The London School of Economics
and Political Science

Essays in Development and Organizations

Guo Xu

A thesis submitted to the Department of Economics of the London School of Economics for the degree of Doctor of Philosophy. London, April 2017.

Declaration

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that this thesis consists of approximately 40,000 words.

I certify that chapter 2 of this thesis was co-authored with Marianne Bertrand, Robin Burgess and Arunish Chawla. I contributed 33% of the work.

Abstract

This thesis contains three essays on development and organizations.

Chapter 1 asks how much discretion should be given to politicians in the allocation of public positions. The discretionary allocation of positions by patronage remains widespread both in developing and developed countries; how patronage affects organizational performance, however, remains understudied. Using historical personnel and public finance data from the administration of the British Empire, I study how a civil service reform affected the allocation and performance of governors who are socially connected to their superior.

Chapter 2 focuses on the role of career incentives in explaining performance differences among modern Indian bureaucrats. While rigid progression rules - such as seniority-based promotions and age-based retirement - prevent favoritism by shielding bureaucrats from political interference, these rigidities may also disincentivize: high performers cannot be fast-tracked, and low performers must be retained. We combine administrative data from the Indian Administrative Service (IAS) with survey data on the perceived effectiveness of civil servants to study how the combination of rigid entry, progression and retirement rules acts to disincentivize modern day civil servants.

Chapter 3 moves beyond public organizations to study the role of collective reputation in a private organization. Using data from an online labour market where the country of residence is the salient group characteristic, I document a mechanism through which collective reputation perpetuates group inequality. Using an instrumental variables strategy, I identify reputational externalities between an employer's first hire and the propensity to contract more workers from the same country. I provide empirical evidence that collective reputation serves as a coordination device, enabling workers to positively sort with employers.

Acknowledgments

I am indebted to Oriana Bandiera and Robin Burgess for their supervision, support and continued guidance. I thank my colleagues and peers at the LSE and beyond for their helpful comments and fruitful discussions.

Contents

List of Tables

List of Figures

Chapter 1

The Costs of Patronage: Evidence from the British Empire

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.

1.1 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 peformance 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,

[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).

[2] This 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).

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 allocating 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

[3]More 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).

[4]In 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

results contribute to further unbundling institutions by tracing out a specific institutional channel through which patronage shaped contemporary state capacity (Acemoglu et al., 2001, 2004; 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]

The remainder of this paper is organized as follows. Section 1.2 introduces the historical background, the data and the measurement of social connections. Section 1.3 discusses a conceptual framework that guides the empirical analysis. The results are presented in four blocks. Section 1.4 presents evidence on how social connections affect the allocation of governors. Section 1.5 and 1.6 present evidence on the performance of governors within and across appointments. Section 1.7 discusses the long-run estimates. Section 1.8 concludes.

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).

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

1.2 Empirical context and data

1.2.1 Background and natural experiment

The organization under study is the British Colonial Office. The Colonial Office was founded 1854[6] 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 1.1). 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

[6]From 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.

[7]British 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.

[8]The 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*.

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 progressively 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

[9]The 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).

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

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.

1.2.2 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 A.2 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

[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).

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 A.1). 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 A.1 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 1.2.3.

[Table 1.1 here]

Table 1.1 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 A.6).

1.2.3 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]

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

[13] Networks 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)

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 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 algorithm[16] 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

[14] I exclude relatedness through marriage that occured after entry into the colonial service.

[15] The 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.

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

[17] To 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

to maximize the switcher sample (Appendix Figure A.4). 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 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 A.5). In my later analysis, I combine all measures into a single measure of connectedness.

---

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.

[18]Oxford 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).

1.3 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 1.4, 1.5 and 1.6.

1.3.1 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 $w_{S(c)}$,

$$U^S = f + gW + \sigma_1 w_{S(1)} + \sigma_0 w_{S(0)} \tag{1.1}$$

[19] Allowing the return to effort to vary by colony size does not affect results (See Appendix Section A.1.1). I assume constant returns to effort to simplify the exposition.

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 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,

$$U^G(e_c) \quad = \quad w_s + \beta V(y_s(e_c), c, g) - \frac{1}{2\theta_c} e_c^2 \tag{1.2}$$

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

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

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.

1.3.2 Effort choice and allocation decision

The governor chooses effort levels $e_c \geq 0$ to maximize utility as shown in equation (1.2). The first order condition yields governor c's optimal effort,

$$e_c^* = \theta_c \beta V_y(c,g) \tag{1.3}$$

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,

$$g\Big(y_{11}(e_1^*) + y_{00}(e_0^*)\Big) + \sigma_1 w_1 > g\Big(y_{01}(e_0^*) + y_{10}(e_1^*)\Big) + \sigma_1 w_0 \tag{1.4}$$

1.3.3 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,

$$\frac{\sigma_1}{g} \geq -\Big(\theta_1 - \theta_0\Big)\frac{m_1 - m_0}{w_1 - w_0}\kappa \tag{1.5}$$

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 ($g \to 0$), 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 ($g \to \infty$), the assignment is increasingly merit-based.

Equation (1.5) 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 1.4. 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,

$$\text{Incentive effect} = \Delta y^*|_{\theta,s} = \Delta e^*|_{\theta,s} = \theta\beta\Big(V_y(1,g) - V_y(0,g)\Big) \tag{1.6}$$

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 $V_y(0,g) > 0$.

[21] The 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 1.5. Furthermore, estimating equation (1.6) 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,

$$\begin{aligned} &\text{Incentive + Selection effect} = \\ &\Delta y^*|_s = \kappa m_s(\theta_1 - \theta_0) + \beta\Big(\theta_1 V_y(1,g) - \theta_0 V_y(0,g)\Big) \end{aligned} \tag{1.7}$$

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 (1.5). 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 (1.6) 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 1.6 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.

1.4 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.

1.4.1 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:

$$\log w_{ist} = \beta \times c_{it} + \theta_i + x'_{it}\gamma + \tau_t + \varepsilon_{ist} \tag{1.8}$$

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 A.2 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 A.2 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 candidiates. 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 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]

[22] In terms of descriptive statistics, the "switchers" are between those who are always connected and never connected (Appendix Table A.7).

[23] The only predictor of turnover are elections (Appendix Table A.4). The results are robust to using only variation in connections induced by elections (Appendix Table A.14, Column 2).

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 1.2 here]

The results are presented in Table 1.2 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]

[24]The 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 A.18.

[25]See Appendix Table A.5 for the correlation matrix for all measures.

[26]The results are robust when dropping one of the four dimensions in turn (Appendix Table A.13).

[27]An 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 A.14, 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).

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]

The large increase in salaries for connected governors is striking as salaries within bureaucracies are typically fixed across positions. Table 1.3 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 1.3 here]

Column 1 reports the salary premium based on the combined measure of connections (the same as in Table 1.2, 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 A.6), 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

[28]See Appendix Figure A.5 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.

(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]

1.4.2 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

[29]If 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 A.6.

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 oveseeing 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]

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

[31] Interestingly, 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

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:

$$\log w_{ist} = \beta_0 \times c_{it} + \beta_1 \times c_{it} \times \mathbf{1}[t \geq 1930] + x'_{it}\gamma + \theta_i + \tau_t + \varepsilon_{ist} \quad (1.9)$$

where w_{ist} is the wage and $c_{it} = \{0, 1\}$ is the dummy for connectedness. This specification 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 1.4 and Figure 1.2 here]

The results in Table 1.4 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

in Britain's new and sizeable African acquisitions, patronage in its sense of family favouritism and personal protÃ©gÃ©s would be insufficient to find enough staff".

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 1.2 provides visual evidence by plotting the salary gap for social connections over time. The gap is estimated using an augmented version of (1.9) 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.

1.5 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.

[32] As 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.)

[33] By 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

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]

$$y_{ist} = \beta \times c_{it} + \gamma' x_{it} + \nu_{is} + \tau_t + \varepsilon_{ist} \tag{1.10}$$

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 A.3 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 A.3 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 1.5 here]

Table 1.5 reports the key result. Under patronage, governors perform worse when connected to their superior.[36] The same governor in the same

revenue locally to pay for the local costs of colonial governance [...] and the construction of public works", was a direct response (Gardner, 2012).

[34]The results are also robust when using revenue growth instead of levels (Appendix Table A.15).

[35]Again, the results are robust to alternative clustering strategies. See Appendix Table A.18).

[36]This 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

colony generates 4% lower annual revenue in years connected compared to years unconnected to the Secretary of State (Panel A, Column 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 1.1). The point estimate for internal revenue is negative but insignificant (Columns 3 to 4).

Turning to the expenditure side (Table 1.5, 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

sign depends on whether connections and effort are complements or substitutes. Appendix Table A.8 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.

[37] The 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.

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 appointment 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 A.3 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 A.16). 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 A.17).

[38]There is also no statistically significant difference in the survival curves for governors when connected and unconnected (Appendix Figure A.6).

1.5.1 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 A.2). 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 1.5). Dropping the large part of the

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

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

full sample, while 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 1.6 here]

The results are summarized in Table 1.6. 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 1.5). 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

[41] See Appendix Figure A.2 for an example of such laws.

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.

1.5.2 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 A.2.3 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

[42]Despite 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.

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 1.7 here]

Table 1.7 summarizes the results using alternative measures of performance. To be consistent, I use the same double-differences specification as in (1.10). 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

[43]The sentiment analysis is implemented using R's *qdap polarity*. See Appendix for a detailed description.

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.

1.6 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 1.3 and 1.4 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

[44] While 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).

variable and a variant of the specification (1.10), 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 1.3), the association is significant and positive for post-patronage appointments (Figure 1.4).[46] The figure 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 1.3 and 1.4 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).

1.6.1 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:

$$g_y = \beta \times C_{st} + \gamma' x_{st} + \mu_s + \varepsilon_{st} \tag{1.11}$$

where $g_y = \log\left(y_{st+n_{st}}/y_{st}\right)$ and $C_{st} = \Sigma_t^{t+n_{st}} c_{I(s,t)st}$ 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.

[45] The 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.

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

[47] Similar 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.

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 1.5 shows, the majority of the governorships indeed end in the sixth year.

[Figure 1.5 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 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,

$$p_t = \frac{\Sigma_i \mathbf{1}[T_{it} \geq 6] \times c_{it}}{\Sigma_i \mathbf{1}[T_{it} \geq 6]} \tag{1.12}$$

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 A.3 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 (1.11) then is,

$$C_{st} = \alpha \times p_{t-1} + \eta' x_{st} + \nu_s + \epsilon_{st} \tag{1.13}$$

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

[48] Monotonicity 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.

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

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

[51] Early or late termination of previous appointments is uncorrelated with the share of available connected governors (Appendix Table A.20).

[Table 1.8 here]

To demonstrate the relevance of the instrument, Column 1 of Table 1.8 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 1.9]

The reduced form and IV estimates are summarized in Table 1.9 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

[52]The results are similar when using two period leads and lags but this substantially reduces the number of observations (Appendix Table A.19).

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, 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.

1.7 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 1.5). Second, the reduction in customs revenue coincided with a larger number of legislation on trade taxes, especially exemptions (Section 1.5.1). 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.

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

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

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

outcome y_s is:

$$y_s = \beta \times C_s + \gamma' x_s + \mu_{R(s)} + \varepsilon_s \tag{1.14}$$

where $C_s = \Sigma_t c_{I(s,t),t}$ 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,

$$P_s = \Sigma_t p_{t-1} \times \mathbf{1}[T_{I(s,t),t} = 1] \tag{1.15}$$

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 1.6.1). 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,

$$C_s = \alpha \times P_s + \delta' x_s + \nu_{R(s)} + \epsilon_s \tag{1.16}$$

where the cumulative number of connected years is instrumented by the expected number of connected appointments P_s constructed as described in equation (1.15). 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.

1.7.1 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 A.9 provides the summary statistics for the modern outcomes and explanatory variables.

The results are reported in Table 1.10 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 1.10 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 1.7 stands in stark contrast to the strong negative relationship from the patronage period (Figure 1.6).

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 A.11).

[55] The first-stage is reported in Appendix Table A.10.

[56] The 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.

[57] As Appendix Table A.10 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.

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 1.6 and 1.7 here]

The main results are robust: as the partial correlation in Figure 1.6 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 1.8). 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 A.21).[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 A.9). 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%.

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

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

[60] In 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 A.12).

[61] In 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

[Table 1.11 here]

Table 1.11, 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.

1.7.2 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 1.5). Table 1.11, 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

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.

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 cnnected governors after the removal of patronage has no long-run impact (Column 12).

[Table 1.11 here]

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.

[63] See Appendix Figure A.7 for the corresponding first-stage.

[64] Let 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, $\log(\Sigma_i^N N^{-1}|X_{is} - Z_{is}|)$.

[65] Consistent 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 A.12).

1.8 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

[66] As 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).

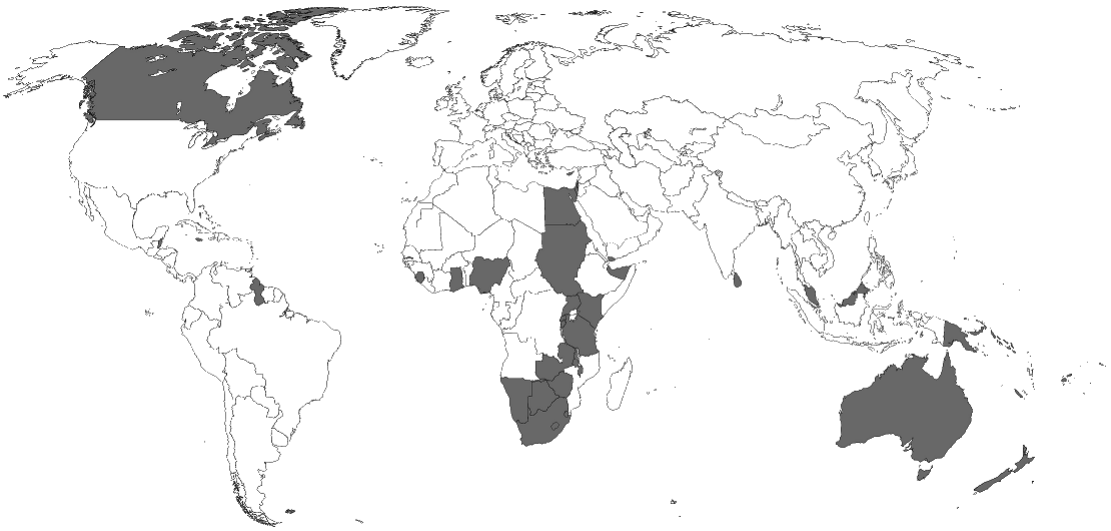
therefore goes beyond the existing body of literature that focuses on lower level bureaucrats and front-line providers who are unlikely to have discernible individual effects on macroeconomic outcomes.

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.

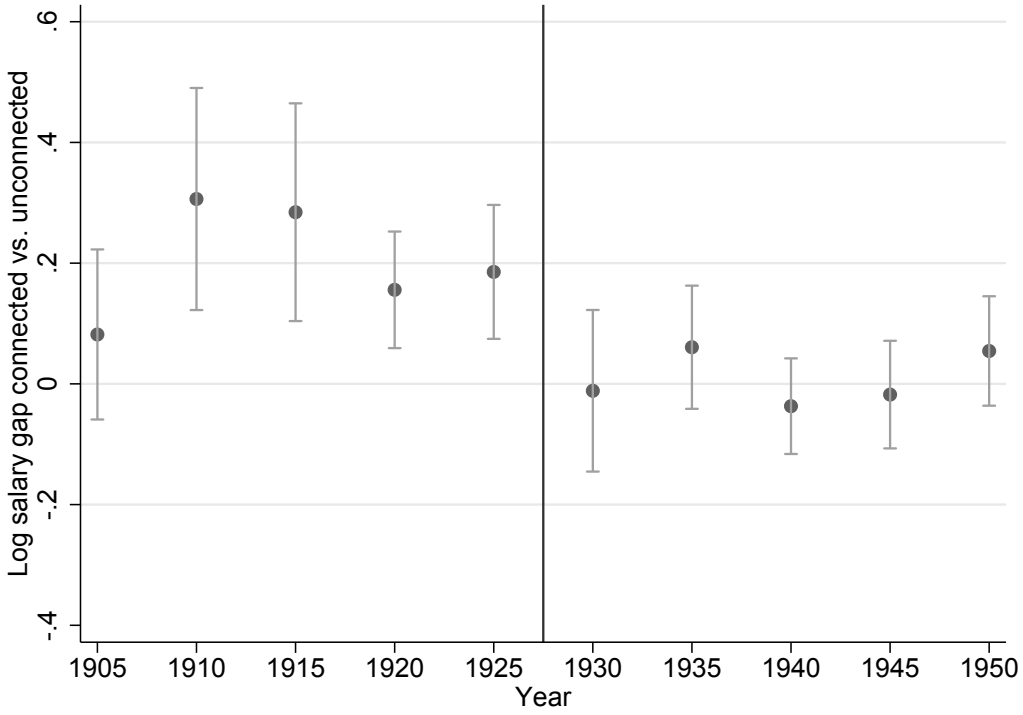
1.9 Tables and Figures

Figure 1.1: Territories administered by the Colonial Office - 1905



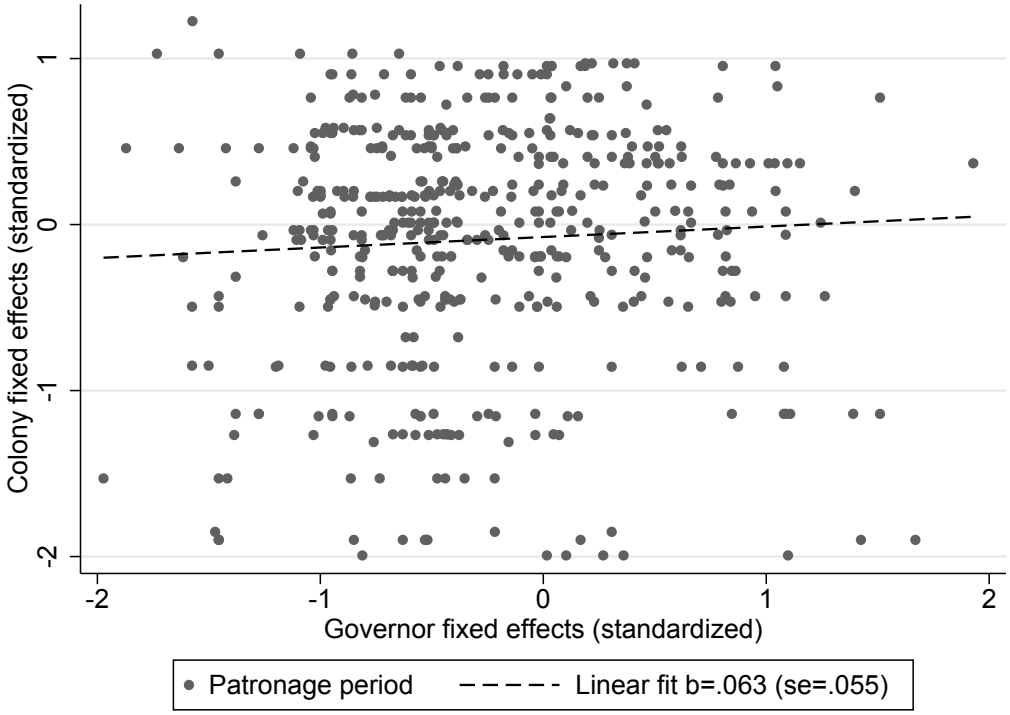
Notes: British territories administed by the Colonial Office in 1905.

Figure 1.2: Salary gap and the removal of patronage (Warren Fisher Reform 1930)



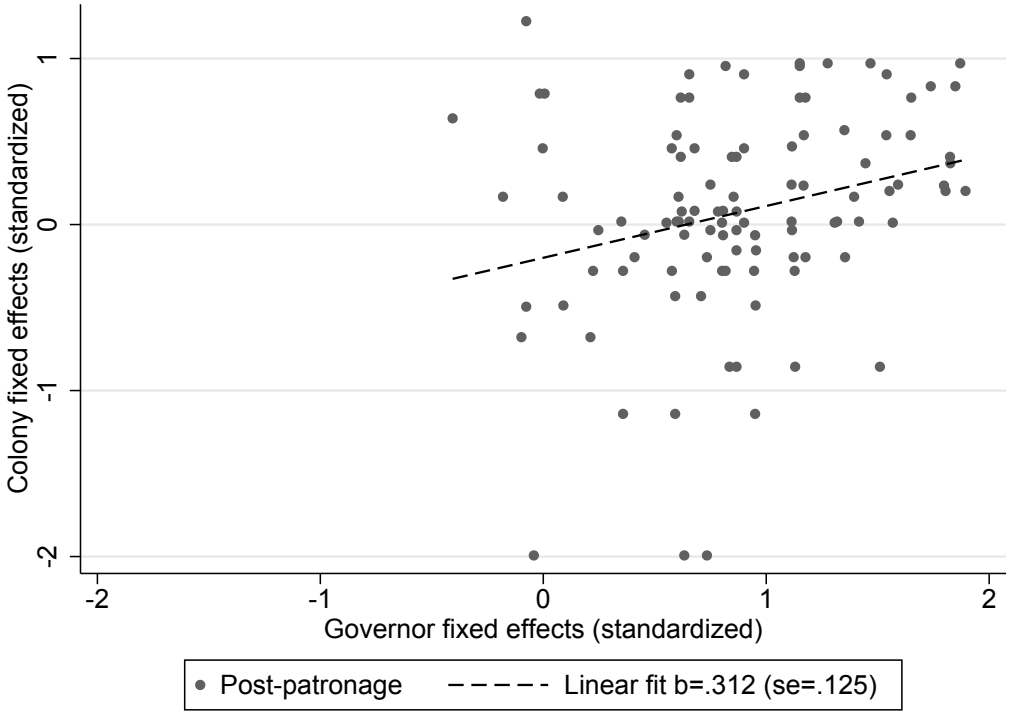
Notes: Difference in (log) salaries for connected and unconnected governors around the Warren Fisher Reform 1930 (solid vertical line). The salary gaps are estimated with an extension of specification (1.10), where connectedness is allowed to vary by five year bins.

Figure 1.3: Matching assortativeness before the removal of patronage



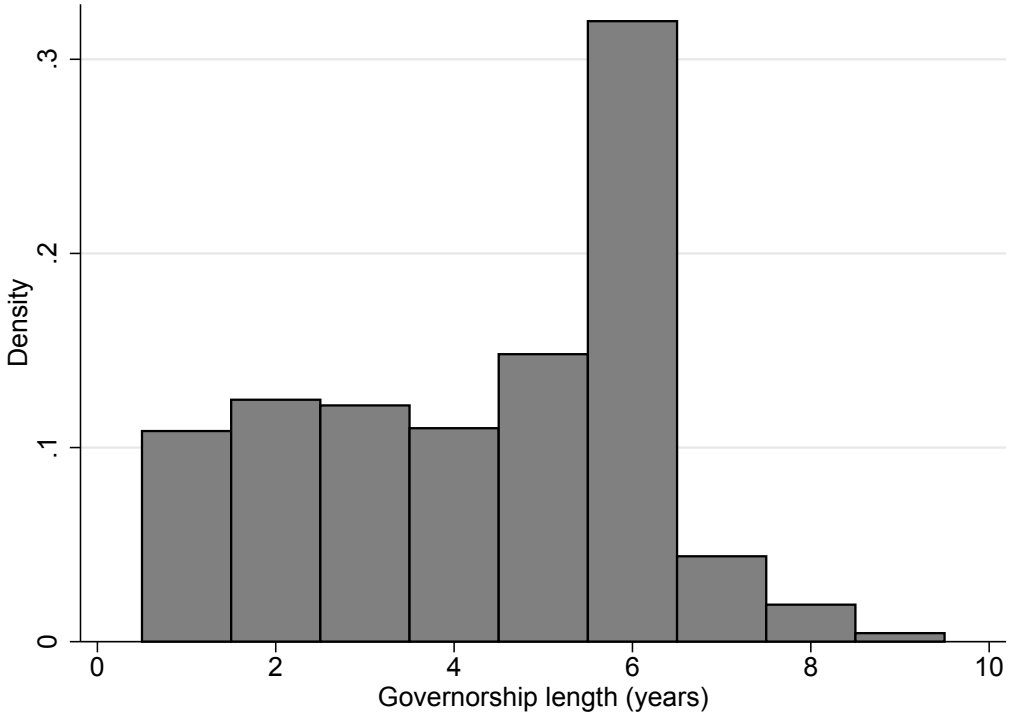
Notes: The relationship between colony and governor fixed effects for the patronage period (1854-1929). Reporting the estimated slope and corresponding robust standard errors.

Figure 1.4: Matching assortativeness after the removal of patronage



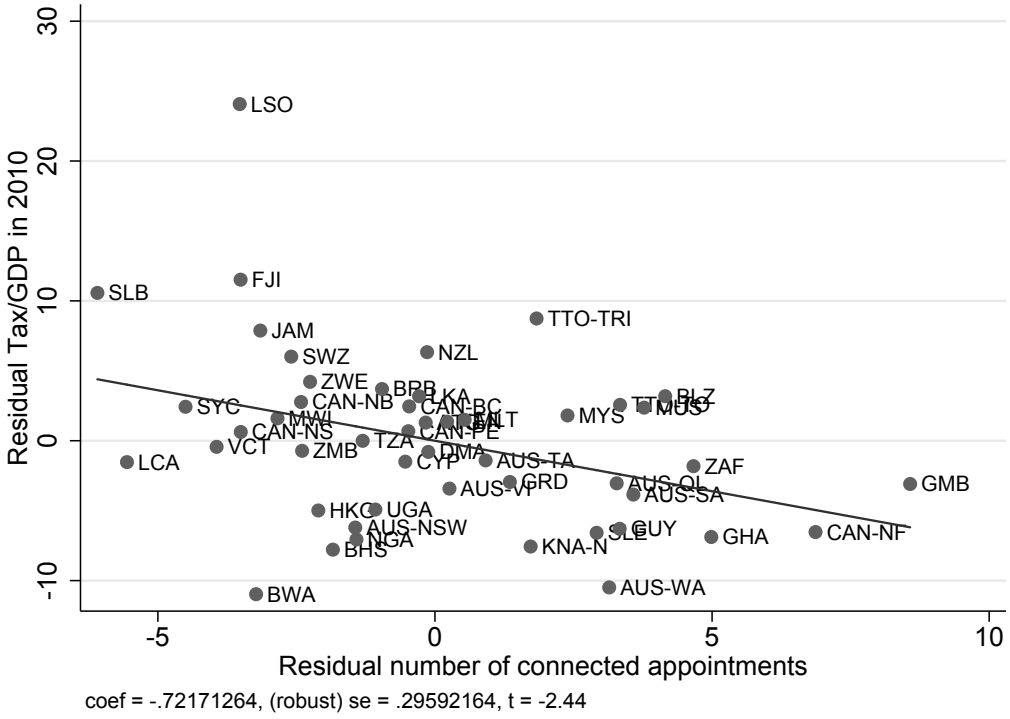
Notes: The relationship between colony and governor fixed effects for the post-patronage period (1930-1966). Reporting the estimated slope and corresponding robust standard errors.

Figure 1.5: Distribution of tenure length for completed governorships



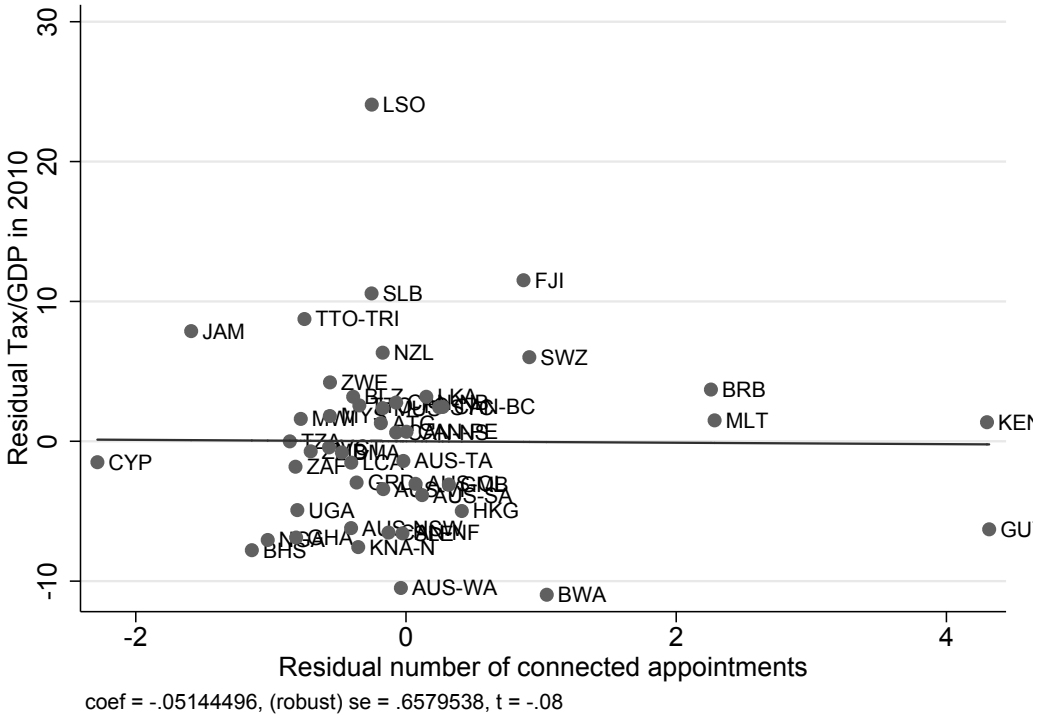
Notes: Distribution of tenure length for completed governorships between 1854-1966. The statutory term limit is six years.

Figure 1.6: Current Tax/GDP and connected appointments under patronage



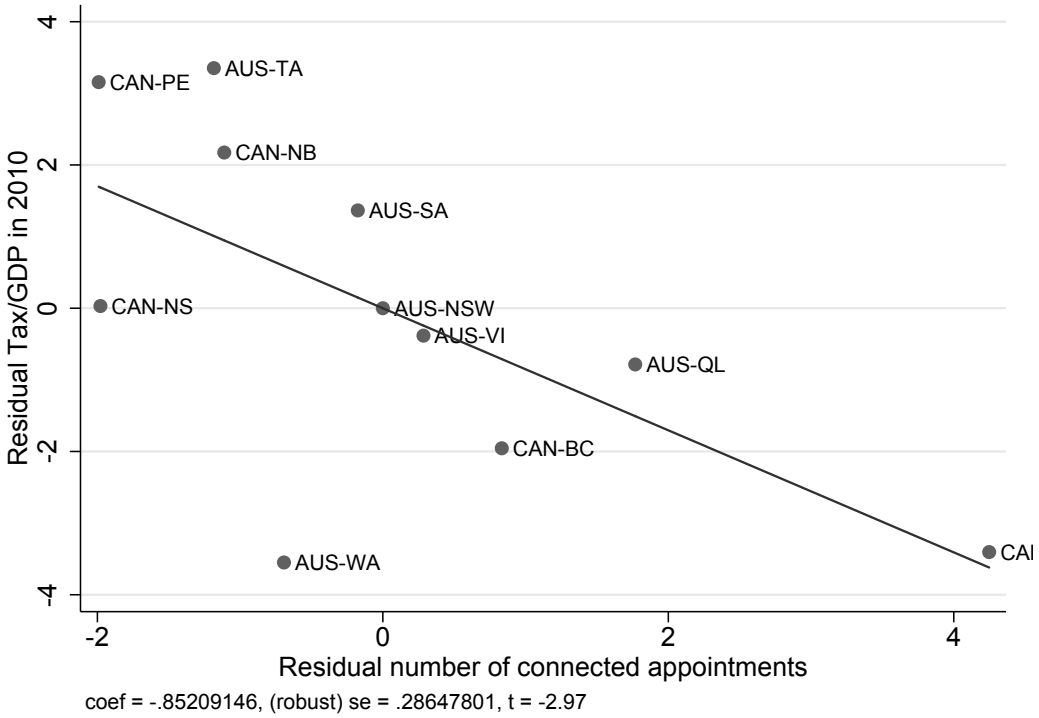
Notes: Second stage (IV) partial correlation between the regional Tax/GDP ratio in 2010 and the number of connected appointments 1854-1930. Controlling for the years under British rule, (log) initial governorship salary, the share of land area within tropics and absorbing continent fixed effects. Robust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

Figure 1.7: 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 1.8: 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.

Table 1.1: 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 1.2: 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***	0.222***	0.223***	0.222***	0.224***	0.224***
	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)
Shared Ancestors	0.103**				0.093**	
	(0.047)				(0.046)	
Both Aristocrats		0.214*			0.175	
		(0.124)			(0.121)	
Both Eton			0.132*		0.117	
			(0.077)		(0.081)	
Both Oxbridge				0.072	0.074	
				(0.047)	(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 1.3: Transfers and connectedness to Secretary of State

	(1)	(2)	(3)	(4)	(5)
			Fixed colony characteristics		
	log Governor salary (GBP)		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***	0.034	0.737***	-0.017	0.063**
	(0.035)	(0.019)	(0.095)	(0.025)	(0.029)
Connected	0.098***	0.011	0.177*	0.014	-0.019
	(0.036)	(0.017)	(0.099)	(0.029)	(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 1.4: Warren Fisher 1930 - Removal of Patronage

	(1)	(2)	(3)	(4)
		Governor salary		
Mean of dep. var	7.929	7.929	7.929	7.929
Connected	0.097***	0.127***	0.205***	0.169***
	(0.036)	(0.043)	(0.059)	(0.060)
Reform dummy × Connected		-0.123**	-0.222***	-0.182**
		(0.056)	(0.079)	(0.084)
Connected + Reform dummy × Connected	-	0.004	-0.017	-0.013
		(0.040)	(0.043)	(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 1.5: 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 ×		0.061*		
Reform dummy		(0.033)		
Connected + Connected ×	-	0.005	-	-
Reform dummy		(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 ×		0.053		
Reform dummy		(0.034)		
Connected + Connected ×	-	0.010	-	-
Reform dummy		(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 1.6: 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**	0.048	0.068**	0.202***	0.004	-0.011
	(0.037)	(0.031)	(0.031)	(0.063)	(0.027)	(0.019)
Connected	-0.083**	-0.051	-0.066**	-0.369***	-0.003	0.013
× Reform dummy	(0.037)	(0.032)	(0.031)	(0.137)	(0.029)	(0.019)
Connected + Connected ×	0.001	-0.003	0.002	-0.167	0.001	0.002
Reform dummy	(0.005)	(0.004)	(0.004)	(0.125)	(0.005)	(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 1.7: Alternative performance measures and connectedness

	(1)	(2)	(3)	(4)
	Social	Parliamentary debates		Highest
	unrest	Mentioned	Sentiment	award
Mean of dep. var	0.049	0.724	0.097	0.021
Connected	0.038*	0.029	-0.045*	-0.031**
	(0.022)	(0.028)	(0.024)	(0.015)
Connected	-0.037*	-0.040	0.039	-0.007
× Reform dummy	(0.022)	(0.031)	(0.029)	(0.028)
Connected + Connected ×	0.001	-0.010	-0.006	-0.037
Reform dummy	(0.002)	(0.015)	(0.016)	(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	Hansard		Who's Who
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 1.8: 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***	0.871***	0.715**	0.808*
	(0.065)	(0.274)	(0.354)	(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 1.9: 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	0.173	0.173	0.166
Connected years	-0.007			-0.129		
	(0.005)			(0.091)		
Prob. connected		-0.115**	-0.092*		-0.101*	-0.055
appointment		(0.054)	(0.052)		(0.057)	(0.066)
Prob. connected					0.054	0.021
× Reform dummy					(0.121)	(0.121)
Connected years +	-	-	-	-	-0.047	-0.034
Connected years × Reform dummy					(0.106)	(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 1.10: 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	-0.196	-0.722***		-0.721***	-0.744*	-0.871**	-0.936*
1854-1930	(0.173)	(0.263)		(0.261)	(0.443)	(0.443)	(0.490)
Connected years			-0.051	-0.049	2.067*	1.475	2.089**
1931-1966			(0.579)	(0.969)	(1.140)	(0.937)	(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 efects 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 1.11: 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	Non-tax	Direct	Indirect tax revenue		
	revenue	revenue	tax	Total	GST	Trade
Mean of dep. var	20.62	5.326	9.897	10.64	7.473	3.258
Connected years	-0.427**	0.170	0.092	-0.523***	-0.117	-0.488***
1854-1930	(0.187)	(0.163)	(0.097)	(0.153)	(0.096)	(0.135)
Connected years	0.426	-0.601**	0.220	0.010	0.164	-0.102
1931-1966	(0.597)	(0.292)	(0.242)	(0.417)	(0.252)	(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	# tariff	Customs	Customs	Tax	Trade/
	rate	lines	misreporting	hours	hours	GDP
Mean of dep. var	7.061	74.765	12.030	3.511	5.052	0.898
Connected years	-0.442**	4.234***	0.088***	0.060***	0.025*	-0.053***
1854-1930	(0.218)	(1.070)	(0.028)	(0.022)	(0.014)	(0.017)
Connected years	0.483	-4.730	0.005	-0.083	0.017	0.042
1931-1966	(0.299)	(3.552)	(0.061)	(0.059)	(0.053)	(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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 2

The Costs of Bureaucratic Rigidity: Evidence from the Indian Administrative Service

Using a stakeholder survey and rich administrative data we study elite civil servants in India. We find that officers that enter state cadres older and in larger cohorts are less effective and more likely to be suspended. We argue that this is due to weaker promotion prospects and career incentives. We demonstrate that states containing a higher proportion of these officers grow less quickly and that these effects are driven by senior officers and felt most acutely in the organized industrial and service sectors of the economy. Career concerns of bureaucrats therefore affect both their effectiveness and aggregate economic performance.

