	11.689	1.995	10.823	12.037	12.071	13.052
(log) Governorship salary	7.928	0.795	7.739	7.961	8.078	8.877
Area tropics	0.652	0.423	0.564	0.591	0.720	0.742
(log) Distance from London	8.562	0.612	8.464	8.608	8.567	8.577
Observations	3,510 (2,595)		-	-	-	-
Number of colonies	70 (54)		42 (28)	39 (30)	37 (30)	10 (3)

Notes: Panel A reports descriptive governor characteristics for all years, and 1860, 1900, 1930 and 1960. Peerage is a dummy that is 1 if the governor is a Duke, Marquess, Earl, Viscount or Baron. Civil servant/military/politician are dummies that are 1 if the governor served as a civil servant/in the military/as a politician before assuming the first governorship. Eton/Oxford/Cambridge are dummies that are 1 if the governor was educated in the named institutions. Age at entry is the age of the governor at time of first governorship. Panel B reports descriptive colony-level statistics. Total revenue and expenditures are in nominal terms. Share of customs revenue is the share of external (trade) taxes over total revenue. Area tropics is the share of the colony within the tropics. Distance from London is the distance from London to the nearest port in the colony. Number in parentheses denotes the minimum number of observations across all variables.

Table 2: Governor salary and connectedness to Secretary of State

	(1)	(2)	(3)	(4)	(5)	(6)
	log Governor salary in GBP					
Mean of dep. var	7.929	7.929	7.929	7.929	7.929	7.929
No. colonies served	0.221*** (0.035)	0.222*** (0.035)	0.223*** (0.035)	0.222*** (0.035)	0.224*** (0.035)	0.224*** (0.035)
Shared Ancestors	0.103** (0.047)				0.093** (0.046)	
Both Aristocrats		0.214* (0.124)			0.175 (0.121)	
Both Eton			0.132* (0.077)		0.117 (0.081)	
Both Oxbridge				0.072 (0.047)	0.074 (0.045)	
Connected						0.098*** (0.036)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Governor FEs	Yes	Yes	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,510	3,510	3,510	3,510	3,510	3,510

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) salary in GBP paid to a governorship in a given year. No. of colonies served is the number of colonies the governor has served in up to the given year. Connected is a dummy that is 1 if the governor and Secretary of State share either common ancestry, are both aristocrats, both went to Eton or studied at Oxford or Cambridge. Spell length FEs are dummies for each year of the term. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Transfers and connectedness to Secretary of State

	(1)	(2)	(3)	(4)	(5)
	log Governor salary (GBP)		Fixed colony characteristics		
			log Initial revenue	Area in tropics	log Distance London
Mean of dep. var	7.929	7.929	10.74	0.653	8.563
No. colonies served	0.224*** (0.035)	0.034 (0.019)	0.737*** (0.095)	-0.017 (0.025)	0.063** (0.029)
Connected	0.098*** (0.036)	0.011 (0.017)	0.177* (0.099)	0.014 (0.029)	-0.019 (0.033)
Year FEs	Yes	Yes	Yes	Yes	Yes
Governor FEs	Yes	Yes	Yes	Yes	Yes
Colony FEs	-	Yes	-	-	-
Spell length FEs	Yes	Yes	Yes	Yes	Yes
Observations	3,510	3,510	3,510	3,510	3,510

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) salary in GBP paid to a governorship in a given year. No. of colonies served is the number of colonies the governor has served in. Connected is a dummy that is 1 if the governor is connected to the Secretary of State, defined as either sharing ancestry, both belonging to the peerage or having attended the same elite schools (Eton/Oxford/Cambridge). Initial revenue is the (log) initial revenue in GBP of the colony, area in tropics is the share of the colony's land area in tropics and distance to London is the (log) distance in km to London. Spell length FEs are dummies for each year of the term. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Warren Fisher 1930 - Removal of Patronage

	(1)	(2)	(3)	(4)
Mean of dep. var	7.929	7.929	7.929	7.929
Connected	0.097*** (0.036)	0.127*** (0.043)	0.205*** (0.059)	0.169*** (0.060)
Reform dummy × Connected		-0.123** (0.056)	-0.222*** (0.079)	-0.182** (0.084)
Connected + Reform dummy × Connected	-	0.004 (0.040)	-0.017 (0.043)	-0.013 (0.048)
Year FEs	Yes	Yes	Yes	Yes
Governor FEs	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes
Connected × Trend (centered 1930)	-	-	Yes	Yes
Reform dummy × Governor characteristics	-	-	-	Yes
Observations	3,510	3,510	3,510	3,027

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) salary of a governorship. Connected is a dummy that is 1 if the governor is connected to the Secretary of State. Reform dummy is a dummy that is 1 after 1930. Time-varying controls comprise the number of colonies the governor has served in. Governor characteristics comprise: dummies for previous career track prior to first governorship (civil servants, military, politician) and number of colonies served. Connected × Trend interacts the connected dummy with a linear time trend which is centered around 1930. Controls × connected interacts all these controls with the connected dummy. Spell length FEs are dummies for each year of the term. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Revenue performance and connectedness to Secretary of State

Panel A: Revenue	(1)	(2)	(3)	(4)
Colony-level Public Finance				
Public revenue				
	Overall		Trade	Internal
Mean of dep. var	12.31	12.31	11.47	11.58
Connected	-0.040**	-0.055***	-0.053**	-0.043
	(0.017)	(0.021)	(0.026)	(0.032)
Connected × Reform dummy		0.061*		
		(0.033)		
Connected + Connected × Reform dummy	-	0.005	-	-
		(0.026)		
Year FEs	Yes	Yes	Yes	Yes
Governor-Colony FEs	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes
Observations	3,510	3,510	2,670	2,652
Panel B: Expenditure	(5)	(6)	(7)	(8)
Public expenditure				
	Overall		Tax	Works
Mean of dep. var	12.33	12.37	9.015	10.32
Connected	-0.029	-0.042*	-0.089*	-0.107*
	(0.019)	(0.023)	(0.053)	(0.062)
Connected × Reform dummy		0.053		
		(0.034)		
Connected + Connected × Reform dummy	-	0.010	-	-
		(0.025)		
Year FEs	Yes	Yes	Yes	Yes
Governor-Colony FEs	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes
Observations	3,510	3,510	1,742	2,588

Notes: Unit of observation is the governor-year. Sample period 1854-1966. The dependent variable in Panel A is the (log) total revenue (Column 1-2), trade (customs) revenue (Column 3) and internal revenue (Column 4). Panel B reports the overall expenditure (Column 5-6), expenditures for revenue services (Column 7) and public works (Column 8). Columns 2 and 6 interact connectedness with a reform dummy that is 1 after 1930. Connected is a dummy that is 1 if the governor is connected to the Secretary of State. Time-varying controls comprise the number of colonies the governor has served in. Spell length FEs are dummies for each year of the term. Standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Tax ordinances, exemptions and connectedness to Secretary of State

	(1)	(2)	(3)	(4)	(5)	(6)
	Legislation	Broken down by ordinance type				
	ordinances	Direct tax	Customs	Exemptions	Social	Works
Mean of dep. var	0.020	0.0105	0.0140	0.226	0.0122	0.00698
Connected	0.085** (0.037)	0.048 (0.031)	0.068** (0.031)	0.202*** (0.063)	0.004 (0.027)	-0.011 (0.019)
Connected × Reform dummy	-0.083** (0.037)	-0.051 (0.032)	-0.066** (0.031)	-0.369*** (0.137)	-0.003 (0.029)	0.013 (0.019)
Connected + Connected × Reform dummy	0.001 (0.005)	-0.003 (0.004)	0.002 (0.004)	-0.167 (0.125)	0.001 (0.005)	0.002 (0.003)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Governor-Colony FEs	Yes	Yes	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes
Data source	National Archives			Blue Book	National Archives	
Observations	573	573	573	405	573	573

Notes: Unit of observation is the governor-year. The sample is restricted to the “switchers” of serving governors who experience a change in connections within the position. In Column 1, the dependent variable is the number of ordinances issued, as recorded by the National Archive catalogue. Columns 2-6 provide more detailed breakdowns. This is broken down by topic of the ordinances: tax related (Column 2), customs related (Column 3), social i.e. education/health/poor relief related (Column 5) and public works related (Column 6). Column 4 is a dummy that is 1 if an exemption was added to the import tariff schedule. Connected is a dummy that is 1 if the governor is connected to the Secretary of State. Reform dummy is a dummy that is 1 after 1930. Time-varying controls comprise the number of colonies the governor has served in. Spell length FEs are dummies for each year of the term. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Alternative performance measures and connectedness

