

What Explains the Geographic Variation in Corporate Investment?*

Shohini Kundu[†] Nishant Vats[‡]

September 9, 2021

Abstract

We show that history can explain the geographic concentration of investment over and above traditional agglomerative forces, geography, and expectations. We use spatial variation in direct and indirect British rule to identify differences in historical circumstances. Using this within-country variation in historical circumstances, combined with a local identification approach and instrumental variable strategy, we explain the spatial differences in investment. Differences in historical origins can explain 13% of total geographic variation in investment. Moreover, investment is 8-10% lower in direct ruled areas. Our results indicate that history can have long-run consequences through its effect on economic organizations and state capacity.

*We thank Rainald Borck, Francesco D'Acunto, Zhiguo He, Lakshmi Iyer, Ralph Koijen, Yueran Ma, Nagpurnanand Prabhala, Raghuram Rajan, James Robinson, Amir Sufi, Michael Weber, Xiao Zhao, and Eric Zwick for helpful comments and suggestions. We are thankful to the Fama-Miller Center at the University of Chicago, the NSE-NYU Stern Initiative on the Study of Indian Capital Markets, and the Wheeler Institute for Business and Development at the London Business School for financial support. We thank seminar participants at the Webinar series in Finance and Development (WEFIDEV), 2021 Transatlantic Doctoral Conference at the London School of Business, 10th European Meeting of the Urban Economic Association and the NSE-NYU Conference on Indian Financial Markets, 2020. We do not have any conflicts of interest to disclose. We take responsibility for all errors.

[†]Shohini Kundu is at the Anderson School of Management, University of California, Los Angeles. email: shohini.kundu@anderson.ucla.edu

[‡]Nishant Vats (corresponding author) is at the Booth School of Business, University of Chicago. Send correspondence to 5807 S Woodlawn Ave, Chicago, IL, USA. email: nvats@chicagobooth.edu

1 Introduction

What explains geographic concentration in investment? What role does history play? It has been established that investment is geographically concentrated. From Steel City to Silicon Valley, the formation of economic clusters has garnered attention from academics and policymakers alike. Understanding the forces behind the development of such clusters is fundamental to understanding the micro-foundations of geographic inequality – why are certain areas richer than others, and what can be done to address these differences? Thus far, the literature has focused extensively on the role of agglomerative forces – the availability and cost of factors of production – and geographic advantages to explain the rise and fall of such clusters. In this paper, we diverge from the extant literature, focusing on a novel aspect that can explain the geographic concentration of investment: history. We consider how the eventual investment concentration depends on the initial starting point, set by historical factors. Specifically, we emphasize the role of the destruction of well-established economic organizations and emergence of state capacity, resulting from differences in historical circumstances, as key factors in explaining the investment concentration today.

The central role of history may seem obvious if investment is assumed to follow a path-dependent process. However, establishing the empirical relationship between history and investment is difficult. A key element in the theoretical models of firms' choice of location is the existence of multiple equilibrium. [Krugman \(1991\)](#) argues that the eventual choice of the equilibrium can either be driven by the history or self-fulfilling expectations. Hence, the empirical challenge lies in disentangling the two forces. Moreover, to clearly establish the relevance of history, one needs to rule out the effect of confounding unobservables such as agglomerative forces and geographic advantage.

In this paper, we attempt to address this issue and show that historical circumstances can explain geographic concentration over and above the traditional agglomerative forces, geographic advantages and self-fulfilling expectations. We do this by using a two-pronged approach. First, we combine within country analysis with plausibly exogenous variation in historical circumstances, originating from direct and indirect colonial rule. Second, in addition to using conventional approaches of comparison, we use (1) a local identification approach of comparing neighboring areas to rule out concerns related to differences in agglomerative forces, geography and expectations, and

(2) an instrumental variable strategy to rule out concerns related to selection bias and simultaneity. Hence, our empirical strategy is better adept at solving the difficult identification problems associated with establishing a direction of causality between history and economic activity, in general, and, in particular, investment concentration.