2.1 Introduction

Bureaucrats are a core element of state capacity. They are responsible for implementing policy and may therefore have a critical bearing on societal outcomes. Bureaucratic effectiveness is particularly important in developing economies. Many have recently adopted economic and social reform programs that are aimed at promoting structural change and have the potential to substantially raise living standards. The eventual success or failure of these programs depends centrally on how they are implemented in the field.

Yet, despite their centrality to development and poverty reduction, the incentives civil servants face within bureaucracies are seldom studied. It is striking, for example, how the study of bureaucrats, and professional bureaucracies in general, has lagged well behind that of politicians or private sector managers. While the influence of political leaders on economic growth (Jones and Olken, 2005) and the impact of CEOs on firm performance (Bertrand and Schoar, 2003) have been extensively studied, we know very little about how bureaucrats affect the growth and development processes.

This paper tries to address this gap in the literature by studying the elite cadre of civil servants in India - the Indian Administrative Service (IAS). The IAS, often called the "steel frame" of India, is responsible for running all key government departments at the state and federal levels as well as a range of public sector enterprises and corporations.[1] This makes them a particularly interesting set of bureaucrats to study as they oversee the implementation of a range of policies that have the potential to affect aggregate economic outcomes.

A few key features distinguish professional bureaucratic organizations from other organizations: selection through competitive examinations, a virtual absence of discretionary firing (and hence limited exit) and, our focus in this paper, seniority-based progression rules and a fixed retirement age. These rules are a direct response to earlier patronage systems, where appointments

[1] For much of its post-Independence history India has been a centrally planned economy with IAS officers being responsible for implementing successive five year plans. Post-1991 they were responsible for liberalising the economy and dismantling the planning architecture that had built up since the 1950s (Aghion et al., 2008).

and promotions were decided based on personal or political favors.[2] The reliance on objective selection criteria is meant to ensure that the most talented, as opposed to the best connected, are recruited. Once recruited, clear progression rules and limits on discretionary firing are meant to restrict wasteful lobbying or influence activities by agents who seek to affect the principal's decisions (Milgrom, 1988). Seniority-based promotion rules also reduce principals' and politicians' ability to engage in favoritism, patronage and corruption by providing objective, impartial criteria for career progression in settings where performance is difficult to measure (Iyer and Mani, 2012).[3] As Weber (1922) notes, "bureaucracy develops more perfectly, the more it is dehumanized, the more completely it succeeds in eliminating from official business love, hatred, and all purely personal, irrational, and emotional elements which escape calculation" (p. 975).

The IAS shares these classic characteristics of modern professional bureaucracies. Selection into the IAS, as for many other civil services around the world, is based on a competitive entry examination, with the top 100-150 scorers on the exam being admitted each year (out of about 450,000 exam takers). Once selected, IAS officers are allocated to a state, also known as a "cadre", through a quasi-random allocation process and officers stay part of

[2]The earliest modern bureaucracies go back to the British Northcote-Trevelyan (1854) report which recommended that recruitment into the civil service be by open examination, that the entry age window be between 19 to 25 years, that entrants should be recruited into a unified, permanent civil service and that promotion should be based on merit, not preferment, patronage, purchase or length of service. Many of the recommendations in the report were influenced by the earlier Macaulay reforms in the Indian Civil Service, the predecessor of the modern IAS which was the first of the British civil services to abolish patronage. The Macaulay Report recommended the replacement of the patronage-based system of appointment in the Indian Civil Service by open and competitive examinations (which were made open to Indians), the establishment of a permanent civil service, and an age window for new entrants of 18-25 years. After recruitment, candidates underwent two years of training - one year of formal training in the UK and one year of district training in India - similar to the training structure of the modern IAS (Fulton, 1968; Arora and Goyal, 1996; **?**). See also Bai and Jia (2016) for a discussion of the Chinese recruitment system for elite civil servants and its impact on political outcomes.

[3]Objective performance measures are also confronted with the multi-tasking problem where bureaucrats exert effort only on measurable dimensions (Holmstrom and Milgrom, 1991). Rasul and Rogger (2016), for example, show that the introduction of monitoring can result in excessive "box ticking" activities that are detrimental to project completion rates.

the same cadre throughout their career. Promotions within the IAS are subject to tenure-based rules, with promotion waves occurring at 4, 9, 13, 16, 25 and 30 years of service. As Figure 2.1 shows promotions are based on seniority according to age at entry. Officers do not move to to a higher payscale until the required number of years of experience have been achieved. While the timing of actual promotions closely tracks the promotion grid for junior officers (< 16 years of service), senior officers (> 16 years of service) have to wait beyond the minimum tenure levels to access the top ranks of the bureaucracy.

In the absence of firing and performance pay, career concerns are one of the few sources of incentives for bureaucrats. Promoting bureaucrats predominantly based on seniority can therefore weaken the link between effort and return, blunting a critical source of career incentives. A wide entry age window[4], combined with seniority-based progression and a fixed retirement age (see Figure 2.2), implies that those who enter older will face barriers to reaching senior payscales (see Figure 2.3). These limited progression prospects may demotivate officers and reduce their effectiveness.

This is the issue we take up in this paper. We make two contributions. First, we empirically assess whether rigidities in promotion affect IAS officers' on-the-job effectiveness. Second, we examine whether bureaucratic-rule induced variation in the effectiveness of elite civil servants influences the aggregate economic performance of Indian states.

To make progress we must confront the key difficulty associated with studying civil servants - the lack of reliable individual performance measures. Politicians need to win elections and the performance of CEOs may be reflected in sales or stock prices. What the "output" of civil servants is, is much less clear particularly for generalists like IAS officers who work in a variety of departments across their career.[5] We get around this difficulty by polling a group of stakeholders who operate in the same state as an IAS officer and elicit their perception of the effectiveness of that named civil servant. The key stakeholders we survey include IAS officers, state civil servants, elected

[4] 21 to 30 for general candidates, extended to 35 for lower caste candidates.

[5] In our data the average posting length of an IAS officer is 16 months and officers careers typically involve postings in a large variety of departments.

politicians, representatives of business associations, local TV and print media, and civil society organisations. For each IAS officer they know, we ask stakeholders to grade them on a 1 (low) to 5 (high) scale for: effectiveness, probity, the ability to withstand illegitimate political pressures, pro-poor orientation and overall rating. We gather this information in the 14 main states of India and cover the majority of centrally recruited IAS officers in each state.

Figure 2.4, which is based on these surveys, motivates much of our subsequent analysis. The figure shows the raw relationship between an IAS officer's perceived effectiveness in the stakeholder survey and the officer's age at the time of entry into the IAS. As expected if a lack of promotion prospects is particularly demotivating for officers that enter the service older, we find a negative relationship between officers' perceived effectiveness and their age at enty into the IAS. This negative correlation is robust to controlling for a rich vector of fixed effects and background information derived from the administrative data for each of the IAS officers, such as their gender, rural/urban background, caste affiliation, education, work experience, and scores on the entry exam and training marks.

We also find that officers that enter state cadres older *and* as part of a larger cohort of individuals allocated to the same state cadre in the same year are deemed to be less effective. This, we argue, is because, irrespective of age, officers are considered with members of their entry cohort at each of the promotion stages. An older officer that enters in a relatively small cohort will be encouraged by the fact that they will face few competitors and less delays in reaching the higher echelons of the state bureaucracy. The reverse would be true for an older officer entering in a large cohort. Short-run fluctuations in cohort sizes driven by the number of vacancies in the year of entry, therefore, can have long-lasting impacts on career incentives by locking in officers with a larger or smaller pool of "competitors."

We validate our subjective performance measures by showing that age at entry interacted with cohort size is also positively related to the number of suspensions a given officer has experienced. In addition, holding constant age at entry and cohort size, we find that officers which enter the IAS in cohorts with a higher proportion of younger officers are perceived to be less effective.

This is consistent with them being disincentivized by having to compete with officers with longer career spans. Finally, we exploit a natural experiment induced by the 1998 pension reform, which increased the retirement age for IAS officers by two years from 58 to 60. From a career perspective, the reform disproportionately benefited older entrants as these officers became more likely to qualify for senior positions. We find that, after the pension reform, those officers who entered the service older were less likely to be suspended.

We then leverage this core finding that officers that enter the IAS older and in larger cohorts are deemed to be less effective to address the question of whether bureaucratic effectiveness affects state-level economic performance in India. We focus on the 1990-2011 liberalization period when extensive reforms were being implemented and aggregate age at entry and cohort size among serving IAS officers to the state-year level. We argue that, due to the quasi-random manner in which officers are allocated to states at the start of their careers, age at entry and cohort size at entry are exogenous to contemporaneous state-level economic performance. This empirical set-up enables us to conduct a state-year panel analysis to examine whether having a state cadre which contains a higher fraction of IAS officers which entered older and in larger cohorts adversely affects state-level economic performance.

We find that states containing officers that entered older and in larger cohorts grow less quickly. This is due to effects on the organised industrial and service sectors of the economy which are more dependent on policies controlled by IAS officers. Agriculture, in contrast, which is largely unorganised, is unaffected by the composition of IAS state cadres. Structural change thus appears to proceed more slowly when there is a higher proportion of demotivated, ineffective officers in a state cadre. When we break the cadre into junior officers and senior officers, we find that the effects on economic performance are driven by the latter. This is consistent with the fact that senior officers head up the key government departments in a state and therefore have greatest purchase over policies that might influence state economic performance.

Taken together, we find compelling evidence that the career incentives bureaucrats face influence their effectiveness, and that this has wider impacts on the economic performance of the states over which they have jurisdiction. Our paper thus shines a light on the costs associated with rigid progression rules in public organizations. Given a range of public services from health and education through to the diplomatic services are organized like the IAS, understanding these costs and gaining insights into how bureaucrats might be better motivated represents an important undertaking. Indeed it is central to improving the implementation of public policy, to promoting economic performance and to improving societal outcomes.

The remainder of the paper is organized as follows. Section 2 provides details about the institutional background and introduces our data sources. In Section 3, we test the impact of bureaucratic rigidities on individual performance. Section 4 moves on to investigating the aggregate effects on state-level economic outcomes. We conclude in Section 5.

2.2 Background and data

2.2.1 The Indian Administrative Service

The Indian Administrative Service (IAS), the successor of the Indian Civil Service (ICS), is the elite administrative civil service of the Government of India. In 2014 the IAS had an overall strength of around 3,600 centrally recruited officers. These officers are civil service leaders, occupying key positions critical for policy implementation. The most senior civil service positions - the Cabinet Secretary of India, the Chief Secretary of States, heads of all state and federal government departments - are occupied by IAS officers. Senior IAS officers also oversee major state-owned enterprises and state-run corporations. Senior IAS officers are known and publicly visible.

The recruitment of officers is based on performance in the Civil Service Exam, which is organized annually by the Union Public Service Commission (UPSC). Entry into the IAS is extremely competitive, with several hundred thousand applicants competing for a small number of spots. In 2015, for

example, 465,882 UPSC exam takers applied for only 120 IAS slots. The highest performing exam takers are typically offered slots in the IAS. Those who do not qualify for the IAS may obtain positions in less competitive civil service streams such as the Indian Police Service (IPS), the Indian Forest Service (IFS), the Indian Revenue Service (IRS) or the state civil services. There are quotas for the reserved castes, namely the Other Backward Castes (OBC), Scheduled Castes (SC) and Scheduled Tribes (ST).

The age limit for entry into the IAS in our study period lies between 21 and 30 years. This constraint is relaxed for reserved groups, who can enter up to 35 years of age. Once selected, IAS officers are allocated to a state cadre at entry into training. The assignment to a state is typically fixed for life,[6] and officers are attached to their state cadre even when serving in Delhi or abroad. After selection and allocation to the state cadre, IAS officers undergo training at the Lal Bahadur Shastri National Academy of Administration (LBSNAA) and in the states they have been assigned to. The two-year training consists of one year of academic training at the LBSNAA ("course work") and one year of practical training ("district training"). After training, recruits are initially placed in district administration (e.g. as district collectors), and are subsequently promoted to higher level positions. Promotion is seniority based occuring after 4, 9, 13, 16, 25 and 30 years. The discrepancy between minimum and actual tenure required to enter a higher payscale increases for later promotions (Figure 2.1), which are subject to more stringent performance review and depend on the availability of vacancies (see Appendix Table B.1). Finally, retirement occurs at 60 years of age for both male and female officers (58 years before 1998). Figure 2.2 shows the distribution of age at exit for the set of retired IAS officers. There is very little exit before the designated retirement age - 20% of all officers exit before 58 years of age, and only 8% of officers exit with fewer than 50 years of age.

A wide entry age window, combined with seniority-based progression and a fixed retirement age implies that those who enter older will face barriers to

[6]The only exception which allows for transfers across states is in the case of marriage to another IAS officer. These cases, however, have to be approved on a case-by-case basis and are rare.

reaching senior payscales (see Figure 2.3). This may disincentivize effort and lower effectiveness. The potential cost of this bureaucratic rigidity, indeed, has been acknowledged by both the Government of India and the media. The 10th Report of the Administrative Reform Commission, for example, points out that a higher age at entry mechanically implies a "shorter service span, which means [old entrants] may not have adequate opportunities to contribute to policy-making at higher levels".[7] Similarly, media reports frequently point to the disadvantages of combining seniority-based promotion with a fixed retirement age, suggesting that "seniority is an objective basis for promotion but often an ineffective one" [8] and "the problem goes down to the age of entry, since [...] promotions go as much by seniority as merit alone."[9] In line with the recommendations of the Administrative Reform Commission, these media articles call for more flexibility, concluding that the service must "put the best people, irrespective of age, in the right positions", and that "from [an] age-based [retirement] system, we should move to fixed tenures [...] for all civil servants irrespective of joining age" [10]. However, despite repeated calls by these Commissions and other bodies to lower the maximum age at which officers can enter the service,[11] the actual window has been widening over time (see Appendix Figure B.1).[12]

[7] Administrative Reform Comission (ARC, 2008), Chapter 5, page 96.

[8] The Indian Express (1 April 2015), "A new kind of babu" , by Manish Sabharwal.

[9] The First Post India (22 December 2012), "Quotas: How bias in favour of SC/STs works against them", by R. Jagannathan.

[10] The Hindu (8 September 2012), "Fixed tenure a way forward on promotions", by Vivek Katju.

[11] See Administrative Reform Commission (2010), p.105.

[12] The age at entry window for the Indian Civil Service (ICS), the colonial precursor of the IAS, was fixed between 21 and 24 years of age just before Independence and geared primarily towards fresh British graduates from Oxford and Cambridge. After Indian Independence this narrow window was maintained into the early 1970s; however, mounting political pressure to include poor and disadvantaged candidates and those from non-elite academic institutions (who it was argued need more preparation time) has pushed the entry window steadily outwards and away from the 19-25 window recommended by Northcote-Trevelyan (1854). The pressure to extend age at entry continues today - the age limit was extended to 32 years for general candidates and 37 years for reserved groups in 2014.

2.2.2 Measuring bureaucrat performance

We collected cross-sectional data on the subjective assessments of IAS officers in the 14 main states of India[13] for 2012-13. IAS officers were assessed on five dimensions: (i) effectiveness on the job, (ii) probity,[14] (iii) ability to withstand illegitimate political pressure, (iv) pro-poor orientation, and (v) overall rating.[15] All dimensions are scored on a 5 point integer scale, where 1 is the lowest and 5 the highest performance.

To obtain assessments from a wide range of stakeholders, we elicited these subjective assessments from respondents of six societal groups in each state: (i) a random sample of IAS officers, (ii) a random sample of state civil servants, (iii) politicians, drawn from a random sample of members of the legislative assembly (MLA), (iv) industry, business and professional associations, comprised of the highest representatives for the major associations,[16] (v) print and TV media, comprised of key journalists covering politics for the largest newspapers and TV stations by circulation and viewership respectively, and finally (vi) civil society, comprised of the highest representatives of major NGOs, trade unions[17] and think-tanks. For each state, we sampled about 10 respondents from each of the groups.[18]

We compiled a list of all centrally recruited IAS officers for each state. In each state, interviewers then systematically worked through the list, asking respondents to provide assessments for each known candidate. We excluded

[13]These states are: Andhra Pradesh, Bihar, Gujarat, Haryana, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh and West Bengal. We excluded joint cadres (Union Territories, Assam - Meghalaya, Manipur - Tripura), as well as the smaller states (Jammu & Kashmir, Nagaland) and the new cadres resulting from state splits in 2000 (Jharkhand, Uttarakhand, Chhattisgarh) from the sample.

[14]Note that a higher value on the scale corresponds to less corruption.

[15]The exact questions are: (i) "How would you rate his/her effectiveness in his/her assignment?" (ii) "How much do you feel this officer uses his/her official position for making money?" (iii) "How much do you feel this officer can withstand illegitimate political pressure?" (iv) "How sensitive is this officer to the needs of the poor and weaker sections in society?" (v) "What is your overall rating of this officer?".

[16]Confederation of Indian Industry (CII), the Federation of Indian Chambers of Commerce and Industry (FICCI), the Associated Chambers of Commerce and Industry of India (ACCI).

[17]All India Trade Union, Secretariat Employees Union.

[18]For logistical reasons, we were unable to survey state civil servants in Gujarat and IAS officers in Punjab.

junior officers with less than 8 years' tenure as they are often in district postings and less visible. Finally, we recorded the source of information to account for reporting biases, differentiating between information obtained through personal exposure, friends or social networks, or the media.

Table 2.1 provides summary statistics of the 360 degrees[19] measures. The sample sizes range from $N = 15,153$ for the probity measure to $N = 17,753$ for the effectiveness measure. The number of complete assessments across all dimensions is $N = 14,037$. We were able to elicit scores for about 70% of all IAS officers in our sample. All dimensions are correlated, with the highest correlation being between pro-poor orientation and the ability to withstand illegitimate political pressure.

A known concern regarding subjective measures is whether these capture actual information or merely biased perceptions (Prendergast, 1999; Olken, 2009). Respondents, when providing assessments, may compress ratings around norms or systematically provide positive or negative ratings to all assessed officers ("centrality bias"). In the presence of halo effects, a respondent's overall impression of an officer may also affect the assessments on each of the performance dimensions. Respondents may also base their assessment on public information, such as media reporting, generating an "echo chamber."

We address concerns of reporting biases in three ways. First, we purge respondent-specific biases in measurement. Accounting for level differences in reported effectiveness is important: IAS officers, for example, tend to rate their colleagues more highly, while media representatives provide, on average, more negative ratings (Appendix Table B.2). Second, we control for source of information fixed effects to alleviate "echo chamber" biases, namely that those who did not know IAS officers personally merely repeat (potentially biased) perceptions originating in the media. For example, IAS officers known personally by a respondent tend to receive higher effectiveness rating than those rated based on knowledge through media or social networks (Appendix Table B.3). Third, we account for interviewer fixed effects to ensure our

[19]The term "360 degree" feedback refers to multi-source feedback used by organizations to elicit information about employees' work-related performance.

results are not driven by artifacts of the data collection method. Finally, in Section 3.3.2, we also provide evidence on actual suspensions, a relatively clear-cut objective measure of (non-)performance, to complement the subjective measures.

2.2.3 Administrative data

To study the determinants of effectiveness, we combine our 360 degree survey data with administrative data obtained from the LBSNAA, the facility where IAS recruits undergo training before their first posting. We use three sources of administrative data.

First, we draw upon the descriptive rolls of 5,635 IAS officers who entered between 1975-2005. This dataset contains a rich set of individual background characteristics ranging from year and location of birth to caste, family background, educational degrees and work experience, allowing us to examine how pre-determined characteristics at point of entry into IAS correlate with later effectiveness.

Second, we use data on internal rankings[20] which covers 4,107 IAS officers from 1972-2009. This dataset provides information about the initial allocation of officers to cadres, the size of their cohorts in a given entry year, their scores on the entry exam as well as their marks on the training courses.

Finally, on-the-job outcome measures are derived from the executive record sheets of 10,817 IAS officers who entered between 1949-2014. These record sheets contain detailed information about the postings (e.g. job title, department and duration) and payscales of each officer throughout his or her career. This dataset allows us to track suspension episodes for each officer. The data is provided by the Ministry of Personnel, Grievances and Pensions and is publicly available.

Table 2.2 summarizes the IAS officers' background characteristics for the cross-section of 2012-13, providing a snapshot of the IAS at the time of our survey. The typical IAS officer is about 24.5 years old at the time of entry into the IAS. A large majority of IAS officers are male (86 percent). More than a

[20]In the IAS, these lists are referred to as the "inter-se-seniority" lists.

quarter of IAS officers are drawn from minority castes (OBC = 8 percent; SC = 14 percent; ST = 5 percent). Nearly three quarters of the IAS officers come from an urban background. A large share of IAS officers (32%) had previously obtained tertiary degrees in Science, Technology, Engineering, Mathematics, Statistics (STEM) or Economics. Among those that have worked prior to joining the IAS, a third held public sector jobs.[21] About 3 percent of IAS officers had previously worked in another branch of the All India Services (AIS), such as the Indian Police Service (IPS) or the Indian Forest Service (IFS) before joining the IAS.

Finally, in order to examine the effects of bureaucratic effectiveness on state-level outcomes, we construct a state-year panel covering the 14 Indian states for which we collected survey data. We assemble state-level time series for GDP covering the post-reform period 1990-2011 from data published by the Reserve Bank of India (RBI).[22] To examine, in greater detail, outcomes over which bureaucrats exert control, we use data from the Annual Survey of Industries (ASI). We also use data from the Public Finance Statistics to examine the impact of bureaucrats on revenue generation and spending. Finally, when relevant, we use population data from the decennial Census of India to derive per capita measures.[23]

2.3 Effects on individual effectiveness

2.3.1 Age at entry and individual effectiveness

As shown in Figure 2.1, the IAS is characterized by rigid seniority-based promotion rules. Officers are only promoted to jobs in the top two payscales if they have at least 25 (second highest payscale) or 30 (highest payscale) years of tenure in the IAS. As the figure also suggests, access to the senior jobs is delayed - likely due to the limited number of vacancies at higher payscales.

[21] The most frequent jobs in this category comprise junior positions in the Indian Railway Service, Income Tax Service, Customs and Telecommunications.

[22] The data is obtained from the RBI's online Data warehouse, available at http://dbie.rbi.org.in.

[23] We (log-)linearly interpolate the annual state-level population between the Census years.

For example, among officers that have 32 years of tenure in the IAS, essentially all of them are still in the second highest payscale even though they are already eligible for a promotion to the top payscale. Such seniority-based promotion rules, combined with substantial delays in actual promotions and forced retirement at 60 years of age (58 years of age prior to 1998), implies that individuals who enter the IAS at a relatively older age face a mechanical barrier to promotion - they will reach the compulsory retirement age before a job at the highest payscale becomes available for them. In fact, based on the evidence in Figure 2.3, any individual that enters the IAS above the age of 28 (26 pre 1998) will have almost no chance of getting promoted to the highest payscale.

This suggests that bureaucratic rules may be a first order factor in explaining the negative correlation, observed in Figure 2.4, between age at entry and effectivenesss. Individuals who enter the IAS at an older age may be less motivated to do well on the job as they do not expect to progress into the top echelons of the bureaucracy, even if they have stellar report cards. This rigid progression rule stands in stark contrast to the private sector, where the practice of fast-tracking high performers is often considered to be "good" management practice (Bloom et al. 2016).

While Figure 2.4 is consistent with the hypothesis that those who are older at entry are more likely to be disincentivized by the seniority-based progression rule, the obvious threats to this interpretation are omitted variable bias and selection. For example, those entering older may be of differential ability or more likely to come from minority castes. We therefore move beyond the univariate correlation and assess the robustness of the correlation to a battery of officer-specific controls which might be correlated with age at entry. For individual i with k years of tenure rated by respondent j, we estimate the relationship between the perceived effectiveness and age at entry as:

$$score_{ijk} = \alpha \times age\_entry_i + \mathbf{x_i}'\beta + \mathbf{z_{ij}}'\gamma + \tau_k + \theta_j + \varepsilon_{ijk} \tag{2.1}$$

where $score_{ijk}$ is the subjective rating of officer i (who has k years of tenure) by respondent j, age_entry_i denotes the age at entry and $\mathbf{x_i}$ the vector of officer-

specific background characteristics. z_{ij} are source of information dummies that differentiate between knowing the rated officer personally, through networks or through media reports. The coefficients θ_j are respondent fixed effects. As no respondent rates officers in more than one state, these respondent fixed effects also control for any state-specific differences in ratings. The coefficients τ_k are fixed effects for each year of tenure k, which absorb seniority-specific profiles in the ratings. We also include interviewer fixed effects in the vector $\mathbf{x_i}$.[24] Finally, ε_{ijk} is the error term, which is clustered on the respondent level.[25]

We consider four sets of background characteristics in the vector $\mathbf{x_i}$ in addition to age at entry that may have some bearing on how effective bureaucrats are in performing their duties. These include individual socio-economic background characteristics, education, work experience and entry exam and training scores. Individual socio-economic background characteristics include, in addition to age at entry: gender, dummies for the reserved caste (which we know to be mechanically correlated with age at entry due to the higher age eligibility for reserved castes), and a dummy for whether the IAS officer is coming from an urban area. The set of education characteristics include a dummy for a STEM or Economics degree as well as a dummy for having received an academic distinction, as measured by a first-class honours in undergraduate or a distinction in graduate studies (equivalent to a GPA above 3.0). The previous work experience controls include dummies for a prior job in education and research, the private sector, the non-AIS public sector or the AIS (IPS and IFS).[26] The omitted category comprises individuals that entered the IAS without any previous work experience. Finally, we include entry and training scores: the standardized UPSC score, the standardized training score, as well as a dummy $improved_i$ that equals 1 if the officer did better in training than on the entry exam. The UPSC and training scores are standardized

[24] As interviewers were trained to collect data using one data collection method (face to face, phone or web), the effects also absorb level differences driven by different data collection methods.

[25] Our results are also robust when clustering on the individual IAS officer level.

[26] The All India Services comprise the Indian Administrative Service, the Indian Police Service and the Indian Forestry Service which are, in that order of importance, the elite branches of the civil service in India.

within each intake year, thus indicating the relative position of an officer in a given cohort. The dummy $improved_i$ equals 1 if the officer's relative position during training - measured as standard deviations from the mean - improved compared to the UPSC score.

The results are summarized in Table 2.3. All columns in Table 2.3 estimate the same regression described in equation (1), except that we vary the dependent variable of interest to span all of the five subjective performance measures considered in our 360 degree survey. To keep the table succinct, we only report the coefficients for the entry exam score which, as a proxy for ability, is likely to be a major confounder of the observed negative relationship. Appendix Table B.4 reports the full set of coefficients.

Despite holding constant such a rich set of background controls in this multivariate setting, individuals who enter the IAS at an older age still receive statistically significantly more negative ratings across all dimensions. Note also that the results show robust correlations between the entry exam score and the subjective performance ratings. Officers that obtained higher scores on the entry exam receive stronger evaluations in our 360 degree assessment survey. This is particularly striking as we are examining a highly selected sample of top ranking exam takers.

In summary, while there are multiple reasons beyond the disincentivizing effects of rigid promotion rules that might explain a negative correlation between age at entry and effectiveness, we conclude from Table 2.3 that the relationship is robust to controlling for other sources of observed heterogeneity between IAS officers. But of course this does not rule out the possibility that the correlation of interest is driven by unobserved sources of heterogeneity. In the following sections, we propose alternative empirical tests that make more direct use of variation in how binding promotion rules are across officers due to the quasi-random assignment of officers to state cadres each year. We also exploit the natural experiment induced by the 1998 pension reform which extended the retirement age by two years and hence mechanically gave officers that entered the service older in affected cohorts a greater chance of reaching the upper echelons of the bureaucracy before retirement.

2.3.2 Age at entry, cohort size and individual effectiveness

We hypothesize that starting in the IAS older reduces individuals' performance, as rigid promotion rules and a fixed retirement age mechanically means that they have a lower chance of reaching the highest echelons of the bureaucracy. Extending this logic further, being an old entrant in a state cadre cohort that is relatively large (i.e. with many officers in the state that have the same tenure) might be particularly demotivating as a large cohort further reduces the chance of being promoted in any given year. An older officer that enters in a smaller cohort knows that he or she will be one of only very few eligible for promotion when entering the 25th or 30th year in the IAS, and hence their chance of being promoted before having to retire will be higher. In contrast, an older officer that enters in a relatively large cohort should expect more delays and is more likely to be forced into retirement before reaching the top of the bureaucracy. All this suggests an alternative empirical test - there should be a negative correlation between individual effectiveness and the interaction term between the individual's age at entry into the IAS and the size of the state cadre cohort he or she was quasi-randomly allocated to, holding age at entry and cohort size constant. We therefore augment equation (1) by interacting age at entry with the cohort size of the state cadre batch the IAS officer was allocated to:

$$\begin{aligned} score_{ijk} &= \alpha \times age\_entry_i + \beta \times cohort\_size_i + \\ &\quad \gamma \times age\_entry_i \times cohort\_size_i + \\ &\quad \mathbf{x_i}'\delta + \mathbf{z'_{ij}}\eta + \tau_k + \theta_j + \varepsilon_{ijk} \end{aligned} \tag{2.2}$$

where $cohort\_size_i$ is the number of officers that were assigned to the same cadre in the same year as the officer that is being rated. Cohort size is centered around the sample mean to facilitate interpretation of the results. The other variables are as described in equation (1). In particular, we include the same set of individual background controls as in Table 2.3.

Our results are presented in Panel A of Table 2.4. Consistent with the hypothesis that rigid bureaucratic rules might demotivate older entrants, we

find that the negative relationship between age at entry and effectiveness is magnified in the presence of larger cohort sizes (Column 1). The interaction term between age at entry and cohort size is also a negative predictor of an officer's overall rating (Column 5). While also negative, the estimated coefficients on this interaction term and the three other subjective assessments (probity, ability to withstand political pressures and pro-poor orientation) are not statistically significant.

The key advantage of this test over that presented in Table 2.3 is that worries about unobserved correlates of age that might directly affect bureaucratic performance are no longer relevant. However, this specification is not without its own concerns about unobserved heterogeneity. Other individual characteristics may differentially impact an officer's performance in larger versus smaller cohorts. The appeal of this test however compared to that in Table 2.3 is that introspection does not as easily suggest obvious concerns or threats to our proposed career incentive-based interpretation.

To address remaining concerns over unobserved confounders that also vary differentially with cohort size, we have replicated the results in Panel B adding a full vector of interaction terms between cohort size and the rich set of individual background characteristics. The estimated effect on the interaction term of interest (age at entry $\times$ cohort size) is nearly unchanged compared to Panel A and remains statistically significant in Columns 1 (effectiveness) and 5 (overall rating).

2.3.3 Additional evidence

The main threat to the interpretation of our findings as disincentives induced by the rigid seniority-based progression rules remains selection on unobservables. Those who enter older may differ in unobserved characteristics from those who enter young and those characteristics might differentially relate to effectiveness depending on cohort size. Furthermore, while the negative coefficient on age at entry $\times$ cohort size may indeed reflect lower incentives for those who anticipate not being able to climb to the highest echelons, we cannot rule out that older officers perform worse in larger cohorts for other reasons

than those related to the rigidity of promotion rules. Finally, some doubts may remain about the quality of our survey-based effectiveness ratings. In this section, we propose three additional tests that help alleviate these remaining concerns.

Cohort age composition and individual effectiveness

In the first additional test, we exploit cross-sectional variation in the age composition of cohorts to which officers are allocated. As is discussed in more detail in Section 4, the allocation of groups of officers to a given state cadre in a given year is a quasi-random process. This means that, holding an officer's age at entry and the size of his or her cohort constant, there will be as good as random variation in the share of officers in the batch that are younger or older than the given officer. This variation can be exploited to assess the impact of the bureaucratic rigidity on effectiveness. Indeed, having many officers in one's cohort that will reach retirement age later than oneself mechanically decreases one's chance of reaching the highest bureaucratic echelons while having many officers in one's cohort that will reach retirement age earlier than oneself mechanically increases one's chance of reaching the highest bureaucratic echelons. Hence, we would expect an officer to perform worse as the number of younger officers in his or her cohort increases. The appeal of this additional test is that it can be implemented by holding constant age at entry as the test only relies on the comparison of relative age among officers in the same cohort.

We use the specification described in equation (1), controlling for the same set of background controls (individual background characteristics, education, previous work experience and entry exam performance), as well as the same set of fixed effects described in Section 3.1. We add two additional independent variables that capture the cadre age composition: the number of younger and older officers in the individual's cadre. Since the variation in the number of younger and older officer relies on the age composition of the cohort, we can hold constant the individual's age at entry using fixed effects.

The results are presented in Table 2.5. We find that officers who entered at the same age but have a larger number of younger officers in their cohort are deemed statistically significantly less effective (Column 1), less able to withstand illegitimate political pressure (Column 3) and receive a lower overall rating (Column 5). In contrast, exposure to a larger number of same age or older officers is not associated with differential perceived performance, even though all the point estimates are positive.

Direct measures of performance and longitudinal data

A different potential concern about our results relates to the quality of our 360 degree survey ratings. Yet, while systematic biases might likely exist in the ratings when it comes to background characteristics (e.g. negative views against women or minority castes might translate into negative subjective performance assessment absent evidence for such negative assessments), it is more difficult to think about such systematic biases driving the ratings for a variable such as age at entry $\times$ cohort size. In other words, while there might be unfair discrimination against some groups of officers, it is difficult to imagine what would drive systematic negative biases against older officers in larger cohorts.

Nonetheless, we also use suspensions as a direct measure of non-performance.[27] Unlike the subjective assessments, suspension data is available for all IAS officers, providing an additional robustness check to alleviate concerns of sample selectivity. In particular, using the publicly available executive record sheets, we can study suspensions for all centrally recruited IAS officers over the period 1980-2012. This is the period for which the executive record sheets cover all IAS officers.

While objective in terms of measurement, we note that this measure also has its own limitations as suspensions may be politically motivated. An officer that is unwilling to countenance the corruption of top state politicians, for example, may be more likely to be suspended. We also note that suspensions

[27] Appendix Table B.6 shows that officers that have been suspended are percieved as less effective and indeed perform worse on all 360 degree performance dimensions.

are rare events and hence only provide a very crude measure of an officer's effectiveness. In fact, it is our lack of confidence in what these measures are capturing that originally motivated the 360 degree evaluations.

Notwithstanding these caveats, we use the individual-level panel data to run the following regression. For individual i in state s with k years of tenure in year t, we estimate:

$$\begin{aligned} y_{ikst} &= \alpha \times age\_entry_i + \beta \times cohort\_size_i + \\ &\quad \gamma \times age\_entry_i \times cohort\_size_i + \\ &\quad \mathbf{x_i}'\beta + \tau_t + \theta_s + \kappa_k + \varepsilon_{ikst} \end{aligned} \tag{2.3}$$

The unit of observation in the regression is now the IAS officer i with tenure k in state s and year t. The dependent variable is defined as the cumulative number of suspensions experienced by officer i up to year t. The regression also includes state fixed effects (θ_s), year fixed effects (τ_t) and year of tenure fixed effects (κ_k). The vector $\mathbf{x_i}$ is the rich set of individual background characteristics from equation (1) when we limit the sample to the subset of observations where we have both information on suspension from the executive record sheets as well as information from the descriptive rolls and internal rankings. The vector $\mathbf{x_i}$ is limited to gender when we extend the analysis to all centrally recruited officers over the period 1980 to 2012, as this is (in addition to age) the only officer-specific background characteristic available in the executive record sheets. Cohort size is again centered around the sample mean for ease of interpretation. Standard errors are clustered at the individual-level to account for the serially correlated nature of the panel data.

The results are presented in Table 2.6. In Columns 1 to 3, the sample is restricted to officers for which we have descriptive rolls data. The sample more than doubles in Columns 4 to 6 when we include all officers in the executive record sheets. All columns indicate that there is a positive relationship between age at entry and number of suspensions. However, the relationship is only significant (at the 10 percent level) in the full sample (Columns 4 to 6). More importantly, consistent with the results in Panel A of Table 2.4, we find that the number of suspensions for officers that entered older increases when these

officers were assigned to larger cohorts at entry. This pattern is robust to allowing for a richer set of fixed effects that confine the identifying source of variation to variation between IAS officers in the same state and of the same tenure (i.e. fixed effects for each state-tenure year group; Columns 2 and 5). In Columns 3 and 6, we show that this pattern is also robust to allowing for interactions between other individual background characteristics and cohort size (as in Panel B Table 2.4).

Pension reform and individual effectiveness

Finally, we provide evidence from the 1998 pension reform that relaxed the age at entry constraint as a source of rigidity in progression. In particular, the reform increased the retirement age for IAS officers by two years from 58 to 60 (see Figure 2.2). While all active IAS officers enjoyed two additional years in service, the pension reform disproportionally benefited those who entered older - under the new retirement schedule, previously age-constrained officers became more likely to qualify for senior promotions. To illustrate this, consider an IAS officer who entered the service at 29 years of age. Before the pension reform, this officer was mechanically barred from qualifying for promotions into the highest payscale which only open up after 30 years in service, since at this point they will have to retire. After the pension reform, however, this mechanical constraint is removed and the officer has one more year before retirement to qualify for the most senior position. Although an officer entering at 21 years of age will also enjoy two more years, the "return" to these two additional years are relatively lower for him or her. More generally, under the view that the negative correlation between individual effectiveness and age at entry captures negative incentives induced by the bureaucratic rigidity, we would expect this negative correlation to be weakened after the pension reform.

We implement this empirical test as a difference-in-differences (DD) regression. For officer i with tenure k in state s and year t, we estimate the impact of

age at entry on the performance outcome y_{ikst} as:

$$\begin{aligned} y_{ikst} &= \alpha \times age\_entry_i + \beta \times age\_entry_i \times post1998_t + \\ &\quad \mathbf{x_i}'\beta + \tau_t + \theta_s + \kappa_k + \varepsilon_{ikst} \end{aligned} \tag{2.4}$$

where $post1998_t = 1$ after the pension reform and 0 otherwise. We define the dependent variable as a dummy variable that equals 1 if officer i with tenure k in state s was suspended in year t, and 0 otherwise. We note that for this particular test, suspension is the only available individual performance measure, as our 360 degree survey data is not historically available. The coefficient β captures the differential impact of an additional year of age at entry on the propensity to be suspended after the pension reform in 1998 and is the coefficient of interest. Again, the standard errors are clustered at the individual level to take into account the serially correlated nature of the data.