	(1)	(2)	(3)	(4)
	Social unrest	Parliamentary debates Mentioned	Sentiment	Highest award
Mean of dep. var	0.049	0.724	0.097	0.021
Connected	0.038* (0.022)	0.029 (0.028)	-0.045* (0.024)	-0.031** (0.015)
Connected × Reform dummy	-0.037* (0.022)	-0.040 (0.031)	0.039 (0.029)	-0.007 (0.028)
Connected + Connected × Reform dummy	0.001 (0.002)	-0.010 (0.015)	-0.006 (0.016)	-0.037 (0.024)
Year FE	Yes	Yes	Yes	Yes
Governor-Colony FEs	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes
Data source	News	Who's Who	Hansard	
Observations	3,510	3,510	2,481	3,510

Notes: Unit of observation is the governor/state-year. Sample period 1854-1966. Dependent variables are a dummy for reported unrests in London newspapers (Column 1), whether a colony has been mentioned in the parliamentary debates (Column 2), the mean sentiment in the debates (Column 3) and a dummy for being awarded a GCMG/GCB, the highest distinction class (Column 4). Connected is a dummy that is 1 if the governor is connected to the Secretary of State. Reform dummy is a dummy that is 1 after 1930. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Predicting connected appointments - First-stage

	(1)	(2)	(3)	(4)
	Connected	Connected years		
Mean of dep. var	0.304	1.460	1.457	1.423
Prob. connected appointment $t - 2$				0.233 (0.451)
Prob. connected appointment $t - 1$	0.215*** (0.065)	0.871*** (0.274)	0.715** (0.354)	0.808* (0.430)
Prob. connected appointment t				0.222 (0.352)
Colony FEs	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Prev. spell FEs	Yes	Yes	Yes	Yes
Observations	598	598	591	537

Notes: Unit of observation is the appointment. Sample period 1854-1966. Dependent variable connected is a dummy that is one if the governor was connected at time of appointment (Column 1) and the years the under a connected governors (Column 2-4). Prob. of connected appointment 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. Column 4 includes one period leads and lags. Controls comprise the (log) governor salary at the start of the appointment and the appointment spell length. Previous spell FEs are dummies for the previous appointment's length. Robust standard errors are clustered at the year and state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Public finance and the impact of connected appointments

	(1)	(2)	(3)	(4)	(5)	(6)
	Public finance growth over the appointment					
	Public revenue				Expenditure	
Mean of dep. var	0.173	0.173	0.173	12.45	0.173	0.166
Connected years	-0.007 (0.005)			-0.129 (0.091)		
Prob. connected appointment		-0.115** (0.054)	-0.092* (0.052)		-0.101* (0.057)	-0.055 (0.066)
Prob. connected × Reform dummy					0.054 (0.121)	0.021 (0.121)
Connected years + Connected years × Reform dummy	-	-	-	-	-0.047 (0.106)	-0.034 (0.099)
Estimation	OLS	OLS	OLS	IV	OLS	OLS
Colony FEs	No	No	Yes	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Prev. spell FEs	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F	-	-	-	4.506	-	-
Observations	598	598	591	591	591	589

Notes: Unit of observation is the appointment. Sample period 1854-1966. Dependent variable is the growth in public revenue (Columns 1-5) and the expenditures (Column 6) over the entire appointment. Connected years is the number of years the appointment was administered by a connected governor. Prob. of connected appointment 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. Reform dummy is a dummy that is 1 after 1930. Controls comprise the (log) salary at the start of the appointment, a linear time trend and spell length FEs. Previous spell FEs are dummies for the previous appointment's length. Robust standard errors are clustered at the year and state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Connected governors (pre/post patronage) and fiscal capacity in 2010

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share of tax revenue (% of GDP) - Subnational tax/GDP						
	All former colonies in 2010				Balanced sample		
					1990	2000	2010
Mean of dep. var	19.76	19.76	19.76	19.76	20.58	18.95	19.77
Connected years 1854-1930	-0.196 (0.173)	-0.722*** (0.263)		-0.721*** (0.261)	-0.744* (0.443)	-0.871** (0.443)	-0.936* (0.490)
Connected years 1931-1966			-0.051 (0.579)	-0.049 (0.969)	2.067* (1.140)	1.475 (0.937)	2.089** (1.018)
Estimation	OLS	IV	IV	IV	IV	IV	IV
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Continent FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-stat	-	25.497	7.767	3.850	3.243	3.243	3.243
Observations	48	48	48	48	29	29	29

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Connected years is the number of connected years the country/province was administered by connected governors between 1854-1930 (under patronage) and 1930-1966 (post-patronage). The dependent variables is the regional tax/GDP ratio in 2010 (Columns 1 to 4) as well as for a balanced sample for 1990, 2000 and 2010 (Columns 5 to 7). The number of connected years between 1854-1930/1931-1966 is instrumented by the expected number of connected appointments calculated based on the share of available governors the year before the appointment separately calculated for 1854-1930/1931-1966. All specifications include continent fixed effects for Africa, Europe, North America, Latin America, Asia and Oceania as well the years of British colonization, the initial governor salary of the historical colony and the share of the region/state within the tropics as controls. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Connected governors, revenue sources and the quality of tax systems in 2010

Panel A:	(1)	(2)	(3)	(4)	(5)	(6)
Share of revenue (% of GDP) in 2010						
	Tax revenue	Non-tax revenue	Direct tax	Indirect tax revenue		
				Total	GST	Trade
Mean of dep. var	20.62	5.326	9.897	10.64	7.473	3.258
Connected years 1854-1930	-0.427** (0.187)	0.170 (0.163)	0.092 (0.097)	-0.523*** (0.153)	-0.117 (0.096)	-0.488*** (0.135)
Connected years 1931-1966	0.426 (0.597)	-0.601** (0.292)	0.220 (0.242)	0.010 (0.417)	0.164 (0.252)	-0.102 (0.399)
Estimation	IV	IV	IV	IV	IV	IV
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Continent FEs	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-stat	3.850	3.850	3.850	3.850	3.799	3.799
Data source	International Centre for Tax and Development (ICTD)					
Observations	48	48	48	48	47	47
Panel B:	(7)	(8)	(9)	(10)	(11)	(12)
	Tariff rate	# tariff lines	Customs misreporting	Customs hours	Tax hours	Trade/GDP
Mean of dep. var	7.061	74.765	12.030	3.511	5.052	0.898
Connected years 1854-1930	-0.442** (0.218)	4.234*** (1.070)	0.088*** (0.028)	0.060*** (0.022)	0.025* (0.014)	-0.053*** (0.017)
Connected years 1931-1966	0.483 (0.299)	-4.730 (3.552)	0.005 (0.061)	-0.083 (0.059)	0.017 (0.053)	0.042 (0.047)
Estimation	IV	IV	IV	IV	IV	IV
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Continent FEs	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-stat	4.373	2.493	3.659	3.817	3.817	3.850
Data source	World Integrated Trade Solution			Doing Business		WB
Observations	48	43	45	46	46	48

Notes: Unit of observation is the post-independence country. Connected years is the number of connected years the country was administered by connected governors between 1854-1930 (under patronage) and 1930-1966 (post-patronage). The dependent variables are: the share of tax revenue over GDP (Column 1), the share of non-tax (including natural resources) revenue over GDP (Column 2), the share of direct tax (Column 3), the share of indirect taxes (Column 4) and its breakdown by goods and services tax (Column 5) and trade taxes (Column 6). In Panel B, the dependent variables are the weighted tariff rate (Column 7), the total number of tariff lines in 1,000 (Column 8), the (log) mean of absolute discrepancy between import values reported at the importing and exporting country (Column 9). Customs hours is the (log) hours needed to clear customs (Column 10). Tax hours is the (log) hours needed to comply with tax regulation (Column 11) and Trade/GDP is the sum of the import and export value divided by GDP (Column 12). The number of connected years is instrumented by the expected number of connected appointments calculated based on the share of available governors the year before the appointment. Controls include the years of British colonization, the initial governor salary of the historical colony and the share of the region/state within the tropics. Continent fixed effects include dummy for Africa, Europe, North America, Latin America, Asia and Oceania. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Figures: Additional material

Figure A1: Sample of comparative revenue statement for Fiji 1854 (Blue Book)

[30] COMPARATIVE YEARLY STATE.