This paper uses the within-country geographic variation in historical circumstances to explain spatial differences in investment. Colonial occupation of India provides such an environment. Before the onset of the British occupation of India starting in 1757, the Indian subcontinent was governed by local rulers. During the colonial era, different areas of the Indian subcontinent fell under the “direct rule” of the British, or, “indirect rule,” under the administration of native rulers. All areas, regardless of their colonial history, were brought under an identical legal and administrative framework after independence in 1947. Moreover, we verify that the direct and indirect ruled areas were similar across several observable dimensions before the onset of colonial rule. Therefore, India provides an ideal laboratory to examine the consequences of differences in historical circumstances on geographic variation in investment concentration in the present.

We begin with an aggregate analysis, showing that investment is concentrated within Indian states. Using data on district-level corporate investment, we compute a state-level measure of investment concentration, using the Herfindahl-Hirschmann Index (HHI). We compare the investment HHI with three benchmark measures – (1) equal investment in all districts, $\frac{1}{N}$, (2) investment proportional to geographical area of district relative to the state, and (3) investment proportional to the population of a district relative to the state. We conclude that investment is geographically concentrated within states, relative to a frictionless spatial equilibrium. Moreover, we show that states with a larger proportion of districts historically under direct British rule exhibit a higher geographic concentration of investment. Specifically, the level of within-state investment concentration is 20 percentage points higher relative to any of the benchmarks, and the proportion of districts under direct rule can explain 13% of total variation in within-state investment concentration.

States with a larger proportion of direct ruled districts exhibit greater investment concentration. We argue that greater investment concentration in states with a larger proportion of direct ruled districts is driven by higher investment in indirect ruled districts. A direct comparison of direct and indirect ruled districts combined with project level data indicates that the size of investment projects in direct ruled areas is 8.8% lower than the size of investment projects in indirect ruled

areas within a state. This estimate is robust to the inclusion of firm \times year and state \times year fixed effects. Moreover, we characterize that the differences in investment occur at both the intensive and the extensive margins. On the extensive margin, we find that projects are 25% less likely to be announced in direct ruled districts relative to indirect ruled districts, within a state. On average, total investment is ₹ 16 billion (\$ 0.2 billion) lower in direct ruled districts relative to indirect ruled districts within the same state.

We verify that our baseline findings are unlikely to be driven by geographic differences across districts, differences in law, differences in the distances to the state capitals, and spatial trends. Moreover, we conduct a placebo test, in which we run 10,000 simulations randomizing whether a district was under direct rule or not. We are able to generate an effect of a size at least as large as the baseline estimate only in 0.1% of cases, thereby ruling out issues related to the spuriousness and spatial autocorrelation of investment. The [Oster \(2019\)](#) test for our baseline analysis suggests that the baseline results are unlikely to be driven by omitted variables. Moreover, we verify that the direct and indirect ruled areas are similar across several observable characteristics, such as geography, distance from state capital, the religion of precolonial rulers, and the colonial population composition.

While these tests are informative and raise confidence in our results, we cannot completely rule out differences among districts along all observable and unobservable dimensions. Our baseline analysis assumes that one district in India is as good a control as any other district within a state. A direct comparison of direct and indirect districts may potentially bias our inference in the presence of systematic differences between direct and indirect ruled districts. Hence, we address concerns of selection and omitted variables by focusing on contiguous direct-indirect ruled district pairs, separated only by administrative borders within a state. We compare a firm's investment in direct and indirect ruled districts within a district-pair. Specifically, we include firm \times district-pair \times year fixed effect, allowing us to identify the estimate using variation in the size of investment projects announced by the same firm within a district-pair. Such an approach allows us to implicitly control for traditional agglomerative forces of [Marshall \(1920\)](#), geographic features, investment opportunities, and expectations that are likely to be similar across contiguous district-pairs. Moreover, whether a district within a contiguous direct-indirect ruled pair was under direct rule during the colonial period is likely to be a matter of chance. Hence, indirect ruled districts

are a valid counterfactual to the contiguous direct ruled districts. Our local identification approach suggests that the projects announced in direct ruled districts are 10.8% smaller in size relative to the projects announced in indirect ruled districts by the same firm within a contiguous district-pair.