We estimate this equation on the full panel of IAS officers as in the last three columns of Table 2.6 covering all centrally recruited IAS officers over the period 1980-2012. We however exclude from the sample all officer-year observations in which the officer is older than 58, the retirement age before the pension reform. This ensures that our results are not contaminated by the fact that officers are mechanically older in the post-pension reform period than in the pre-pension reform period.

The results of this analysis are presented in Table 2.7 and suggest that the extension of the retirement age by two additional years effectively mitigated the negative impact of age at entry on suspension.

While increasing the age at entry by one additional year is associated with a 0.09 percentage point higher suspension rate, the increase in the propensity of older officers to be suspended disappears after 1998 (Column 1). Column 2 shows that this result is robust to allowing for a richer set of fixed effects that allow the suspension rate to differ systematically across officers from the same state and year of tenure. While decreasing precision, allowing for a differential trended impact of age at entry on suspension (Column 3) and introducing individual fixed effects (Column 4) does not substantially shift the point estimates of the coefficient of interest.

To further assess whether the results in Table 2.7 are indeed driven by the pension reform, we also estimate a more flexible version of equation (4) where we allow the age at entry coefficient to vary year by year. Figure 2.5 summarizes the results by plotting the estimated coefficients on age at entry for each year. The figure provides evidence consistent with the view that the diminishing effect of age at entry on suspension coincides with the pension reform. While the impact of age at entry is smoothly trending up over time, we observe a large and discontinuous decrease after the pension reform. Taken together, the evidence in this section is consistent with the view that entering the IAS at an older age reduces individual effectiveness (and increases the probability of suspension) at least in part due to weaker promotion prospects and hence weaker career incentives.

2.4 Effects on state-level economic performance

What are the implications of having a less motivated group of civil servants for aggregate economic outcomes? This is the question we take on in the final section of the paper. In Section 3.2, we suggested that officers that enter the IAS at an older age might be particularly demotivated when "competing" with a larger group of officers of the same tenure in their state as this reduces their chance of ever reaching the top echelons of the bureaucracy. Because IAS officers run all the key government departments and public sector corporations and enterprises, and are central to the implementation of policy reforms, it is possible that having a higher proportion of such demotivated officers (i.e. older officers in larger batches) in a state cadre may adversely affect economic performance.[28] In a subset of years, we observe age at entry and cohort size

[28] As a legacy of central planning and the percieved need of the state to control the commanding heights of the economy, large parts of the economies of Indian states still remain in the public sector and under state control. IAS officers head up important state enterprises (e.g. Bharat Petroleum Limited (Ministry of Petroleum & Natural Gas), Hindusthan Machine Tools (Ministry of Heavy Industries & Public Enterprises) or Indian Rare Earth Limited (Department of Atomic Energy)), run public corporations responsible for infrastructure (e.g. state electricity boards, electricity regulatory commissions, power generation and distribution companies) and oversee state banks and insurance companies (e.g. Industrial Development Bank of India, National Insurance Co. Ltd. (Ministry of Finance)). The influence of IAS officers

for the universe of IAS officers in each state cadre. Combined with data on state-level economic outcome measures, this allows us to assess the empirical relevance of this possibility.

For this state level exercise, we focus on the 1990 to 2011 period, a period of large-scale liberalization reforms. Because the IAS oversees the implementation of these reforms, it is possible that the motivation of its officers is germane to the successful implementation of these reforms and ultimately to the genesis of economic growth (Weber, 1922; Rauch, 1995; Rauch and Evans, 2000; Dal Bo et al., 2013). As for the 360 degree survey data collection, we focus on the 14 main states in India. Moreover, we again focus on officers with at least eight years of tenure, as they are more likely to be in charge of policy implementation at the state level (rather than at the district level).

The allocation rule for IAS officers is central to our analysis in this section. As indicated previously, the allocation process follows a set of rules that effectively generates a quasi-random allocation of IAS officers across states. The process follows three stages (see the online Appendix for a more detailed description): (i) officers are assigned serial numbers in order of merit, as determined by the civil service exam, (ii) vacancies determine the number of officers needed in each state (i.e. cohort size) and (iii) officers are then allocated to these vacancies by cycling through the list of states. Separate number lines denoting caste status and insider/outsider status are used to match officers to vacancies.[29] The order of states rotates across years, ensuring that all states have their turn at receiving the best talent.

Appendix Table B.5 formally tests for the quasi-random allocation of the IAS officers across the 14 main states of India. For this table, the sample of officers is restricted to the group for which we have rich individual background data - those officers we observe in the descriptive rolls and internal ranking data. This corresponds to all intake years between 1972 and 2005. We regress individual officers' characteristics on assignment state fixed effects and entry

thus extends beyond government departments and into large swathes of the productive parts of state economies.

[29]Thus while IAS officers can indicate their preference for home state, the quasi-random allocation process ensures that only a small minority of IAS officers are allocated to their home state.

year fixed effects. We then test for the equality of the estimated state fixed effects. The corresponding p-values of the test are presented in Column 1 of Appendix Table B.5. Based on our rich set of observable individual characteristics, we cannot statistically reject that states receive, on average, officers that are statistically indistinguishable as regards age at entry, gender, rural/urban background, caste affiliation, education, work experience, scores on the entry exam and training marks.

Of course, due to the relatively small number of officers assigned to a state each year, there will be variation in officer characteristics within states over time, and within intake year across states. Appendix Figure B.6 and Appendix Figure B.7 show the exam entry score and age at entry of IAS officers allocated to the major states by year of intake. On average, and as formally demonstrated in Appendix Table B.5, we see that all states receive officers of the same quality (as proxied for by the exam score) and age at entry. But there is within-state fluctuation around these means. In other words, by chance, a given state may have, for example, a disproportionate share of older officers serving in its senior ranks at a given point in time. This variation is clearly exogenous to state economic outcomes at that point in time. This is true both because of the quasi-random allocation process at entry but also because this allocation process takes place many years before these officers have reached the positions from which they are implement state-level policies.

There is also within-state variation over time in the entry cohort sizes, as is evident from Appendix Figure B.8. The year-on-year fluctuation in entry cohort sizes within a state is determined by the number of vacancies that arise in that state and year. While one might argue that this variation might be endogenous to economic conditions in a state at the time of entry, this variation is arguably more exogenous to state economic conditions at the time these cohorts are eight or more years into their tenures.

Our key variable of interest in this analysis, as in the individual data analysis in Section 3.2, is the interaction between the age at entry of serving officers and the size of their cohort. Figure 2.6 shows this variation across states by intake year. Given the logic above, we argue that this variation is exogenous to state economic conditions, and in particular to state economic conditions by

the time the officers in those intake years are eight or more years into their tenures.

Having argued that the allocation of IAS officers to states provides the basis of a credible research design, we now lay out the precise empirical analysis we perform in the state-year panel. Let s denote the state in which officer i is serving at time t. The year of tenure is given by k. The average age at entry of a state cadre s at least eight years into the IAS in year t is:

$$\overline{age\_entry}_{st} = \frac{\Sigma_{\{(i,t)|S=s \wedge k \geq 8\}} age\_entry_i}{\Sigma_{\forall(i,t)} \mathbf{1}[S = s \wedge k \geq 8]} \tag{2.5}$$

In words, we aggregate the individual-level data to the state-year level by calculating the mean age at entry of all active IAS officers with tenure ≥ 8 in that state and year. We perform this aggregation for each year between 1990-2011. We use the same aggregation method as described in (5) to compute the average state cohort size in a given state and year, $\overline{cohort}_{st}$. Mirroring the individual level analysis in Section 3.2, the key independent variable of interest in the state-year panel is the interaction between $\overline{cohort}_{st}$ and $\overline{age\_entry}_{st}$.

We note that, in computing these state-year means, we ignore transfers to the central government or leave abroad (e.g. a posting at an international organization or training assignment), which do not affect membership of the state cadre. As secondments to Delhi and leaves are likely to be endogenous to current state economic conditions, we focus on the $(\overline{cohort}_{st})(\overline{age\_entry}_{st})$ variation induced by all state cadre officers irrespective of whether they are present in the state or not. A potential concern, however, lies in the endogenous exit or transfers of IAS officers to other states. If older officers are more likely to exit or transfer when growth is fast, the state-level correlation between age at entry and cohort size may be spurious. Since compliance with the strict retirement age is high and transfers to other state cadres are *de facto* negligible, we argue that this is unlikely to be a major source of bias. We also verified that deviations from the retirement age are not correlated with contemporaneous state-level economic performance.

For state s and time t, we estimate the following reduced form state-level regression:

$$ln(Y)_{st} = \beta_0 \times \overline{age\_entry}_{st} \times \overline{cohort}_{st} + \beta_1 \times \overline{age\_entry}_{st} + \quad (2.6)$$

$$\beta_2 \times \overline{cohort}_{st} + \bar{x}'_{st}\gamma + \theta_s + \tau_t + \varepsilon_{st} \quad (2.7)$$

where the dependent variable Y_{st} is the state-level outcome of interest, and the independent variables, such as $\overline{age\_entry}_{st}$, are constructed as described in (5). Following the standard specification in a growth regression framework, we add state fixed effects θ_s and year fixed effects τ_t. The vector x_{st} controls for the (log) overall state cadre size, which also includes IAS officers below eight years of tenure and recruited from the state civil services, as well as region-specific linear time trends. The standard errors are clustered at the state-level. While we would in practice like to include in $\bar{x}_{st}$ other characteristics of the average IAS officer active in a state in a given year, the only other characteristic available for this state-level analysis is gender. This is because the descriptive rolls and internal ranking data, which contain the rich individual level characteristics, only start with the 1972 intake. Many officers active in the 1990s and even the early 2000s had joined the IAS before 1972.

A key alternative specification we perform in the analysis below separates all active officers with tenure ≥ 8 into senior (at least 16 years of tenure) and junior (8-15 years of tenure). A priori, it is the senior officers that we would expect to have the greater influence on economic outcomes within a state and we can empirically verify that. The key constructs above ($\overline{cohort}_{st}$, $\overline{age\_entry}_{st}$ and their interaction) can be constructed for these two groups of officers using the same aggregation approach as above but appropriately restricting the values of k.

2.4.1 Effects on GDP

Table 2.8 reports the impact of average age at entry $\times$ average cohort size on state-level GDP per capita. The dependent variable is either total state-level GDP per capita or a sectoral GDP per capita component (agriculture, industry or services). The average cohort size is centered around the sample mean.

We first briefly comment on the estimated coefficient on the direct effect of age at entry. In the individual-level analysis in Section 3.1, we showed that being older at entry was correlated with lower effectiveness. While we argued that this correlation might in part be due to the lower career incentives IAS officers that enter older face due to the bureaucratic rigidity, we also discussed the obvious issues in separating such incentive effects from issues related to selection and omitted variable biases. These interpretational concerns are also present in these state-level regressions as we cannot control for the rich vector of other individual level characteristics as we could in Section 3.1 due to the data constraints discussed above. In particular, a direct effect of average age at entry of active IAS officers on state outcomes may reflect an effect of other average characteristics of the active IAS officers that are correlated with their age. Moreover, even if age at entry is indeed the key characteristic of the IAS officers leading to differential state outcomes, this could be due to selection rather than career concerns induced by the bureaucratic rigidity.

With these caveats, we find that the estimated coefficient on average age at entry is negative across all specifications but statistically insignificant. Focusing on the point estimates, the magnitudes suggest that increasing average age at entry of the state cadre by one year is associated with a 10% reduction in total output per capita (Column 1). While this estimated magnitude appears large, the actual variation in the average age at entry is small. A one standard deviation increase in the average age at entry corresponds to about 0.3 years. Put differently, while the statutory age at entry window increased by 4 years in our sample period of 1990 to 2011, the actual average age at entry only increased by about one year showing that it is difficult to move the average entry age of the state cadre (Appendix Figure B.1). Historically for the period 1960-2011, an increase in the maximum age at entry by one year is associated with an increase of the actual average age at entry by 0.16 years.[30] Using a back of the envelope calculation, this elasticity would imply that an increase in the statutory maximum age at entry by one

[30]This elasticity is estimated by regressing the average actual age at entry on the maximum statutory age at entry for regular entrants and controlling for a linear trend.

year would be associated with a one-off decrease in state-level GDP per capita by 1.6%.

Of more interest to us is the estimated coefficient on the interaction term between average age at entry and average cohort size, which, as we argued in Section 3.2, puts a sharper spotlight on the bureaucratic rule-induced career concerns that may demotivate older IAS officers. This key interaction term is statistically significant and negative in Column 1, suggesting that the negative impact of average age at entry is magnified in the presence of larger average cohorts. This result mirrors the individual-level results and is consistent with the view that the career incentives of IAS officers have a direct bearing on the performance of Indian states, affecting the lives of millions of people. The magnitude of this effect is non-trivial. A one standard deviation in average age at entry (0.3 years) reduces overall state-level GDP by about 3.9% when combined with a one standard deviation in average cohort size (2.6).

If IAS officers indeed have a bearing on state-level outcomes, we expect their impact to primarily affect sectors over which the state has purchase (Kocchar et al. 2006). When breaking down the aggregate GDP into its components, we find that the average age at entry $\times$ cohort size only affects the non-agricultural sector (see Columns 3 and 4 in Table 2.8). The impact is largest on the industrial sector (Column 3). This is perhaps not surprising as the Indian industrial sector is well known for its large organized components, where government policies such as those related to regulation, taxation and public good provision may affect private firms and where state-owned enterprises still play a major role (Basu and Maertens 2007). The service sector is also significantly affected. The impact on agricultural output, however, is statistically indistinguishable from zero (Column 2). We reconcile this pattern using the fact that the bulk of agricultural production in India is subsistence agriculture, which is largely unorganized.

Figure 2.7 presents visual evidence for the relationship we have uncovered in Table 2.8. The figure plots the partial correlation between age at entry $\times$ cohort size and state-level outcomes, which are either overall state-level GDP per capita or sector-specific (agriculture, industry, services). We compute the partial correlations, stripping away state and year fixed effects as well as the

impact of all other control variables before plotting the remaining correlation. The figure visually confirms that states, in years when serving officers entered older and in larger cohorts, exhibit lower overall GDP and non-agricultural GDP per capita.

Since it is the senior IAS officers who occupy the key positions at the state-level, one may expect the variation in average age at entry $\times$ average cohort size among these IAS officers to matter more. To empirically test this, we replicate the specification in equation (6) but separately compute the state-year aggregates (average age at entry, average cohort size and their interaction, share of female officers) for active officers with 8-15 years of tenure (which we label as junior officers) and for active officers with 16 or more years of tenure (which we label as senior officers). The cut-off at 16 is chosen to reflect the move from junior to "supertime" scale, which is equivalent to senior positions such as Joint Secretaries. This split also corresponds to the median years of tenure.

Panel A of Table 2.9 reports the results of this analysis. We focus on the two key interaction terms of interest: average age at entry among senior (junior) active officers interacted with average cohort sizes among senior (junior) active officers. The results confirm that the observed negative impact of a higher average age at entry when cohort size increases is indeed driven by the senior IAS officers. In Figure 2.8, we once again provide the corresponding visual evidence. While the relationship for senior IAS officers is negative, the relationship is flat for junior IAS officers indicating that the effect is coming through senior officers.

2.4.2 Effects on other economic outcomes

Panels B to D of Table 2.9 replicate the analysis in Panel A of that Table, probing deeper into various components of state economic performance.

Column 3 of Table 2.8, as well as Column 3 of Panel A of Table 2.9, suggest particularly large effects in the industrial sector. In Panel B, we drill into the industrial sector by breaking down its components. We find that officers who are older at entry and in larger cohorts are particularly detrimental for

the subset of industries that are dominated by state organizations, and hence under tighter control from the IAS. In particular, we find that variation in the state cadre composition of IAS officers affect the economic performance of the mining and quarrying sector (Column 1), which makes up slightly less than 10% of India's industrial GDP in 2011. This is a sector subject to substantial regulation and licensing requirements, with a large number of public sector undertakings, such as Bharat Industries or the Oil & Natural Gas Corporation, that are under the oversight of ministries such as the Ministry of Mining, Ministry of Coal or the Ministry of Petroleum & Natural Gas. As in Panel A, it is the variation among senior officers that has the largest bearing on outcomes.

The dissection of manufacturing into registered (Column 2) and unregistered (Column 3) sectors corroborates our findings from Section 4.1, whereby IAS officers primarily affected growth through the organized sectors (i.e. not in agriculture). The registered sector includes all firms with more than 10 workers that are officially registered under the Factories Act of 1948, accounting for approximately 70% of the average total state-level manufacturing output in 2011. Consistent with the fact that IAS officers have greater purchase over formal industries (Besley and Burgess, 2004; Asher and Novosad, 2016), a higher average age at entry and cohort size has a negative bearing on registered manufacturing while having no discernible impact on the unregistered, informal, manufacturing sector. We also find negative impacts on utilities, which comprise electricity, gas and water supply which are often state-run and under the control of senior IAS officers (Column 4).

In Panel C of Table 2.9, we attempt to validate our findings of impacts on GDP in the formal manufacturing sector with evidence from another independent dataset. We draw on state-level data from the Annual Survey of Industries (ASI) for the same period 1990-2011. The ASI is a census of all registered manufacturing establishments using more than 10 (20) workers when (not) using power. The ASI provides detailed information about production, employment and input costs. The results in Panel C suggest impacts on the expansion of formal manufacturing and employment. States with cadres of higher entry age and cohort size see a slower expansion of industries, as measured by the number of manufacturing establishments

(Column 1) and by total employment (Column 2). These are two margins that are subject to tight regulation. Workers in the registered manufacturing sector are covered by the Industrial Disputes Act (IDA) of 1947, which places substantial constraints on the extent to which firms can lay off workers (Besley and Burgess, 2004). While statistically insignificant, the point estimate for the impact on net value-added, a proxy for productivity, is negative (Column 3). Finally, the negative impact on industrial output, as measured by the ASI (Column 4), validates our main state-level result (Table 2.8). Overall, the evidence from the ASI is consistent with an effective bureaucracy being conducive to state-level industrial growth by facilitating new business creation or existing business expansion (Aghion et al., 2008).

In Panel D of Table 2.9, we zoom into the service sector to explore channels through which bureaucrats may impact its growth. The results in Panel D show primarily impacts on state-dominated service sectors, consistent with the previous breakdowns by agricultural versus non-agricultural (Table 2.8) and registered versus unregistered manufacturing (Panel B). We find significant impacts on the segment of the sector encompassing transport, storage and communications (Column 2), which make up about 11% of the service sector in 2011, the bulk of which is attributable to economic activity in railways and road transportation. There is also a large impact on the banking and insurance sub-sector (Column 4), which - despite deregulation - remains dominated by state owned banks. Again, in both cases, the effect is driven by more senior officers. In contrast, there is no statistically significant impact on retail trade, hotels and restaurants (Column 3) and construction (Column 1) which, in India, are largely unorganised.

Overall, the state-level results paint a robust and coherent picture of how variation in the motivation of bureaucrat cadres, as induced by bureaucratic rigidity, can impact economic outcomes at the state-level. The breakdown by dimensions such as junior versus senior, as well as the disaggregation by sectors, provides evidence consistent with senior officers exerting disproportionate control, and having disproportionate impact, over regulated sectors of the economy.

In the remaining two panels of Table 2.9, we conclude this state-level analysis by examining potential impacts on public expenditures (Panel E) and public revenue (Panel F), two key dimensions of state capacity (Besley and Persson, 2009). A less motivated group of senior bureaucrats may negatively impact economic outcomes at the state level by doing less, which would translate in lower public spending (e.g. new schemes are not being implemented, or are slow in being implemented) and lower revenue. In Panel E, we consider possible impacts on total public expenditures (Column 1) but also isolate social and economic expenditures. Social expenditures comprise spending on education, health and welfare, while economic expenditures comprise spending on rural development, special area programmes, energy, industry, transport and communications. Consistent with the GDP analysis, we find that age at entry $\times$ cohort size has negative impacts on total public expenditures (Column 1), and that the effect appears particularly large for economic expenditures, which might be particularly conducive to industrial growth (Column 3). Again, it is the composition of the senior segment of the bureaucracy that appears to matter. In Column 4 of Panel E, we examine whether the number of large scale development projects, as measured by the number of World Bank funded projects, is also affected. While of the expected sign (negative), the estimated coefficient is not statistically significant.

Finally, Panel F of Table 2.9 suggests that state cadres with a higher average age at entry and larger average cohort size are also associated with lower revenue generation. The effect is primarily driven by lower tax revenue, either coming straight from state taxes (Column 2) or obtained from taxes levied by central government (Column 3). Again the effects are driven by senior officers who occupy leadership positions in the government departments responsible for collecting both state and central taxes in the Indian states.

2.5 Conclusion

The organization of the state is attracting increasing attention within economics as a central determinant of economic performance (Besley and Persson, 2009;

Finan et al., 2015). Bureaucrats and bureaucracies are considered to be the backbone of the modern state but have been little studied. A recent wave of papers that has studied selection and incentives of public servants has tended to focus on lower level bureaucrats and frontline providers (Ashraf et al., 2014; Dal Bo et al., 2013; Deserranno, 2016; Khan et al., 2015; Nath, 2015; Rasul and Rogger, 2016). This contrasts with an older literature that sees bureaucrats and bureaucracies as central to the industrialization and growth processes (Northcote and Trevelyan, 1854; Tullock, 1965; Weber, 1922; Rauch, 1995; Rauch and Evans, 2000).

Our paper fits between these two literatures by studying the Indian Administrative Service. IAS officers hold positions at the apex of government that grant them significant influence over the implementation of policies, rules and regulations that may affect growth in a country of over a billion people. How well incentivized or motivated they are may have far ranging effects as their effort and actions affect not only the actions of subordinates but also, via policy, the actions of individuals, households and firms which jointly determine economic growth.

By fielding a large-scale survey in fourteen states in 2012-13 we were able to open the black box of what determines bureaucratic effectiveness. Our individual-level results, which leverage several empirical strategies, suggest that the combination of seniority based promotion rules and a fixed retirement age reduce bureaucrats' effectiveness by dimming promotion prospects and weakening career incentives. In a state-year panel, we find that state cadres containing a higher proportion of officers who entered older and in larger cohorts, and for whom the combination of seniority based promotion rules and a fixed retirement age might be particularly demotivating, experience worse economic outcomes. Moreover, these effects are driven by the impact on industry and services which are the more organized sectors in India and therefore more likely to be affected by policies controlled by IAS officers. We also show that it is the senior IAS officers that appear responsible for these effects.

Overall, our paper sheds light on some of the costs associated with the rigid rules that govern bureaucracies such as the IAS. However, our paper does

not answer the broader question as to whether the top Indian civil service would be more effective if freed of these rules. Indeed, it is possible that a first-order reduction in patronage, favoritism and influence activities, that a bureaucratic system is meant to confront, dominate what might only be the second-order costs highlighted in our study. This broader question is beyond the scope of our paper. However, by isolating costs, our study certainly calls for further work on trying to isolate those potential benefits.[31] Only then would one be able to engage in a robust policy discussion about possible better ways to organize an elite civil service such as the IAS.

More humbly, our study does suggest that some more marginal, and politically realistic, changes in the organization of the IAS might be beneficial. In particular, our study provides empirical support for the concern raised by the Administrative Reform Commission (ARC, 2008) about the rising maximum eligible age at entry into the IAS. Our study also suggests that a reform that would move away from an age-based retirement system towards a system with fixed tenures for all civil servants irrespective of joining age should be seriously considered.

[31] Xu (2016), for example, provides evidence for an improved selection and allocation of governors following the removal of patronage in the British colonial administration.

2.6 Tables and Figures

Table 2.1: Descriptive statistics of 360 performance measures

	(1) Mean	(2) SD	(3) Ratings	(4) Officers	(5) Coverage
Effectiveness on the job	3.730	1.077	17,753	1,472	71.14%
Probity of IAS officer	3.670	1.105	15,153	1,451	70.13%
Withstanding illegitimate pressure	3.523	1.094	16,728	1,471	71.09%
Sensitive towards poorer	3.527	1.141	17,047	1,471	71.09%
Overall rating	3.646	1.057	17,698	1,472	71.14%

Notes: Performance scores for the cross-section of rated IAS officers in 2012-13. Reporting the descriptive statistics (mean and standard deviation) for the subjective measures, where the scores range from 1 (lowest) to 5 (highest). Column 3 and 4 report the total number of ratings and the total number of rated officers. Column 5 reports the coverage rate for the sample population of all active, centrally recruited IAS officers with at least 8 years of tenure in 2012/13.

Table 2.2: Individual characteristics of IAS officers in 2012

	(1)	(2)	(3)	(4)	(5)	(6)
			Percentile			
	Mean	SD	10%	50%	90%	Obs.
Age at entry	24.474	2.088	22	24	27	1,472
Female	0.141	0.349	0	0	1	1,472
Other backward caste (OBC)	0.081	0.273	0	0	0	1,472
Scheduled caste (SC)	0.141	0.349	0	0	1	1,472
Scheduled tribe (ST)	0.052	0.222	0	0	0	1,472
Urban background	0.737	0.439	0	1	1	1,472
Academic distinction	0.326	0.468	0	0	1	1,472
STEM or Economics degree	0.324	0.468	0	0	1	1,472
Previous job: Education/research	0.168	0.374	0	0	1	1,472
Previous job: Private/SOE	0.121	0.326	0	0	1	1,472
Previous job: Public sector	0.324	0.468	0	0	1	1,472
Previous job: Public AIS	0.033	0.181	0	0	0	1,472
Ranking in year of intake	53.896	35.549	10	49	104	1,472
Cohort size	7.334	3.971	3	7	12	1,472

Notes: Mean, standard deviation and percentiles of IAS officers in 2012-13. Sample covers the cross-section of centrally recruited IAS officers in 2012-13 with performance ratings. Urban background denotes officers from urban backgrounds, Academic distinction is a dummy for having received an academic distinction. STEM is a dummy for graduates of Science, Technology, Engineering and Mathematics and Economics degrees. Previous job denotes the sector of employment previous to entry into IAS (Education/research, Private sector/State-owned-enterprise, Public sector-Non All India Service, Public sector-All India Service). Ranking is the rank of the officer among all who entered in a given year based on the entry and training scores. Cohort size is the number of IAS officers allocated to the same state in a given year.

Table 2.3: Age at entry and effectiveness

	(1) Effective	(2) Probity	(3) Pressure	(4) Pro-Poor	(5) Overall
Mean of dep. var	3.730	3.671	3.524	3.528	3.647
Age at entry	-0.009**	-0.009**	-0.015***	-0.007*	-0.010**
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Entry score	0.041***	0.021	0.023**	0.021*	0.041***
	(0.010)	(0.013)	(0.011)	(0.012)	(0.010)
Background controls	Yes	Yes	Yes	Yes	Yes
Interviewer FEs	Yes	Yes	Yes	Yes	Yes
State-specific respondent FEs	Yes	Yes	Yes	Yes	Yes
Tenure year FEs	Yes	Yes	Yes	Yes	Yes
Source of information FEs	Yes	Yes	Yes	Yes	Yes
Observations	17,750	15,138	16,719	17,043	17,695

Notes: Unit of observation is the score for a given IAS officer in 2012-13 with at least 8 years of tenure. Relating the five performance measures (effectiveness, probity, ability to withstand illegitimate political pressure, pro-poor orientedness and overall rating) to age at entry. Entry score is the standardized UPSC score in the year of intake with a mean of 0 and a standard deviation (SD) of 1. Background controls are: a female dummy, caste dummies (OBC, SC, ST), a dummy for coming from an urban background, having received an academic distinction, a STEM or Economics degree, having worked in education/research, private sector/SOEs, public sector, public AIS, a standardized training score, as well as a dummy that is 1 if the officer improved the ranking in the training relative to the entry exam. The summary statistics of the background controls are shown in Table 2.2. Interviewer FEs are dummies for each interviewer. State-specific respondent FEs are fixed effects for each respondent. Tenure year FEs are dummies for each year since entering the IAS. Source of information FEs are dummies for whether the respondent knows the officer personally, through friends or only through media. Robust standard errors in parentheses, clustered at the respondent level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.4: Age at entry, cohort size and effectiveness

Panel A	(1)	(2)	(3)	(4)	(5)
	Effective	Probity	Pressure	Pro-Poor	Overall
Mean of dep. var	3.730	3.671	3.524	3.528	3.647
Age at entry	-0.013***	-0.012**	-0.017***	-0.008*	-0.014***
	(0.005)	(0.006)	(0.005)	(0.004)	(0.005)
Cohort size (centered)	0.064**	0.047	0.044	0.027	0.075**
	(0.028)	(0.033)	(0.031)	(0.029)	(0.029)
Age at entry × Cohort size	-0.003**	-0.002	-0.002	-0.001	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Interviewer FEs	Yes	Yes	Yes	Yes	Yes
State-specific respondent FEs	Yes	Yes	Yes	Yes	Yes
Tenure year FEs	Yes	Yes	Yes	Yes	Yes
Source of information FEs	Yes	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes	Yes
Observations	17,750	15,138	16,719	17,043	17,695
Panel B	Effective	Probity	Pressure	Pro-Poor	Overall
Mean of dep. var	3.730	3.671	3.524	3.528	3.647
Age at entry	-0.012***	-0.012**	-0.016***	-0.009**	-0.014***
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)
Cohort size (centered)	0.066**	0.041	0.030	0.048	0.073**
	(0.029)	(0.035)	(0.032)	(0.031)	(0.034)
Age at entry × Cohort size	-0.003**	-0.001	-0.001	-0.002	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Interviewer FEs	Yes	Yes	Yes	Yes	Yes
State-specific respondent FEs	Yes	Yes	Yes	Yes	Yes
Tenure year FEs	Yes	Yes	Yes	Yes	Yes
Source of information FEs	Yes	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes	Yes
Controls × Cohort size	Yes	Yes	Yes	Yes	Yes
Observations	17,750	15,138	16,719	17,043	17,695

Notes: Unit of observation is the score for a given IAS officer in 2012-13 with at least 8 years of tenure. Panel A relates the five performance measures (effectiveness, probity, ability to withstand illegitimate political pressure, pro-poor orientedness and overall rating) to age at entry, cohort size and their interaction, where cohort size is the size of the state cohort in which the officer was allocated to, centered around the sample mean. Background controls are: a female dummy, caste dummies (OBC, SC, ST), a dummy for coming from an urban background, having received an academic distinction, a STEM or Economics degree, having worked in education/research, private sector/SOEs, public sector, public AIS, standardized scores for the (UPSC) entry and training scores, as well as a dummy that is 1 if the officer improved the ranking in the training relative to the entry exam. Interviewer FEs are dummies for each interviewer. State-specific respondent FEs are fixed effects for each respondent. Tenure year FEs are dummies for each year since entering the IAS. Source of information FEs are dummies for whether the respondent knows the officer personally, through friends or only through media. Panel B interacts all background characteristics with cohort size. Robust standard errors in parentheses, clustered at the respondent level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.5: Effectiveness and own cohort age composition

	(1)	(2)	(3)	(4)	(5)
	360 ratings - 2012/13				
	Effective	Probity	Pressure	Pro-Poor	Overall
Mean of dep. var.	3.730	3.671	3.524	3.528	3.647
Cohort size: Younger	-0.009*	-0.005	-0.014**	-0.009	-0.016**
	(0.005)	(0.007)	(0.007)	(0.006)	(0.006)
Cohort size: Older	0.000	0.002	0.000	0.004	0.000
	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)
Interviewer FEs	Yes	Yes	Yes	Yes	Yes
State-specific respondent FEs	Yes	Yes	Yes	Yes	Yes
Tenure year FEs	Yes	Yes	Yes	Yes	Yes
Source of information FEs	Yes	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes	Yes
Age at entry FEs	Yes	Yes	Yes	Yes	Yes
Observations	17,750	15,138	16,719	17,043	17,695

Notes: Unit of observation is the score for a given IAS officer in 2012-13 with at least 8 years of tenure. Relating the age composition of an IAS officer's cohort to the five measures of performance (effectiveness, probity, ability to withstand illegitimate political pressure, pro-poor orientedness and overall rating). Cohort size: Younger denotes the number of offices in the same state cohort that are younger, and Cohort size: Older denotes the number of officers in the same state cohort that are older. Background controls are: a female dummy, caste dummies (OBC, SC, ST), a dummy for coming from an urban background, having received an academic distinction, a STEM or Economics degree, having worked in education/research, private sector/SOEs, public sector, public AIS, standardized scores for the (UPSC) entry and training scores, as well as a dummy that is 1 if the officer improved the ranking in the training relative to the entry exam. Interviewer FEs are dummies for each interviewer. State-specific respondent FEs are fixed effects for each respondent. Tenure year FEs are dummies for each year since entering the IAS. Source of information FEs are dummies for whether the respondent knows the officer personally, through friends or only through media. Robust standard errors in parentheses, clustered at the respondent level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.6: Age at entry, cohort size and suspension 1980-2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of suspensions					
Mean of dep. var	0.0583	0.0583	0.0583	0.0637	0.0637	0.0637
Age at entry	0.001	0.001	0.001	0.004*	0.004*	0.004*
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Cohort size (centered)	-0.037*	-0.035	-0.039*	-0.009	-0.013	-0.012
	(0.021)	(0.021)	(0.023)	(0.011)	(0.011)	(0.011)
Age at entry × Cohort size	0.002*	0.002*	0.002*	0.001	0.001*	0.001*
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	-	-	Yes	-	-
Tenure year FEs	Yes	-	-	Yes	-	-
State-Tenure year FEs	-	Yes	Yes	-	Yes	Yes
Controls × Cohort size	-	-	Yes	-	-	Yes
Sample	Rich controls			Full sample		
Observations	42,629	42,605	42,567	108,725	108,663	108,663

Notes: Unit of observation is the IAS officer in a given year. The dependent variable is the cumulative number of suspensions of an IAS officer up to the given year. Columns 1-3 report the estimates based on the restricted sample which controls for rich individual-level background characteristics: a female dummy, caste dummies (OBC, SC, ST), a dummy for coming from an urban background, having received an academic distinction, a STEM or Economics degree, having worked in education/research, private sector/SOEs, public sector, public AIS, standardized scores for the (UPSC) entry and training scores, as well as a dummy that is 1 if the officer improved the ranking in the training relative to the entry exam. Columns 4-6 is based on the full sample for 1980-2012 and controls only for gender. Standard errors are clustered at the individual IAS officer level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.7: Impact of pension reform on suspensions - 1980-2012

	(1)	(2)	(3)	(4)
	Suspended ($\times$ 100)			
Mean of dep. var	0.740	0.740	0.740	0.741
Age at entry	0.089***	0.084***	0.083*	
	(0.028)	(0.029)	(0.048)	
Age at entry $\times$ Post 1998	-0.102**	-0.093**	-0.091	-0.103
	(0.040)	(0.040)	(0.072)	(0.073)
Age at entry $\times$ Year			-0.000	0.009*
			(0.003)	(0.005)
Year FEs	Yes	Yes	Yes	Yes
State FEs	Yes	-	-	-
Tenure year FEs	Yes	-	-	-
State-Tenure year FEs	-	Yes	Yes	Yes
Individual FEs	-	-	-	Yes
Individual control	Yes	Yes	Yes	-
Observations	107,540	107,537	107,537	107,369

Notes: Unit of observation is the IAS officer in a given year between 1980-2012. Relating the differential impact of age at entry before and after the pension reform in 1998 to suspensions. Suspension is scaled by 100 to improve readability. Individual control holds constant gender of the officer. Age at entry $\times$ Year allows age at entry to trend linearly. The year is centered around 1998. Standard errors are clustered at the individual level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.8: State-level GDP per capita and age at entry × cohort size

	(1)	(2)	(3)	(4)
	(log) State-level GDP per capita 1990-2011			
		Disaggregated by sector		
	Overall	Agriculture	Industry	Service
Age at entry	-0.102	-0.071	-0.168	-0.090
	(0.107)	(0.128)	(0.207)	(0.133)
Cohort size (centered)	1.324***	0.311	2.760***	1.577***
	(0.362)	(0.466)	(0.817)	(0.429)
Age at entry × Cohort size	-0.051***	-0.014	-0.115***	-0.058***
	(0.015)	(0.020)	(0.034)	(0.018)
State FEs and Year FEs	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes
Observations	305	305	305	305

Notes: Unit of observation is the state-year. Relating (log) state-level GDP per capita to the average state cadre age at entry, cohort size and its interaction. Cohort size is centered around the sample mean. Background controls are: the (log) overall cadre size, which includes all active IAS officers in a given state (including those recruited from the state civil services and below 8 years of tenure), and the share of female. Region-specific trends allow for linear trends that vary by 5 regions: North India, Northeast India, South India, and West India. Standard errors in parentheses, clustered at the state-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.9: State-level GDP per capita and age at entry × cohort size by seniority