Specify each separate Tax or Duty.	Amount collected in the Year 1854 in Pounds Sterling.		Amount collected in the Year 1853 in Pounds Sterling.	
	£	s. d.	£	s. d.
Customs Revenue	69,412	5 7	69,756	9 4
Light, Duval. . . .	2,358	12 7	2,437	10 3
Rents of Crown Lands Proceeds of Crown Land and Rents Redeemed	821	14 3	721	11 7
Licence Fund Fines and Forfeitures. . .	789	1 5	1,152	8 2
Fees from Public Offices From North American Clergy Estimate. . .	334	18 7	565	17 8
Raised by Loan under Colonial Acts. . . .	6,000	" "	2,425	" "
Treasury Notes. . .	"	" "	3,466	13 4
Loan from Savings Bank	"	" "	12,403	9 4
From Estate of late Treasurer	"	" "	300	" "
Postal Revenue. . .	261	1 4	309	8 2
Patents.	10	" "	20	" "
Cashier of Savings Bank	65	" "	"	" "
Telegraph (Labourers) Through Post Office. .	620	5 5	"	" "
Norwegian Brig Bunde	39	4	"	" "
Total	81,007	6 10	93,857	17 10

Notes: Sample of comparative revenue statement for Fiji 1854 from the Blue Book. Each row records the revenue for a specific source (e.g. customs revenue). The two columns report the revenue in the current (1854) and the previous year (1853).

Figure A2: Sample of trade tax exemption laws (Blue Book)

9

Specification of Taxes, Duties, &c.

EXEMPTIONS.—Continued.

Machinery.—Under Ordinance No. 2 of 1859, and Minute of 8th June, 1865.

Specification of Taxes, Duties, &c.	Act of Colonies which levied.
All descriptions of Marine, Locomotive, Stationary, and Portable Steam Engines, their Boilers, and Gear, whole, or in parts	} Free.
Machinery adapted to Windmills	
Every description of Machinery for lifting, forcing, and conducting water	
Machinery for the manufacture of Sugar, Hydraulic, and Screw presses	
Cranes, Crab Winches, and Screw Jacks	
Pulpers, Peelers, and Winnowing Machines, whole, or in parts	
Tile, Brick, and Pipe making machines	
Printing and Lithographic Presses	
Fibre, Cotton Carding, Weaving and Spinning Machines	
Nasmyth's Hammers	
Lathes	
Punching, Drilling, Shearing, Planing, Sawing, and Screw making Machines	
All materials and plant imported for the construction of the Railway between Colombo and Kandy, certified by the Chief Resident Engineer as required for that purpose	
Manures	
Paper	
Pepper, Black	
Regimental Clothing, Necessaries, and Accoutrements, imported for the use of Her Majesty's Land and Sea Forces	
Seeds intended for Agricultural and Horticultural purposes, including Plants	
Specimens illustrative of Natural History	
Tanks (Iron)	
Whale Oil	

EXPORT DUTIES.

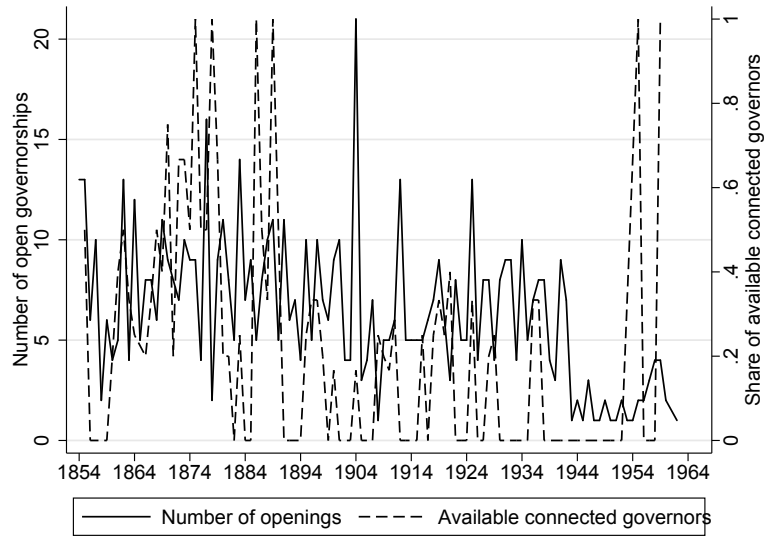
DUTIES OF CUSTOMS payable on Goods, Wares, and Merchandize, being the Growth, Produce, or Manufacture of the Island of Ceylon, exported to parts beyond seas.

ARTICLES.	RATE OF DUTY.		
	£	s.	d.
Arrecanuts	-	-	- the cwt. 0 0 4
Cinnamon	-	-	- the Bale of 100 lbs. net. 0 2 0
Coffee	-	-	- the cwt. 0 1 0
Coin Yarn, Fibre, Rope, and Junk	-	-	- the cwt. 0 0 3

Ordinances No. 18 of 1852, No. 9 of 1853, No. 2 of 1856, No. 2 of 1859, and No. 3 of 1862.

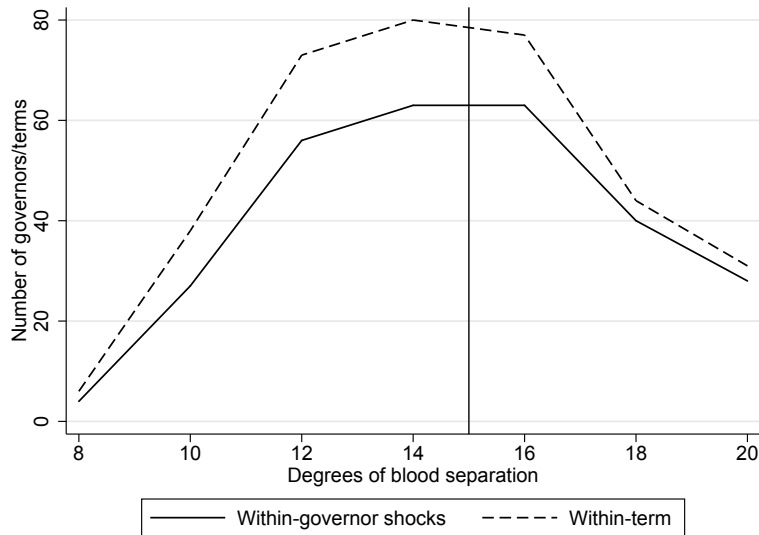
Notes: Sample of customs tax exemptions laws from the 1869 Ceylon Blue Book.

Figure A3: Number of openings and share available governors who are connected



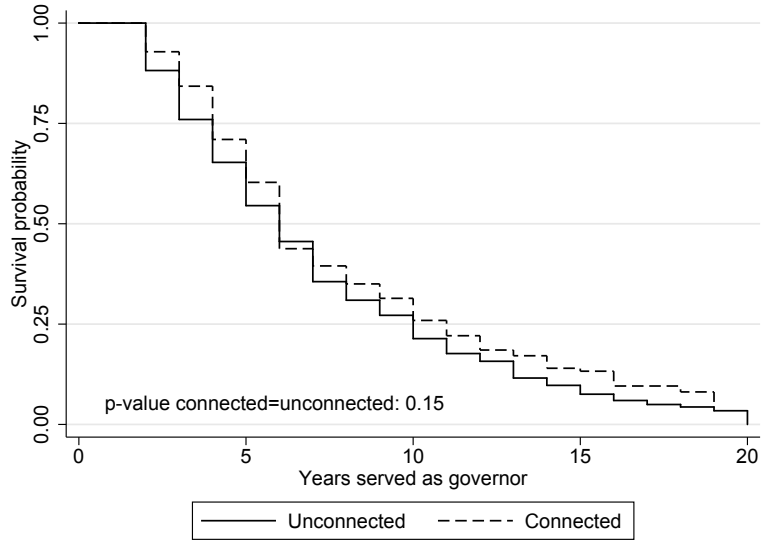
Notes: Number of governorships that need to be filled (i.e. are beyond the statutory six year term limit) and the share of available connected governors. The share of available connected governors is defined as the proportion of serving governors who are connected and beyond the statutory six year term limit.