The comparison of direct ruled districts with indirect ruled districts may still be prone to selection bias, hindering our ability to interpret the baseline effect as causal. While the low value of the Moran I statistic indicates that selection is likely to be of little concern, it may be unreasonable to assume that the direct control by British in India was random. We address concerns of selection through an instrumental variable strategy.

We exploit a unique feature of British annexation policy in India, the Doctrine of Lapse. The Doctrine of Lapse was in effect between 1848 and 1856, which allowed the governor-general of British India to annex Indian princely states where the ruler died without a natural heir. The relevance condition posits that the death of a ruler without a natural heir is associated with a territory coming under direct British rule. We verify the relevance condition associated with this instrument in the first stage. Our two stage least square estimates (2SLS) indicate that investment is lower in direct ruled districts relative to indirect ruled districts within a state, validating our interpretation of the relation between contemporary corporate investment and direct British rule being causal. An important assumption of this test is the exclusion restriction, which posits that the death of a ruler without a natural heir affects current corporate investment only through the territory being under direct British rule. We verify this assumption through two falsification tests. In the first falsification test, we directly study the relation between investment and the death of the ruler without a natural heir in indirect ruled districts in periods when the policy was not applicable. In the second falsification test, we directly study the relation between investment and the death of the ruler with a natural heir in indirect ruled districts when the Doctrine of Lapse was applicable. Both tests yield null results, supporting the exclusion assumption.

We explore the underlying mechanism through which direct British rule affects investment. We focus on two key channels – (1) the destruction of well-established economic organizations, and (2) differences in colonial state capacity which persist to date. We rule out alternative mechanisms that may explain our results including the geographic differences in law and enforcement, provision of public goods, and trust.

We show that the East India Company (EIC) consolidated economic power through the

annexation of cotton producing territories. Specifically, a cotton producing district was 40% more likely to be under direct British rule, relative to indirect rule. This allowed the British to directly control the supply of cotton, securing a monopoly on the supply of Indian goods and products (Sahoo (2015)). The direct control of cotton producing territories allowed the British to meet their objectives of protecting the interests of the British textile industry and increase Britain's share of global trade. Specifically, we argue that areas under direct colonial rule were subject to economic policies and practices that dismantled the well-established local economic organizations centered around the cotton industry. The destruction of flourishing and dominant economic industries resulted in significant economic losses, borne by the native population. Using precolonial cotton production as an instrument for direct British rule, we show that a firm reduces its project size by 20% in direct ruled areas relative to indirect ruled areas, within a pair of adjacent districts. We argue that the economic losses have endured to the present as the destruction of strong economic organizations (1) hampers the intergenerational transfer of skills and knowledge, and (2) disrupts the natural Marshallian process that develops over time and explains the agglomeration and dominance of industries.

Apart from the differences in the extent of economic exploitation, differences in the colonial occupation of India through direct and indirect rule altered the political incentives of the precolonial elites. While the British directly controlled the direct ruled areas in India, native rulers had considerable autonomy over the internal affairs of their dominion in indirect ruled areas. Iyer (2010) argues that the rulers in indirect ruled areas were under a constant threat of deposition by the British on account of misrule. Hence, local rulers exerted tremendous effort to improve state capacity, to avoid the slightest hint of misrule. Moreover, the rulers of indirect ruled districts had longer tenures which granted them incentives to plan and invest for long-term development. While the colonial geographic differences in de jure institutions were eliminated after the Indian Independence of 1947, we argue and verify that precolonial geographic differences in state capacity affect contemporary state capacity, especially the ability of the state to provide public goods in a timely fashion. We show this using data on delays in road construction. Our results show that road construction projects are delayed by 10.2% more in direct ruled districts relative to a contiguous indirect ruled districts. This result is different from Iyer (2010) as we focus on differences in the timely provision of public goods, rather than differences in the quantity of public goods. We do not

find any differences in the latter for our sample period.