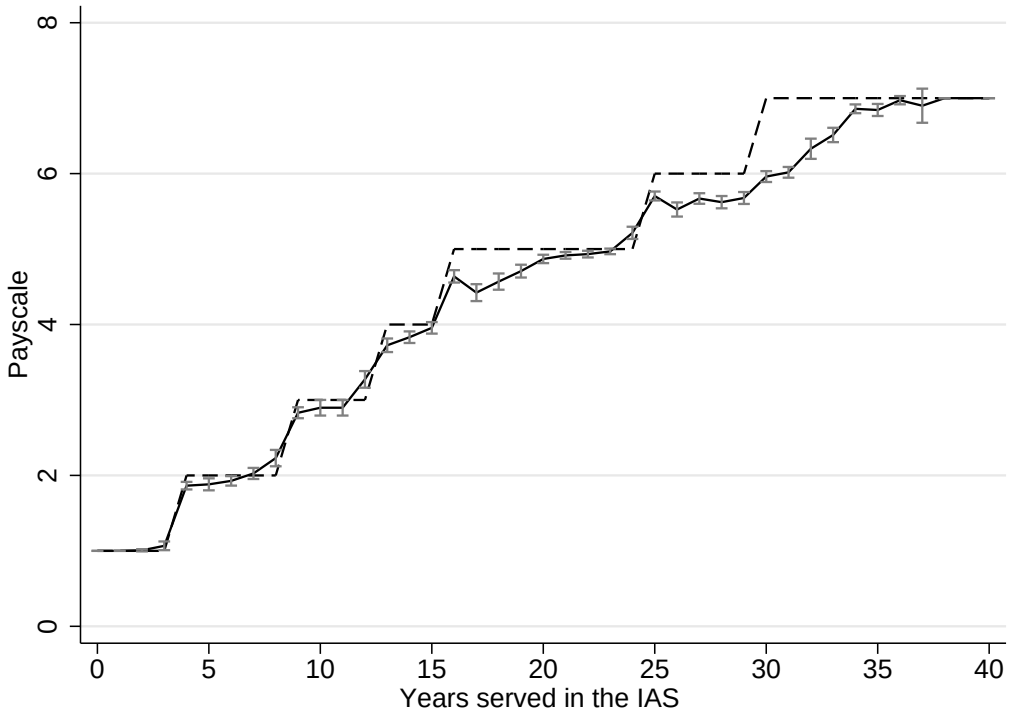
	(1)	(2)	(3)	(4)
Panel A: State GDP per capita	Overall	Agriculture	Industry	Service
Senior avg. age at entry ×	-0.036*	-0.001	-0.101**	-0.059**
Avg. cohort size	(0.017)	(0.025)	(0.036)	(0.023)
Junior avg. age at entry ×	0.003	0.003	0.016	0.006
Avg. cohort size	(0.008)	(0.014)	(0.020)	(0.009)
State FEs and Year FEs	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Observations	305	305	305	305
Panel B: Industrial GDP	Mining	Registered	Unregistered	Utilities
Senior avg. age at entry ×	-0.181**	-0.142***	-0.005	-0.239***
Avg. cohort size	(0.061)	(0.036)	(0.033)	(0.045)
Junior avg. age at entry ×	-0.070**	0.024	0.043	-0.099**
Avg. cohort size	(0.027)	(0.028)	(0.026)	(0.040)
State FEs and Year FEs	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Observations	305	305	305	305
Panel C: ASI	Factories	Workers	Value added	Output
Senior avg. age at entry ×	-0.070***	-0.074**	-0.050	-0.094***
Avg. cohort size	(0.016)	(0.033)	(0.072)	(0.027)
Junior avg. age at entry ×	0.013	0.032**	0.042	0.011
Avg. cohort size	(0.009)	(0.013)	(0.041)	(0.019)
State FEs and Year FEs	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Observations	304	304	304	301
Panel D: Service sector GDP	Construction	Transport	Trade	Banking
Senior avg. age at entry ×	0.024	-0.093***	-0.048	-0.066***
Avg. cohort size	(0.066)	(0.020)	(0.052)	(0.021)
Junior avg. age at entry ×	-0.018	-0.001	-0.000	-0.009
Avg. cohort size	(0.035)	(0.013)	(0.019)	(0.012)
State FEs and Year FEs	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Observations	305	305	305	305
Panel E: Public expenditure	Total	Social	Economic	World Bank
Senior avg. age at entry ×	-0.033*	-0.052**	-0.106***	-0.056
Avg. cohort size	(0.019)	(0.022)	(0.029)	(0.104)
Junior avg. age at entry ×	-0.012	0.003	0.001	-0.023
Avg. cohort size	(0.012)	(0.018)	(0.030)	(0.061)
State FEs and Year FEs	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Observations	305	305	305	305

(continued)

Panel F: Public revenue	(1) Total	(2) State own	(3) Central	(4) Non-tax
Senior avg. age at entry ×	-0.039**	-0.078***	-0.053**	-0.023
Avg. cohort size	(0.014)	(0.025)	(0.023)	(0.035)
Junior avg. age at entry ×	0.003	0.011	0.019	-0.020
Avg. cohort size	(0.008)	(0.015)	(0.013)	(0.020)
State FEs and Year FEs	Yes	Yes	Yes	Yes
Background controls	Yes	Yes	Yes	Yes
Region-specific trends	Yes	Yes	Yes	Yes
Observations	305	305	305	305

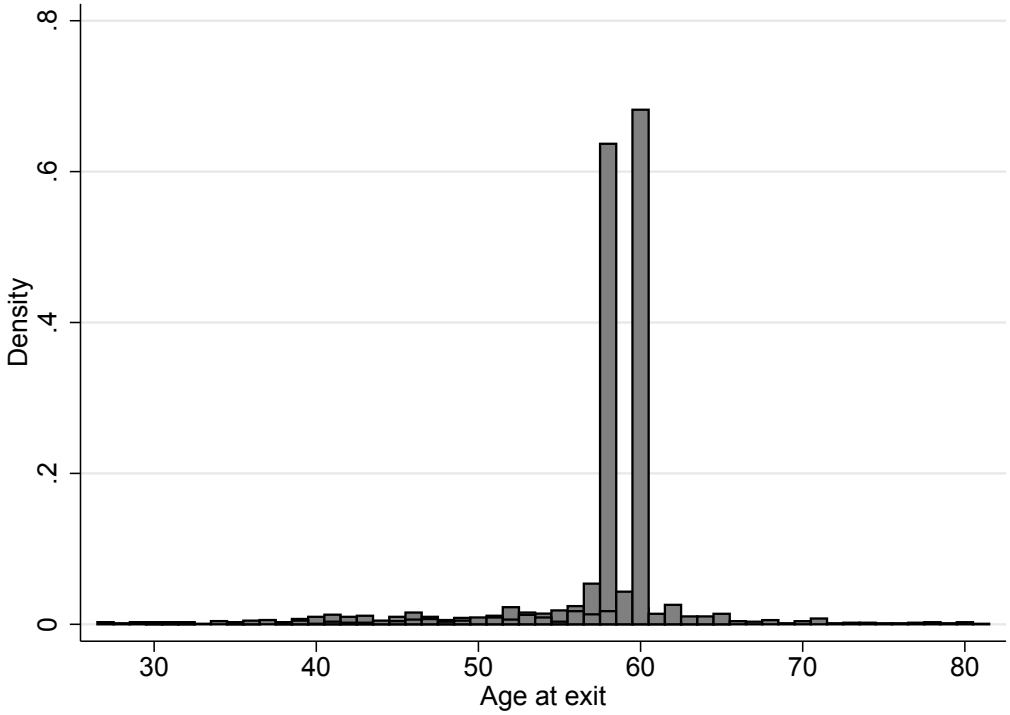
Notes: Unit of observation is the state-year. Relationship between (log) state-level outcomes and the junior/senior average state cadre age at entry, cohort size (centered around sample mean), their interaction, as well as the share of females among junior/senior IAS officers, controlling for the (log) overall cadre size and allowing for region-specific linear trends. In Panel A, the dependent variable is the (log) total state-level GDP per capita and its breakdown by sectors. In Panel B, the dependent variable are (log) state-level GDP components of the industrial sector, broken down by mining, registered and unregistered manufacturing and utilities. In Panel C, the dependent variables are industry-level outcomes from the Annual Survey of Industries (ASI): the (log) number of factories, (log) number of workers, the (log) value added and (log) industrial output. In Panel D, the dependent variables are (log) state-level GDP components of the service sector: construction, transport (railroads, road transport, water transport, air transport etc.), trade (trade and repair services, retail, hotel and restaurants), and banking (financial services and insurance). In Panel E, the dependent variable is (log) state-level expenditure: total expenditure, social (education, health, welfare, housing, relief) and economic expenditures (rural development, special area programmes, irrigation, energy, industry, transport and communications), and number of new World Bank projects. In Panel F, the dependent variables are: (log) total revenue, state-own revenue (taxes on income, property and capital transactions), central revenue (corporation tax, income tax, estate, duty) and non-tax revenue (interest receipts, dividends and profits). Standard errors in parentheses, clustered at the state-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 2.1: Seniority based progression: Average payscale and years of tenure



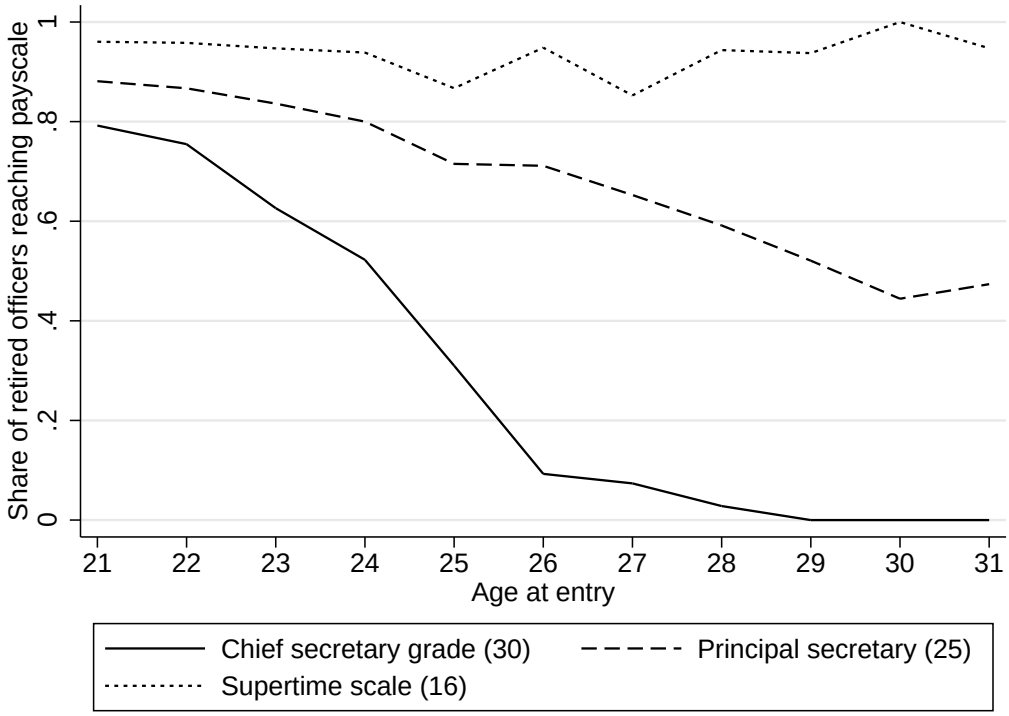
Notes: Average payscale of IAS officers as a function of the years served in the IAS (solid line) for the cross-section of all centrally recruited IAS officers active in 2012. The dashed line marks the payscale as predicted using the IAS promotion guidelines.

Figure 2.2: Distribution of age at retirement pre/post-1998



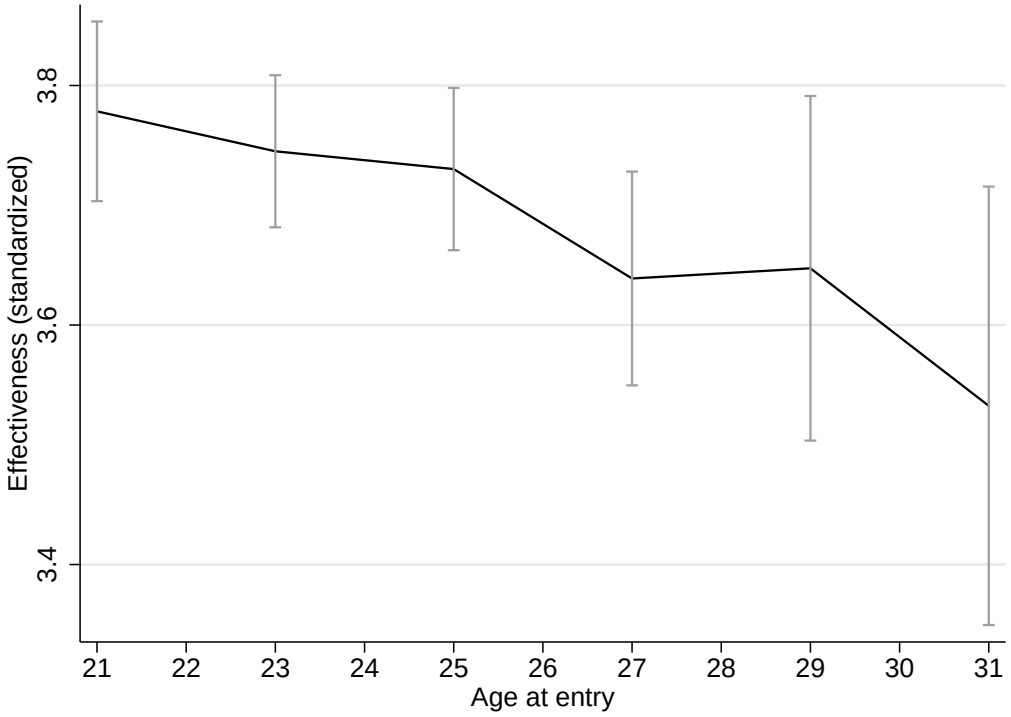
Notes: Distribution of age at exit from IAS among retired officers in 2012. Grey (black) bars denote retirement before (after) 1998. The retirement age was raised from 58 to 60 in 1998.

Figure 2.3: Share of retired officers reaching senior payscales as a function of age at entry



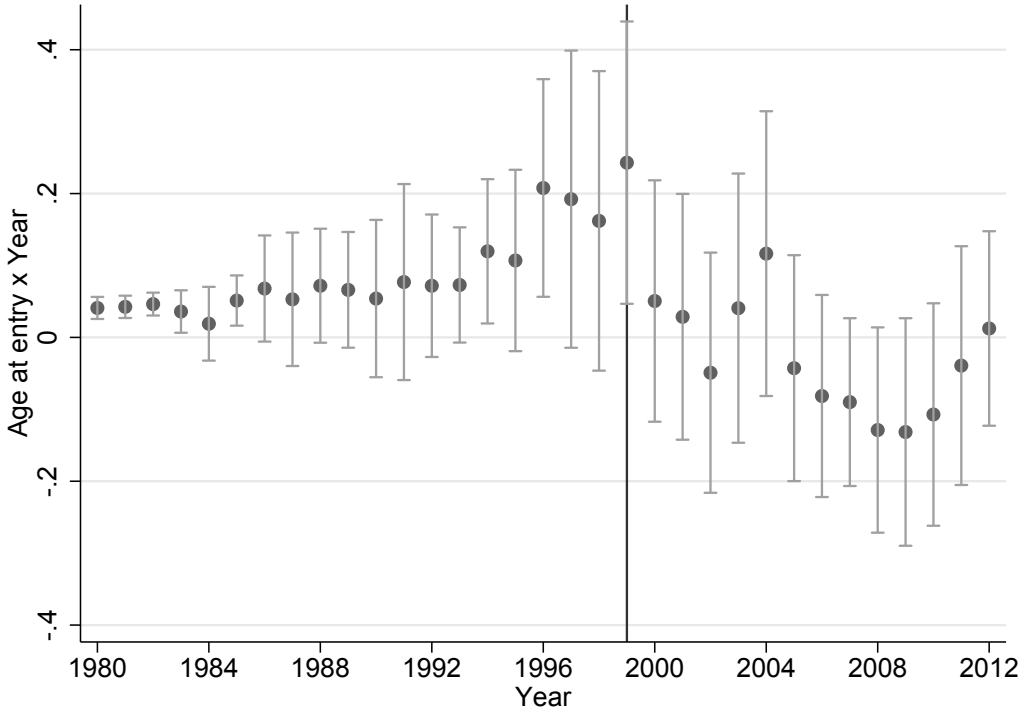
Notes: Share of retired officers in 2012 reaching senior payscales as a function of age at entry. Number in parentheses indicates the minimum number of years to qualify for the position.

Figure 2.4: Effectiveness score and age at entry



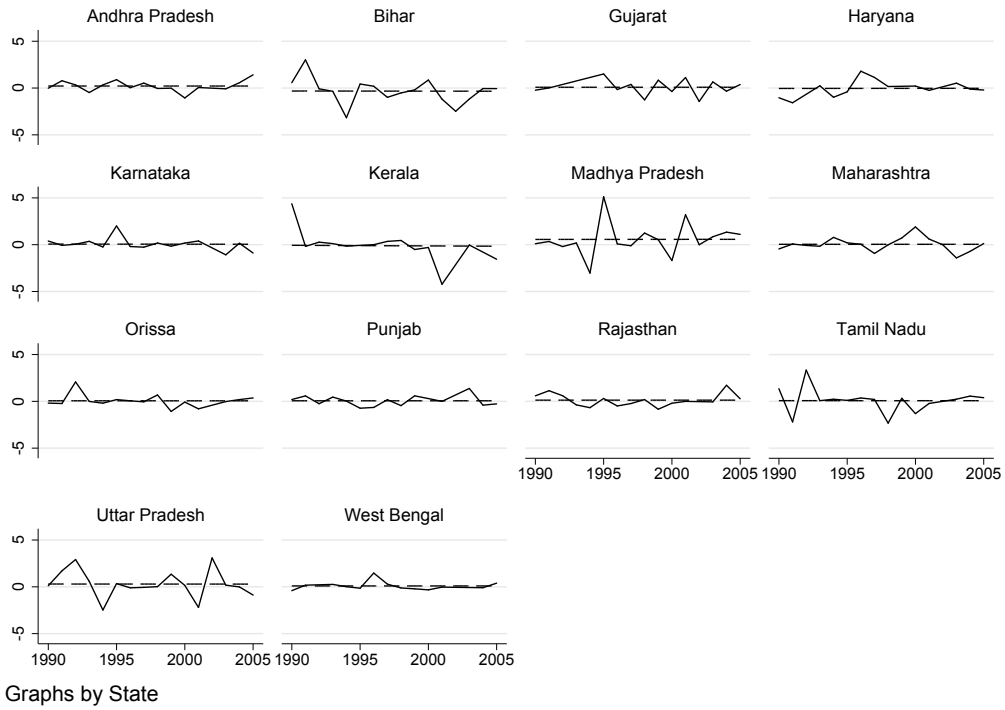
Notes: Raw correlation between the standardized effectiveness score and age at entry. Standard errors used are clustered at the respondent-level.

Figure 2.5: Pension reform 1998 - Age at entry and suspensions by year



Notes: Summarizing the interaction coefficients for a regression of the suspension dummy on age at entry interacted with each year dummy between 1985-2012. The regression includes year FEs and state-specific tenure year FEs. The coefficients are rescaled by 100 to improve readability. The solid line marks the pension reform. Standard errors used for computation of the 95% confidence intervals are clustered at the individual-level.

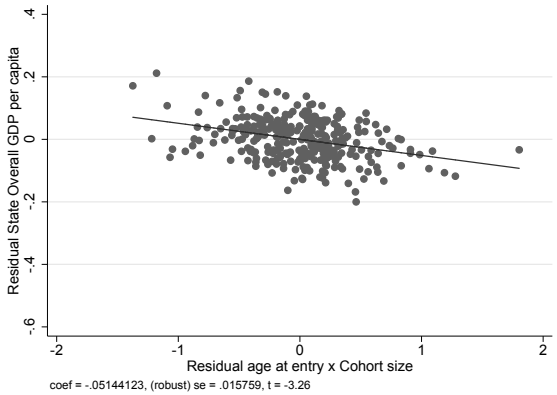
Figure 2.6: Quasi-random allocation across states: Age at entry × Cohort size



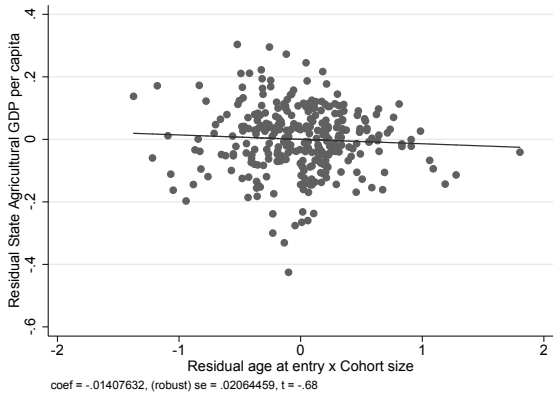
Notes: Interaction between average age at entry and cohort size (standardized relative to their year of intake) 1972-2009. The trend line is fitted as a non-parametric local polynomial.

Figure 2.7: State-level GDP per capita and age at entry × cohort size

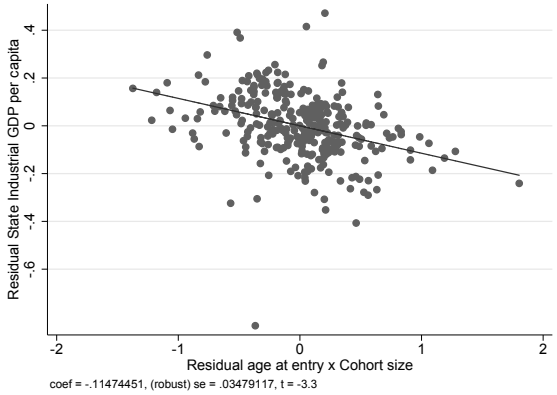
(a) Total GDP per capita



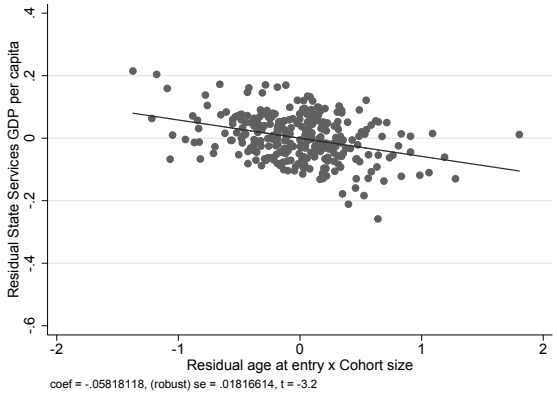
(b) Agricultural GDP per capita



(c) Industrial GDP per capita



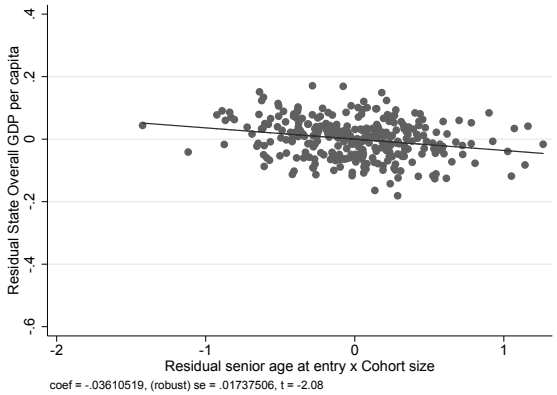
(d) Service GDP per capita



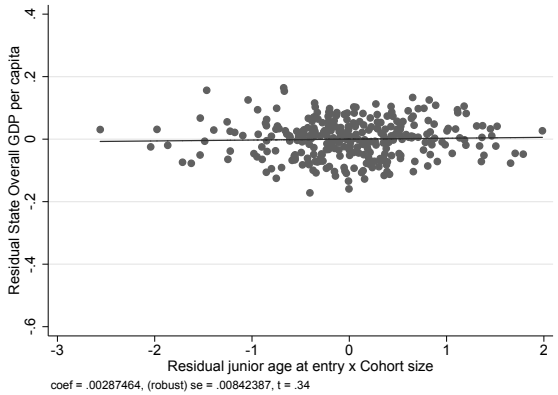
Notes: The unit of observation is the state-year. Reporting the partial (residual) correlation between age at entry × cohort size and (real) state-level GDP per capita 1990-2011.

Figure 2.8: State-level GDP per capita and Age at entry $\times$ ln(Cohort size), by seniority

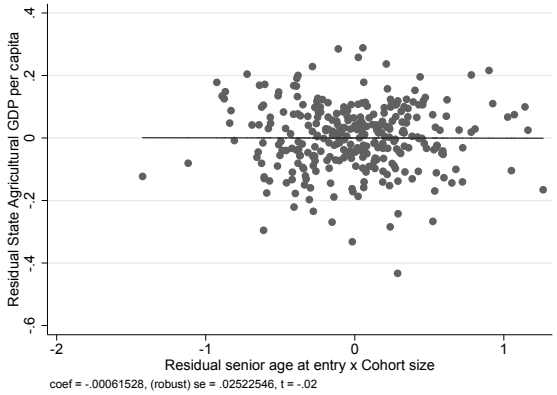
(a) Total GDP per capita, Senior cadre



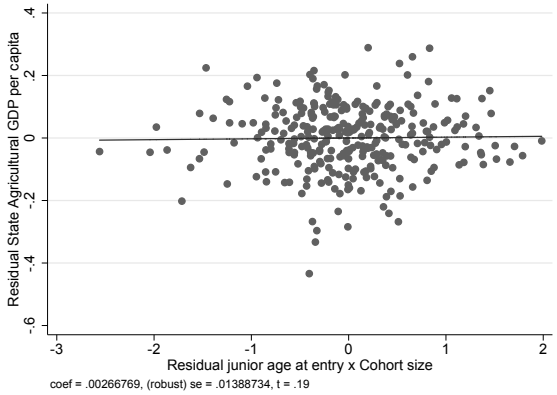
(b) Total GDP per capita, Junior cadre



(c) Agricultural GDP per capita, Senior cadre

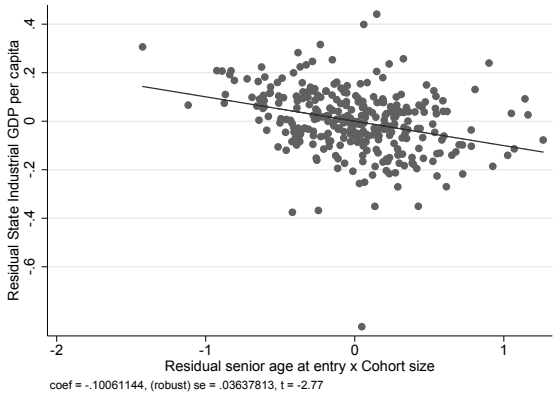


(d) Agricultural GDP per capita, Junior cadre

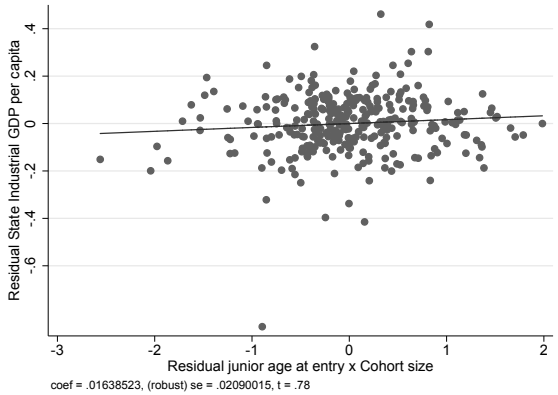


(continued)

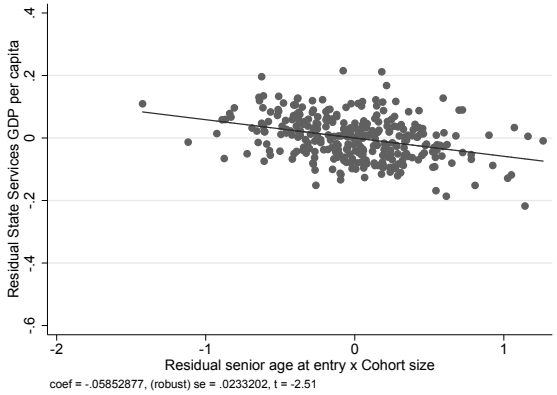
(a) Industrial GDP per capita, Senior cadre



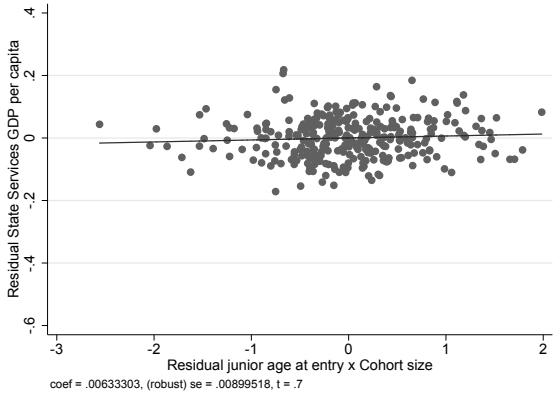
(b) Industrial GDP per capita, Junior cadre



(c) Service GDP per capita, Senior cadre



(d) Service GDP per capita, Junior cadre



Notes: Unit of observation is the state-year. Reporting the partial correlation between age at entry $\times$ cohort size and state-level GDP per capita 1992-2011, broken down by effectiveness of junior (8-15 years tenure) and senior ($\geq$ 16 years tenure) officers and sectors (agriculture, industry and services).

Chapter 3

How Does Collective Reputation Affect Hiring? Selection and Sorting in an Online Labour Market

Using data from an online labour market where the country of residence is the salient group characteristic, we document a mechanism through which collective reputation perpetuates group inequality. Using an IV strategy, we identify reputational externalities between an employer's first hire and the propensity to contract more workers from the same country. Employers, contingent on their first worker's performance, continue to almost exclusively hire from the same country. This coincides with a positive sorting response: Observing their predecessor's success, workers from the same country disproportionately apply and are of higher quality. Employers, facing better applicants, in turn provide higher ratings. Collective reputation hence appears to serve as a coordination device that enables workers to positively sort with employers: Good workers then attract more good workers from the same country and vice versa.

3.1 Introduction

Technological advances like broadband and cloud storage have greatly facilitated remote work and outsourcing. In recent years, online labour markets such as oDesk, Guru, Elance, Rentacoder, Freelancer or Amazon MTurk have enabled businesses to conveniently tap into a global pool of IT workers. The potential gains from trade in these global markets are huge, as employers and workers can match across great distance in a near "zero gravity" environment, taking advantage of large wage differentials across regions and countries.

While varied in design, a common characteristic shared across all virtual markets is incomplete information: Participants on these markets never physically meet and often use aliases. Skills stated are hard to verify. In these settings, reputation mechanisms have proven successful: By allowing participants to publicly rate each other and provide feedback, dynamic incentives are introduced to alleviate moral hazard that is otherwise pervasive in environments of imperfect information (Stanton and Thomas, 2012; Cabral, 2012; Ghani et al., 2014).

In addition to measures of individual reputation, online markets typically provide group-specific information such as country of residence or gender. These collective traits can provide added information to employers (Phelps, 1972; Altonji and Pierret, 2001), but also enable workers to sort along a dimension which, ex-ante, may be unrelated to worker productivity. The possibility for workers to coordinate on a collective trait can, in theory, give rise to inefficient "self-fulfilling stereotypes" (Coate and Loury, 1993; Tirole, 1996; Moro and Norman, 2004). While the role of individual reputation in mitigating moral hazard is well documented, much less is empirically known about the role of collective reputation[1] in facilitating or inhibiting market transactions.

How does collective reputation affect hiring and selection into jobs? This paper delves into the role of collective reputation in a global online labour

[1]Following the definition of individual reputation (Mailath and Samuelson, 2006; Cabral, 2005), collective reputation is "the situation when agents believe another particular agent to be something", depending on an observable group-specific (collective) trait. Collective reputation is hence typically discussed within as (dynamic) statistical discrimination framework (Blume, 2006; Kim and Loury, 2014).

market, where the country of residence is the salient group characteristic. Using detailed hiring data on all public transactions, we first examine reputational externalities between an employer's very first hire and the propensity to contract more workers from the same country (the "first hire" country). In absence of collective reputation, the individual rating given to the first worker should not extend to others. After empirically establishing the presence of reputational externalities, we examine changes in the applicant composition and the employers' final choices to shed light on the underlying mechanisms at play.

A recurring challenge in any hiring setting is the lack of experimental sources of variation that enables us to rule out alternative, observationally equivalent explanations. To obtain causal estimates, we introduce a novel instrumental variable (IV) strategy, where we exploit plausibly exogenous variation in vertical traits within the first applicant pool to predict the actual hire. The availability of rich data on the applicant pool level for over 25,000 employers is key to the empirical strategy, allowing us to take an unusually close look at the mechanisms through which collective reputation emerges and translates into market outcomes.

We present three key findings: First, we document the persistence of the "first hire" country. Our instrumental variable estimate suggests that employers are 3.1% points more likely to continue to hire from their "first hire" country than from other countries in their first applicant pool (mean: 10%). This observed persistence is primarily driven by the positive first rating given, consistent with employer learning and a reputational externality. Second, we document a strong and positive supply-side sorting response: Workers from the "first hire" country disproportionately apply after observing the first hire's successful outcome: Following an earlier positive rating, workers from the same country are 5.1% points more likely to apply than in response to a negative rating (mean: 27.3%). These workers also tend to be of higher quality, as measured by their previous rating and experience. Finally, the sorting response amplifies the positive first hiring: Employers, faced with more high quality workers from the successful "first hire" country, are in turn 11.1% points more likely to continue providing top ratings for their later hires

(mean: 90%). Overall, the results provide empirical evidence for the role of coordination in creating "self-fulfilling stereotypes": An employer's first rating given to a worker with an observable collective trait serves as a group-specific signal upon which later workers coordinate. Depending on the first experience, good workers attract more good workers from the same country and vice versa. The provision of collective traits hence perpetuates initial group inequalities by creating a "herding effect". Most importantly, the results suggest that collective traits such as country of residence - even if uncorrelated to *any* economic fundamentals ex-ante - can persistently shape the way workers sort and apply to jobs.

We rule out competing explanations that appear observationally equivalent to collective reputation. To ensure that the observed persistence is indeed coming from a reputational externality across workers, we excluded rehires throughout the analysis. Other than collective reputation, the sorting response could however also reflect learning about the country-specific match productivity, revealed through the first hire's country and rating. In this case, we expect the effect to decline once we control for bilateral confounds or remove language related tasks (e.g. translation). To ensure that the persistence is not driven by same-country referral networks, we also explicitly omit private transactions that are exclusive for invited workers, focusing only on public transactions which are competitive and open to all. A remaining concern is that employers "signal" their preference for certain groups by providing a high rating in their first hire. If workers indeed sort in response to the employer's revealed taste for the collective trait, we expect the sorting effect to be even stronger when the first worker appears particularly unattractive along vertical traits, such as the individual rating or experience. We find no evidence for these alternative explanations and provide further robustness checks to support our main findings.

3.1.1 Related literature and implications

The results contribute to several strands of literature: First, our paper adds to the emerging literature on online labour markets (Thomas, 2012; Cabral,

2012). While a large body of literature has documented the importance of individual reputation, as measured by online ratings and feedback (Resnick et al., 2006; Cabral and Hortacsu, 2006; Hortacsu et al., 2009; Moreno and Terwiesch, 2013; Agrawal et al., 2013; Pallais, 2014), few papers have examined collective reputation. The closest papers to ours are Ghani et al. (2014) and Nosko and Tadelis (2015). Ghani et al. (2014) provide evidence from oDesk that ethnic diaspora Indians are more likely to initially outsource to India, and more likely to continue hiring from the first country of hire when the experience was positive. We confirm the correlations and provide causal evidence for a sorting response through which the country of hire may appear persistent. Nosko and Tadelis (2015) provide experimental evidence from eBay that buyers overly rely on the first transaction to learn about platform quality, creating a reputational externality. While the persistence of the first transaction appears observationally equivalent, we provide evidence for another mechanism on a two-sided market. In the labour market studied, the persistence is driven by the supply-side response of the workers (or sellers, in their terminology) who sort based on the rating and country of an employer's first hire.

Second, this paper relates to the personnel economics literature on recruitment and selection. While a large body of literature has documented moral hazard and policies to motivate workers on the job, relatively little is known about how to attract the "right" workers in the first place (Lazear and Oyer, 2007; Paul and Scott, 2011). In contrast to the literature that examines selection by varying financial incentives (Dal Bo et al., 2013; Deserranno, 2016; Ashraf et al., 2014), we focus on the impact of reputational externalities in attracting or deterring applicants.

More broadly, the contribution adds value by documenting statistical discrimination on the labour market (Altonji and Pierret, 2001; Bertrand and Mullainathan, 2004; Riach and Rich, 2006; Mill and Stein, 2015). Our results, in particular, suggest that the provision of collective traits such as gender, race or nationality in applications (e.g. on CVs) may further amplify existing group inequalities as workers, anticipating discrimination, refrain from applying altogether. The sorting response documented, in particular, sheds light on a mechanism that could partly explain occupational sorting and persistent

labour market differences along collective traits such as gender or ethnicities (Glover et al., 2015; Bertrand, 2011; Botticini and Eckstein, 2013).

The remainder of this paper is organized as follows: In Section 2, we introduce the empirical context and the data. Section 3 discusses the identification strategy. Section 4 presents the main results on the persistence of the first hire. Section 5 discusses the mechanisms by turning the focus to the applicant pool composition and final choices. Section 6 provides robustness checks and Section 7 concludes.

3.2 Context and market structure

3.2.1 Empirical context

The empirical evidence is from rentacoder.com, one of the largest first-generation virtual markets for outsourcing. In recent years, virtual markets have become increasingly attractive for scholars to study, mainly due to the availability of large datasets and the presence of information asymmetries (Cabral, 2012; Thomas, 2012). The availability of the applicant pools in this context is key for the implementation of our instrumental variables strategy.

In the market studied, employers contract out service jobs to workers who compete by bidding a fixed wage and revealing observable measures of quality.[2] The market chosen is appealing for several reasons: As one of the largest markets, it is representative of a range of competing outsourcing markets. Up to its acquisition by a competitor in 2012, the market under study was one of the five largest virtual markets for outsourcing, with 1.3 million tasks posted by employers and cumulative worker earnings of $139 million. Typical tasks on the platform are data entry tasks, small programming tasks (e.g. creating websites) or simple design tasks.