Figure A4: Size of switcher sample and cut-off for shared ancestry



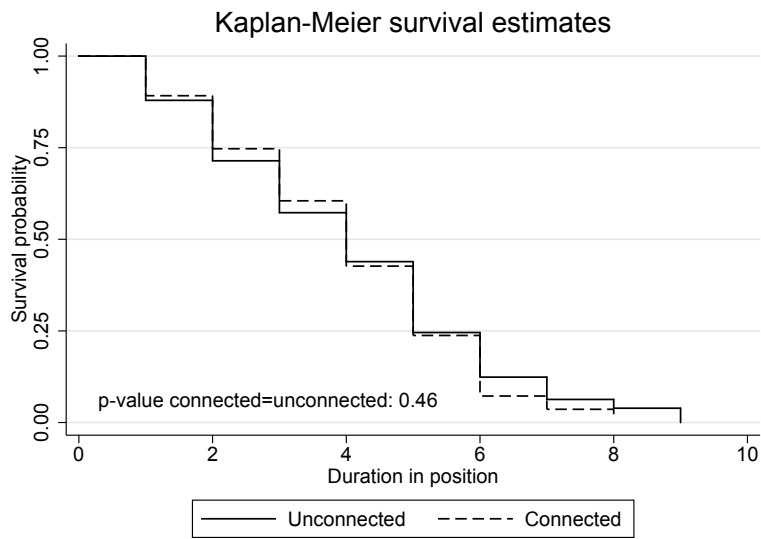
Notes: Number of governors/governor-colony spells that experience a within-shock to connections as a function of the cut-off for connectedness

Figure A5: Retirement by connectedness - Survival estimates



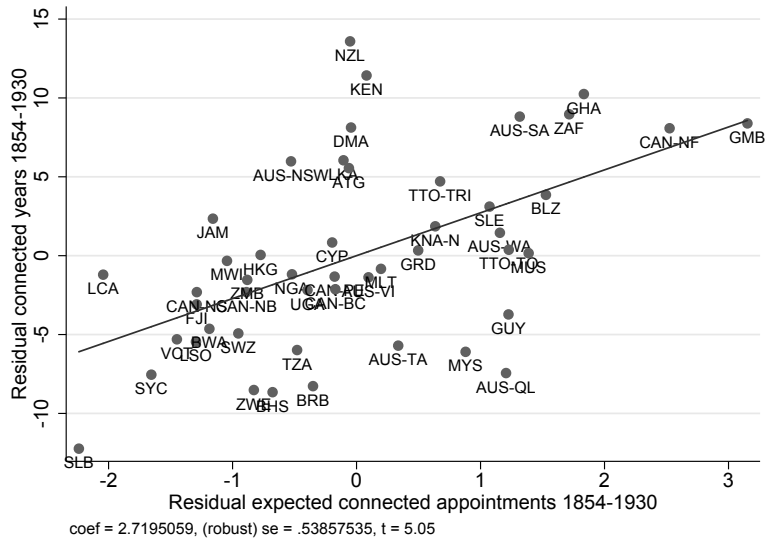
Notes: Kaplan-Meier survival estimates. Absorbing state is retirement from Colonial Office. Reporting the p -value for test of equality of survivor functions.

Figure A6: Exit (governor-colony) by connectedness - Survival estimates



Notes: Kaplan-Meier survival estimates. Absorbing state is exit from position. Reporting the p -value for test of equality of survivor functions.

Figure A7: Connected years and expected number of appointments (First stage)



Notes: Partial correlation between the connected years and the expected number of appointments 1854-1930, first-stage, controlling for the years under British rule, (log) initial governorship salary, the share of land area within tropics and absorbing continent fixed effects. Robust standard errors.

Appendix Tables: Additional material

Table B1: British colonies and territories ($N = 70$)

Colony	Start	End	Modern territory (+ marks still dependent)
Antigua	1854	1871	Part of Antigua & Barbuda
Bahamas	1854	1964	Bahamas
Barbados	1854	1884	Barbados
Basutoland	1884	1946	Lesotho
Bechuanaland	1891	1941	Botswana
Bermuda	1854	1941	Bermuda+
British Columbia	1860	1866	Province of Canada
British Guiana	1854	1964	Guinea
British Honduras	1854	1942	Honduras
Cape of Good Hope	1854	1908	Part of South Africa
Cayman Islands	1919	1939	Cayman Islands+
Ceylon	1854	1944	Sri Lanka
Cyprus	1879	1955	Cyprus
Dominica	1856	1932	Dominica
Falkland Island	1854	1959	Falkland Island (Islas Malvinas)+
Fiji	1876	1940	Fiji
Gambia	1854	1945	Gambia
Gibraltar	1854	1947	Gibraltar+
Gold Coast	1850	1946	Ghana
Grenada	1854	1946	Grenada
Heligoland	1854	1889	Part of Germany
Hong Kong	1854	1959	Hong Kong (SAR, PR China)
Ionian Islands	1854	1863	Part of Greece
Jamaica	1854	1960	Jamaica
Kenya	1922	1962	Kenya
Labuan	1856	1887	Part of Malaysia
Lagos	1862	1904	Part of Nigeria
Leeward Islands	1885	1945	Dissolved into Antigua & Barbuda, British Virgin Islands, Montserrat, St. Kitts & Nevis, Anguilla and Dominica
Malta	1854	1960	Malta
Mauritius	1854	1946	Mauritius
Montserrat	1858	1888	Montserrat+
Natal	1854	1907	Part of South Africa

Nevis	1854	1882	St. Kitts & Nevis
New Brunswick	1854	1865	Province of Canada
New South Wales	1854	1901	State of Australia
New Zealand	1854	1920	New Zealand
Newfoundland	1855	1932	Province of Canada
Nigeria	1914	1939	Nigeria
Northern Nigeria	1900	1913	Part of Nigeria
Northern Rhodesia	1924	1948	Zambia
Nova Scotia	1854	1866	Province of Canada
Nyasaland	1903	1938	Malawi
Palestine	1921	1944	Israel, State of Palestine
Prince Edward Island	1854	1871	Province of Canada
Queensland	1860	1901	State of Australia
Seychelles	1903	1939	Seychelles
Sierra Leone	1854	1943	Sierra Leone
Solomon Islands	1920	1941	Solomon Islands
Somaliland	1902	1938	Somalia
South Australia	1854	1902	State of Australia
Southern Nigeria	1900	1913	Part of Nigeria
Southern Rhodesia	1924	1932	Zimbabwe
St. Christopher	1854	1893	St. Kitts & Nevis
St. Helena	1854	1958	St. Helena, Ascension & Tristan da Cunha+
St. Lucia	1854	1959	St. Lucia
St. Vincent	1854	1986	St. Vincent & Grenadines
Straits Settlements	1865	1938	Malaysia
Swaziland	1906	1947	Swaziland
Tanganyika	1920	1961	Tanzania
Tasmania	1854	1909	State of Australia
Tobago	1854	1898	Part of Trinidad & Tobago
Trinidad	1854	1899	Part of Trinidad & Tobago
Trinidad & Tobago	1899	1945	Trinidad & Tobago
Turks & Caicos	1851	1946	Turks & Caicos
Uganda	1901	1945	Uganda
Vancouver Island	1862	1863	Part of Canada
Victoria	1855	1899	State of Australia
Virgin Islands	1856	1932	British Virgin Islands+
Western Australia	1854	1913	State of Australia
Zululand	1887	1986	Part of South Africa

Table B2: Within-governor - switcher sample

Average for	(1)	(2)	(3)
	Demeaned within governor		<i>p</i> -value
	Connected	Unconnected	diff
Total years served	0.054	-0.072	0.764
mean: 7.379	(4.619)	(4.290)	
Duration in position	0.049	-0.066	0.357
mean: 2.369	(1.943)	(1.823)	
Transfer	-0.008	0.011	0.191
mean: 0.108	(0.289)	(0.322)	
Retire	-0.007	0.009	0.322
mean: 0.098	(0.282)	(0.301)	
Exit	-0.015	0.021	0.065*
mean: 0.199	(0.381)	(0.412)	
Observations	559	418	977 (28%)
No. governors			96 (21%)

Notes: Average characteristics (demeaned within governor) for the same governor when connected and unconnected. Showing mean and standard deviations (in parentheses). Total years served is the total years served as a governor in the Colonial Office. Duration in position is the years in the current governorship. Transfer is a dummy that is 1 if the governor was transferred to another colony. Retire is a dummy that is 1 if the governor exited the Colonial Office. Exit is a dummy that is 1 if the governor either retired or transferred. *p*-value for mean comparison is computed with robust standard errors, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Within-appointment - switcher sample

	(1)	(2)	(3)
Average for	Demeaned within position Connected	Unconnected	<i>p</i> -value diff
Total years served mean: 5.907	0.012 (1.898)	-0.016 (1.800)	0.876
Duration in position mean: 2.543	0.010 (1.877)	-0.014 (1.782)	0.895
Transfer mean: 0.086	-0.003 (0.252)	0.004 (0.264)	0.718
Retire mean: 0.104	-0.015 (0.267)	0.021 (0.309)	0.142
Exit mean: 0.182	-0.017 (0.360)	0.023 (0.397)	0.183
Observations	333	248	581 (17%)
No. governors			89 (20%)
No. governor-colony			112 (15%)

Notes: Average characteristics (demeaned within governor-colony/appointment) for the same governor in the same colony when connected and unconnected. Showing mean and standard deviations (in parentheses). Total years served is the total years served as a governor in the Colonial Office. Duration in term is the years in the current governorship. Transfer is a dummy that is 1 if the governor was transferred to another colony. Retire is a dummy that is 1 if the governor exited the Colonial Office. Exit is a dummy that is 1 if the governor either retired or transferred. *p*-value for mean comparison is computed with robust standard errors, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. $p < 0.01$.