Related Literature: This paper is primarily related to three strands of the literature: geographic concentration of economic activity, long-run effects of colonial rule, and the emergence and effects of state capacity.

First, we contribute to the literature examining the determinants of geographic concentration of economic activity by introducing a novel aspect that can explain the geographic concentration of investment: historical circumstances. The extant literature since [Marshall \(1920\)](#) has focused on the role of the cost and availability of the factors of production and geographic advantages as the primary determinant of emergence of clusters ([Ellison and Glaeser \(1997\)](#), [Duranton and Overman \(2005\)](#), [Ellison, Glaeser and Kerr \(2010\)](#)). We argue that the historical episodes of direct colonial rule can determine the concentration of investment over and above these traditional agglomerative forces. The results support the theoretical models of [Ethier \(1982\)](#), [Panagariya \(1986\)](#), [Arthur \(1986\)](#), and [Krugman \(1987\)](#), which highlight the role of history in the eventual choice of an economy's equilibrium. Our local identification approach, using firm \times district-pair \times year fixed effects, allows us to disentangle the importance of differences in historical circumstances from differences in traditional agglomerative forces of [Marshall \(1920\)](#), and, differences in geographic features. Moreover, our local identification approach allows us to distinguish between the history versus expectations effect discussed in [Krugman \(1991\)](#), as we compare adjacent districts that are likely to have same similar investment opportunities, hence, have similar expectations, but differ in their historical exposure to direct British rule.

Second, this paper is related to the literature examining the long-run effects of the presence of a large foreign enterprise. Specifically, we show that foreign enterprises with high market power can have negative long-run consequences on local investment. This occurs through the dismantling of pre-existing and competing economic organizations. This result is in contrast with prior work which shows that foreign enterprises with high market power can have positive long-run effects on local economic development in the presence of high labor mobility ([Méndez-Chacón and Van Patten \(2019\)](#)), or when they organize colonial economic activity to maximize their economic returns ([Dell and Olken \(2020\)](#)). Our primary result is consistent with other works ([Nunn \(2008\)](#), [Dell \(2010\)](#), [Bobonis and Morrow \(2014\)](#), [Lowes and Montero \(2020\)](#)), which document negative, persistent development effects of exploitative foreign enterprises. These papers focus on the coercion of labor

– the threat or use of force to compel workers to enter into an employment relationship – as the key driver of trust which explains persistent detrimental effects. In contrast, using the case of the cotton industry, we show that the long-run detrimental effects of foreign enterprises can originate from the dismantling of pre-existing economic organizations, which are likely to pose a threat to the monopoly power of foreign enterprises. We argue that the long-run detrimental effects of the destruction of well-established economic organizations originate from the destruction of stable human and financial capital stock.

Third, our work is related to the burgeoning literature examining the emergence of state capacity and its subsequent impact on economic development. Specifically, we show that in addition to the destruction of well-established economic organizations, differences in state capacity between direct and indirect ruled areas have long-run consequences. The combination of a longer tenure, as well as the threat of annexation, contributed to greater state capacity in indirect ruled areas during colonial rule, relative to their direct ruled counterparts. We show that these differences in colonial state capacity can explain present day differences in state capacity across direct and indirect ruled areas. We show that differences in corporate investment across direct and indirect ruled districts, in the present, can partly be explained by differences in state capacity that trace their roots to the colonial period. An efficient state has a greater ability to expand the productive capacity of an area by increasing resources and enhancing the productivity of existing resources through timely public capital investment. Hence, our work provides micro-evidence wherein current state capacity can be explained by historical state capacity. Hence, our work complements the literature that highlights the importance of elites in the development of state capacity via governance and politics ([Michalopoulos and Papaioannou \(2013\)](#)), conflict resolution ([Acemoglu et al. \(2020\)](#)), and land governance ([Banerjee and Iyer \(2005\)](#)).