The online platform offers two types of hiring for task assignment: Public market transactions, which are competitive and open to all workers, and

[2]This type of mechanism is often also referred to as multi-attribute auctions, as sellers do not only compete on price but several dimensions (e.g. rating and experience). In contrast to a *scoring auction* (Asker and Cantillon, 2008, 2010), the scoring rule of the employer is unknown to the bidding workers.

private transactions, which restrict the set of potential workers to those invited by the employer. These private transactions almost exclusively comprise rehires or referrals of previous workers, where individual reputation is likely to be more important (Stanton and Thomas, 2012). Since we focus on collective reputation, however, our main focus is on transactions made on the large and competitive public market.

With a public online market where entry is free and unregulated, information asymmetries are particularly pervasive (Agrawal et al., 2013; Moreno and Terwiesch, 2013): Anyone can sign up and bid as a worker and there is no minimum skill requirement, rendering types unobservable to employers. To sustain transactions given these information asymmetries, the market runs a reputation management system where participants rate each other after completion of tasks.

A typical public market transaction can be described as follows: Employers post jobs requests on the market along detailed specifications about the deliverables, contract type (e.g. fixed or hourly paid) and time frame. The task is then reviewed by the market operator. When approved, it is listed on the market and workers can bid for the job by submitting a wage for which they would be willing to deliver it. Workers do not observe other bidding workers, so there is limited scope for strategic interaction among them, a critical feature for our empirical strategy. The employer observes a list of all applicant workers along a narrow set of variables and chooses his preferred worker (the applicant pool). The set of well-defined variables comprise the bid wage, a rating for past quality and the number of tasks completed. The salient collective trait shown is the country of residence, as indicated by a country flag and the location of the worker.[3] The employer can obtain additional information by viewing the complete profile of each worker. Figure 3.1 presents a typical list of bids.

[Figure 3.1 here]

[3]While the display of sub-national locations may suggest a localized role of collective reputation (e.g. city-level reputation for outsourcing hubs Gurgaon vs. Bangalore in India), the within country variation of locations is too small to allow for a conclusive test for sub-national collective reputation.

Once an employer chooses a worker, the payment agreed upon is transferred from the employer to an escrow account to mitigate moral hazard on the employer's side. The worker then begins with the job, which can end in two ways: Once the worker reports the task complete within the time agreed upon, the worker is asked to upload the deliverables which will then be checked by the employer. If the employer is satisfied with the results, the task is reported complete in which case the payment (minus a commission to the market provider) is transferred to the worker. If unsatisfied, the employer may ask the worker to revise the work or cancel the task in which case the money is returned to the employer and the worker receives nothing. Similarly, when the worker failed to deliver on time, the employer may either extend the time frame or cancel the task. After completion or cancellation of the task, employers and workers are given the opportunity to rate each other on an integer scale from 1 (lowest) to 10 (highest). The rating is only revealed once both have submitted their rating or if two weeks have passed.[4] Once revealed, the rating is visible to anyone on the market. The employers' hiring history is hence visible to potential applicant workers in later hires.

While entry is free and unregulated, re-entry is relatively costly. Employers and workers are required to provide their full contact and bank details to ensure payment. The contacts are verified by phone calls and double or fake accounts, when detected, are suspended. Finally, off-site communication is discouraged as they are not legally binding and will not be taken into account when tasks fail and employers report the case to the market provider for arbitration.

3.2.2 Data and descriptive statistics

We collected data for all public market transactions between 2001 and 2012, covering the entire period of market operation. We restrict the sample to fixed wage bids, which make up 99% of all market transactions. The resulting core

[4]This is to avoid that poor workers, for example, do not strategically hide their anticipated bad rating by not submitting their own bid.

dataset contains 271,783 bids made by 60,083 workers for the respective first job of each of the 25,652 employers.

The online market is international, with the main direction of contracting from high income to low and medium income countries. Almost half of the employers are based in the United States, followed by the United Kingdom, Canada, Austria and Germany. Workers are primarily based in India, the United States, Romania and Pakistan.

The main measure of individual reputation is the average rating assigned to a worker. While ratings range between 1 (lowest) to 10 (highest), the majority of workers receive the highest ratings. This highly skewed distribution of ratings is common across virtual markets, as documented by Nosko and Tadelis (2015) and Dellarocas and Wood (2008). In order to obtain informative variation, we only focus on the top margin, examining the share of workers who have obtained a top rating of 10/10. This corresponds to about 70% of the cases.[5]

Using the binary measure for top ratings, we break down the average distribution of ratings by countries to examine cross-country differences in the individual reputation score. In Figure 3.2, we plot the average share of top rated workers for the largest countries, ranking them in descending order. The plot indicates substantial variation in average ratings across country: More than 85% of workers from Argentina, Germany and Bulgaria obtained top ratings, while the share of top ratings is almost 10% points lower for Bangladesh, Sri Lanka and India. These differences are jointly significant: The country fixed effects are jointly significant, even when controlling for employer and all observable individual worker characteristics (See Appendix C.1)

[Figure 3.2 here]

The collective trait, hence, appears to contain added information to predict the performance of workers. The presence of differences in group-reputation alone, however, need not indicate collective reputation: For one, these differences could simply be taste-based. Some countries are more popular than others. Alternatively, the group differences could be an endogenous

[5]In later robustness checks, we confirm the robustness of the results to the continuous measure as well as alternative dummies, e.g. based on above median rating (See Section 3.6).

equilibrium outcome as workers sort across employers and tasks. Put differently, these average group differences do not inform us to whether these reflect actual (ex-ante) differences or are themselves a result, for example of self-fulfilling stereotypes.

To make progress, our empirical strategy explicitly focuses on the employers' very first hire on the market. This allows us to examine how the first exposure endogenously shapes subsequent hiring outcomes. More specifically, we first exploit a quasi-random source of initial assignment of employers to workers from various countries. We then use this first country-specific exposure to see how shocks to individual reputation extend to others to subsequently generate differences in collective hiring outcomes. We discuss the empirical strategy in the next section but before provide additional descriptive statistics about the first applicant pool.

The data allows us to examine composition changes among applicants. For each of the jobs, we observe the applicant pools with the characteristics of the competing applicant workers. These characteristics range from the wage bid submitted, to a rating (1 low to 10 high), the number of jobs completed, a measure of experience, to the country of residence. Table 3.1 summarizes the characteristics of the first applicant pools: For the pooled sample of all employers, the average number of bids in the first applicant pool is about 10.6, with bidding workers from an average of 5.7 countries. Furthermore, employers appear to be responsive to price and measures of reputation: The average bid price is $323.3, but the chosen bid is only $170.7 on average. Employers are also more likely to choose highly rated workers (an average share of 24% vs. 31% chosen), and workers with more experience on the market (average of 36.3 previous jobs vs. 59.4 chosen). These patterns are in line with studies of hiring determinants on online markets (Ghani et al., 2014; Moreno and Terwiesch, 2013).

[Table 3.1 here]

Only about a third of the employers continue to hire beyond the first hire. While unusual for physical labour markets, the high numbers of entry and exit are typical features of online markets (Nosko and Tadelis, 2015). The

unconditional means are correspondingly low, with buyers on average hiring only 1.34 times beyond the first hire. The unconditional mean for the average subsequent amount paid per job is $29.15 and the total volume is $93.08.

3.3 Empirical model and identification

3.3.1 Regression model

We first test for reputational externalities between an employer's very first hire and the propensity to contract workers from the same country in the later hires. In absence of collective reputation, the first hiring decision does not extend to workers sharing the same collective trait.

With the worker's country as the salient collective trait, we collapse the data to the employer-worker country level. This allows us to compare employer-specific differences in hiring outcomes between countries. Since the majority of employers only hire once or twice, the simplification comes at no major empirical loss. For each of the $i = 1...N$ employers, we compute the bilateral hiring intensity with respect to the $j = 1...M_i$ worker countries from the first applicant pool. We then compare if an employer's likelihood to hire from one of the M_i countries depends on the first country choice. Employer i's overall hiring intensity vis-à-vis worker country j then is:

$$y_{ij} = \beta \cdot \widehat{first\_hire}_{ij}(z_{ij}) + c_i + d_j + \mathbf{x'_{ij}}\gamma + \epsilon_{ij} \tag{3.1}$$

where y_{ij} is a measure of the frequency of hiring between employer i and country j. To capture the extensive margin in hiring, we first use a dummy where $y_{ij} = 1$ if employer i hires any workers from country j for his second or later jobs. We also use the number of hires and average wages paid to workers to examine potential impacts on the intensive margin. The independent variable of interest is $first\_hire_{ij}$, a bilateral dummy that is 1 if the employer's first hire was from country j. In presence of a reputational externality, we would expect $H_0 : \beta \neq 0$.

The main challenge in this non-experimental setting is to rule out confounds that may bias the estimation. In particular, there are three main sources of bias that the identification strategy must address: First, there may be sorting among workers and employers. If only workers from j select into applicant pools of large employers (who are also more likely to remain on the market), for example, employers are mechanically more likely to hire from country j both in the first and later hires. We address selectivity by comparing only the set of countries that select into the same employer's first applicant pool, implemented using the employer fixed effects c_i.

Second, there may exist time-invariant differences across worker country j that could drive the differential probability of hiring across all hires: The likelihood of a worker from country j submitting the lowest bid, for example, increases mechanically with the size (in terms of workers on the market) of the country. Similarly, if workers from country j consistently submit more competitive applications, for example due to cross-country wage differences, a spurious persistence may be created by unobserved cross-country heterogeneity. As before, we address this econometric concern by introducing worker country fixed effects d_j that absorb cross-country level differences. In robustness checks, we also allow these country fixed effects to vary over time by using country-year and country-year-month fixed effects.[6]

Finally, a spurious effect may also appear in presence of bilateral employer-worker country confounders, inducing a correlation between $first\_hire_{ij}$ and the error term ϵ_{ij}. If an employer simply has a preference for country j, the taste-based persistence in hiring will be observationally equivalent to a reputational externality. We address this issue by proposing a novel IV.

3.3.2 Predicting the first hire: IV

We use an instrumental variable strategy to generate exogenous variation in the country of first exposure. The intuition for the instrumental variable strategy can be described in three steps: First, since employers are ceteris paribus more

[6]The results are robust to country-year and country-year-month fixed effects. The results also remain robust when omitting the first five nascent years of the online market where large changes (e.g. in terms of country composition) may have occurred (Table C.3).

willing to select workers with cheaper bids, we can predict the first hire country using the variation in wage bids among workers in the first applicant pool (relevance). Second, given the institutional features, the variation in wage bids within applicant pools is noisy, especially once absorbing cross-country heterogeneity through the worker fixed effects. Third, this noisy variation in the first applicant pool is unlikely to be correlated with the variation in wage bids in future applicant pools, other than through the first hire (exclusion).

More specifically, we exploit the residual variation in the realized distribution of wage bids to instrument for the first hire country. To see how the instrument can be implemented, let w_{ijk} denote the wage bids of workers $k = 1...K_i$ from country j in employer i's first applicant pool. We simply create a bilateral dummy that is 1 if a worker from country j submitted the cheapest bid in employer i's first applicant pool:

$$z_{ij} = \mathbf{1}[w_{ij.} = \min(w_{i..})] \tag{3.2}$$

More generally, our instrumental variable strategy predicts the probability of a worker from country j being hired as a function of the first applicant pool's *realized* distribution of bids, with cross-country differences partialled out. In robustness checks, we also create IVs using other functions of the distribution of bids (e.g. the average deviation from the mean wage bid) but focus on the order statistic, the simplest instrument for the purpose of exposition. Figure 3.3 summarizes the final source of variation used to construct the instrument. The figure shows the distribution of (standardized) bids after partialling out both cross-applicant pool and cross-worker country differences. The residual variation in bids remains large. We exploit this arguably idiosyncratic source of variation to predict the country of first hire.

[Figure 3.3 here]

As described, the exclusion restriction in this context is that future (residual) realizations of the distribution of bids are uncorrelated with the first realization. In other words, the exclusion restriction maintains that the fact an employer first hired a worker from country j *just* because workers from other countries

were more expensive is uncorrelated with the relative competitiveness among bid countries in later hirings, other than through the first hire. We argue that the exclusion restriction is reasonable in this empirical context. The market structure creates a high degree of uncertainty: Workers neither know the number, bids and type of other workers when applying for a job. Similarly, the nature of tasks varies across hiring pools, so there exists uncertainty about the actual costs required to deliver the project. Since the instrument uses the relative variation in wage bids *within* a given pool, we argue that this variation is very likely to be uncorrelated with the subsequent relative variation created by a very different set of workers, employers and application pool.

To complete the discussion of the empirical specification, we also include a vector $\mathbf{x_{ij}}$ that controls for bilateral country-level confounders which may determine both first and subsequent hiring. In our context, the main measures are shared common languages, time zone differences and the geographic distance between the employer and worker country.[7] Finally, the standard errors are clustered at the employer-level.

3.4 Results

3.4.1 Reputational Externalities

The main results are reported in Table 3.2. Column 1 and 2 report the OLS estimates. As expected, the inclusion of worker country fixed effects accounts for the upward bias driven by cross-country level differences. Column 3 reports the instrumental variable estimate: Compared to countries from which workers applied but were not chosen from, employers are 3.2% points more likely to continue hiring another worker from the first hire country. The point estimate is nearly unchanged when adding the common language, time zone differences and (log) distance as bilateral controls, adding support to the validity of the instrumental variable strategy (Column 4). In Column

[7]The bilateral data is drawn from the CEPII's Gravity Dataset, see Mayer and Zignago (2011).

5 and 6, we report the results by replacing the binary outcome with the subsequent number of hires (Column 5) and the total wage payments (Column 6). Employers are more likely to hire and pay higher wages to countries of first hire, compared to countries from which workers applied initially but were not chosen from. The first stage of the two-stage estimation is strong: Employers are 27.2% points more likely to hire from a given country if one of the country's worker submitted the lowest (residual) wage bid. The first-stage conveniently passes conventional tests of weak instruments, with a (Kleibergen-Paap) F-statistic of 5214.54.

[Table 3.2 here]

The estimates are economically significant: With the mean of hiring continuation at 10%, the increase by 3.1% points reflects a sizeable increase of 31% when evaluated at the mean of the dependent variable. For the total amount paid, the increase is even larger and at 56% when evaluated at the mean of $16.35. Since the sample also includes a large number of employers who do not continue after the first hire, the coefficients are even larger when conditioning on the set of employers who at least hired twice (Table C.3, Column 8). Since continuation itself is likely to be endogenous, however, our interpretation relies on the unconditional, lower estimates. Notice also that the effect does not include the rehiring of the same workers (as in first hire), hence capturing solely the externality generated by the shared group trait, the country of residence.

There are two potential reasons for why the instrumental variable estimates are larger than the OLS estimates. First, the results may be driven by measurement error, for example due to misreporting in the country of residence. Second, if the treatment effect is heterogeneous, the instrumental variable estimate will reflect a local average treatment effect (LATE) of those employers who are particularly responsive to variations in prices, especially to the cheapest price. In Section 6.2, we examine the complier population and the robustness of the estimates upon alternative construction of the instrument further (Table C.9). For the main result, however, it is assuring that the OLS

point estimate - which we suspect to be upward biased - is even smaller than the point estimate of the instrumental variable procedure.

To explore how persistent the reputational externality is, we focus on a subset of employers who continue to hire beyond the sixth hire (5% of all employers) and estimate the probability of hiring from the first hire country at each hire up to the sixth. We use the same specification as for the main result (3.4.1). The estimates are reported in Figure 3.4. The country of hire in the first job has a large impact on the probability of hiring from the same country in the second job. Conditioning on the set of employers who continue to hire, the point estimate is about twice the size of the overall unconditional first country of hire effect in Table 3.2. While the coefficient magnitude drops in the third hire, we observe an uptick and statistically significant effects for the fourth and fifth hire, despite the imprecise estimates due to a substantially smaller sample. The results provide evidence for a persistent effect of the reputational externality that tapers off at the sixth hire. Although the duration of persistence appears short, the number of rehires is generally low on the market. About 90% of all employers hire only up to four times, which is in the range where the persistence remains large.

[Figure 3.4 here]

3.4.2 Treatment heterogeneity

The persistence identified in Section 3.4.1 suggests that the country-specific first exposure predicts later hiring patterns. In presence of a reputational externality, we also expect employers to learn and update their beliefs about the first worker's country depending on his or her performance (Altonji and Pierret, 2001). We therefore expect the persistence to be even stronger when differentiating by the first job's outcome.

To investigate this further, we extend the main regression model (3.1) by allowing for treatment heterogeneity. Employer i's hiring intensity vis-à-vis

country j then is:

$$\begin{aligned} y_{ij} &= \beta_0 \cdot \widehat{first}_{ij}(z_{ij}) + \beta_1 \cdot \widehat{first}_{ij}(z_{ij}) \cdot s_{ij} + \\ & c_i + d_j + \mathbf{x'_{ij}}\gamma + \epsilon_{ij} \end{aligned} \tag{3.3}$$

We interact the first hire dummy $\widehat{first}_{ij}$ with another dummy s_{ij} denoting the success of the first job. We define success ($s_{ij} = 1$) as having obtained a top rating of 10/10, which corresponds to 70% of the cases. With most of the jobs rated as a success, our margin of comparison here is hence to compare top rated workers to the rest.[8] While the success of the first job s_{ij} is potentially endogenous, we argue that the interaction $\widehat{first}_{ij} \times s_{ij}$ is more likely to be quasi-random. The specification can also be interpreted as a difference-in-differences: intuitively, we now compare the subsequent hiring outcome of workers from two first hire countries, with the added difference that one worker received a high rating and the other a low rating.

Table 3.3 summarizes the results, now only reporting the instrumental variable estimates. In line with a reputational externality, the results suggest that the first hire effect is solely driven by the job's outcome, as measured by the rating given. In Column 2, the interaction between the first hire and the top rating is positive and significant: As expected, the point estimate increases in contrast to the previous first hire effect reported in Section 3.4.1. Only conditional on success does the first hire translate into future hires.

[Table 3.3 here]

One potential concern is that the interaction is not capturing the actual rating given for the first job but the individual quality of the hired worker: If chosen workers with a high rating are more likely to succeed (and hence rated highly), the key interaction could be spuriously driven by the individual quality of the first worker. To address this concern, we control for the

[8] In robustness checks, we also use different definitions of success, such as stricter definitions (e.g. rating of at least 2/10...7/10, 8/10... 10/10) or a more continuous measure (See Table C.5)

observable measures of individual reputation: In Column 3, we interact the first hire country with the (ex-ante) individual rating of the chosen worker at time of hire. Reassuringly, the interaction term is insignificant with a substantially smaller point estimate. In Column 4, we use experience as the second measure of individual quality but find a similarly insignificant result.[9] In Column 5, we add all three interactions: The results confirm that it is indeed the experience with the first worker, as measured by the first rating given, that is driving the persistence.

In Column 6 and 7, we replace the dummy dependent variable with continuous variables. The results remain comparable: Conditional on success in the first hire, the times traded with a given country are significantly higher (Column 6). We also find the same result for the cumulative payments (Column 7).

To further explore how the persistence varies with the first rating given, we allow for a finer breakdown by ratings. If the heterogenous effect indeed reflects a reputational externality and employer learning, we expect the updating to be stronger the clearer the first signal, as captured by the rating, is. We therefore break down the rating into five groups: Worst rating (Below 4), bad rating (4-6), neutral rating (7-8), good rating (9) and best rating (10).[10] We estimate the effects relative to the neutral group.

[Figure 3.5 here]

The results are summarized in Figure 3.5, where the effects are plotted by the constructed bins. The effect is symmetric: Employers are more (less) likely to continue hiring from the first country of hire if the first rating was positive (negative) relative to a neutral rating. This is consistent with learning, as employers react accordingly to both positive and negative signals. While the estimates for the negative ratings are somewhat noisier, this is due to the small number of bad ratings observed.

[9] Experience is a dummy than is 1 if the worker's number of jobs completed is above median (17).

[10] The bins are chosen to reflect the classifications provided by the market and to ensure sufficient numbers of observations by bin. As discussed, rating distributions are typically left-skewed, with most of the mass concentrated among the highest ratings. The results, however, do not change when we use alternative groupings (See Table C.5).

3.5 Discussion and mechanisms

In the previous section, we found causal evidence consistent with a reputational externality: Shocks to the past performance of a worker extend over to the entire group. We now explore the underlying mechanisms of this persistence by exploring the role of coordination and worker sorting.

For a positive shock to collective reputation to generate the strong observed persistence, we require the shock to endogenously shift the composition of applicants so as to "rationalize" the employer's belief about a given country's average worker quality. In theory, we require a sufficiently strong strategic complementarity between the employer's positive hiring response and the incentives of higher ability workers from the first hire country to apply. We test this condition by studying if the employer's first exposure is indeed accompanied by a positive supply-side sorting response: More and better applicants apply in response to a positive shock to collective reputation. We conclude this section by switching to the employer's side, comparing the final choices employers make. This allows us to assess how shifts in the applicant composition coincide with changes in the final choices.

3.5.1 Sorting and applicant composition

We use the same specification as before in (3.4), but now turning to the worker's side by examining the applicant pool. The results are reported in Table 3.4.

We first examine if the propensity of workers to apply in later jobs relates to the employer's first exposure (Column 1-2). The results show that workers are more likely to apply to employers who initially hired from their country. Comparing among countries in the first hire, workers from the same country as the employer's first hire are 3% points more likely to apply (Column 1). Again, the effect increases when we allow for treatment heterogeneity (Column 2), suggesting that the effect is primarily driven by the first positive rating given. In Column 3 and 4, we replace the binary measure with the number

of applicants.[11] The results remain similar and confirm the effect also in the intensive margin.

[Table 3.4]

In Column 5-8, we explore whether the composition shift among countries is also accompanied by a change in the quality of the applicants, as measured by the individual rating and past number of jobs completed. The results suggest that the sorting response is positive: Following a positive first exposure to a country, workers from the same country do not only increasingly apply but are also of higher quality. Compared to a negative first exposure, the number of top rated applicants per country is 0.63 higher after a positive first exposure. Evaluated against the mean number of 0.6 per country, this increase is large.

Interestingly, the results for experience also show that workers tend to sort along the (ex-ante) experience level of the first hired worker. An experienced worker in the first hire increases the average number of experienced workers in later applicant pools by 0.3. This suggests that the employer's first choice may also signal the preferences for certain type of workers. We discuss this alternative mechanism in Section 3.6. Once again, the point estimates are larger when we condition on the set of employers who do not exit. As exit itself is endogenous, however, we prefer the unconditional specification.

Overall, the results are consistent with the role of the first hire in solving a coordination problem: As shocks to collective reputation induce positive sorting, the employer's first impression is likely to be subsequently confirmed.

3.5.2 Comparing chosen workers and ratings

How does the composition change in the applicant pool affect the final choice? In order to make that comparison, and in contrast to previous specifications, we now condition on the endogenous set of final hires. For each of the i employers, we enumerate the t subsequent hires separately. The hires correspond to

[11] Since we hold constant the numbers of subsequent hires using the employer fixed effects, there is no need to deflate these numbers by the overall numbers of applicants or jobs.

workers, indexed j and from country c. We estimate:

$$y_{ijct} = c_i + \beta \cdot \widehat{first}_{ij}(z_{ij}) + \beta_1 \cdot \widehat{first}_{ij}(z_{ij}) \times s_{ij} + d_c + \epsilon_{ijct} \quad (3.4)$$

As before, we include employer fixed effects c_i and worker country fixed effects d_c. The identifying variation is therefore comparable to the previous specification, but now only among the final hires (See Section 3.3.1). We also use the same instrumental variable strategy. The standard errors remain clustered at the employer-level.

The results are reported in Table 3.5. Among the set of hired workers, those from a first hire country with a positive first rating are more likely to receive a top rating themselves. Compared to a negative first rating, a positive first rating increases the probability of receiving a top rating by 11.1% (Column 2). This effect appears to be driven by the higher quality of selected workers from the first hire country (Column 6). Followed by a positive first rating, later hires from the country are 12.5% points more likely to have a top rating. In terms of wage and experience, however, the final choices are comparable (Column 4 and 8).

[Table 3.5]

The results confirm the presence of strong strategic complementarities that give rise to a "herding effect": Higher ability workers, observing an employer's first positive experience with workers from their country, disproportionately apply as they anticipate a higher probability of being chosen. The increased entry of workers from the first hire's country, however, drives down their "country premium" so that workers from the first hire country do not receive higher average payments.[12] Faced with a larger pool of better applicants from successful first hire countries, however, employers are more likely to select a higher rated worker. These high reputation workers in turn perform better, further amplifying the initial public group-specific signal, thus endogenously

[12] In Appendix C.6, we use an alternative specification by comparing the characteristics of the final choice relative to the applicant pool average. The results confirm the absence of the "country premium".

creating persistent differences between countries. This closes the loop that gives rise to self-reinforcing differences in application rates across groups.

3.6 Alternative mechanisms and Robustness

3.6.1 Alternative mechanisms

We consider several competing mechanisms that may appear observationally equivalent to collective reputation. We first explore an alternative channel through which the applicant composition change could be driven by. While employers may not change their selection rule, they could strategically delay their job postings to local hours where workers from the first hire country are most likely to be awake. Akin to many online markets, workers respond timely to job posts as these are displayed chronologically. We explore whether employers change their time of job posting in order to increase the number of applications from the first worker country. The test is implemented by constructing a dummy for the hours when workers from the given country are most likely to be awake. The window of active local hours is defined to lie between 9am to 5pm local time in the worker's country. For countries with several time-zones, we average across all zones as an approximation. The results are reported in Appendix Table C.2 and suggest that employers are not more likely to strategically delay their job postings in response to the first exposure. This suggests that the applicant pool shift is indeed driven by the increased selection of first hire country workers into later applications.

Other than collective reputation, the sorting response could reflect learning about the country-specific match productivity, revealed through the first hire's country and rating. First of all, however, it is unclear why there should be country-specific match heterogeneity in the first place. Most of the jobs on the platform are small tasks such as data entry or programming. In presence of country-specific match heterogeneity, however, we expect the effect to decline once we control for bilateral confounds or remove language related tasks (e.g. translation). Indeed, the share of direct language-related tasks like translation is less than 2% and the removal of these do not affect our results. Furthermore,

we include a range of bilateral country measures and interact them with the first hire country to account for observable bilateral heterogeneity. The results, again, remain unchanged (Table C.7).

The results may also be driven by referrals networks. If employers are more likely to hire referrals from the first hire country, the estimates would recover same-country propensities in friendship and referral networks. While we cannot definitely rule out unobserved referral networks, we argue that this mechanism is unlikely to be important in our context:[13] In the market under study, referral hirings typically take place in private transactions, where employers can invite selected workers to bid. We explicitly exclude these private transactions throughout our analysis and focus only on public market transactions that are competitive and open to all workers.

Another final concern is that employers "signal" their preference for certain groups by providing a high rating in their first hire. If workers indeed sort in response to the employer's revealed taste for some countries, however, we expect the sorting effect to be even stronger when the first worker appears particularly unattractive along vertical traits, such as the individual rating or experience. In Table 3.3 and Table 3.4, we allowed for the first hire effect to vary along the first worker's (ex-ante) individual rating and experience. If employer's signal their preference for workers from a certain country, we expect the sorting response to increase when the first hire was particularly low ranked in reputation and experience. At most we find the opposite: Experienced workers are more likely to select in if the first hire was particularly experienced.

3.6.2 Robustness

We conclude the results with a wide range of robustness checks. First, we show that the main results are robust upon alternative measures of hiring intensity, such as measures for wage payments above the 9th decile, or the ranking of overall payment flows (Table C.3, Column 2-4). We split the sample by the

[13] Other markets, e.g. oDesk, explicitly allow for referrals by enabling workers to "affiliate" with established intermediaries (Stanton and Thomas, 2012). This feature however does not apply to the market under study.

median year and confine the sample to only US employers (about half of the sample) and employers who hire at least twice (Column 5-8). For transparency, we report the simple OLS and the reduced form regressions (Table C.4). We also report results using alternative definitions of high/low rating (Table C.5).

As another exercise, we experiment with alternative instruments and characterize the complier population in order to facilitate the interpretation of the LATE. Instead of using the order statistic (cheapest worker) to construct the cheapest wage as an instrument, we re-estimate the regression using a dummy for when at least one worker from the given country submitted a bid below the average wage bid (Table C.8). We also create a continuous measure that captures the number of workers from a country with bids below average.

The different instruments provide similar results. In terms of magnitude of the first hire effect, the alternative wage instruments provide comparable point estimates ranging between 3% to 5% points. These differences in estimates are likely to reflect different complier groups: Compared to the first hire (treated) and untreated populations, the size of the complier population varies across the instruments (See Table C.9). Note that our preferred instrument constitutes the lower bound of our range of estimates.

3.7 Conclusion

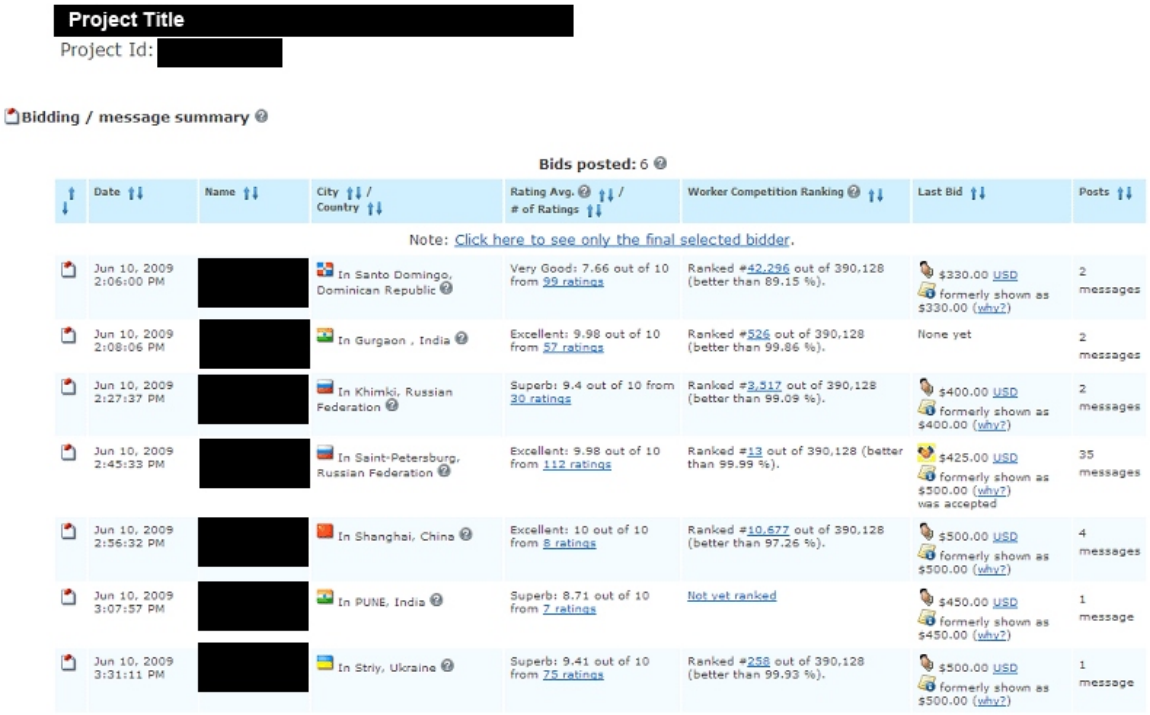
In this paper, we provide causal evidence for collective reputation as a coordination device. An employer's first rating given to a worker with an observable collective trait serves as a group-specific signal upon which later workers coordinate. Depending on the first experience, good workers attract more good workers from the same country and vice versa. The provision of collective traits hence perpetuates initial group inequalities by creating an (unintended) "herding effect". As the provision of collective traits like country of origin or gender is pervasive across online markets, our results are likely to extend beyond the online labour market under study.

More broadly, our results trace out a channel through which collective traits - even if uncorrelated to *any* economic fundamentals ex-ante - can persistently

shape the way workers sort and apply to jobs. While the online labour market setting limits the external validity of our findings, the sorting mechanism documented is likely to also apply to physical labour markets. Our results, for example, suggest that the requirement to disclose collective traits such as gender, race or nationality in applications (e.g. on a CV) may create inefficiencies as workers from some groups, anticipating discrimination, refrain from applying to certain employers altogether.

3.8 Tables and Figures

Figure 3.1: Example of a typical applicant pool



Project Title

Project Id: [redacted]

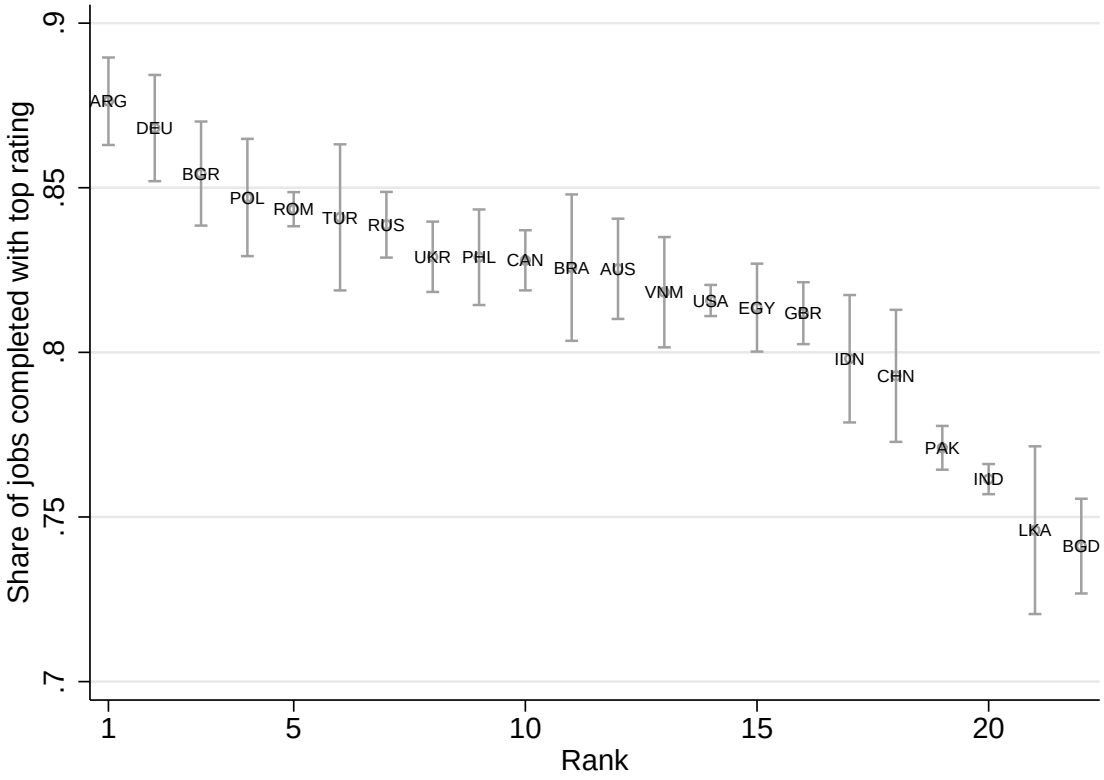
Bidding / message summary

Bids posted: 6

	Date	Name	City / Country	Rating Avg. / # of Ratings	Worker Competition Ranking	Last Bid	Posts
	Note: Click here to see only the final selected bidder.						
	Jun 10, 2009 2:06:00 PM	[redacted]	In Santo Domingo, Dominican Republic	Very Good: 7.66 out of 10 from 99 ratings	Ranked #42,296 out of 390,128 (better than 89.15 %).	$330.00 USD formerly shown as $330.00 (why?)	2 messages
	Jun 10, 2009 2:08:06 PM	[redacted]	In Gurgaon , India	Excellent: 9.98 out of 10 from 57 ratings	Ranked #526 out of 390,128 (better than 99.86 %).	None yet	2 messages
	Jun 10, 2009 2:27:37 PM	[redacted]	In Khimki, Russian Federation	Superb: 9.4 out of 10 from 30 ratings	Ranked #3,517 out of 390,128 (better than 99.09 %).	$400.00 USD formerly shown as $400.00 (why?)	2 messages
	Jun 10, 2009 2:45:33 PM	[redacted]	In Saint-Petersburg, Russian Federation	Excellent: 9.98 out of 10 from 112 ratings	Ranked #13 out of 390,128 (better than 99.99 %).	$425.00 USD formerly shown as $500.00 (why?) was accepted	35 messages
	Jun 10, 2009 2:56:32 PM	[redacted]	In Shanghai, China	Excellent: 10 out of 10 from 8 ratings	Ranked #10,677 out of 390,128 (better than 97.26 %).	$500.00 USD formerly shown as $500.00 (why?)	4 messages
	Jun 10, 2009 3:07:57 PM	[redacted]	In PUNE, India	Superb: 8.71 out of 10 from 7 ratings	Not yet ranked	$450.00 USD formerly shown as $450.00 (why?)	1 message
	Jun 10, 2009 3:31:11 PM	[redacted]	In Striy, Ukraine	Superb: 9.41 out of 10 from 75 ratings	Ranked #258 out of 390,128 (better than 99.93 %).	$500.00 USD formerly shown as $500.00 (why?)	1 message

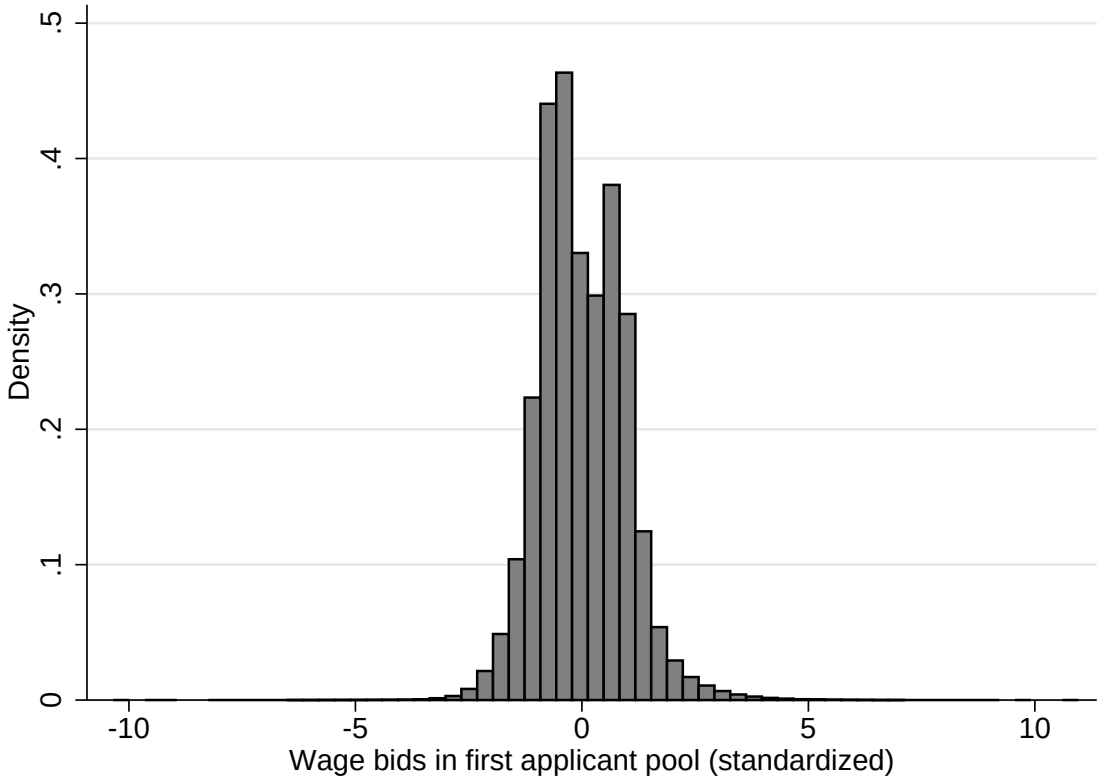
Names are anonymized but typically are aliases that do not enable employers to directly infer to underlying worker quality. The salient collective trait in this context is the country of residence, as indicated by the flag and the country name.