Table B4: Change in Secretary of State, political turnover and colony performance

	(1)	(2)	(3)	(4)
	New Secretary of State			
Mean of dep. var	0.366	0.366	0.361	0.361
New Party $t - 1$	0.462*** (0.11)			0.485** (0.23)
New Prime Minister $t - 1$		0.336*** (0.10)		0.007 (0.21)
Revenue growth $t - 1$			0.462 (0.94)	0.702 (0.96)
Decade FEs	11	11	11	11
Linear trend	Yes	Yes	Yes	Yes
Observations	109	109	108	108

Notes: Unit of observation is the year. Sample period 1854-1966. Dependent variable is a dummy for whether a new Secretary of State was appointed in given year. New party (New Prime Minister) is a dummy if the ruling party (prime minister). Revenue growth is the average revenue growth in the colonies. All explanatory variables are lagged (contemporaneous effects are all insignificant). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B5: Connectedness between Secretary of State and governor: Correlation matrix

	(1)	(2)	(3)	(4)	(5)
	Shared ancestry	Both aristocrats	Both Etonian	Both Oxbridge	Connected
(1) Shared ancestry	1.000	0.424	0.135	0.048	0.818
(2) Both aristocrats	0.424	1.000	0.252	0.120	0.392
(3) Both Etonian	0.135	0.252	1.000	0.083	0.273
(4) Same Oxbridge	0.048	0.120	0.083	1.000	0.482
(5) Connected	0.818	0.392	0.273	0.482	1.000

Notes: Unit of observation is the Secretary of State-governor pair ($N = 1,518$). Sample period 1854-1966. Reporting the correlation coefficient between the different measures of connectedness. Connected is the combined dummy that is 1 if the governor and Secretary of State share either common ancestry, are both aristocrats, both went to Eton or studied at Oxford or Cambridge.

Table B6: Determinants of governor salaries

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: log Governor salary in GBP					
Mean of dep. var	7.929	7.922	8.262	7.929	8.250	8.250
log Revenue in GBP	0.355*** (0.022)				0.279*** (0.042)	0.276*** (0.043)
log Population		0.295*** (0.041)			0.064* (0.035)	0.082** (0.037)
log Settler mortality			-0.113*** (0.040)		-0.001 (0.036)	-0.054 (0.055)
log Distance to London				0.164 (0.183)	-0.083 (0.131)	-0.402 (0.337)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Continent FEs	-	-	-	-	-	Yes
Observations	3,510	3,270	2,213	3,510	2,096	2,096
Within R^2	0.768	0.531	0.106	0.0136	0.730	0.760

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) salary in GBP paid to a governorship in a given year. (log) Revenue is the total annual revenue in the colony. (log) Population is the total population size in the colony. (log) Settler mortality is the log settler mortality rate from Acemoglu et al. (2001). (log) distance to London is the log distance (in km) to London from the colony's capital to London. Continent fixed effects include dummy for Africa, Europe, North America, Latin America, Asia and Oceania. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B7: Descriptive statistics between within-governor switchers and always/never connected

	(1)	(2)	(3)	(4)
	Full governor sample $N = 456$		Mean difference connection switching ($N = 96$) –	
	Mean	Standard deviation	Always connected	Never connected
Peerage	0.085	0.280	-0.422***	0.024*
Civil servant	0.274	0.446	0.173**	0.198***
Military	0.360	0.480	0.275**	-0.065
Politician	0.087	0.283	-0.186	0.058**
Eton	0.109	0.312	-0.126*	0.154***
Oxford	0.178	0.383	-0.038	0.250***
Cambridge	0.150	0.358	-0.001	0.138***
Age at entry	49.153	9.855	-0.219	-1.638
Age at retirement	56.697	9.054	3.902***	1.663
Years served	7.697	5.410	3.537***	3.036***
Colonies served	1.793	1.263	0.858***	1.832***
Average salary	3655.38	2148.62	-709.848**	1213.911***
Highest salary	4085.20	2379.15	-495.494	1585.237***
Lowest salary	3205.70	2158.85	-1128.178***	738.612***
Award received	0.020	0.058	-0.009	0.003
Years connected	2.317	4.368	-0.817	5.822***

Notes: Descriptive governor characteristics: mean, standard deviation (in parentheses) and mean comparison between switchers and always connected governors (Column 3) and never connected governors (Column 4). Peerage is a dummy that is 1 if the governor is a Duke, Marquess, Earl, Viscount or Baron. Civil servant/military/politician are dummies that are 1 if the governor served as a civil servant/in the military/as a politician before assuming the first governorship. Eton/Oxford/Cambridge are dummies that are 1 if the governor was educated in the named institutions. Age at entry (retirement) is the age of the governor at time of first (last) governorship. Years served is the total number of years served as governor. Colonies served is the number of colonies served as governor. Average (highest/lowest) salary is the mean (highest/lowest) salary earned throughout the governor career. Award received is the share of governors who received the highest distinction of GCMG/GCB. Years connected is the total number of years connected to the Secretary of State. Number in parentheses denotes the minimum number of observations across all variables.

Table B8: Promotions, connectedness and revenue performance

	(1)	(2)	(3)	(4)	(5)
		Promoted		Retire	Transfer
Mean of dep. var	0.0623	0.0623	0.0623	0.129	0.0702
Connected	0.023** (0.009)	0.024** (0.009)	0.024** (0.009)	-0.028** (0.011)	0.029*** (0.010)
Average growth		0.028 (0.060)	0.028 (0.065)	0.359*** (0.103)	0.000 (0.069)
Average growth \times Connected			-0.001 (0.147)	0.026 (0.204)	0.065 (0.158)
Colony FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Spell length dummies	Yes	Yes	Yes	Yes	Yes
Observations	3,510	3,311	3,311	3,311	3,311

Notes: Replicating Jia et al. (2015). Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) salary in GBP paid to a governorship in a given year. No. of colonies served is the number of colonies the governor has served in up to the given year. Connected is a dummy that is 1 if the governor is connected to the Secretary of State. Revenue growth is the growth in revenue in the colony of the serving governor up to the given year. Robust standard errors in parentheses, clustered at the bilateral governor-secretary of state level. Revenue growth is defined as the (log) change in revenue between last year and the first year of appointment. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: Descriptive statistics: Modern outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>N</i>	Mean	SD	Percentile		
				25%	50%	75%
Connected years 1854-1930	48	12.979	12.767	1	9.5	19.5
Exp. # connected appointments 1854-1930	48	2.261	1.535	0.633	2.95	3.516
Connected years 1931-1966	49	2.959	4.082	0	0	5
Exp. # connected appointments 1931-1966	49	0.163	0.354	0	0	0.333
Tariff rate (weighted) in % (WITS)	48	7.060	5.795	1.9	7.205	10.38
Number of tariff lines in 1,000 (WITS)	44	73.716	73.323	24.362	41.226	107.812
Avg import-export reporting gap (WITS)	45	12.025	1.657	10.817	11.511	13.889
(log) Clearing customs 2015 (DB)	48	3.467	1.391	2.944	3.705	4.479
(log) Paying taxes 2015 (DB)	48	5.055	0.479	4.700	4.985	5.322
Trade as share of GDP 2010 (WB)	48	89.806	63.187	55.108	74.847	103.458
Tax/GDP 2010 (subnational)	48	19.760	8.604	13.635	18.725	24.148
Tax/GDP 2010 (country-level, ICTD)	49	20.331	7.306	13.531	22.572	26.195
Non-tax revenue/GDP 2010 (ICTD)	49	5.707	4.838	1.950	5.092	7.401
Direct tax revenue/GDP 2010 (ICTD)	49	9.755	5.940	5.454	7.819	15.581
Indirect tax revenue/GDP 2010 (ICTD)	49	10.499	4.777	7.576	9.231	13.109
Goods and sales tax revenue/GDP 2010 (ICTD)	48	7.348	2.958	5.150	7.187	9.009
Trade tax revenue/GDP 2010 (ICTD)	48	3.231	3.899	0.430	1.825	5.457