Taken together, our two channels – destruction of well-established economic organizations and state capacity – supplement the longstanding literature, exploring the long-run effects of colonial rule by identifying the mechanism through which colonial regimes can affect contemporary economic outcomes. This adds to the several other mechanisms through which colonial regimes persist such as mistrust ([Nunn and Wantchekon \(2011\)](#)), economic and physical infrastructure ([Donaldson \(2018\)](#), [Dell and Olken \(2020\)](#), [Berger \(2009\)](#)), health and education infrastructure ([Huillery \(2009\)](#)), legal regimes and property protection ([Acemoglu, Johnson and Robinson \(2001\)](#)),

[Porta et al. \(1998\)](#), [Banerjee and Iyer \(2005\)](#))).

Our paper is closest to [Iyer \(2010\)](#), which examines the long-run consequences of direct colonial rule in India. However, we differ from [Iyer \(2010\)](#) on two accounts. First, we identify distinct sources of differences in state capacity. Second, we highlight a new channel of colonial destruction of well-established industries that can explain long-run consequences of direct rule. Using data on public goods provision across Indian districts from 1961 until 1991, [Iyer \(2010\)](#) documents differences in public goods provision across direct and indirect ruled districts. These differences diminish substantially over time, exhibiting convergence, consistent with policies of equalization followed by postcolonial governments ([Iyer \(2010\)](#), [Banerjee and Somanathan \(2007\)](#))). Hence, such differences in public goods provision are muted for our sample period – which we verify – and are unlikely to explain the persistent differences in investment concentration, even after more than two decades since 1991. Instead, we argue that differences in investment concentration are partly driven by differences in the speed with which the state can provide public goods. This is an important distinction because the delays in the provision of such public goods can reflect fundamental faults in state efficiency that can hamper utilization of time-sensitive investment opportunities.

This paper is organized as follows. Section 2 provides a short description of the historical setting. Section 3 describes the datasets that are utilized in this project. Section 4 presents our key results. Section 5 presents the robustness results. Section 6 presents the evidence from bordering districts. Section 7 presents the results from the instrumental variable regressions. Section 8 discusses the mechanism and section 9 concludes.

2 Historical Setting

In this section, we provide a short description of the setting for the paper. We chronicle British annexation of India and expound on the process of colonialism in Appendix A. The Indian subcontinent was initially governed by local rulers. During the colonial era, districts came either under indirect rule or direct rule. We argue that direct and indirect ruled districts differ on two dimensions. First, the direct ruled districts faced greater economic exploitation. Second, districts that fell under indirect rule, where precolonial rulers retained administrative powers, had greater state capacity, while districts that fell under direct rule of the British had lower state capacity ([Iyer \(2010\)](#))).

2.1 An Overview of India under British Rule (1757-1947)

British colonial rule over the Indian subcontinent began in 1757 and lasted until 1947. After the decline of the Mughals and the Maratha Empire, India was divided into several small states ruled by royal families, referred to as *princely states*. The British Empire consolidated its grip on political control across present-day India, Bangladesh, Burma, and Pakistan in “British India” under three broad policies of annexation, namely, the Subsidiary Alliance, the Doctrine of Lapse, and Annexation under Misrule. With these policies, certain areas that had been annexed by the British were integrated under direct rule, while remaining areas, were granted indirect rule, in which authority was delegated to local princes (princely states). Figure 1 presents the geographic distribution of direct and indirect ruled areas.