Figure 3.2: Average share of top rated jobs by worker country



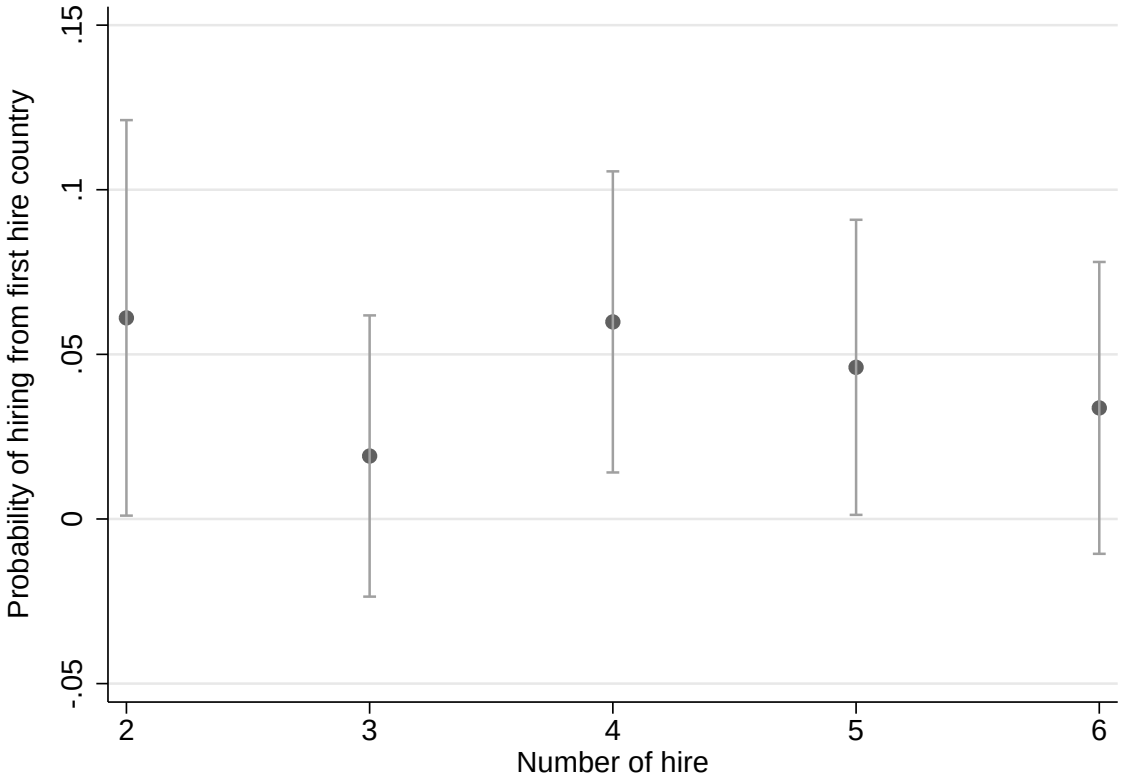
Average share of top rated jobs over the full sample period 2001-2012, by the worker's country (ISO-3 code) in descending order. 95% confidence intervals.

Figure 3.3: Residual *within* applicant pool distribution of wage bids



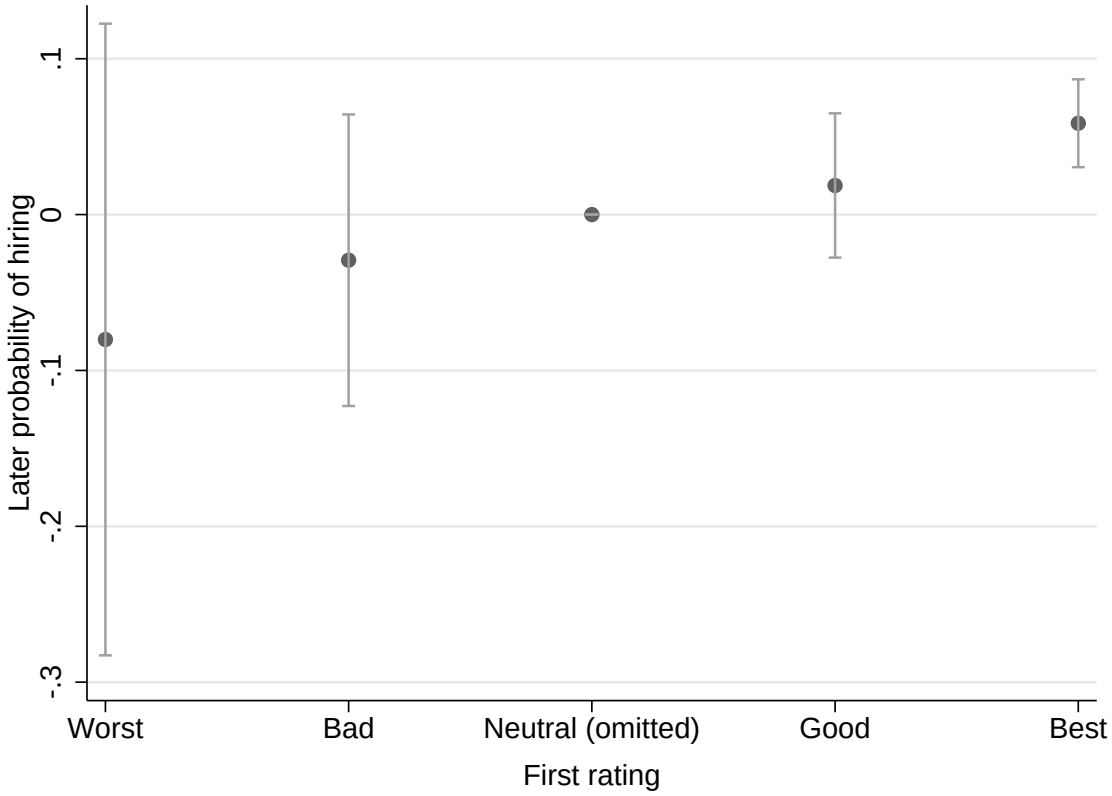
Residual *within* applicant pool variation in standardized (log) wage bids, with worker country fixed effects partialled out. Full sample period 2001-2012.

Figure 3.4: Times hired: Persistence of the first hire effect



Probability of hiring from the first hire country at the 2nd, 3rd, 4th, 5th and 6th hire among employers who hire beyond the 6th hire (5% of all employers). $N = 12498$.

Figure 3.5: Treatment heterogeneity: Probability of hiring again from the country of first hire as a function of the first rating given



Point estimates of the first hire effect as a function of the first rating given. Estimates are reported relative to the neutral rating (7-8 points). Worst rating is defined as a rating below 4 points. A bad rating is defined as a rating between 4-6 points. A good (best) rating is equivalent to a rating of 9 (10) points. 95% confidence intervals.

Table 3.1: First applicant pool characteristics and descriptive statistics of later hiring patterns

Panel A: Employer level	Applicant pool characteristics of the first job									
	All employers (N=25805)					Non-exit (N=8219)				
	Mean	SD	10%	50%	90%	Mean	SD	10%	50%	90%
Number of applicants	10.60	10.78	2	7	24	11.99	11.25	2	8	26
Number of countries	5.66	3.60	2	5	11	6.23	3.70	2	5	11
Wage bid ($)	323.24	5925.94	23.12	96.52	591.65	228.43	1154.68	21.10	81.87	466.93
- Chosen worker	171.01	469.45	12.75	68	424.15	132.09	327.07	10.2	51	296.65
Top rating	0.24	0.19	0	0.22	0.50	0.24	0.18	0	0.22	0.50
- Chosen worker	0.31	0.46	0	0	1	0.32	0.46	0	0	1
Number of completed jobs	36.34	51.86	2	18.54	90.13	33.78	49.36	2	17.03	84.06
- Chosen worker	59.44	134.82	0	14	156	55.56	132.58	0	13	142
Panel B: Employer-country level	Employers hiring from countries of their first applicant pool, after the first hire									
	All employers (N=146273)					Non-exit (N=51545)				
	Mean	SD	10%	50%	90%	Mean	SD	10%	50%	90%
Hires from country after first job	0.100	0.301	0	0	1	0.286	0.452	0	0	1
- First hire country	0.141	0.348	0	0	1	0.440	0.496	0	0	1
Times hired	0.236	1.495	0	0	1	0.671	2.460	0	0	2
- First hire country	0.356	1.717	0	0	1	1.111	2.893	0	0	3
Total pay ($)	16.35	163.34	0	0	0	46.41	272.61	0	0	85
- First hire country	24.92	198.82	0	0	12.75	77.79	345.39	0	0	170

Panel A reports the descriptive statistics for the employer's first applicant pool. *Number of applicants* is the number of applicant workers in the employers' first job applicant pool. *Number of countries* is the number of distinct countries from which workers applied in the first applicant pool. *Wage bid* is the fixed wage bid for completing the job in USD ($). *Top rating* is a dummy that is 1 if the worker has a top rating and 0 otherwise (workers with no previous rating are hence coded 0). *Number of completed jobs* is the number of previous jobs completed on the platform. *Chosen worker* shows the summary statistics for the hired workers among the applicants. **Panel B** reports descriptive statistics for the employer's subsequent transactions with countries from the applicant pool of the first job. *Hires from country after first job* is a dummy that is 1 if the employer ever hired from the country after the first hire. *Times hired* is the number of times the employer hired from the country after the first job. *Total pay* is the cumulative payments an employer made to workers from the given country, in USD ($). *First hire country* shows the summary statistics for the country the employer first hired from. Reporting mean, standard deviation (SD), the 1st decile (10%), median (50%) and 9th decile (90%).

Table 3.2: Reputational externalities: First country hire and later hiring

Panel A: OLS and IV	(1)	(2)	(3)	(4)	(5)	(6)
	Dummy = 1 if employer ever hires from country after the first job				Times hired	Total pay
Mean dep. var.	0.100	0.100	0.100	0.100	0.236	10.026
First hire country	0.037***	0.011***	0.032***	0.031***	0.081**	9.270**
	(0.00)	(0.00)	(0.01)	(0.01)	(0.03)	(4.21)
Estimation	OLS	OLS	IV	IV	IV	IV
Panel B: First stage	(8)	(9)	(10)	(11)	(12)	(13)
			First hire country			
Mean dep. var.			0.175	0.175	0.175	0.175
Cheapest			0.273***	0.272***	0.272***	0.272***
			(0.004)	(0.004)	(0.004)	(0.004)
Employer FEs	X	X	X	X	X	X
Worker country FEs		X	X	X	X	X
Bilateral controls				X	X	X
Kleibergen-Paap F-statistic			5250.67	5214.54	5214.54	5214.54
Observations	146273	146273	146273	146273	146273	146273

The unit of observation is the employer-worker country pair. The dependent variable is a dummy that is 1 if the employer ever hired from the country from which workers applied in the first applicant pool *after* the first hire. *Times hired* is the number of times the employer hired from the country after the first hire. *Total pay* is the cumulative wages paid by an employer to workers in the given country after the first hire. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. *Cheapest* is a dummy that is 1 if applicant workers from the country submitted the cheapest (residual) wage bid. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. The test for weak instruments is the Kleibergen-Paap Wald F-statistic. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.3: Treatment heterogeneity: Later hiring contingent on the first hire's rating

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dummy = 1 if employer ever hires from country after the first job					Times hired	Total pay
Mean dep. var.	0.100	0.100	0.100	0.100	0.100	0.236	10.026
First hire country	0.031***	-0.006	0.026***	0.036***	-0.005	-0.040	1.101
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.06)	(2.47)
First hire country × Top rating given		0.055***			0.054***	0.136**	4.229*
		(0.01)			(0.01)	(0.06)	(2.44)
First hire country × Previous: Top rating			0.014		0.006	0.051	3.029
			(0.01)		(0.01)	(0.07)	(2.77)
First hire country × Previous: Experienced				-0.013	-0.007	0.033	-1.875
				(0.01)	(0.01)	(0.07)	(2.73)
Estimation method	IV	IV	IV	IV	IV	IV	IV
Employer FEs	X	X	X	X	X	X	X
Worker country FEs	X	X	X	X	X	X	X
Bilateral controls	X	X	X	X	X	X	X
First-stage Kleibergen-Paap F-statistic	5214.54	1771.78	2105.11	903.50	419.57	419.57	419.57
Observations	146273	146273	146273	146273	146273	146273	146273

The unit of observation is the employer-worker country pair. The dependent variable is a dummy that is 1 if the employer ever hired from the country from which workers applied in the first applicant pool *after* the first hire. *Times hired* is the number of times the employer hired from the country after the first hire. *Total pay* is the cumulative wages paid by an employer to workers in the given country after the first hire. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. A *Top rating* is defined as a rating of 10/10. *Previous: Top rating* is a dummy that is 1 if the applicant worker received a top rating in previous jobs. *Previous: Experience* is a dummy that is 1 if the worker has completed more than a median amount (17) of jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. The test for weak instruments is the Kleibergen-Paap Wald F-statistic. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.4: Mechanisms: Change in applicant pool composition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Applied		No. applicants		Top rating		High exp.	
Mean dep. var.	0.273	0.273	0.155	0.155	0.573	0.573	0.196	0.196
First hire country	0.030***	-0.006	0.301	-0.621	0.205*	-0.409**	0.049	-0.245**
	(0.01)	(0.01)	(0.22)	(0.39)	(0.12)	(0.21)	(0.06)	(0.10)
First hire country × Top rating given		0.051***		1.092***		0.634***		0.256**
		(0.01)		(0.41)		(0.22)		(0.12)
First hire country × Previous: Top rating		0.009		0.214		0.194		0.038
		(0.01)		(0.51)		(0.26)		(0.12)
First hire country × Previous: Experienced		-0.003		0.291		0.312		0.278**
		(0.01)		(0.48)		(0.25)		(0.12)
Estimation method	IV	IV	IV	IV	IV	IV	IV	IV
Employer FEs	X	X	X	X	X	X	X	X
Worker country FEs	X	X	X	X	X	X	X	X
Bilateral controls	X	X	X	X	X	X	X	X
Observations	145281	145281	145281	145281	145281	145281	145281	145281

The unit of observation is the employer-country pair. *Applied* is a dummy that is 1 if workers from the country from which workers applied in the first applicant pool applied *after* the first hire. *No. of applicants* is the number of applicant workers from the country after the first hire. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. *Top rating* is the number of top rated applicant workers (10/10) from the country after the first hire. *High exp.* is the number of applicants with more than a median amount (17) of completed jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.5: Mechanisms: Effect on later hiring outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Characteristics of hired worker					
	Top rating given		(ln) Wage		Top rating		Experienced	
Mean of dep. var.	0.900	0.900	3.677	3.677	0.772	0.772	0.314	0.314
First hire country	0.004	-0.072	0.037	0.132	-0.006	-0.136*	0.003	0.026
	(0.02)	(0.05)	(0.07)	(0.17)	(0.04)	(0.07)	(0.03)	(0.07)
First hire country × Top rating given		0.111**		-0.034		0.125*		-0.057
		(0.05)		(0.18)		(0.08)		(0.08)
First hire country × Previous: Top rating		0.002		-0.148		0.045		-0.062
		(0.04)		(0.16)		(0.08)		(0.08)
First hire country × Previous: Experienced		-0.030		-0.038		0.039		0.112
		(0.04)		(0.16)		(0.08)		(0.07)
Estimation method	IV	IV	IV	IV	IV	IV	IV	IV
Employer FEs	X	X	X	X	X	X	X	X
Worker country FEs	X	X	X	X	X	X	X	X
Bilateral controls	X	X	X	X	X	X	X	X
Observations	20761	20761	20761	20761	20761	20761	20761	20761

The unit of observation is the completed job. *Top rating given* is a dummy that is 1 if the employer provided the highest rating (10/10) to the worker. *ln(Wage bid)* is the (log) wage paid to the hired worker. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. *Top rating* is a dummy that is 1 if the hired worker had a top rating at time of application (before receiving the rating for the current job). *Experienced* is a dummy that is 1 if the chosen worker completed more than a median amount (17) of completed jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Bibliography

Acemoglu, D., Johnson, S., and Robinson, J. (2004). Institutions as the fundamental cause of long-run growth. *National Bureau of Economic Research Working Paper Series*, pages 10481+.

Acemoglu, D., Johnson, S., and Robinson, J. A. (2001). The colonial origins of comparative development: An empirical investigation. *American Economic Review*, 91(5):1369–1401.

Acemoglu, D. and Robinson, J. (2006). *Economic Origins of Dictatorship and Democracy*. Cambridge University Press.

Aghion, P., Burgess, R., Redding, S., and Zilibotti, F. (2008). The unequal effects of liberalization: Evidence from dismantling the license raj in india. *American Economic Review*, 98(4):1397–1412.

Aghion, P. and Tirole, J. (1997). Formal and Real Authority in Organizations. *Journal of Political Economy*, 105(1):1–29.

Agrawal, A. K., Lacetera, N., and Lyons, E. (2013). Does information help or hinder job applicants from less developed countries in online markets? Working Paper 18720, National Bureau of Economic Research.

Alesina, Alberto, e. a. (2003). Fractionalization. *Journal of Economic Growth*, 8(2):155–94.

Altonji, J. G. and Pierret, C. R. (2001). Employer Learning And Statistical Discrimination. *The Quarterly Journal of Economics*, 116(1):313–350.

Arora, R. and Goyal, R. (1996). *Indian Public Administration: Institutions and Issues*. Wishwa Prakashan.

Asher, S. and Novosad, P. (2016). Politics and Local Economic Growth: Evidence from India. *mimeo*.

Ashraf, N., Bandiera, O., and Lee, S. (2014). Do-gooders and go-getters: career incentives, selection, and performance in public service delivery. *STICERD Working Paper No. 54*.

Asker, J. and Cantillon, E. (2008). Properties of scoring auctions. *The RAND Journal of Economics*, 39(1):69–85.

Asker, J. and Cantillon, E. (2010). Procurement when price and quality matter. *RAND Journal of Economics*, 41(1):1–34.

Bai, Y. and Jia, R. (2016). Elite recruitment and political stability: The impact of the abolition of china's civil service exam. *Econometrica*, 84(2):677–733.

Bandiera, O., Barankay, I., and Rasul, I. (2009). Social Connections and Incentives in the Workplace: Evidence From Personnel Data. *Econometrica*, 77(4):1047–1094.

Bandiera, O., Barankay, I., and Rasul, I. (2010). Social Incentives in the Workplace. *Review of Economic Studies*, 77(2):417–458.

Banton, M. (2008). *Administering the Empire, 1801-1968: A Guide to the Records of the Colonial Office in the National Archives of the UK*. University of London, Institute of Historical Research.

Bertrand, M. (2009). CEOs. *Annual Review of Economics*, 1(1):121–150.

Bertrand, M. (2011). *New Perspectives on Gender*, volume 4 of *Handbook of Labor Economics*, chapter 17, pages 1543–1590. Elsevier.

Bertrand, M. and Mullainathan, S. (2004). Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination. *American Economic Review*, 94(4):991–1013.

Bertrand, M. and Schoar, A. (2003). Managing with style: The effect of managers on firm policies. *The Quarterly Journal of Economics*, 118(4):1169–1208.

Besley, T. and Burgess, R. (2004). Can labor regulation hinder economic performance? evidence from india. *The Quarterly Journal of Economics*, 119(1):91–134.

Besley, T., Ilzetzki, E., and Persson, T. (2013). Weak States and Steady States: The Dynamics of Fiscal Capacity. *American Economic Journal: Macroeconomics*, 5(4):205–35.

Besley, T. and Persson, T. (2009). The Origins of State Capacity: Property Rights, Taxation, and Politics. *American Economic Review*, 99(4):1218–44.

Besley, T. and Persson, T. (2010). State Capacity, Conflict, and Development. *Econometrica*, 78(1):1–34.

Blume, L. (2006). The dynamics of statistical discrimination. *Economic Journal*, 116(515):F480–F498.

Botticini, M. and Eckstein, Z. (2013). *The Chosen Few: How Education Shaped Jewish History, 70-1492*, volume 1. Princeton University Press, 1 edition.

Burks, S. V., Cowgill, B., Hoffman, M., and Housman, M. (2015). The Value of Hiring through Employee Referrals. *The Quarterly Journal of Economics*, 130(2):805–839.

Cabral, L. (2005). The economics of trust and reputation: A primer. *mimeo*.

Cabral, L. (2012). Reputation on the internet. *The Oxford Handbook of the Digital Economy*.

Cabral, L. and Hortacsu, A. (2006). The dynamics of seller reputation: Evidence from ebay. Technical report.

Cameron, C. and Miller, D. (2014). Robust inference for dyadic data. *mimeo*.

Card, D., Heining, J., and Kline, P. (2013). Workplace Heterogeneity and the Rise of West German Wage Inequality. *The Quarterly Journal of Economics*, 128(3):967–1015.

Coate, S. and Loury, G. C. (1993). Will affirmative-action policies eliminate negative stereotypes? *The American Economic Review*, 83(5):pp. 1220–1240.

Dal Bo, E., Finan, F., Folke, O., Persson, T., and Rickne, J. (2016). Who becomes a politician? *mimeo*.

Dal Bo, E., Finan, F., and Rossi, M. A. (2013). Strengthening State Capabilities: The Role of Financial Incentives in the Call to Public Service. *The Quarterly Journal of Economics*, 128(3):1169–1218.

Dellarocas, C. and Wood, C. A. (2008). The sound of silence in online feedback: Estimating trading risks in the presence of reporting bias. *Management Science*, 54(3):460–476.

Deserranno, E. (2016). Financial Incentives as Signals: Experimental Evidence from the Recruitment of Village Promoters in Uganda . *mimeo*.

Dewatripont, M., Jewitt, I., and Tirole, J. (1999). The economics of career concerns, part ii: Application to missions and accountability of government agencies. *Review of Economic Studies*, 66(1):199–217.

Dijkstra, E. W. (1959). A note on two problems in connexion with graphs. *Numerische Mathematik*, 1:269–271.

Dincecco, M. and Prado, M. (2012). Warfare, fiscal capacity, and performance. *Journal of Economic Growth*, 17(3):171–203.

Dippel, C., Greif, A., and Trefler, D. (2015). The Rents From Trade and Coercive Institutions: Removing the Sugar Coating. *NBER Working Paper 20958*.

Drugov, M. (2015). Optimal patronage. CEPR Discussion Papers 10343, CEPR Discussion Papers.

Finan, F., Olken, B. A., and Pande, R. (2015). The Personnel Economics of the State. *NBER Working Paper 21825*.

Fisman, R. (2001). Estimating the Value of Political Connections. *American Economic Review*, 91(4):1095–1102.

Fisman, R. and Wei, S.-J. (2004). Tax Rates and Tax Evasion: Evidence from "Missing Imports" in China. *Journal of Political Economy*, 112(2):471–500.

Francis, M. (1992). *Governors and Settlers: Images of Authority in the British Colonies, 1820-60*. Canterbury University Press.

Fulton, J. (1968). *The Civil Service. Vol 1. Report of the Committee 1966-68*.

Gardner, L. (2012). Taxing Colonial Africa: The Political Economy of British Imperialism. *Oxford University Press*.

Ghani, E., Kerr, W. R., and Stanton, C. (2014). Diasporas and outsourcing: evidence from oDesk and India. *Management Science*, 60(7):1677–1697.

Glover, D., Pallais, A., and Pariente, W. (2015). Discrimination as a Self-Fulfilling Prophecy: Evidence from French Grocery Stores. *mimeo*.

Grindle, M. (2012). *Jobs for the Boys: Patronage and the State in Comparative Perspective*. Harvard University Press.

Guardado, J. (2016). Office-selling, corruption and long-term development in peru. *mimeo*.

Holmstrom, B. and Milgrom, P. (1991). Multitask principal-agent analyses: Incentive contracts, asset ownership, and job design. *Journal of Law, Economics and Organization*, 7(0):24–52.

Hortacsu, A., Martinez-Jerez, F. A., and Douglas, J. (2009). The Geography of Trade in Online Transactions: Evidence from eBay and MercadoLibre. *American Economic Journal: Microeconomics*, 1(1):53–74.

Iyer, L. and Mani, A. (2012). Traveling Agents: Political Change and Bureaucratic Turnover in India. *The Review of Economics and Statistics*, 94(3):723–739.

Jeffries, C. (1938). *The Colonial Empire and its Civil Service*. Cambridge University Press.

Jia, R. (2014). Pollution for promotion. *mimeo*.

Jia, R., Kudamatsu, M., and Seim, D. (2015). Political selection in china: The complementary roles of connections and performance. *Journal of the European Economic Association*, 13(4):631–668.

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

Khan, A. Q., Khwaja, A. I., and Olken, B. A. (2015). Tax farming redux: Experimental evidence on performance pay for tax collectors. *The Quarterly Journal of Economics*.

Kim, Y.-C. and Loury, G. (2014). Collective reputation and the dynamics of statistical discrimination. Technical report, mimeo.

Kirk-Greene, A. (2000). *Britain's Imperial Administrators, 1858-1966*. Palgrave.

Kramarz, F. and Thesmar, D. (2013). Social networks in the boardroom. *Journal of the European Economic Association*, 11(4):780–807.

Laidlaw, Z. (2005). *Colonial Connections 1815-1845: Patronage, the Information Revolution and Colonial Government*. Studies in Imperialism. Manchester University Press.

Lazear, E. P. and Oyer, P. (2007). Personnel economics. Working Paper 13480, National Bureau of Economic Research.

Lucas, R. E. (1978). On the Size Distribution of Business Firms. *Bell Journal of Economics*, 9(2):508–523.

Lyttle, G. and Orgel, S. (1981). *Patronage in the Renaissance*. Princeton University Press.

Mailath, G. and Samuelson, L. (2006). *Repeated games and reputations: long-run relationships*. Oxford scholarship online. Oxford University Press.

Manski, C. F. (1993). Adolescent econometricians: How do youth infer the returns to schooling? In *Studies of Supply and Demand in Higher Education*, NBER Chapters, pages 43–60. National Bureau of Economic Research, Inc.

Mayer, T. and Zignago, S. (2011). Notes on CEPII's distances measures: The GeoDist database. Working Papers 2011-25, CEPII research center.

Milgrom, P. R. (1988). Employment Contracts, Influence Activities, and Efficient Organization Design. *Journal of Political Economy*, 96(1):42–60.

Mill, R. and Stein, L. (2015). Race, skin color, and economic outcomes in early twentieth-century america. *mimeo*.

Moreno, A. and Terwiesch, C. (2013). Doing business with strangers: Reputation in online service marketplaces. *mimeo*.

Moro, A. and Norman, P. (2004). A general equilibrium model of statistical discrimination. *Journal of Economic Theory*, 114(1):1–30.

Morris, S. and Coate, S. (1999). Policy Persistence. *American Economic Review*, 89(5):1327–1336.

Nath, A. (2015). Bureaucrats and Politicians: How Does Electoral Competition Affect Bureaucratic Performance? *mimeo*.

North, D. and Weingast, B. (1989). Constitutions and commitment: The evolution of institutions governing public choice in seventeenth-century england. *The Journal of Economic History*, 49(04):803–832.

Northcote, S. and Trevelyan, C. (1854). *Report on the Organization of the Permanent Civil Service*.

Nosko, C. and Tadelis, S. (2015). The limits of reputation in platform markets:an empirical analysis and field experiment. Working Paper 20830, National Bureau of Economic Research.

Olken, B. A. (2009). Corruption perceptions vs. corruption reality. *Journal of Public Economics*, 93(7-8):950–964.

Pallais, A. (2014). Inefficient hiring in entry-level labor markets. *American Economic Review*, 104(11):3565–99.

Paul, O. and Scott, S. (2011). Chapter 20 - personnel economics: Hiring and incentives. volume 4, Part B of *Handbook of Labor Economics*, pages 1769 – 1823. Elsevier.

Persson, P. and Zhuravskaya, E. (2016). The Limits of Career Concerns in Federalism: Evidence from China. *Journal of the European Economic Association*, 14(2):338–374.

Phelps, E. S. (1972). The statistical theory of racism and sexism. *American Economic Review*, 62(4):659–61.

Prendergast, C. (1999). The Provision of Incentives in Firms. *Journal of Economic Literature*, 37(1):7–63.

Prendergast, C. and Topel, R. H. (1996). Favoritism in Organizations. *Journal of Political Economy*, 104(5):958–78.

Prichard, W. (2016). Reassessing tax and development research: A new dataset, new findings, and lessons for research. *World Development*, 80:48 – 60.

Rasul, I. and Rogger, D. (2016). Management of Bureaucrats and Public Service Delivery: Evidence from the Nigerian Civil Service. *Economic Journal*.

Rauch, J. E. (1995). Bureaucracy, Infrastructure, and Economic Growth: Evidence from U.S. Cities during the Progressive Era. *American Economic Review*, 85(4):968–79.

Rauch, J. E. and Evans, P. B. (2000). Bureaucratic structure and bureaucratic performance in less developed countries. *Journal of Public Economics*, 75(1):49–71.

Resnick, P., Zeckhauser, R., Swanson, J., and Lockwood, K. (2006). The value of reputation on ebay: A controlled experiment. *Experimental Economics*, 9(2):79–101.

Riach, P. A. and Rich, J. (2006). An experimental investigation of sexual discrimination in hiring in the english labor market. *The B.E. Journal of Economic Analysis & Policy*, 5(2):1–22.

Robinson, J. A., Acemoglu, D., and Johnson, S. (2005). Institutions as a fundamental cause of long-run growth. *Handbook of Economic Growth*, 1A:386–472.

Sequeira, S. and Djankov, S. (2014). Corruption and firm behavior: Evidence from African ports. *Journal of International Economics*, 94(2):277–294.

Spolaore, E. and Wacziarg, R. (2009). The diffusion of development. *The Quarterly Journal of Economics*, 124(2):469–529.

Stanton, C. and Thomas, C. (2012). Landing the first job: The value of intermediaries in online hiring. *Available at SSRN 1862109*.

Thomas, C. (2012). *The Economics of Online Recruitment*, volume 6 of *The New Palgrave Dictionary of Economics*, chapter 1. Palgrave Macmillan.

Tilly, C. (1990). *Coercion, Capital, and European States, AD 990-1990*. B. Blackwell.

Tirole, J. (1996). A theory of collective reputations (with applications to the persistence of corruption and to firm quality). *Review of Economic Studies*, 63(1):1–22.

Tullock, G. (1965). *The politics of bureaucracy*. Public Affairs Press.

Vanden-Eynde, O., Kuhn, P., and Moradi, A. (2016). Trickle-down ethnic politics: Drunk and absent in the kenya police force (1957-1970). *mimeo*.

Weber, M. (1922). *Wirtschaft und Gesellschaft*. Mohr Verlag.

World Bank (1997). World development report 1997: The state in a changing world. *World Bank Group*.

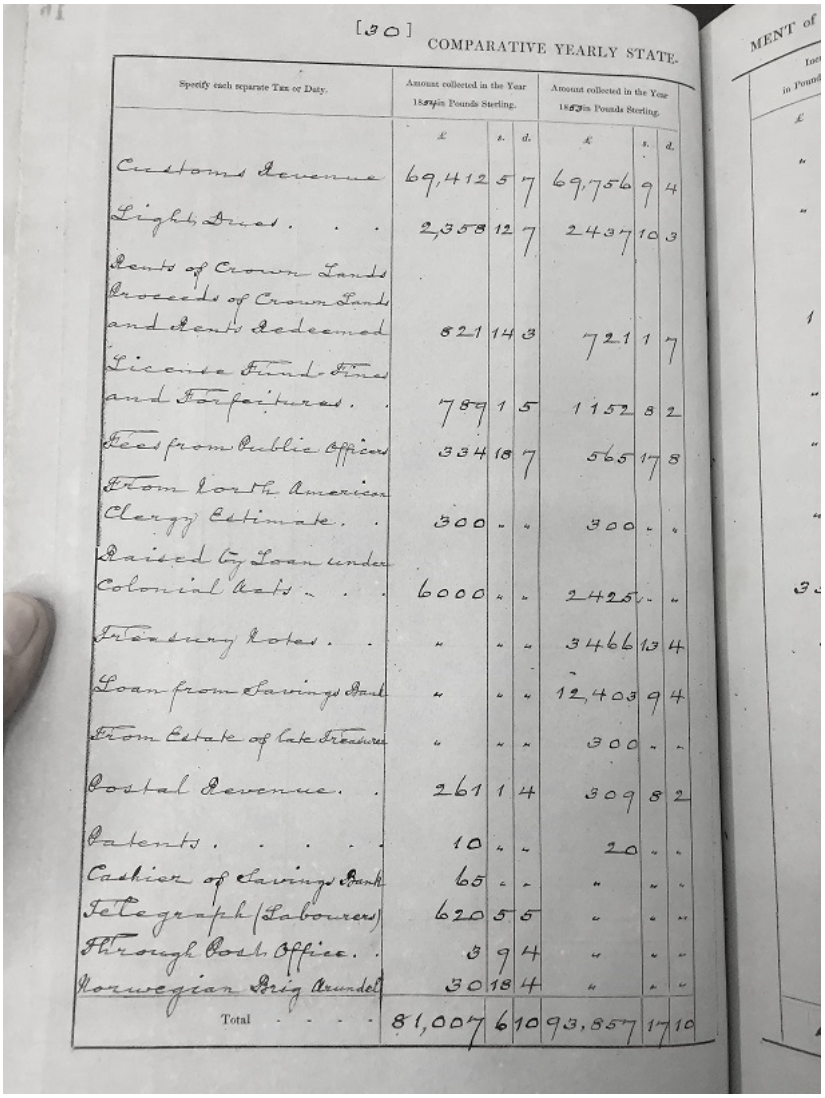
World Bank (2008). Public sector reform: What works and why? *World Bank Group*.

Appendices

Appendix A

Appendices to Chapter 1: The Costs of Patronage: Evidence from the British Empire

Figure A.1: Sample of comparative revenue statement for Fiji 1854 (Blue Book)



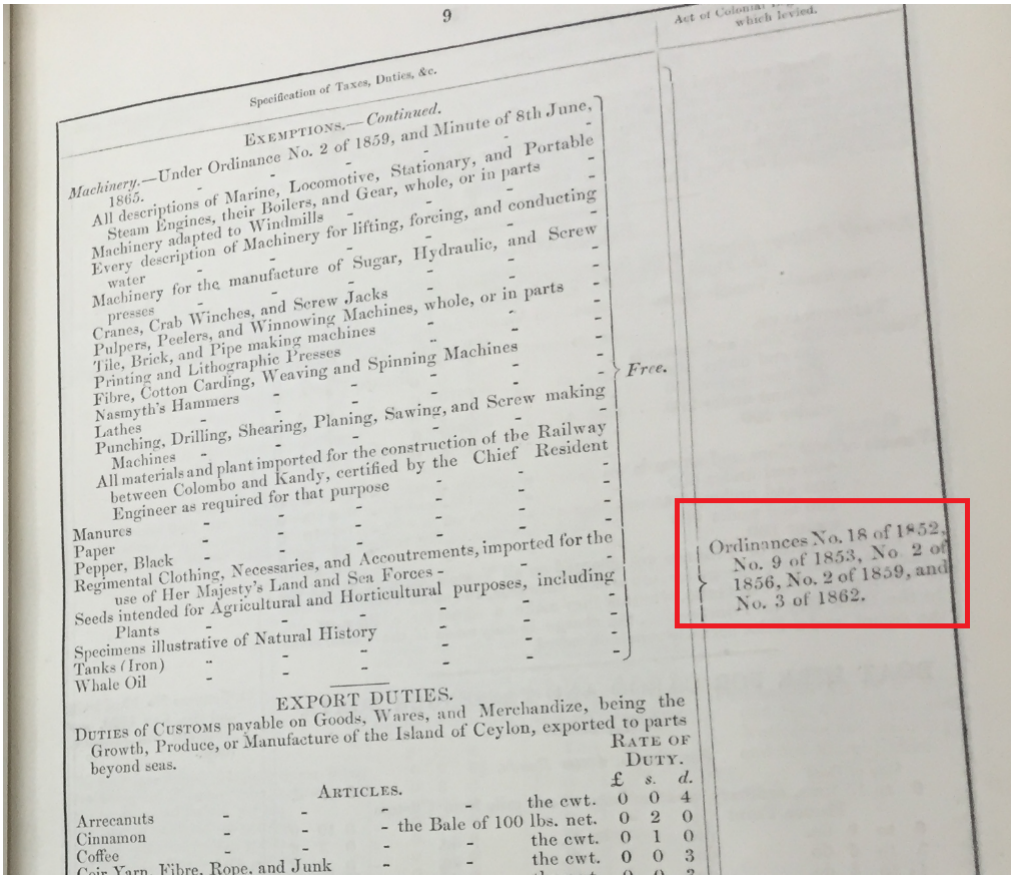
[30]

COMPARATIVE YEARLY STATE.