Notes: Unit of observation is the region/state. Descriptive statistics for the cross-section of modern-day outcomes for the sample of independent states. Connected years is the number of years under a connected governor in the colonial period 1854-1930. Expected # connected appointments is the number predicted using the share of available governors. Tariff rate is the weighted average tariff rate. Number of tariff lines is the total number of tariff lines in 1,000. Average import-export reporting gap proxies for the extent of customs misreporting, calculated as: $\log(\sum_i^N N^{-1} |X_{is} - Z_{is}|)$. WITS = World Integrated Trade Solutions database. Clearing customs is the days needed to clear customs, defined as the average days to comply with border regulation for both import and exports. Paying taxes (hours) is the hours needed to comply with tax regulation. Trade as share of GDP is the total imports and exports divided by GDP in 2010. DB = Doing Business Indicators. Subnational Tax/GDP (Rev/GDP) in 2010 is the tax (public revenue) over GDP ratio in 2010. The remaining tax sources come from the ICTD = International Center for Tax and Development: country-level tax revenue over GDP, the share of non-tax (including natural resources) revenue over GDP, the share of direct tax, the share of indirect taxes and its breakdown by goods and services tax and trade taxes.

Table B10: Long-run impact of connectedness (First-stage)

	(1)	(2)	(3)	(4)
	Total connected years 1854-1930		1931-1966	
Mean of dep. var	12.98	12.98	2.875	2.875
Expected # connected appointments 1854-1930	2.720*** (0.539)	2.739*** (0.534)		-0.031 (0.272)
Expected # connected appointments 1931-1966		-0.857 (3.977)	3.734*** (1.340)	3.743*** (1.342)
Controls	Yes	Yes	Yes	Yes
Continent FEs	Yes	Yes	Yes	Yes
Observations	48	48	48	48

Notes: Unit of observation is the region/state. The dependent variable is the total number of years under connected governors between 1854-1930 (and after abolition of patronage 1931-1966). Expected # connected appointments is the expected number of connected appointments between 1854-1930 (1931-1966). Years of British colonization is the years under British rule. Area tropics is the share of land area that lies in the tropics. Initial governor salary is the (log) amount of the first governor salary fixed for the governorship. Continent fixed effects include dummy for Africa, Europe, North America, Latin America, Asia and Oceania. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: Connected governors and fiscal capacity in 2010 - Reduced form

	(1)	(2)	(3)	(4)	(5)	(6)
	Share of tax revenue (% of GDP) - Subnational 2010					
Mean of dep. var	19.76	19.76	19.76	19.76	19.76	19.76
Connected years 1854-1930	-0.196 (0.173)		-0.201 (0.170)			
Connected years 1930-1966		-0.177 (0.322)	-0.202 (0.340)			
Exp. connected years 1854-1930				-1.963** (0.805)		-1.973** (0.824)
Exp. connected years 1930-1966					-0.192 (2.457)	0.435 (2.158)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Continent FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48	48	48	48	48	48

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Connected years is the number of connected years the country/province was administered by connected governors between 1854-1930 (under patronage) and 1930-1966 (post-patronage). The dependent variables is the regional tax/GDP ratio in 2010 (Columns 1 to 4) as well as for a balanced sample for 1990, 2000 and 2010 (Columns 5 to 7). The expected number of connected appointments calculated based on the cumulative share of available governors the year before the appointment, calculated separately for 1854-1930/1931-1966. All specifications include continent fixed effects for Africa, Europe, North America, Latin America, Asia and Oceania as well the years of British colonization, the initial governor salary of the historical colony and the share of the region/state within the tropics as controls. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B12: GDP per capita and measures of institutional quality

	(1)	(2)	(3)	(4)	(5)
		State capacity measures 2010			
	log GDP pc 2010	Days enforce contract	Quality judicial	Days reg. property	Quality land admin
Mean of dep. var	2.227	2.227	6.311	2.534	9.110
Connected years 1854-1930	0.022 (0.037)	0.013 (0.011)	0.013 (0.011)	-0.009 (0.011)	0.001 (0.015)
Connected years 1931-1966	-0.059 (0.101)	-0.023 (0.027)	-0.023 (0.027)	0.018 (0.041)	0.015 (0.058)
Estimation	IV	IV	IV	IV	IV
Controls	Yes	Yes	Yes	Yes	Yes
Continent FEs	Yes	Yes	Yes	Yes	Yes
First-stage F-stat	4.426	4.426	4.426	4.426	4.462
Data source	PWT8.1		Doing Business		
Observations	44	48	48	48	48

Notes: Unit of observation is the post-independence country or sub-national province corresponding to the historical colony. Dependent variables are (log) GDP per capita (PWT8.1, rgdpna series) and Doing Business Indicators (Columns 2-5) for the (log) days needed to enforce contract, an index for the quality of judicial institutions, the days to register property and an index for the quality of the land administration. Connected years is the number of connected years in the colonial sample period 1854-1930. Controls include the years of British colonization, the initial governor salary of the historical colony and the share of the region/state within the tropics. Continent FEs include dummy for Africa, Europe, North America, Latin America, Asia and Oceania. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Tables: Robustness checks

Table C1: Robustness: Governor salary and connectedness, dropping connection types

	(1)	(2)	(3)	(4)	(5)
		log Governor salary in GBP			
Mean of dep. var	7.929	7.929	7.929	7.929	7.929
No. colonies served	0.223*** (0.035)	0.225*** (0.035)	0.224*** (0.035)	0.222*** (0.035)	0.222*** (0.035)
Connected	0.097*** (0.036)				
Connected excl. Ancestry		0.122*** (0.040)			
Connected excl. Aristocrats			0.114*** (0.036)		
Connected excl. Eton				0.076* (0.040)	
Connected excl. Oxbridge					0.098** (0.048)
Year FEs	Yes	Yes	Yes	Yes	Yes
Governor FEs	Yes	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes	Yes
Observations	3,510	3,510	3,510	3,510	3,510

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) salary in GBP paid to a governorship in a given year. No. of colonies served is the number of colonies the governor has served in up to the given year. Connected is a dummy that is 1 if the governor and Secretary of State share either common ancestry, are both aristocrats, both went to Eton or studied at Oxford or Cambridge. The remaining explanatory variables drop one type of connections from the combined measure in turn. Spell length FEs are dummies for each year of the term. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Robustness: Salary, connectedness to PM and heterogeneity

	(1)	(2)	(3)	(4)
	log Governor salary in GBP			
Mean of dep. var	7.929	7.929	7.929	7.929
Connected	0.097*** (0.036)	0.098*** (0.035)	0.101*** (0.036)	0.090** (0.040)
Connected to PM		0.076 (0.133)		
Connected × Election			-0.018 (0.021)	
Connected × Tory party				0.013 (0.040)
Controls	Yes	Yes	Yes	Yes
Governor FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Observations	3,510	3,510	3,510	3,510

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) salary in GBP paid to a governorship in a given year. Connected is a dummy that is 1 if the governor and Secretary of State share either common ancestry, are both aristocrats, both went to Eton or studied at Oxford or Cambridge. Connected to PM is the same measure for the governor and the Prime Minister in office. Election is a dummy that is 1 if there was a general election in the given year. Tory is a dummy that is 1 if the government in power is the Tory/Conservative party. The remaining explanatory variables drop one type of connections from the combined measure in turn. Spell length FEs are dummies for each year of the term. Controls are the no. of colonies served is the number of colonies the governor has served in up to the given year. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: Robustness: Fiscal performance - Growth rates