The three waves of annexation are characterized by three different policies. Under Subsidiary Alliance, the East India Company entered contracts with princely states, providing the latter with the subsidiary militia for protection and necessary defense for payment. In the event of default by the Indian ruler, a part of their territory was surrendered to the British. The second wave of British annexation, *subordinate isolation*, began in 1818, lasting until the Indian Mutiny of 1857. This phase was marked by the policy of Doctrine of Lapse between 1848 and 1856. Under the Doctrine of Lapse, any subordinate princely state would automatically lapse to the East India Company if the ruler died without a natural heir. Power was transferred from the East India Company to the British Crown after the Sepoy Mutiny of 1857 putting a formal end to active British annexation of India.¹ Despite the Queen’s proclamation of 1858 which ruled out any future annexation after the Sepoy Mutiny, the Governor-General did retain the power to interfere in the internal matters of the princely states in case of a “misrule.” Ashton (1977) documents active interference by the British colonial power in the internal affairs of princely states, e.g., Lord Curzon, the Governor-General of India from 1899 to 1905 forced fifteen rulers to abdicate, during his tenure on account of misrule.

Princely states were heterogeneous – some consisted of a few villages, while other kingdoms presided over thousands of square miles. Princely states had varying degrees of legal autonomy, ranging from first-class, wherein the state could try criminal cases to third-class states whereby only small civil cases could be adjudicated by the ruler. Iyer (2010) notes that the princely states

¹This is referred as the Government of India Act 1858 – an Act of the British Parliament, which nationalized the East India Company and granted the British monarch supreme authority over India, as well as power and possessions of the East India Company.

constituted approximately 45% of the total geographic area of present day India, and 23% of total population in 1911. Additionally, she notes the presence of princely states across India with a high concentration in central and western India. The Foreign Office recognized about 680 Indian princely states in the year 1910 (Iyer (2010)).

After Indian Independence in 1947, areas under direct British rule federated. The princely states, however, could choose to join the Indian Union or remain independent. Prime Minister Jawaharlal Nehru and the Home Minister Sardar Vallabhbhai Patel integrated all princely states into the Indian Union by 1950. Thereafter, princely rulers no longer maintained sovereignty, and the indirect ruled districts faced similar political and legal structures as the direct ruled districts within modern-day states.

2.2 Colonial Differences

The differences between direct and indirect British rule originate from several sources, both political and economic.

Politically, rulers of princely states were under constant threat of being annexed in case of a misrule. *Annexation on Misrule* incentivized princely states to provide better governance and institutions. Princely states had between four to five rulers during 1858-1947, whereas states under direct British rule were governed by 24 Governor-Generals during the same period (Iyer (2010)). The longer tenure of native rulers granted them greater incentives to plan and invest for long-term development. Hence, the combination of a longer tenure, as well as the threat of annexation contributed to development of better early institutions and governance policy in indirect ruled areas, resulting in greater state efficiency.

Economically, areas under direct colonial rule faced the brunt of protectionist policies, restrictive contracts, and exploitative practices, causing the native population to experience significant economic losses. The native population in areas under direct rule were also more likely to face physical abuse, affecting their marginal productivity of labor. Additionally, the British neither encouraged the industrialization of indigenous industries in India, nor developed institutions to promote them. These factors are discussed in the context of cotton production in section 8.1, although the mechanism applies in other contexts as well.²

²The EIC became the “de facto ruler of Bengal, where it established a monopoly in grain trading and prohibited local traders and

2.3 What Determines Direct Rule?

In this section, we explore if direct rule can be explained by a multitude of factors, such as geographic features, distance to state capital, religion of the precolonial ruler, and population distribution during colonial times. Table 1 presents results from a linear probability model, regressing the likelihood of an area being under direct rule on these characteristics. Table 1 indicates that the majority of these characteristics cannot predict direct rule except Muslim ruler. The likelihood of a territory being under direct rule increases if the area was ruled by a Muslim ruler. With the exception of the religion of the precolonial ruler, the direct and indirect ruled areas seem evenly balanced on these observable characteristics.

3 Data

This section describes the datasets that are utilized in the paper. We describe the sources of the datasets and our sample.