Specify each separate Tax or Duty.	Amount collected in the Year 1854 in Pounds Sterling. £	s.	d.	Amount collected in the Year 1853 in Pounds Sterling. £	s.	d.
Customs Revenue	69,412	5	7	69,756	9	4
Light Dues . . .	2,358	12	7	2437	10	3
Rents of Crown Lands						
Proceeds of Crown Lands and Rents Redeemed	821	14	3	721	1	7
Licence Fund, Fines and Forfeitures .	789	1	5	1152	8	2
Fees from Public Offices	334	18	7	565	17	8
From North American Clergy Estimate .	300	"	"	300	"	"
Raised by Loan under Colonial Acts . .	6000	"	"	2425	"	"
Treasury Notes . .	"	"	"	3466	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)	620	5	5	"	"	"
Through Post Office .	3	9	4	"	"	"
Norwegian Brig Arundel	30	18	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 A.2: Sample of trade tax exemption laws (Blue Book)



9

Specification of Taxes, Duties, &c.

Act of Colonial ... which levied.

EXEMPTIONS.—*Continued.*

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

All descriptions of Marine, Locomotive, Stationary, and Portable Steam Engines, their Boilers, and Gear, whole, or in parts
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

} *Free.*

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

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

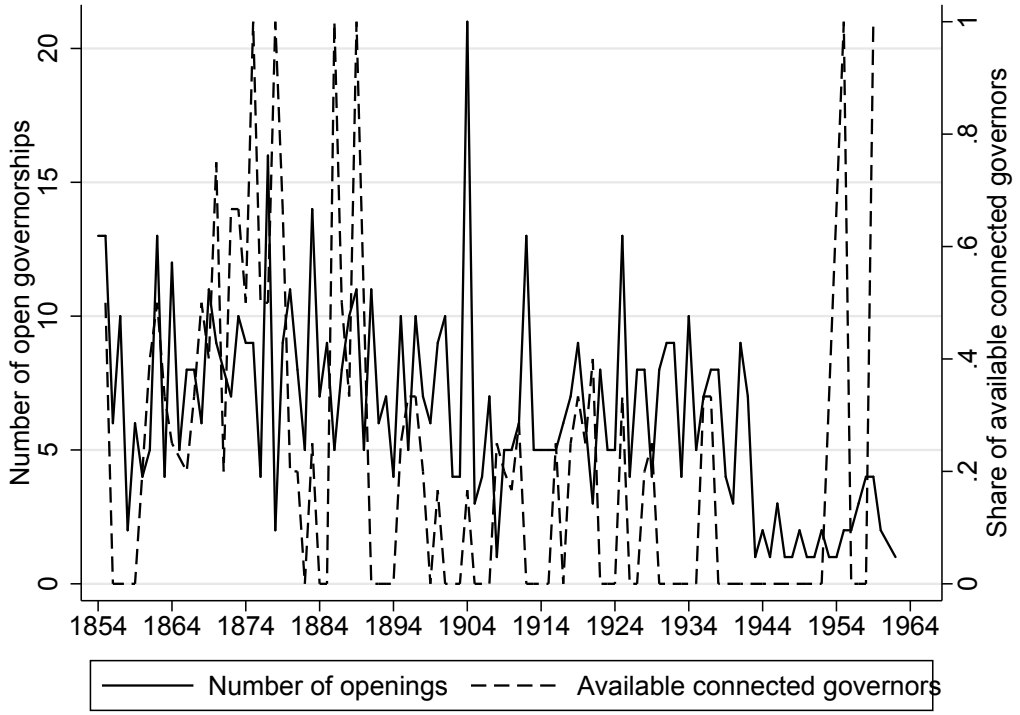
EXPORT DUTIES.

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
Coir Yarn, Fibre, Rope, and Junk	the cwt.	0	0	3

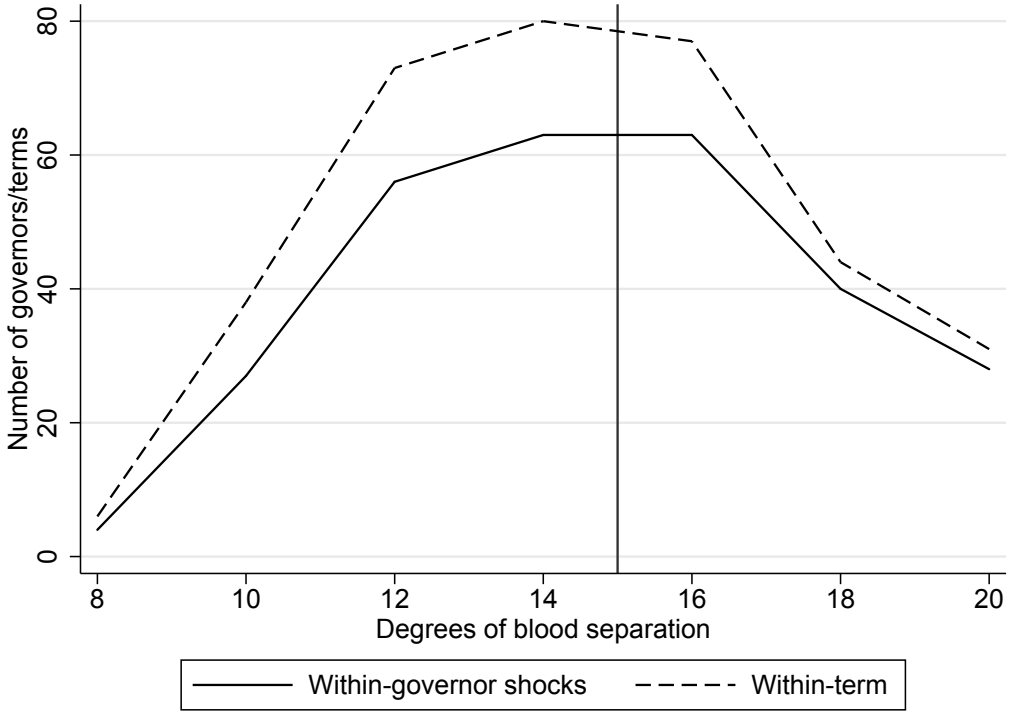
Notes: Sample of customs tax exemptions laws from the 1869 Ceylon Blue Book.

Figure A.3: Number of openings and share available governors who are connected



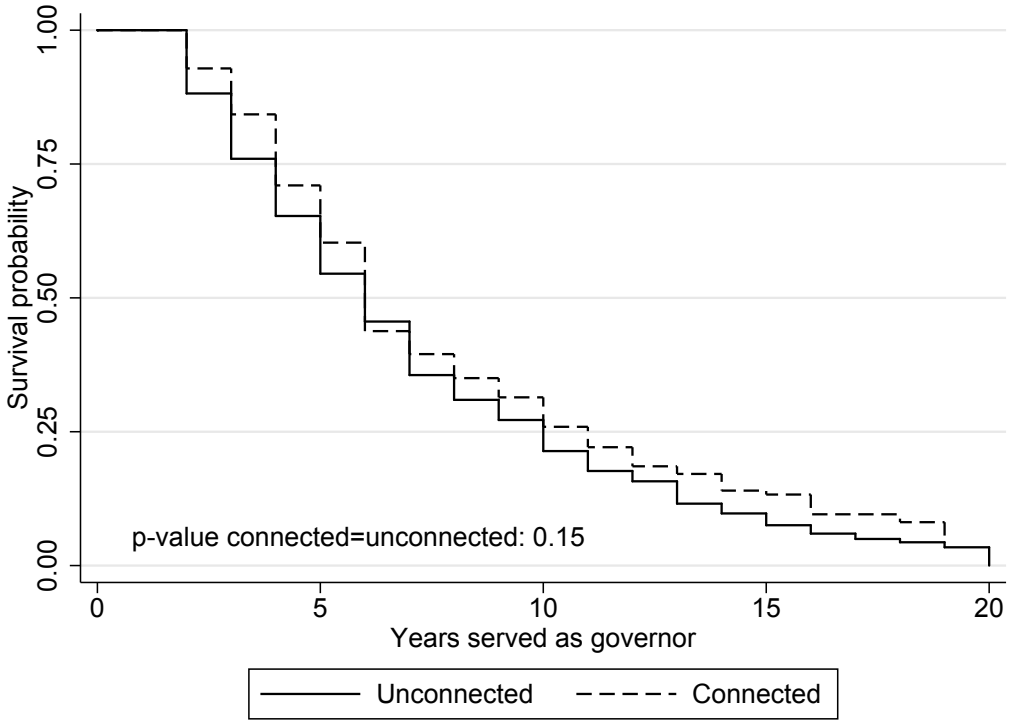
Notes: Number of governorships that need to be filled (i.e. are beyond the statutory six year term limit) and the share of available connected governors. The share of available connected governors is defined as the proportion of serving governors who are connected and beyond the statutory six year term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

Figure A.4: Size of switcher sample and cut-off for shared ancestry



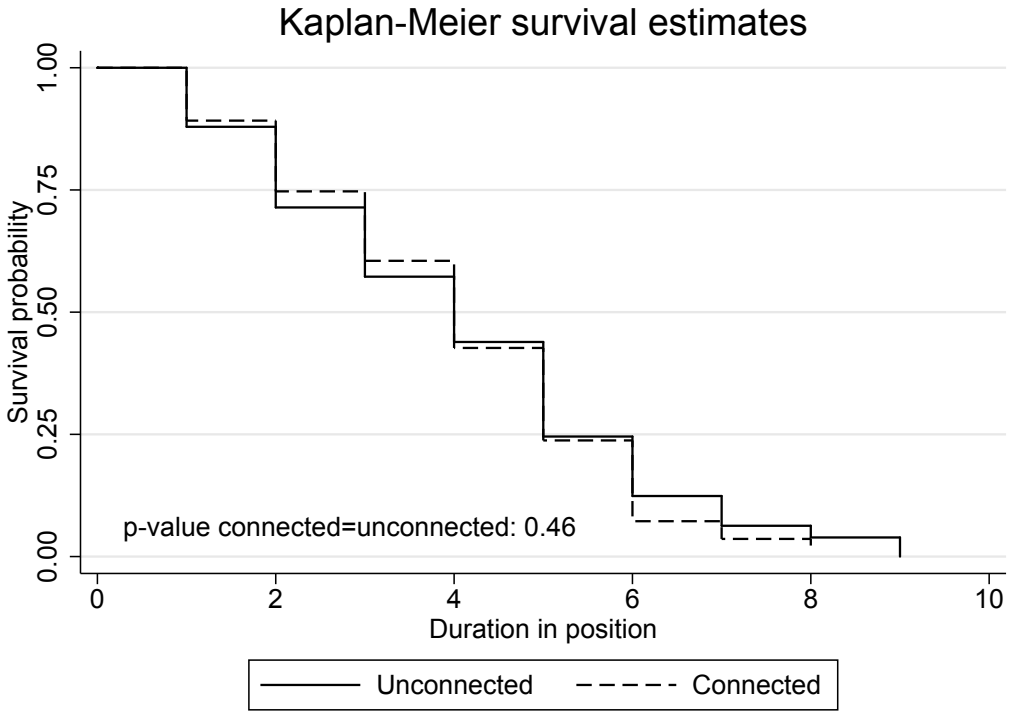
Notes: Number of governors/governor-colony spells that experience a within-shock to connections as a function of the cut-off for connectedness

Figure A.5: Retirement by connectedness - Survival estimates



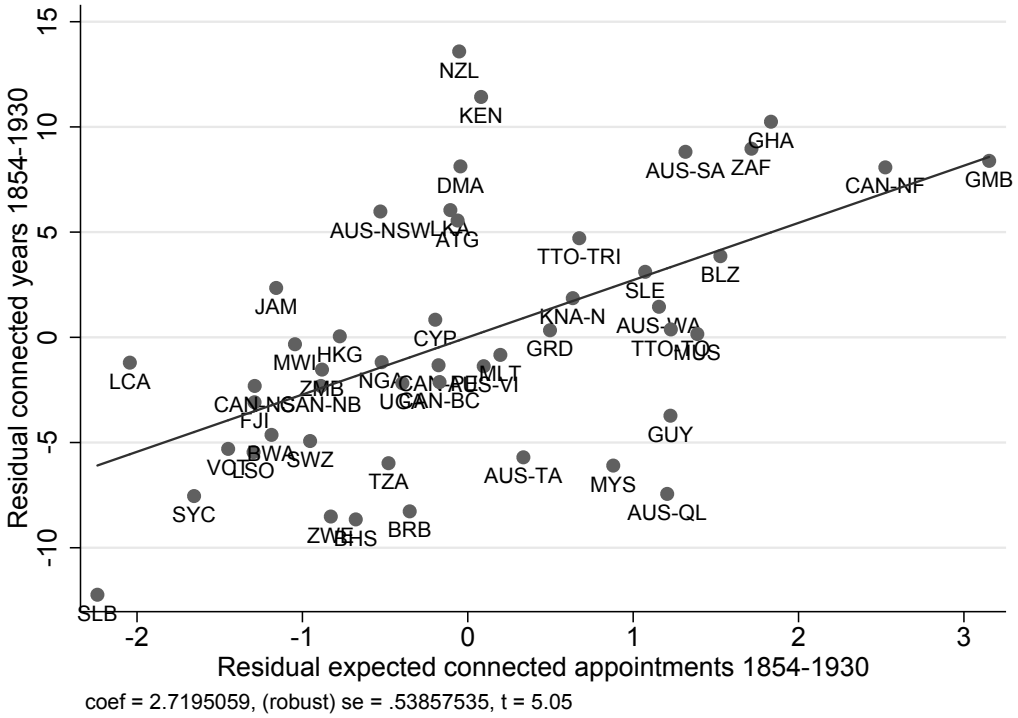
Notes: Kaplan-Meier survival estimates. Absorbing state is retirement from Colonial Office. Reporting the p-value for test of equality of survivor functions.

Figure A.6: 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 A.7: 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.

Table A.1: 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 A.2: Within-governor - switcher sample

	(1)	(2)	(3)
	Demeaned within governor		p-value
Average for	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 A.3: Within-appointment - switcher sample

	(1)	(2)	(3)
	Demeaned within position		p-value
Average for	Connected	Unconnected	diff
Total years served	0.012	-0.016	0.876
mean: 5.907	(1.898)	(1.800)	
Duration in position	0.010	-0.014	0.895
mean: 2.543	(1.877)	(1.782)	
Transfer	-0.003	0.004	0.718
mean: 0.086	(0.252)	(0.264)	
Retire	-0.015	0.021	0.142
mean: 0.104	(0.267)	(0.309)	
Exit	-0.017	0.023	0.183
mean: 0.182	(0.360)	(0.397)	
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 A.4: 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***			0.485**
	(0.11)			(0.23)
New Prime Minister $t-1$		0.336***		0.007
		(0.10)		(0.21)
Revenue growth $t-1$			0.462	0.702
			(0.94)	(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 A.5: Connectedness between Secretary of State and governor: Correlation matrix

	(1) Shared ancestry	(2) Both aristocrats	(3) Both Etonian	(4) Both Oxbridge	(5) 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 A.6: 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***				0.279***	0.276***
	(0.022)				(0.042)	(0.043)
log Population		0.295***			0.064*	0.082**
		(0.041)			(0.035)	(0.037)
log Settler mortality			-0.113***		-0.001	-0.054
			(0.040)		(0.036)	(0.055)
log Distance to London				0.164	-0.083	-0.402
				(0.183)	(0.131)	(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) d istance 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 A.7: 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 A.8: 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**	0.024**	0.024**	-0.028**	0.029***
	(0.009)	(0.009)	(0.009)	(0.011)	(0.010)
Average growth		0.028	0.028	0.359***	0.000
		(0.060)	(0.065)	(0.103)	(0.069)
Average growth × Connected			-0.001	0.026	0.065
			(0.147)	(0.204)	(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 A.9: Descriptive statistics: Modern outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
				Percentile		
	N	Mean	SD	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: $\log(\Sigma_i^N N^{-1}|X_{is} - Z_{is}|)$. 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 A.10: 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	2.720***	2.739***		-0.031
appointments 1854-1930	(0.539)	(0.534)		(0.272)
Expected # connected		-0.857	3.734***	3.743***
appointments 1931-1966		(3.977)	(1.340)	(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 A.11: 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	-0.196		-0.201			
1854-1930	(0.173)		(0.170)			
Connected years		-0.177	-0.202			
1930-1966		(0.322)	(0.340)			
Exp. connected years				-1.963**		-1.973**
1854-1930				(0.805)		(0.824)
Exp. connected years					-0.192	0.435
1930-1966					(2.457)	(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 efects 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 A.12: 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	0.022	0.013	0.013	-0.009	0.001
1854-1930	(0.037)	(0.011)	(0.011)	(0.011)	(0.015)
Connected years	-0.059	-0.023	-0.023	0.018	0.015
1931-1966	(0.101)	(0.027)	(0.027)	(0.041)	(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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: 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***	0.225***	0.224***	0.222***	0.222***
	(0.035)	(0.035)	(0.035)	(0.035)	(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 A.14: 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***	0.098***	0.101***	0.090**
	(0.036)	(0.035)	(0.036)	(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 A.15: 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**	-0.042**	-0.006	0.013
	(0.017)	(0.021)	(0.020)	(0.024)
Reform dummy		0.023		-0.080**
× Connected		(0.032)		(0.038)
Connected + Connected ×	-	-0.019	-	-0.066
Reform dummy		(0.024)		(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 A.16: 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
No. colonies served	0.068	0.322***	0.247**	0.137
	(0.063)	(0.052)	(0.099)	(0.089)
Connected	-0.053***	-0.073***	-0.058*	-0.064**
	(0.020)	(0.024)	(0.031)	(0.031)
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 A.17: 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**	-0.033*	-0.038*	-0.043**	-0.033*
	(0.017)	(0.020)	(0.020)	(0.020)	(0.020)
No. colonies served	0.068	0.066	0.065	0.064	0.082
	(0.063)	(0.063)	(0.063)	(0.063)	(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 A.18: 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*
Standard errors						
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
Standard errors						
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 A.19: 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		-0.072		-0.081
appointment $t-3$		(0.066)		(0.080)
Prob. connected		0.041		0.014
appointment $t-2$		(0.108)		(0.103)
Prob. connected	0.197**	0.225***	0.160*	0.196**
appointment $t-1$	(0.076)	(0.084)	(0.086)	(0.091)
Prob. connected			0.072	0.057
appointment t			(0.084)	(0.083)
Prob. connected			0.060	0.037
appointment $t+1$			(0.109)	(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 A.20: 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 $\times$		-0.135		0.133		0.002
Prob. connected appointment		(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 A.21: 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***	-0.880**	-0.940**	-0.434*	-0.439
	(0.263)	(0.347)	(0.440)	(0.225)	(0.482)
Years of British colonization	0.081**	0.088**	0.108*	0.054*	0.045
	(0.035)	(0.039)	(0.059)	(0.032)	(0.047)
Area tropics	-0.225***	-0.250***	-0.271**	-0.258***	-0.248**
	(0.066)	(0.070)	(0.117)	(0.071)	(0.097)
log Initial governor salary	1.791	2.809	2.448	-0.216	0.392
	(2.170)	(2.765)	(2.664)	(2.172)	(2.982)
Landlocked		-5.787			-4.799
		(5.634)			(5.732)
Ethnic fractionalization			7.327		-5.256
			(12.490)		(7.913)
log Genetic distance				-0.854	-0.141
				(1.927)	(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.1 Model extensions

A.1.1 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,

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

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

A.2 Data appendix

A.2.1 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 A.1). 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.

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.

A.2.2 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 Djikstra's algorithm (Dijkstra, 1959), implemented using Matlab's *graphshortestpath* 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.

A.2.3 Computing additional performance measures

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.

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. In 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.

Appendix B

Appendices to Chapter 2: The Costs of Bureaucratic Rigidity: Evidence from the Indian Administrative Service

Table B.1: IAS Promotion Guidelines - Seniority based progression

Scale	Level	Years	Description	Grade
1.	Junior time scale	0	Entry level	Jr. Time Scale
2.	Senior time scale	4	Committee of Chief Secretary and two supertime scale officers to evaluate and decide suitability of promotion - subject to vacancies	Sr. Time Scale
3.	Jr. Admin. Grade	9	Non-functional, admissable without any screening except when disciplinary proceedings are pending against the officer	Under Secy, Dy Secy Level/JAG, Dy Secy Equiv, Dy Secy, Under Secy Equiv, Under Secy Level
4.	Selection Grade	13	Committee of Chief Secretary and two supertime scale officers (or above) to screen - subject to vacancies	Dir Level/SLJAG, Directory Equiv, Director
5.	Supertime scale	16	Committee of Chief Secretary and two principal secretaries (if unavailable, seniormost supertime scale officer) to screen - subject to vacancies	JS Level/Level-I, Joint Secy, Joint Secy (Ex-Off), Joint Secy Equiv, Addl Secy Level, Addl Secy, Addl Secy (Ex-Off)
6.	Principal secretary	25	Committee of Chief Secretary and one senior most officer on the Chief Secretary level to screen. Subject to vacancies.	Secretary, Secy (Ex-Off), Secy Equiv
7.	Chief Secretary	30	Committee of Chief Secretary, one officer in same grade within state, one officer serving at Centre	Above Secy Level, Cab Secy

Notes: IAS Promotion Guidelines (2000): No. 20011/4/92/AIS-II.

Table B.2: 360 degree measures of effectiveness, by stakeholder group

		(1)	(2)	(3)	(4)	(5)
		Subjective ratings				
		Effective	Probity	Pressure	Pro-Poor	Overall
IAS	Mean	3.921	3.918	3.835	3.882	3.879
	SD	0.990	1.072	0.985	0.992	0.996
	N	4,932	4,217	4,767	4,752	4,955
State Civil Service	Mean	3.943	3.810	3.532	3.802	3.839
	SD	0.988	1.116	1.108	1.089	1.061
	N	2,571	2,041	2,422	2,468	2,611
Large firms	Mean	3.748	3.704	3.553	3.530	3.724
	SD	1.057	0.983	1.040	0.977	0.982
	N	2,708	2,402	2,541	2,575	2,661
MLAs	Mean	3.642	3.518	3.258	3.302	3.512
	SD	1.138	1.185	1.183	1.313	1.036
	N	2,595	2,164	2,367	2,473	2,580
NGOs	Mean	3.535	3.528	3.307	3.283	3.455
	SD	1.125	1.141	1.172	1.162	1.076
	N	1,927	1,694	1,816	1,856	1,930
Media (Print & TV)	Mean	3.421	3.350	3.322	3.060	3.258
	SD	1.116	1.047	1.039	1.124	1.075
	N	3,020	2,635	2,815	2,923	2,961
Pooled	Mean	3.730	3.670	3.523	3.527	3.646
	SD	1.077	1.105	1.094	1.141	1.057
	N	17,753	15,153	16,728	17,047	17,698

Notes: Descriptive statistics (mean, standard deviation (SD) and sample size) of 360 degree measures of effectiveness, broken down by the assessing stakeholder group. The abbreviation MLAs stands for members of the legislative assembly. NGOs stands for non-governmental organization.

Table B.3: 360 degree measures of effectiveness, by source of information

		(1)	(2)	(3)	(4)	(5)
		Subjective ratings				
		Effective	Probity	Pressure	Pro-Poor	Overall
Personal interaction	Mean	3.928	3.772	3.665	3.671	3.786
	SD	0.979	1.069	1.056	1.118	1.038
	N	9,751	8,325	9,407	9,492	9,724
Friends & Networks	Mean	3.179	3.546	3.328	3.306	3.461
	SD	1.239	1.152	1.108	1.107	1.062
	N	3,149	2,673	2,770	2,884	3,143
Media	Mean	3.689	3.545	3.347	3.371	3.486
	SD	1.022	1.124	1.119	1.165	1.052
	N	4,853	4,155	4,551	4,671	4,831
Pooled	Mean	3.730	3.670	3.523	3.527	3.646
	SD	1.077	1.105	1.094	1.141	1.057
	N	17,753	15,153	16,728	17,047	17,698

Notes: Descriptive statistics (mean, standard deviation (SD) and sample size) of 360 degree measures of effectiveness, broken down by source of information. Personal interaction are assessments provided by respondents who know the rated officer personally. Friends & networks are those known through friends or social (work) networks, and media are those known through television, radio or newspaper.

Table B.4: 360 measures and age at entry - Full controls

	(1) Effective	(2) Probity	(3) Pressure	(4) Pro-poor	(5) Overall
Mean of dep. var	3.730	3.671	3.524	3.528	3.647
Age at entry	-0.009**	-0.009**	-0.015***	-0.007*	-0.010**
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Entry score	0.041***	0.021	0.023**	0.021*	0.041***
	(0.010)	(0.013)	(0.011)	(0.012)	(0.010)
Training score	0.020**	0.011	0.017**	0.004	0.016*
	(0.008)	(0.010)	(0.008)	(0.009)	(0.009)
Improved	0.092***	0.058**	0.032	0.045**	0.044**
	(0.019)	(0.023)	(0.020)	(0.021)	(0.021)
Female	0.012	-0.022	-0.041*	0.042*	-0.001
	(0.020)	(0.024)	(0.021)	(0.022)	(0.023)
Caste: OBC	-0.027	-0.117***	-0.074**	-0.033	-0.031
	(0.031)	(0.045)	(0.037)	(0.037)	(0.037)
Caste: SC	0.038	0.001	0.058**	0.020	0.058**
	(0.024)	(0.031)	(0.028)	(0.028)	(0.027)
Caste: ST	-0.102***	-0.142***	-0.065	-0.085**	-0.067
	(0.039)	(0.047)	(0.041)	(0.039)	(0.042)
Urban background	-0.011	0.007	0.023	-0.002	-0.013
	(0.015)	(0.018)	(0.017)	(0.017)	(0.017)
Academic distinction	0.001	0.009	0.007	0.000	0.006
	(0.015)	(0.017)	(0.016)	(0.016)	(0.016)
STEM or Economics	0.013	-0.024	-0.002	-0.007	0.000
	(0.012)	(0.017)	(0.015)	(0.014)	(0.014)
Previous: Education/Research	0.042**	0.013	0.052**	0.036*	0.015
	(0.018)	(0.023)	(0.021)	(0.020)	(0.020)
Previous: Finance/Banking	0.023	-0.003	0.030	0.048	0.026
	(0.029)	(0.038)	(0.031)	(0.031)	(0.034)
Previous: Private/SOE	0.054***	0.020	0.062***	0.033	0.039*
	(0.020)	(0.027)	(0.024)	(0.024)	(0.023)
Previous: Public	0.020	-0.004	0.031*	0.003	-0.007
	(0.017)	(0.020)	(0.018)	(0.018)	(0.018)
Previous: AIS	-0.046	-0.011	0.038	-0.071*	-0.070
	(0.039)	(0.051)	(0.044)	(0.042)	(0.043)
State-specific respondent FEs	Yes	Yes	Yes	Yes	Yes
Source of information FEs	Yes	Yes	Yes	Yes	Yes
Interviewer FEs	Yes	Yes	Yes	Yes	Yes
Tenure year FEs	Yes	Yes	Yes	Yes	Yes
Observations	17,750	15,138	16,719	17,043	17,695

Notes: Unit of observation is the score for a given IAS officer in 2012-13 with at least 8 years of tenure. Relating the five performance measures to age at entry, cohort size and their interaction, where cohort size is the size of the state cohort in which the officer was allocated to, centered around the sample mean. State-specific respondent FEs are fixed effects for each respondent. Tenure year FEs are dummies for each year since entering the IAS. Source of information FEs are dummies for whether the respondent knows the officer personally, through friends or only through media. Standard errors clustered at the respondent level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.5: Test for (quasi-)random allocation across states

H_0: Random allocation across states	(1) p-value
Age at entry	0.143
Female	0.903
Other backward caste (OBC)	0.413
Scheduled caste (SC)	0.173
Scheduled tribe (ST)	0.191
Urban background	0.495
Academic distinction	0.226
STEM and Economics degree	0.506
Previous job: Education/research	0.305
Previous job: Finance/banking	0.256
Previous job: Private/SOE	0.454
Previous job: Public sector	0.103
Previous job: Public AIS	0.660
Ranking in year of intake	0.515
UPSC score	0.215
Training score	0.309
Improved	0.669
Cohort size (centered around state mean)	0.620
Age at entry $\times$ Cohort size (centered around state mean)	0.636

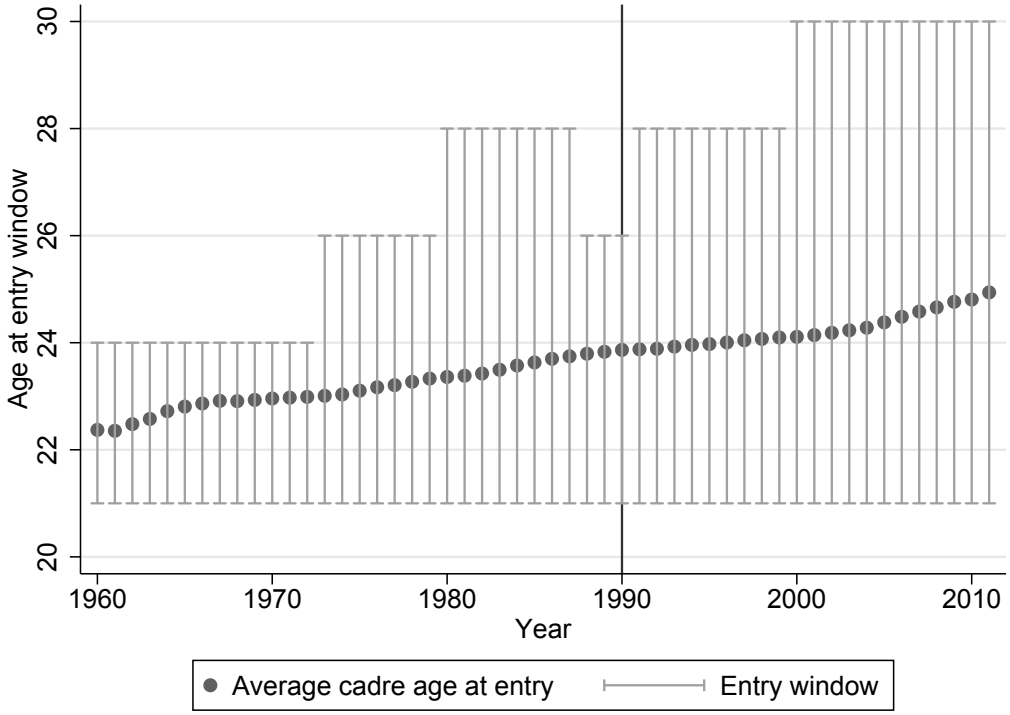
Notes: Test for random allocation across states for each year of intake between 1972-2009. The test is implemented by regressing the individual characteristics of the IAS officers on a set of state fixed effects and cadre fixed effects, and then testing the equality of the estimated state fixed effects. The total number of individuals in the sample is $N = 1,578$. Robust standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.6: 360 measures and suspensions

	(1)	(2)	(3)	(4)	(5)
	360 ratings - 2012/13				
	Effective	Probity	Pressure	Pro-Poor	Overall
On suspension	-0.004***	-0.007***	-0.007***	-0.005***	-0.008***
	(0.08)	(0.09)	(0.08)	(0.07)	(0.10)
Interviewer FEs	Yes	Yes	Yes	Yes	Yes
State-specific respondent FEs	Yes	Yes	Yes	Yes	Yes
Tenure year FEs	Yes	Yes	Yes	Yes	Yes
Source of information FEs	Yes	Yes	Yes	Yes	Yes
Observations	17,750	15,138	16,719	17,043	17,695

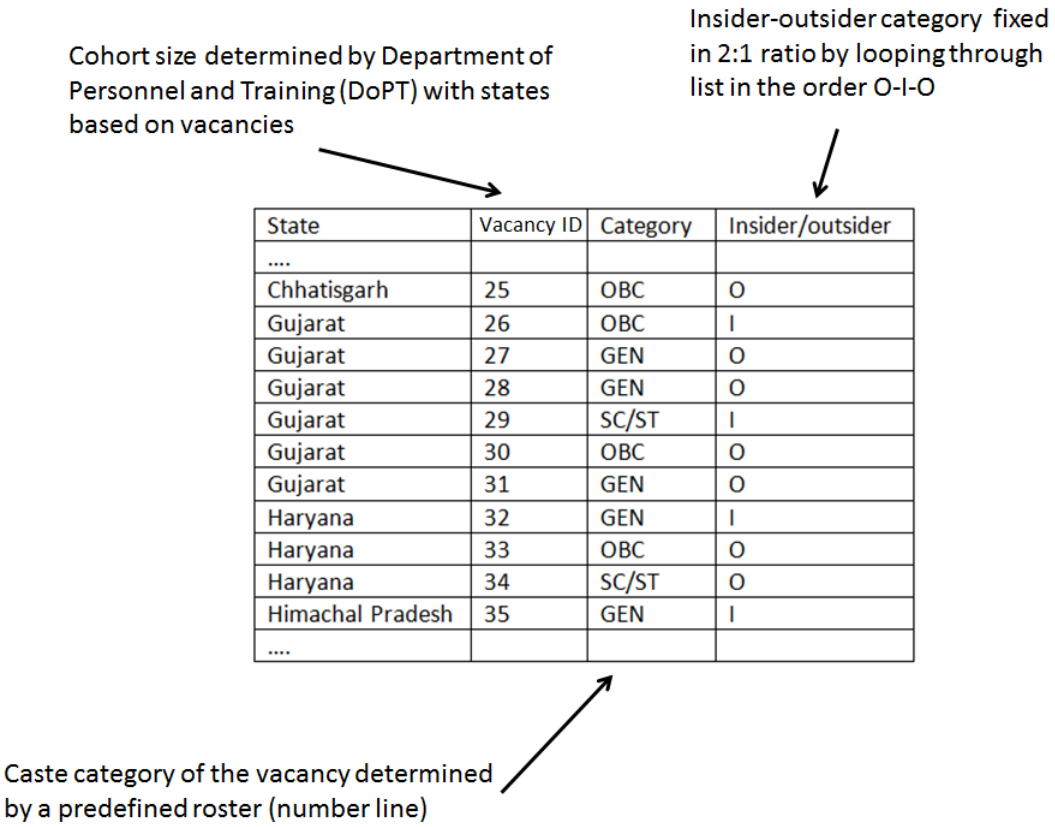
Notes: Unit of observation is the score for a given IAS officer in 2012-13 with at least 8 years of tenure. On suspension is a dummy that is 1 if the IAS officer is suspended in 2012-13. Interviewer FEs are dummies for each interviewer. State-specific respondent FEs are fixed effects for each respondent. Tenure year FEs are dummies for each year since entering the IAS. Source of information FEs are dummies for whether the respondent knows the officer personally, through friends or only through media. Standard errors in parentheses, clustered at the respondent level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure B.1: Statutory age at entry window of the IAS over time



Notes: Statutory age at entry window for general candidates of the IAS over time. Solid line marks beginning of the sample period for the state-level panel.

Figure B.2: Determination of vacancies: Example 2006



State	Vacancy ID	Category	Insider/outsider
....			
Chhatisgarh	25	OBC	O
Gujarat	26	OBC	I
Gujarat	27	GEN	O
Gujarat	28	GEN	O
Gujarat	29	SC/ST	I
Gujarat	30	OBC	O
Gujarat	31	GEN	O
Haryana	32	GEN	I
Haryana	33	OBC	O
Haryana	34	SC/ST	O
Himachal Pradesh	35	GEN	I
....			

Notes: Illustrating the assignment of categories (caste and home preference) to vacancies through the roster randomization for the year 2006. Vacancies are earmarked by caste status (O.B.C. denotes other backward castes, S.C./S.T. scheduled castes/tribes and unreserved the general castes) and home state ("I" denotes insider vacancies reserved for applicants from the same state; "O" denotes outsider vacancies reserved for applicants from other states). The assignment occurs through a number line.

Figure B.3: Assignment of categories (caste and home preference) to vacancies through roster randomization

Cadre Allocation - 2006

Distribution of vacancies to be filled in various cadres/joint cadres of Indian Administrative Service (IAS) on the basis of Civil Services Examination 2006, among Insider and Outsider Vacancies and between categories.

Sl. No.	Name of the State Cadre / Joint Cadre	Unreserved Insider	Unreserved Outsider	OBC Insider	OBC Outsider	SC/ST Insider	SC/ST Outsider	Total
1	A G M U T	1	2	1	0	0	1	5
2	Andhra Pradesh	1	1	0	0	0	0	2
3	Assam Meghalaya	1	2	0	1	1	0	5
4	Bihar	2	1	0	2	1	1	7
5	Chhatisgarh	0	3	1	1	1	0	6
6	Gujarat	0	3	1	1	1	0	6
7	Haryana	1	0	0	1	0	1	3
8	Himachal Pradesh	1	0	0	0	0	0	1
9	Jammu & Kashmir	0	1	0	0	0	0	1
10	Jharkhand	0	1	0	0	0	0	1
11	Karnataka	0	1	1	0	0	1	3
12	Kerala	1	0	0	1	0	0	2
13	Madhya Pradesh	2	1	0	1	0	1	5
14	Maharashtra	1	2	0	1	1	0	5
15	Manipur Tripura	0	3	0	1	1	0	5
16	Nagaland	0	1	0	1	1	0	3
17	Orissa	1	1	0	1	0	1	4
18	Punjab	0	1	1	0	0	1	3
19	Rajasthan	0	1	1	0	0	1	3
20	Sikkim	0	0	1	0	0	1	2
21	Tamil Nadu	0	1	1	0	0	0	2
22	Uttar Pradesh	1	2	0	2	1	1	7
23	Uttaranchal	1	0	0	1	0	1	3
24	West Bengal	0	3	1	0	0	1	5
		14	31	9	15	8	12	89

Notes: The final distribution of vacancies by state and caste/home quota for the year 2006. Vacancies are earmarked by caste status (O.B.C. denotes other backward castes, S.C./S.T. scheduled castes/tribes and unreserved the general castes) and home state (insider vacancies are reserved for applicants from the same state; outsider vacancies are reserved for applicants from other states).