	(1)	(2)	(3)	(4)
	Revenue growth		Expenditure growth	
Mean of dep. var	0.045	0.045	0.045	0.045
Connected	-0.037** (0.017)	-0.042** (0.021)	-0.006 (0.020)	0.013 (0.024)
Reform dummy × Connected		0.023 (0.032)		-0.080** (0.038)
Connected + Connected × Reform dummy	-	-0.019 (0.024)	-	-0.066 (0.030)
Governor-Colony FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Observations	3,412	3,412	3,407	3,407

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the annual revenue growth (Columns 1-2) and expenditure growth (Columns 3-4). Connected is a dummy that is 1 if the governor is connected to the Secretary of State. Reform dummy is a dummy that is 1 after 1930. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C4: Robustness: Revenue performance - Subsamples

	(1)	(2)	(3)	(4)
	log Revenue in GBP			
	Drop moved immediately	Drop first&last year	Appointed connected	Appointed unconnected
Mean of dep. var	12.31	12.29	13.26	13.00
Connected	-0.053*** (0.020)	-0.073*** (0.024)	-0.058* (0.031)	-0.064** (0.031)
No. colonies served	0.068 (0.063)	0.322*** (0.052)	0.247** (0.099)	0.137 (0.089)
Governor-Colony FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Observations	3,465	2,002	987	985

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) total revenue. Connected is a dummy that is 1 if the governor is connected to the Secretary of State. Column 1 drops the switchers who move immediately after experiencing a shock to connections. Column 2 drops the first and last year of the appointment in the switcher sample. Column 3 is the sample of those who are appointed connected. Column 4 is the sample of those who are appointed unconnected. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C5: Robustness: Revenue performance - Bounding selective exit

	(1)	(2)	(3)	(4)	(5)
	log Revenue in GBP				log Exp
	Main	Trend	Trend+2%	Trend+4%	Trend+4%
Connected	-0.040** (0.017)	-0.033* (0.020)	-0.038* (0.020)	-0.043** (0.020)	-0.033* (0.020)
No. colonies served	0.068 (0.063)	0.066 (0.063)	0.065 (0.063)	0.064 (0.063)	0.082 (0.059)
Governor-Colony FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes	Yes
Observations	3,510	3,622	3,622	3,622	3,622

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) total revenue in Columns 1-4. Connected is a dummy that is 1 if the governor is connected to the Secretary of State. Column 2 assumes that revenue growth follows the pre-trend after the governor has exited. Column 3 and 4 assume growth increases by 2% and 4% points above the trend. Column 5 uses (log) total expenditure as the dependent variable. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C6: Robustness: Alternative clustering of standard errors

	(1)	(2)	(3)	(4)	(5)	(6)
	log Salary		log Revenue		log Expenditure	
Mean of dep. var	7.929	7.929	12.31	12.31	12.33	12.33
Connected	0.097***	0.127***	-0.040**	-0.055***	-0.029	-0.042*
<i>Standard errors</i>						
Governor-Secretary of State (dyadic)	(0.036)	(0.043)	(0.017)	(0.021)	(0.019)	(0.023)
Governor & Secretary of State (2 way)	(0.039)	(0.044)	(0.021)	(0.023)	(0.021)	(0.026)
Dyadic & Year (2 way)	(0.036)	(0.044)	(0.019)	(0.022)	(0.020)	(0.023)
Connected × Reform dummy		-0.123**		0.061*		0.053
<i>Standard errors</i>						
Governor-Secretary of State (dyadic)		(0.043)		(0.021)		(0.023)
Governor & Secretary of State (2 way)		(0.057)		(0.039)		(0.032)
Dyadic & Year (2 way)		(0.056)		(0.038)		(0.041)
Governor FEs	Yes	Yes	No	No	No	No
Governor-Colony FEs	No	No	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,510	3,510	3,510	3,510	3,510	3,510

Notes: Unit of observation is the governor-year. Sample period 1854-1966. Dependent variable is the (log) total salary in GBP for the governorship (Columns 1-2), the (log) total revenue (Columns 3-4) and the (log) total expenditure (Columns 5-6). Connected is a dummy that is 1 if the governor and Secretary of State share either common ancestry, are both aristocrats, both went to Eton or studied at Oxford or Cambridge. The asterisks report the preferred (dyadic governor-secretary clustered) standard errors * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Alternative clustering specifications are reported in parentheses. These include two-way clustering on the governor and secretary level, as well as two-way clustering on the dyadic *and* year level.

Table C7: Robustness: Placebo first-stage with leads and lags

	(1)	(2)	(3)	(4)
	Connected appointment			
Mean of dep. var	0.305	0.302	0.305	0.299
Prob. connected appointment $t - 3$		-0.072 (0.066)		-0.081 (0.080)
Prob. connected appointment $t - 2$		0.041 (0.108)		0.014 (0.103)
Prob. connected appointment $t - 1$	0.197** (0.076)	0.225*** (0.084)	0.160* (0.086)	0.196** (0.091)
Prob. connected appointment t			0.072 (0.084)	0.057 (0.083)
Prob. connected appointment $t + 1$			0.060 (0.109)	0.037 (0.115)
Controls	Yes	Yes	Yes	Yes
Colony FEs	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes
Prev. spell FEs	Yes	Yes	Yes	Yes
Observations	591	506	509	462

Notes: Unit of observation is the appointment. Sample period 1854-1966. Dependent variable is a dummy that is 1 if the governor was appointed connected. The independent variable is the share of connected governors who are available for reshuffle (i.e. have served beyond their 5th term) with different leads and lags. Controls include (log) salary of the governor and the spell length. Previous spell FEs are dummies for the previous appointment's length. Robust standard errors in parentheses, clustered at the dyadic governor-secretary of state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C8: Robustness: Strategic non-compliance with six year term limit

	(1)	(2)	(3)	(4)	(5)	(6)
	Duration of previous appointment					
	Less than 6 years		6 years		More than 6 years	
Mean of dep. var	0.596	0.596	0.320	0.320	0.085	0.085
log Salary in GBP	-0.078	-0.040	0.001	-0.036	0.076	0.076
	(0.074)	(0.088)	(0.076)	(0.087)	(0.052)	(0.054)
Prob. connected appointment	-0.047	1.042	0.105	-0.965	-0.058	-0.076
	(0.142)	(0.944)	(0.141)	(1.044)	(0.041)	(0.274)
log Salary in GBP × Prob. connected appointment		-0.135		0.133		0.002
		(0.118)		(0.130)		(0.035)
Colony FEs	Yes	Yes	Yes	Yes	Yes	Yes
Spell length FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	591	591	591	591	591	591

Notes: Unit of observation is the appointment. Sample period 1854-1966. Dependent variables are dummies for whether the previous appointment was terminated early (Columns 1-2), on time (Columns 3-4) and late (Columns 5-6). Prob. of connected appointment 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. Controls comprises the (log) salary for the governorship and the spell length. Robust standard errors are clustered at the year and state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C9: Robustness: Tax/GDP and historical connectedness - controls

	(1)	(2)	(3)	(4)	(5)
		Modern tax/GDP ratio			
Mean of dep. var	19.76	19.76	19.42	20.07	20.07
Connected years	-0.722*** (0.263)	-0.880** (0.347)	-0.940** (0.440)	-0.434* (0.225)	-0.439 (0.482)
Years of British colonization	0.081** (0.035)	0.088** (0.039)	0.108* (0.059)	0.054* (0.032)	0.045 (0.047)
Area tropics	-0.225*** (0.066)	-0.250*** (0.070)	-0.271** (0.117)	-0.258*** (0.071)	-0.248** (0.097)
log Initial governor salary	1.791 (2.170)	2.809 (2.765)	2.448 (2.664)	-0.216 (2.172)	0.392 (2.982)
Landlocked		-5.787 (5.634)			-4.799 (5.732)
Ethnic fractionalization			7.327 (12.490)		-5.256 (7.913)
log Genetic distance				-0.854 (1.927)	-0.141 (2.270)
Estimation	IV	IV	IV	IV	IV
Continent FEs	Yes	Yes	Yes	Yes	Yes
First-stage F-stat	25.50	19.30	13.54	11.17	6.320
Observations	48	48	46	34	34

Notes: Unit of observation is the region/state. The dependent variable is the regional tax/GDP ratio in 2010. Connected years is the number of years with a connected governor between 1854-1930. Years of British colonization is the years under British rule. Area tropics is the share of land area that lies in the tropics. Initial governor salary is the (log) amount of the first governor salary fixed for the governorship. Ethnic fractionalization measures are from Alesina (2003). Genetic distance to UK is from Spolaore and Wacziarg (2009). Continent fixed effects include dummy for Africa, Europe, North America, Latin America, Asia and Oceania. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Model extensions

A.1 Colony-specific return to effort

In the basic model, the return to effort did not depend on the assigned colony. Similar to the span-of-control argument, however, effort could also have a greater impact in the large colony. I therefore extend the revenue generation function to allow effort and colony size to be complements, $y_{cs} = \kappa\theta_c m_s + m_s e_{cs}$. The corresponding effort now varies by colony size, $e_{cs} = \theta_c \beta V_y(c) m_s$. This is because the continuation value depends on revenue, and the same amount of effort now generates a higher revenue return in the larger colony.

The allocation problem remains as before: the Secretary of State chooses the allocation to maximize utility. The only difference is that effort does not cancel out as its return depends on whether it is exerted in the large or small colony.

The resulting condition now extends to following: the Secretary of State will allocate the connected governor to the large colony if the extent of patronage is high,

$$\frac{\sigma_1}{g} \geq -\frac{(m_1 - m_0)}{(w_1 - w_0)} \left((\theta_1 - \theta_0)\kappa + \beta \left(V_y(1) - V_y(0) \right) (m_1 + m_0) \right) \quad (17)$$

In the absence of patronage and ability differences, the connected governor is only promoted if he exerts higher effort ($V_y(1) > V_y(0)$).

B Data appendix

B.1 Historical fiscal data

The main source of historical colonial revenue and expenditure data are the Colonial Blue Books, a set of standardized yearly reports providing detailed information about public revenue and spending, trade and socio-economic indicators for over 80 colonies covering the period 1821-1949 (Banton, 2008). This unique data source has remained largely untapped among economists (with the notable exception of Dippel et al. (2015)) but enables the construction of long series of comparable measures on a wide range of variables (such as sources of revenue income, spending patterns, salaries, education, newspapers). The original set of Blue Books is stored at the National Archives, with incomplete subsets stored at the University Library in Cambridge and the University of London Commonwealth Library.

I digitized data on revenue and expenditures from the full set of 3,905 Blue Books. The main part of interest was the Section “Comparative Statement of Revenue and Expenditure” (Appendix Figure A1). This section provides a breakdown of both revenue and expenditures for two years: the current year of the Blue Book, and the previous year. Since I collected data from all Blue Books, this provided an additional redundancy to validate the quality of the

fiscal data across all the years. All monetary values are typically listed in pounds. When needed, the local currency (e.g. Hong Kong Dollar, Sri Lankan rupees) was converted at the historical exchange rate provided by the Blue Book.

The breakdown broadly follows two patterns: it lists the ordinary expenditures for the colonial bureaucracy (civil establishment) and the extra-ordinary expenditures accruing to the various departments. Ordinary expenditures comprise salaries, allowances and pensions paid to colonial civil servants and are grouped by function (e.g. revenue collection, education, police and gaols). These closely resemble the Ministries in later periods. Extra-ordinary expenditures typically encompass unexpected expenditures (e.g. following natural disasters) or investments in public works. As the Blue Books were not compiled across the entire period of the colonies (with most discontinued shortly after WWII), I extend these series using reported aggregates provided by the Colonial Lists. This allows me to extend the series up to 1966. The disadvantage, however, is that the Colonial Lists only provide aggregates without the fine breakdowns from the Blue Books.

B.1.1 Harmonizing revenue and spending breakdown

I also digitize and construct breakdowns of the aggregate revenue and expenditure. The main challenge here lies in the changing definitions of the subitems. For example, one Blue Book may list a detailed breakdown of each department's disbursed salaries, while the subsequent year may only report the total. Similarly, police expenditures may have been grouped with the spendings for prisons in one year but then reported separately in the other.

To construct consistent series, I digitized the section "Net Abstract of Revenue and Expenditures" from all Blue Books. This is the section that precedes the "Comparative statement". Unlike the "comparative statements", this section only provides the breakdown of the current reporting year. The advantage, however, lies in its finer granularity: positions that may have been grouped in the "Comparative statement" are separately reported in the "Net Abstract".

In the second, step I harmonized the series, focusing on several broad groups: On the revenue side, I distinguish between external and internal revenue. External revenue comprise customs revenue and duties collected at the entry points (typically ports). Internal revenue comprise revenue raised within the colonies, such as income tax, hut taxes, poll taxes, land revenue, fees and duties. On the expenditure side, I focus on two broad groups of spending. First, I focus on expenditures in revenue collection. This comprises expenditures made for the collection of customs, but also the raising of direct taxes. I use this as a direct measure for investments in fiscal capacity. Second, I harmonize expenditure series on public works and infrastructure investments. This position includes public works, expenditures for roads, bridges, repairs for public buildings, as well as spendings on civil engineers.

Despite all my efforts in providing harmonized breakdowns, data constraints and changing definitions still reduce the final sample size of these breakdowns. In the paper, however, I

provide evidence that the main results are robust for the subsample. This alleviates concerns of sample selectivity.

B.2 Identifying social connections

The main source of genealogical data is drawn from the database The Peerage (thePeerage.com), obtained on the 20th June 2015. The data provides a genealogical survey of the peerage of Britain as well as the royal families of Europe, including the family trees of the British elite.

The dataset covers 664,265 individuals over more than 500 years including their family relationships. The data contains the full names and date of birth, as well as the details of the spouse, parents and children. I convert the family trees into 1,271,854 undirected links. To avoid concerns of endogenous network formation, I drop marriage links and focus only on blood-relatedness. Dropping marriages reduces the number of undirected links to 1,008,986.

In the second step, I match each of the 456 governors and 39 Secretary of States for the Colonies to the unique identifiers provided in the Peerage dataset. A match is defined as an identical name and birthday. Ambiguous matches, for example due to changing aristocrat titles, are resolved by consulting the UK Who is Who or the Oxford Dictionary of National Biography. Only two Colonial Secretaries cannot be matched (George Hall, Arthur Jones). Both are politicians of the Labour party not from elite backgrounds. 34% of the governors are reliably matched in the Peerage data. I assume that the missing individuals are not connected. This is not a restrictive assumption as the family trees of the Colonial Secretaries are fully mapped out. A governor not included in the family tree, then, is unconnected.

For governors and Colonial Secretaries matched to the Peerage data, I compute the shortest distance using Dijkstra's algorithm (Dijkstra, 1959), implemented using Matlab's *graphshortest-path* package. Two individuals are *connected* if the degree of separation is less than 16. Finally, to verify the data quality, I drew a random sample of 5 connected governors and manually traced the connection from the governor to the superior Colonial Secretary. In addition, I validated the genealogical data with data provided by Ancestry.com.

B.3 Computing additional performance measures

B.3.1 Sentiment analysis of parliamentary debates

I extracted the full set of parliamentary debates from the *Hansard* to compute the number of times a colony has been mentioned in the parliamentary debates and the associated sentiment of the mention. This allows me to compute a dummy that is 1 if the colony has been mentioned in a given year. To measure the sentiment, I then use the R's *qdap polarity* tool to compute sentiments associated with the mentions. Intuitively, the procedure assigns a positive/negative sentiment to each word and then weights these words depending on the context. For example,

a negative word like “punishment” is amplified if it is preceded by a magnifying adjective, like “severe”. Similarly, the sign is reversed if the word is preceded by a negator, like “not”. See <http://trinker.github.io/qdap> for a detailed description of the procedure. I then compute the average sentiment based on all speeches in a given year that mentioned a given colony.

B.3.2 Social unrest based on newspaper reports

To measure social unrest, I collected data from historical newspapers to generate a dummy that proxies social unrest. The data is drawn from all London-based newspapers found in the *The British Newspaper Archive* in December 2015. For each year between 1854-1966, I count the frequency in which a colony is mentioned in conjunction with following keywords: (i) riot (ii) arrest (iii) killed (iv) murder. For example, the number of times Jamaica was mentioned together with the keyword “killed” spiked at 1008 in 1866, right after the Morant Bay rebellion. To alleviate concerns over measurement errors (e.g. that colony and keywords are mentioned in distinct articles that are mistakenly misclassified), I standardize the frequency of mentions within the colony for each keyword and compute an average for each colony-year based on all four keywords. I then focus on “extreme cases” by defining social unrest to take a value of 1 if the average standardized unrest index exceeds the 95th decile.