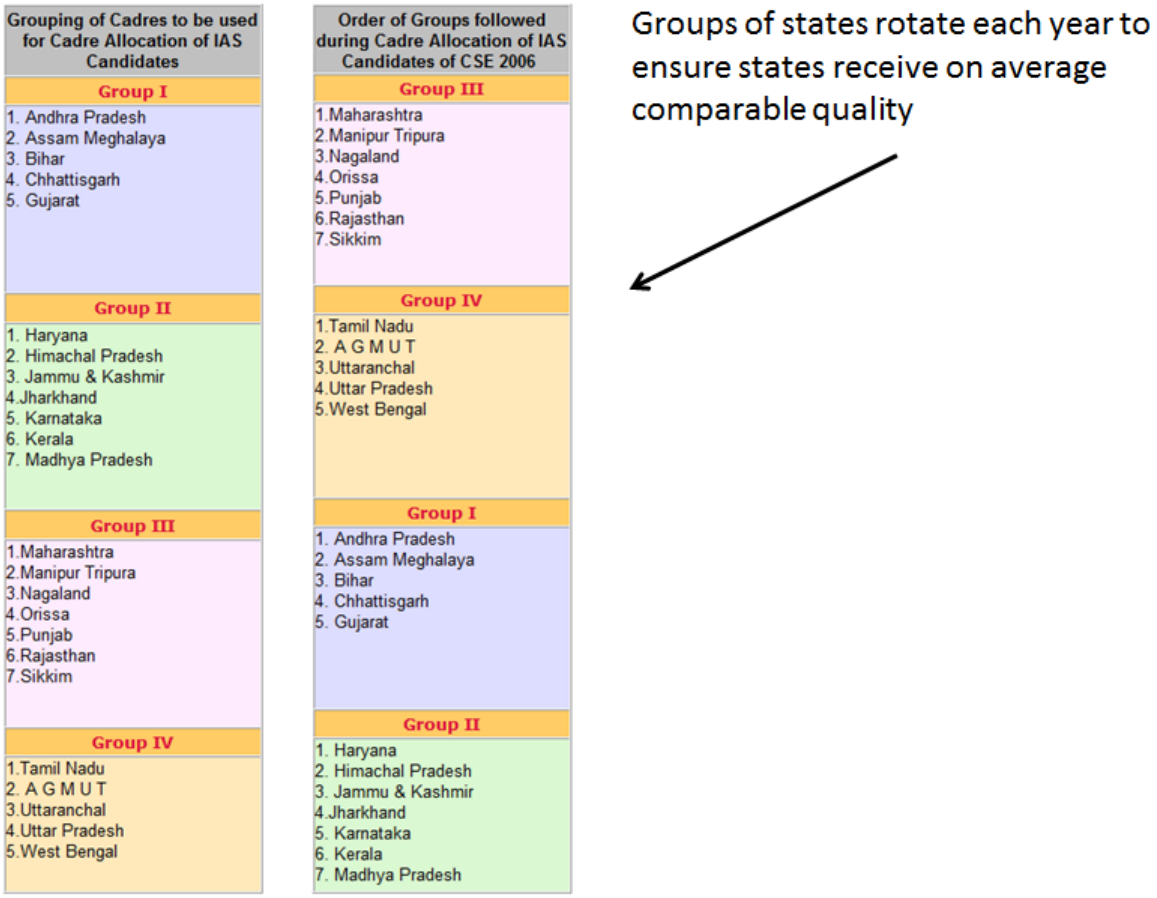
Figure B.4: Merit-based (UPSC rank) allocation based on caste and home preference match

Master Statement in respect of candidates allotted to Indian Administrative Service on the basis of Civil Services (Main) Examination, 2006 for purpose of their Cadre Allocation

Sl. No.	Rank	Name of the Candidate	Home State	Category	Whether Home State Opted?
1	1	MUTYALARAJU REVU	Andhra Pradesh	O.B.C*	Yes
2	2	AMIT SAINI	Punjab	General	Yes
3	3	ALOK TIWARI	Uttar Pradesh	General	Yes
4	4	PRASANTH N	Kerala	General	Yes
5	5	SHASHANK MISRA	Uttar Pradesh	General	Yes
6	6	VYASAN R	Kerala	General	No
7	8	ANINDITA MITRA	Chhatisgarh	General	No
8	9	ARAVIND AGRAWAL	Orissa	General	Yes
9	10	JUHI MUKHERJEE	Chandigarh	General	Yes
10	11	BISHNU CHARAN MALLICK	Orissa	S.C.	Yes
11	12	DEEPAK RAWAT	Uttaranchal	General	Yes
12	13	NILA MOHANAN	Kerala	General	Yes
13	14	JAI SINGH	Uttar Pradesh	General	Yes
14	15	MOUMITA BASU	West Bengal	General	Yes
15	16	SHAMMI ABIDI	Uttar Pradesh	General	Yes
16	17	REMYA MOHAN MOOTHADATH	Kerala	General	Yes
17	18	SHRIMAN SHUKLA	Madhya Pradesh	General	Yes
18	19	SHEETAL VERMA	Uttar Pradesh	S.C.*	Yes
19	20	SHAINAMOL A	Kerala	O.B.C*	Yes
20	21	YASHA MUDGAL	Rajasthan	General	Yes
21	22	ATUL KUMAR	Haryana	General	Yes
22	23	SHUCHI TYAGI	Uttar Pradesh	General	Yes
23	24	ANURAG TEWARI	Uttar Pradesh	General	Yes
24	25	UDIT PRAKASH	Uttar Pradesh	General	Yes
25	26	SACHINDRA PRATAP SINGH	Uttar Pradesh	O.B.C	Yes

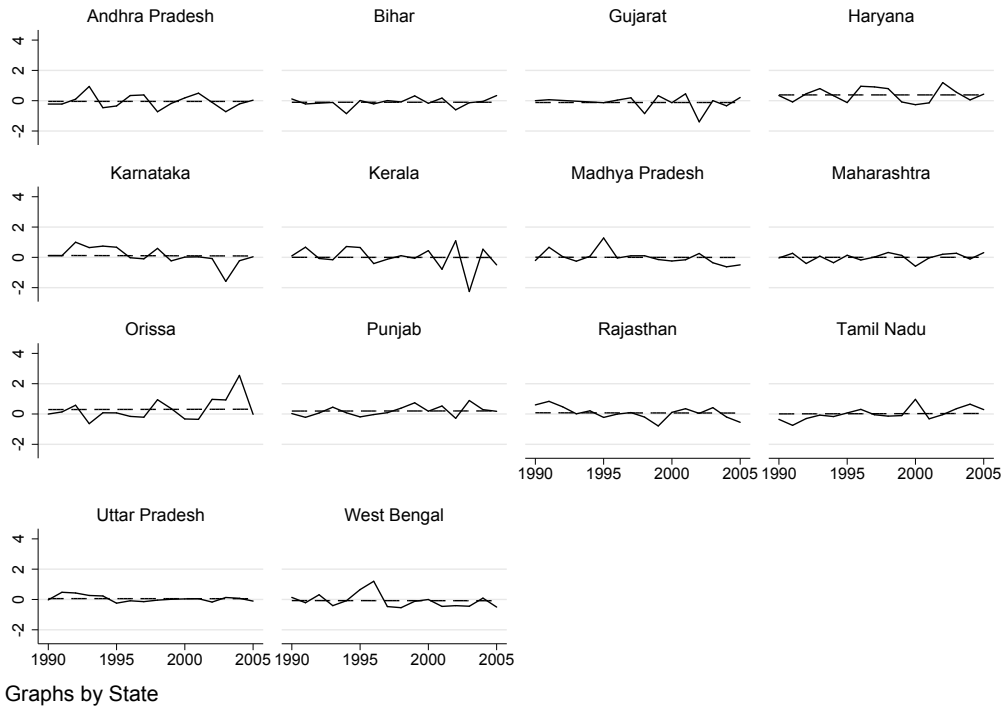
Notes: Illustrating the ranking of candidates using the intake year of 2006. Candidates in a given year of intake are ranked in descending order based on the UPSC entry exam score. Home state denotes the state from which the candidate applied from. Category denotes the caste of the candidate, where O.B.C. denotes other backward castes, S.C. scheduled castes, S.T. scheduled tribes and General the unreserved castes. Whether home state opted denotes if the applicant indicated a preference to be allocated to the home state.

Figure B.5: Rotation of state groups over years



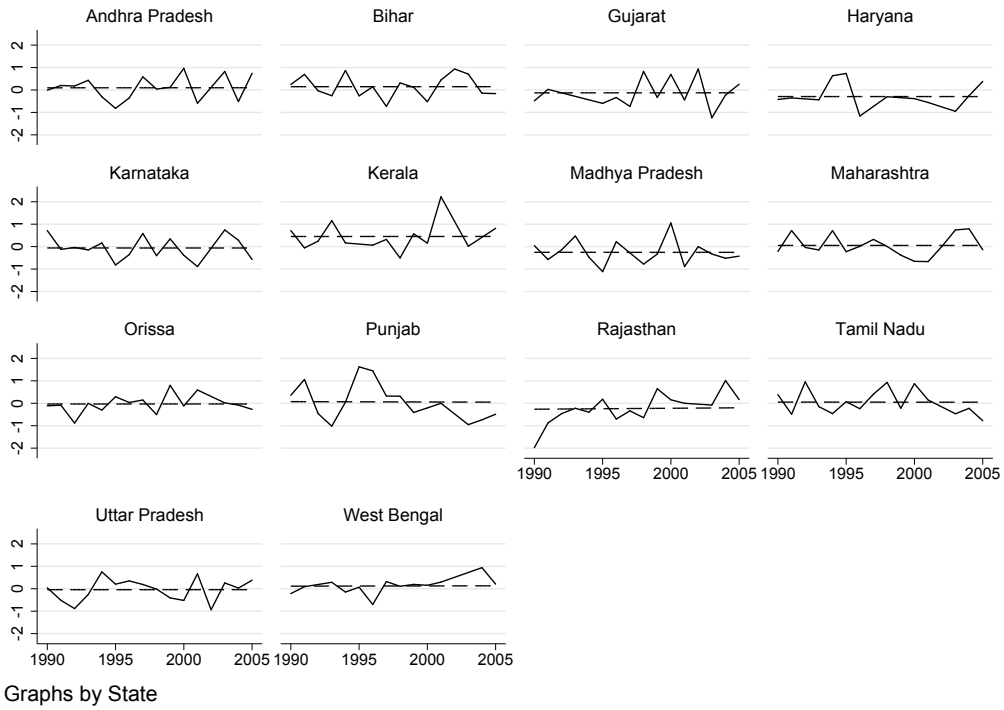
Notes: Division of state cadres into four groups and the rotation of groups in the order of IAS officer allocation over time, as illustrated by the group order in 2006. The groups of states rotate each year. In 2007, for example, the order changes to Group II, Group III, Group IV, Group I.

Figure B.6: Quasi-random allocation across states: UPSC (entry) score



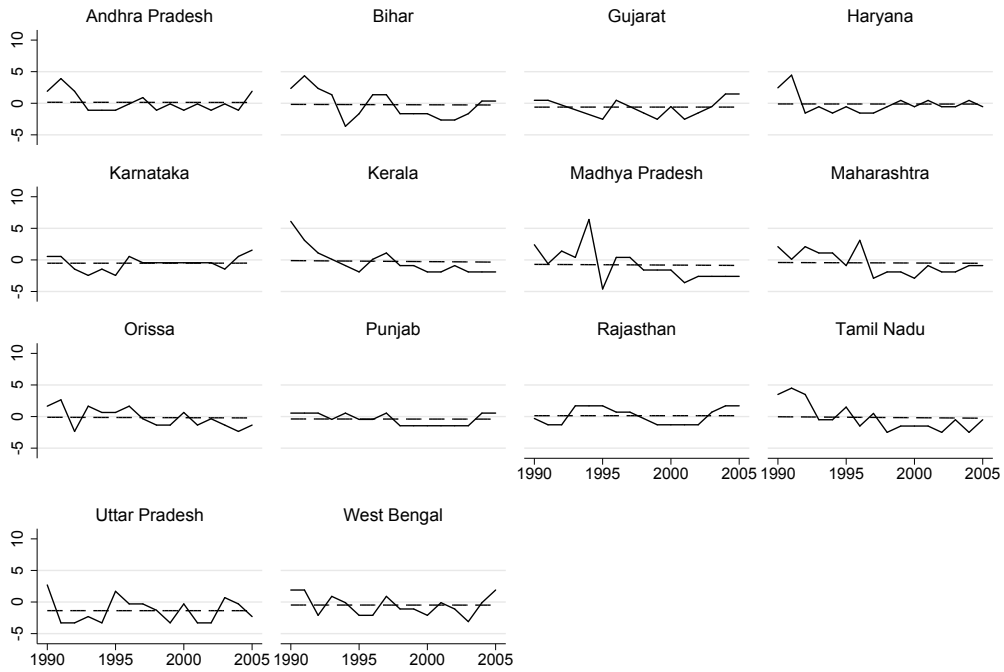
Notes: Average UPSC score of IAS officers (standardized relative to their year of intake) allocated to states 1972-2009. The trend line is fitted as a non-parametric local polynomial.

Figure B.7: Quasi-random allocation across states: Age at entry



Notes: Average age at entry of IAS officers (standardized relative to their year of intake) allocated to states 1972-2009. The trend line is fitted as a non-parametric local polynomial.

Figure B.8: Quasi-random allocation across states: Cohort size



Notes: Average number of officers, i.e. the cohort size (standardized relative to the state average) allocated to states 1972-2009. The trend line is fitted as a non-parametric local polynomial.

B.1 Allocation rule

Key to our empirical analysis is the rigid rule that determines the allocation of IAS officers and the cohort sizes of each state's intake. Here, we briefly summarize the allocation rule. A detailed documentation can be found in the IAS guidelines.[1] Coinciding with our sample period, we focus on the pre-2008 allocation rule, paying particular attention to the sources of variation that give rise to the observed quasi-random allocation of IAS officers across cadres.

After entering the IAS following the UPSC exams, centrally recruited IAS officers are allocated to 24 cadres. These cadres typically map directly into the Indian states. Smaller states, however, are grouped into three joint cadres, which are Assam-Meghalaya, Manipur-Tripura and AGMUT (Arunachal Pradesh, Goa, Mizoram and Union Territories (Delhi). We did not survey states with pooled cadres due to logistical constraints. The cadres we study therefore map directly onto the 14 major states which contain the majority of India's population.

The allocation process can be broadly divided into three steps: In the first step, IAS applicants are asked to declare their preference to remain in their home state (referred to as "insider" preference). In the second step, the overall number of vacancies and the corresponding quotas for castes and "insiders" are determined. In the final step, vacancies and officers are matched in the actual allocation process where merit (as defined by the ranking in the UPSC entry exam), caste status and locational preferences are all taken into account. The interplay of idiosyncrasies in each of these steps gives rise to the observed quasi-random allocation of IAS officers across cadres.

Step 1. IAS officers can declare their cadre preferences by first stating their preference to remain in their state of residence. Nearly all IAS officers exercise this option. The declared preferences however do not guarantee the actual allocation: only 7.5% of all IAS officers are allocated to their home state. The actual allocation depends on the availability of vacancies.

[1] For full details, refer to the original official notifications 13013/2/2010-AIS-I, 29062/1/2011-AIS-I and 13011/22/2005-AIS-I published by the Department of Personnel and Training, Ministry of Personnel, Public Grievances and Pensions, Government of India.

Step 2. The total number of vacancies is determined by the state government with the Department of Personnel and Training. Typically, the overall number of vacancies in a given year depend on the shortfall from the total number of IAS officers designated to a state (the cadre strength). This cadre strength is defined by the "cadre strength fixation rules", whereby larger states are assigned more IAS officers. These rules are seldom revised so the designated state cadre strength is fixed over longer periods. The vacancies are then broken down by quotas on two dimensions: caste and home preference. There are three categories for castes: General (unreserved) caste, Scheduled Caste/Tribes (SC/ST) and Other Backward Castes (OBC). The designation of vacancies to these caste categories are made based on predefined national quotas. The actual assignment of each vacancy to a caste is randomized using a rotating roster. In terms of preferences, vacancies are broken down into "insider" and "outsider" vacancies. Insider vacancies are to be filled by IAS officers from the same state who declared their home state preference at time of application. The ratio of insider to outsider vacancies is 1:2, with the assignment of vacancies to "insider" or "outsider" category following the repeating sequence O-I-O. The determinantion of vacancies is illustrated in Appendix Figure B.2. The result of this procedure is a list denoting the number of vacancies for each state and the corresponding quotas by caste status (SC/ST/OBC) and home state (insider/outsider) as shown in Appendix Figure B.3.

Step 3. The final allocation process is based on merit as determined by the ranking in the UPSC entry exam, the vacancies available and the preference stated.

Before the officers are allocated, the candidates are ranked and assigned a serial number in the order of merit, as determined by the UPSC exam. Appendix Figure B.4 shows this ranking along with the officers' caste and home preference. The highest scoring candidate for the 2006 intake, for example, was Mutyalaraju Revu who belongs to the OBC category and indicated his preference to be assigned to Andhra Pradesh.

The allocation proceeds sequentially. First, the insider vacancies are allocated as far as exact matches along caste and home state preference permit.

If the number of matches exceed the vacancies, the higher ranking IAS officer is given preference. Given the exact match along caste and home state required for slotting, however, many insider vacancies typically remain unfilled. In this case, the caste requirement is successively relaxed. In presence of open unreserved insider vacancies, the unreserved insider vacancy can be allocated to insider IAS officers from SC/ST and OBC (following the exact order) if there is an SC/ST (or OBC) outsider vacancy to allow for the exchange: For example, if Gujarat has received two unreserved insider vacancies but only one Gujarati general caste to fill the first slot, the second slot is opened to Gujarati SC/ST insiders, and if those are not available, to OBC insiders. The reallocation, however, is only permitted when there is a corresponding outsider vacancy that can be converted to an unreserved outsider vacancy to maintain the quota among the caste vacancies. A Gujarati insider SC/ST then can only fill the unreserved insider vacancy if a SC/ST outsider vacancy is available for exchange. Similar rules apply for unfilled SC/ST or OBC insider vacancies. Open SC/ST insider vacancies that could not be filled are first relaxed to allow for OBC insider candidates and then to general candidates. Open OBC vacancies, similarly, can first be filled by SC/ST insider candidates and then by general candidates (in both cases provided there is a corresponding outsider slot for exchange). Any remaining open insider vacancies that could not be filled despite the relaxation of the quotas are converted to outsider vacancies to ensure all vacancies are filled.

The allocation of the outsiders and those who failed to be allocated to their preferred home state (and are consequently converted to outsiders) is done according to a rotating roster system. The roster is created by arranging all 24 cadres in alphabetical order and dividing them into four groups. These groups are devised on the basis of an average intake by each group, which over a period of time is roughly equal:

1. Group I: Andhra Pradesh, Assam-Meghalaya, Bihar, Chhattisgarh and Gujarat

2. Group II: Haryana, Himachal Pradesh, Jammu & Kashmir, Jharkhand, Karnataka, Kerala and Madhya Pradesh

3. Group III: Maharashtra, Manipur-Tripura, Nagaland, Orissa, Punjab, Rajasthan and Sikkim

4. Group IV: Tamil Nadu, AGMUT (UT Cadre), Uttaranchal, Uttar Pradesh and West Bengal

The outsider candidates are allocated in the order of merit across the four groups for the outsider available vacancies (including those that have been converted from insider vacancies). In the first cycle, all candidates are allocated to their matching caste vacancy in the four states of Group I, starting with Andhra Pradesh. In the second cycle, the remaining candidates are allocated to their matching caste vacancies in Group II and so on. Since states who receive officers earlier in the allocation process will receive higher ranked recruits, the order of the groups shuffles each year to ensure that all states receive officers of comparable quality. In Appendix Figure B.5, for example, Group III is the first group in 2006, followed by Group IV, Group I and Group II. In the subsequent year, the groups will rotate and the allocation of outsiders will commence with Group II first, followed by Group III, Group IV and Group I.

Appendix C

Appendices to Chapter 3: How Does Collective Reputation Affect Hiring? Selection and Sorting in an Online Labour Market

Table C.1: Testing the joint significance of Worker country FEs (Full sample 2001-12)

	(1)	(2)	(3)
	Top rating received for job		
Mean dep. var.	0.804	0.804	0.804
Previous: Top rating			0.039***
			(0.001)
Previous: Experienced			0.020***
			(0.001)
Overall R^2	0.008	0.691	0.692
Observations	144516	144516	144516
Worker country FEs $F(21, 109319)$	52.958***	22.962***	19.423***
Employer FEs $F(35175, 109340)$		6.921***	6.729***
Individual background $F(2, 144513)$			223.59***

The unit of observation is the job. Reporting the *F*-test for the joint significance of the collective trait, the worker's country, in predicting job performance, as measured by whether a top rating was received. *Top rating* is a dummy that is 1 if the job was completed with a highest rating by the employer (10/10). *Previous: Top rating* is a dummy that is 1 if the applicant worker received a top rating in previous jobs. *Previous: Experience* is a dummy that is 1 if the worker has completed more than a median amount (17) of jobs on the platform. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Testing for changes in the employer's job posting time

	(1)	(2)	(3)	(4)
	Local time at job posting			
	9am-5pm		8am-8pm	
Mean of dep. var.	0.345	0.345	0.495	0.495
First hire country	-0.051	0.008	-0.026	0.073
	(0.04)	(0.08)	(0.04)	(0.09)
First hire country × Top rating		-0.053		-0.039
		(0.08)		(0.09)
First hire country × Previous: Top rating		-0.018		-0.102
		(0.08)		(0.09)
First hire country × Previous: Experienced		-0.027		-0.080
		(0.08)		(0.09)
Estimation method	IV	IV	IV	IV
Employer FEs	X	X	X	X
Worker country FEs	X	X	X	X
Bilateral controls	X	X	X	X
Observations	20676	20676	20676	20676

The unit of observation is the job. The dependent variable *Local time at job posting* is a dummy that is 1 if the job was posted between 9am-5pm (8am-8pm) in the worker's local country time. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. A *Top rating* is defined as a rating of 10/10. *Previous: Top rating* is a dummy that is 1 if the applicant worker received a top rating in previous jobs. *Previous: Experience* is a dummy that is 1 if the worker has completed more than a median amount (17) of jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Robustness: Alternative dependent variables, time periods and samples

Panel A: First hire effect	(1) Hire after first job	(2) Alt. dep. vars. Highest	(3) Alt. dep. vars. $\geq$ 9th decile	(4) Alt. dep. vars. Rank	(5) Time period Continue Pre 2006	(6) Time period Continue Post 2006	(7) Employer sample Continue US employers	(8) Employer sample Continue $>$ 1 hires
Mean dep. var.	0.100	0.715	0.010	4.477	0.125	0.078	0.099	0.286
First hire country	0.031***	0.027***	0.004*	0.130**	0.031***	0.022**	0.024***	0.073***
	(0.006)	(0.006)	(0.002)	(0.05)	(0.01)	(0.01)	(0.008)	(0.02)
Estimation method	IV	IV	IV	IV	IV	IV	IV	IV
Employer FEs	X	X	X	X	X	X	X	X
Worker country FEs	X	X	X	X	X	X	X	X
Observations	146273	146273	146273	146273	70989	75284	80474	51545
Panel B: Contingent on first outcome	(9) Base Continue	(10) Alt. dep. vars. Highest	(11) Alt. dep. vars. $\geq$ 9th decile	(12) Alt. dep. vars. Rank	(13) Time period Continue Pre 2006	(14) Time period Continue Post 2006	(15) Employer sample Continue US employers	(16) Employer sample Continue $>$ 1 hires
Mean dep. var.	0.100	0.715	0.010	4.477	0.125	0.078	0.099	0.286
First hire country	-0.005	-0.004	-0.004	0.099	-0.001	-0.018	-0.018	0.037
	(0.01)	(0.01)	(0.00)	(0.11)	(0.02)	(0.02)	(0.02)	(0.04)
First hire country $\times$ Top rating	0.054***	0.051***	0.007	0.127	0.043**	0.061***	0.059***	0.073*
	(0.01)	(0.01)	(0.00)	(0.12)	(0.02)	(0.02)	(0.02)	(0.04)
First hire country $\times$ Previous: Top rating	0.006	0.000	0.008	0.015	0.010	-0.003	0.009	-0.002
	(0.01)	(0.01)	(0.01)	(0.13)	(0.02)	(0.02)	(0.02)	(0.04)
First hire country $\times$ Previous: Experienced	-0.007	-0.008	0.002	-0.057	-0.006	0.004	-0.004	-0.044
	(0.01)	(0.01)	(0.01)	(0.13)	(0.02)	(0.02)	(0.02)	(0.04)
Estimation method	IV	IV	IV	IV	IV	IV	IV	IV
Employer FEs	X	X	X	X	X	X	X	X
Worker country FEs	X	X	X	X	X	X	X	X
Observations	146273	146273	146273	146273	70989	75284	80474	51545

The unit of observation is the employer-country pair. **Panel A** re-estimates the main result in Section 3.4.1 with different measures of outcome (Column 2-4), time periods (5-6) and samples (7-8). *Highest* is a dummy that is 1 if an employer's largest cumulative wage payments went to the given country. *$\geq$ 9th decile* is a dummy that is 1 if the cumulative wage payments were above the 9th decile among payments to all hired countries. *Rank* transforms the continuous payments into an ordinal ranking of flows. To facilitate comparison, the measure has been inverted so higher ranks indicate a higher payments made. Column 5-6 splits the sample by the median year (2006). Column 7 restricts the sample to US employers. Column 8 conditions only on employers who hire at least twice. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. A *Top rating* is defined as a rating of 10/10. *Previous: Top rating* is a dummy that is 1 if the applicant worker received a top rating in previous jobs. *Previous: Experience* is a dummy that is 1 if the worker has completed more than a median amount (17) of jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. **Panel B** repeats the same regressions for the secondary result in Section 3.4.2. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Robustness: OLS and reduced forms

Panel A: OLS	(1)	(2)	(3)	(4)	(5)	(6)
	Hire after first job		Times hired		Total pay	
Mean dep. var.	0.100	0.100	0.236	0.236	10.026	10.026
First hire country	0.010***	0.008	0.023**	0.025	1.399***	2.074**
	(0.00)	(0.01)	(0.01)	(0.02)	(0.43)	(0.99)
First hire country × Top rating		0.011**		0.017		0.375
		(0.00)		(0.02)		(0.91)
First hire country × Previous: Top rating		-0.005		-0.014		-0.414
		(0.01)		(0.03)		(1.00)
First hire country × Previous: Experienced		-0.008*		-0.019		-1.594*
		(0.00)		(0.02)		(0.93)
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS
Employer FEs	X	X	X	X	X	X
Worker country FEs	X	X	X	X	X	X
Bilateral controls	X	X	X	X	X	X
Observations	146273	146273	146273	146273	146273	146273
Panel B: Reduced form	(5)	(6)	(7)	(8)	(9)	(10)
	Continue		Times		Total pay	
Mean dep. var.	0.100	0.100	0.236	0.236	10.026	10.026
Cheapest	0.009***	0.001	0.023**	-0.005	1.177***	0.751
	(0.00)	(0.00)	(0.01)	(0.02)	(0.39)	(0.91)
Cheapest × Top rating		0.015***		0.037*		1.128
		(0.00)		(0.02)		(0.80)
Cheapest × Previous: Top rating		-0.000		0.011		0.644
		(0.00)		(0.02)		(0.92)
Cheapest × Previous: Experienced		-0.007		-0.003		-1.121
		(0.00)		(0.02)		(0.85)
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS
Employer FEs	X	X	X	X	X	X
Worker country FEs	X	X	X	X	X	X
Bilateral controls	X	X	X	X	X	X
Observations	146273	146273	146273	146273	146273	146273

The unit of observation is the employer-worker country pair. **Panel A** reports OLS estimates and **Panel B** reports reduced forms. The dependent variable is a dummy that is 1 if the employer ever hired from the country from which workers applied in the first applicant pool *after* the first hire. *Times hired* is the number of times the employer hired from the country after the first hire. *Total pay* is the cumulative wages paid by an employer to workers in the given country after the first hire. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. A *Top rating* is defined as a rating of 10/10. *Previous: Top rating* is a dummy that is 1 if the applicant worker received a top rating in previous jobs. *Previous: Experience* is a dummy that is 1 if the worker has completed more than a median amount (17) of jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Robustness: Using different definitions of job rating

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Continue			
Mean dep. var.	0.100	0.100	0.100	0.100	0.100	0.100	0.100
First hire country	-0.106	-0.106**	-0.048	-0.033	-0.013	-0.002	-0.181***
	(0.07)	(0.05)	(0.04)	(0.03)	(0.02)	(0.01)	(0.06)
First hire country × Rating ≥ 5	0.137*						
	(0.07)						
First hire country × Rating ≥ 6		0.138**					
		(0.05)					
First hire country × Rating ≥ 7			0.080*				
			(0.04)				
First hire country × Rating ≥ 8				0.066**			
				(0.03)			
First hire country × Rating ≥ 9					0.048**		
					(0.02)		
First hire country × Rating ≥ 10						0.039**	
						(0.02)	
First hire country × Rating [0,1]							0.224***
							(0.07)
Estimation method	IV	IV	IV	IV	IV	IV	IV
Employer FEs	X	X	X	X	X	X	X
Worker country FEs	X	X	X	X	X	X	X
Observations	146273	146273	146273	146273	146273	146273	146273

The unit of observation is the employer-worker country pair. The dependent variable is a dummy that is 1 if the employer ever hired from the country from which workers applied in the first applicant pool *after* the first hire. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. A *Rating* $\geq X$ is a dummy that is 1 if a rating is higher than $X/10$. *Rating [0,1]* is the normalized rating that ranges from 0 to 1, where 0 is the absence of rating. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Robustness: Final choice relative to applicant pool average

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Characteristics of hired worker compared to applicant pool							
	Highest rating		Highest experience		Cheapest		Dominant bid	
Mean of dep. var.	0.465	0.465	0.402	0.402	0.476	0.476	0.181	0.181
First hire country	-0.019	-0.090	0.021	-0.124*	-0.013	-0.042	0.015	-0.015
	(0.03)	(0.07)	(0.03)	(0.07)	(0.03)	(0.06)	(0.03)	(0.06)
First hire country × Top rating given		0.097		0.052		0.046		0.017
		(0.07)		(0.07)		(0.07)		(0.06)
First hire country × Previous: Top rating		0.050		0.115		-0.033		0.054
		(0.07)		(0.07)		(0.07)		(0.06)
First hire country × Previous: Experienced		-0.060		0.157**		0.011		-0.008
		(0.07)		(0.07)		(0.07)		(0.06)
Estimation	IV	IV	IV	IV	IV	IV	IV	IV
Employer FEs	X	X	X	X	X	X	X	X
Country FEs	X	X	X	X	X	X	X	X
Bilateral controls	X	X	X	X	X	X	X	X
Observations	20761	20761	20761	20761	20761	20761	20761	20761

The unit of observation is the employer-worker country pair. The dependent variable captures the final choices of employer's relative to their applicant pools: In Column 1-2, the dependent variable is a dummy that is 1 if the employer hired the worker with the highest rating. In Column 3-4 (5-6), we capture whether the employer hired the most experienced (cheapest) worker. In Column 7-8, we summarize the previous three measures using a dummy whether the worker hired the dominant worker, based on the vertical traits of rating, experience and price. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. A *Top rating* is defined as a rating of 10/10. *Previous: Top rating* is a dummy that is 1 if the applicant worker received a top rating in previous jobs. *Previous: Experience* is a dummy that is 1 if the worker has completed more than a median amount (17) of jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Robustness: Controlling for observable country-specific match heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
	Employers			Workers		
	Hire	Times	Total pay	Applied	Top rated	High exp.
Mean of the dep. var.	0.100	0.236	16.357	0.273	0.573	0.196
First hire country	-0.016	0.154	0.618	-0.032	-2.867*	-1.819**
	(0.10)	(0.38)	(17.99)	(0.10)	(1.56)	(0.80)
First hire country × Top rating given	0.053***	0.132**	4.347*	0.051***	0.630***	0.259**
	(0.01)	(0.06)	(2.46)	(0.01)	(0.23)	(0.12)
First hire country × Previous: Top rating	0.004	0.042	3.043	0.008	0.151	0.020
	(0.01)	(0.07)	(2.80)	(0.01)	(0.26)	(0.12)
First hire country × Previous: Experienced	-0.006	0.042	-1.658	-0.002	0.338	0.289**
	(0.01)	(0.07)	(2.74)	(0.01)	(0.25)	(0.12)
First hire country × ln(Distance)	0.003	-0.014	0.334	0.005	0.368*	0.228**
	(0.01)	(0.05)	(2.36)	(0.01)	(0.21)	(0.11)
First hire country × Abs. time difference	-0.000	-0.004	-0.533	-0.001	-0.076	-0.049
	(0.00)	(0.02)	(0.57)	(0.00)	(0.06)	(0.03)
First hire country × Common language	-0.027*	-0.111*	1.821	-0.023	-0.680***	-0.271**
	(0.02)	(0.06)	(2.82)	(0.01)	(0.25)	(0.12)
Estimation	IV	IV	IV	IV	IV	IV
Employer FEs	X	X	X	X	X	X
Country FEs	X	X	X	X	X	X
Bilateral controls	X	X	X	X	X	X
Kleibergen-Paap F-statistic	233.14	233.14	233.14	233.14	233.14	233.14
Observations	146273	146273	146273	145281	145281	145281

The unit of observation is the employer-worker country pair. The dependent variable *hire* is a dummy that is 1 if the employer ever hired from the country from which workers applied in the first applicant pool *after* the first hire. *Times hired* is the number of times the employer hired from the country after the first hire. *Total pay* is the cumulative wages paid by an employer to workers in the given country after the first hire. *Applied* is a dummy that is 1 if workers from the country from which workers applied in the first applicant pool applied *after* the first hire. *No. of applicants* is the number of applicant workers from the country after the first hire. *Top rating* is the number of top rated applicant workers (10/10) from the country after the first hire. *High exp.* is the number of applicants with more than a median amount (17) of completed jobs on the platform. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. A *Top rating* is defined as a rating of 10/10. *Previous: Top rating* is a dummy that is 1 if the applicant worker received a top rating in previous jobs. *Previous: Experience* is a dummy that is 1 if the worker has completed more than a median amount (17) of jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Robustness: Alternative constructions of the wage IV

	(1)	(2)	(3)	(4)	(5)	(6)
	Dummy = 1 if employer ever hires from country after the first job					
Mean of the dep. var.	0.100	0.100	0.100	0.100	0.100	0.100
First hire country	0.031***	-0.005	0.038***	-0.006	0.046***	0.010
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
First hire country × Top rating		0.054***		0.056***		0.101***
		(0.01)		(0.01)		(0.02)
First hire country × Previous: Top rating		0.006		0.027*		-0.003
		(0.01)		(0.02)		(0.02)
First hire country × Previous: Experienced		-0.007		-0.009		-0.061***
		(0.01)		(0.01)		(0.02)
Instrument	Cheapest		Below average		# Below average	
Employer FEs	X	X	X	X	X	X
Country FEs	X	X	X	X	X	X
Bilateral controls	X	X	X	X	X	X
Kleibergen-Paap F-statistic	5214.54	419.57	9043.14	1007.02	1725.42	395.67
Observations	146273	146273	146273	146273	146273	146273

The unit of observation is the employer-worker country pair. The dependent variable *hire* is a dummy that is 1 if the employer ever hired from the country from which workers applied in the first applicant pool *after* the first hire. *Times hired* is the number of times the employer hired from the country after the first hire. *Total pay* is the cumulative wages paid by an employer to workers in the given country after the first hire. *First hire country* is a dummy that is 1 if the worker hired for the first job came from the country. A *Top rating* is defined as a rating of 10/10. *Previous: Top rating* is a dummy that is 1 if the applicant worker received a top rating in previous jobs. *Previous: Experience* is a dummy that is 1 if the worker has completed more than a median amount (17) of jobs on the platform. Bilateral controls include a dummy for the shared official country language, the absolute time difference in hours, and (log) centroid distance between the employer and worker country. *Cheapest* is the preferred instrument when a worker from the given country submitted the lowest (residual) bid. *Below average wage* is a dummy if at least one worker from the country submitted a wage bid that was below the applicant pool average. *# Below average wage* is a continuous measure that captures the number of workers bidding below the applicant pool average wage. The test for weak instruments is the Kleibergen-Paap Wald F-statistic. Robust SEs, clustered at the employer level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: LATE and probabilities of compliance using different instruments

	(1)	(2)	(3)	(4)	(5)	(6)
	LATE		First stage		Compliance probability	
	$\hat{\beta}_1$	$P[D=1]$	$P[D_1 > D_0]$	$P[z=1]$	$P[D1 > D0 \mid D=1]$	$P[D1 > D0 \mid D=0]$
Panel A: Wage bid instrument						
Cheapest	0.031***	0.176	0.333	0.184	0.348	0.149
	(0.01)					
Below average wages	0.038***	0.176	0.200	0.541	0.614	0.525
	(0.01)					

The table summarizes estimates using different instruments (Column 1) and characterizes the population of compliers. Column 5 shows the size of the complier population relative to the "treated" population (countries first chosen). Column 6 shows the size of the complier population relative to the "untreated" population of countries that were not chosen from the first applicant pool.