3.1 Project Announcement Data

We obtain data on project announcements from the CapEx database maintained by the Centre for Monitoring Indian Economy (CMIE). CMIE, a leading business information company in India, was established in 1976 and has been used extensively for India-based academic research. More recently this data has been employed in [Alok and Ayyagari \(2020\)](#) to study the association between the electoral cycle and the corporate investment cycle. The CapEx database serves as the source of annual report – Private Investment Growth and Prospects published by the Indian central bank – the Reserve Bank of India. All projects announced by private and public firms and government entities that cost more than ₹ 10 mn (\approx \$0.2 mn) are recorded in the database. CMIE collects project announcement information from multiple sources including the annual reports of firms, media reports, and government agencies. Although CapEx has sporadic coverage of historical project announcements, the data is systematically available from 1995. The dataset provides information on the firm announcing the project, location (district, and state) of the project, size of the project, and time of project announcement. This dataset also allows tracking of the project outcomes.

dealers from ‘hoarding’ rice” ([Zingales \(2017\)](#)). The combination of high taxation and monopolistic power contributed to the Great Bengal Famine, during which one out of three Bengalis died of starvation.

The data employed in this paper covers project announcements across all states for the universe of all firms in India spanning between fiscal year 1996 and 2018.³ However, we include 17 states in our sample because the empirical strategy in the paper exploits a within state estimation. Hence, states with at least one direct and indirect ruled district are included in the sample.⁴ Figure 2 plots the geographic distribution of the total amount (in ₹ billion) and number of projects announced by all firms. Project announcements, in both number and amount, exhibit a great degree of geographic dispersion within 427 districts in the sample. Appendix B.1 presents an overview of the sample in terms of its distribution over time, amount, and project type.

3.2 Historical Geography of British rule

The historical data on the boundaries of British rule, precolonial states, dates of annexation, mode of annexation by the British and details of the death of kings in precolonial states comes from [Iyer \(2010\)](#). We verify the historical data through primary data sources including [Baden-Powell \(1892\)](#), [Chakrabarti \(1896\)](#), [Hunter and Bartholomew \(1908\)](#), and [Lee-Warner \(1910\)](#). [Iyer \(2010\)](#) provides a crosswalk between precolonial boundaries and districts of India as per the 1961 Census. We manually match the 1961 Indian districts to the districts of India in 2014. The detailed list of 2014 Indian districts is obtained from the Environmental Systems Research Institute (ESRI), Inc. The 1961 districts are matched to the 2014 districts using the mapping discussed in [Kumar and Somanathan \(2009\)](#), and current district websites. District websites often include a webpage that details the history of a district, and accounts for administrative splits and name changes. If the district website does not have a history page, we refer to the Wikipedia page and verify information about splits or name changes through government notices. If a 2014 district matches with more than one 1961 district, we restrict the match to the 2014 district that reports the greatest geographic area. The 2014 district names are used to identify the districts where projects are announced in the CapEx database. Lastly, we collect data on precolonial cotton production, religion of the ruler, and 1911 census demographics at the district-level from [Lee \(2017\)](#).

Figure 1 shows the colonial boundaries of the areas under direct and indirect rule with

³A fiscal year in India starts on 1st of April and ends on 31st of March in the next calendar year. As an example, fiscal year 2018 starts on 04/01/2017 (mm/dd/yyyy) and ends on 03/31/2018. Fiscal year is a reasonable unit of time as private and government enterprises manage their annual budgets within a fiscal cycle.

⁴The states included in the sample are Andhra Pradesh, Chhattisgarh, Gujarat, Haryana, Himachal Pradesh, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Odisha, Punjab, Rajasthan, Tamil Nadu, Telangana, Uttar Pradesh, Uttaranchal, and West Bengal. Telangana and Andhra Pradesh were one state until 2014.

the 2014 administrative state boundaries. *Direct ruled areas* consist of regions under the direct administrative rule of the British until the Indian independence in 1947. *Indirect ruled areas* are the native states under the administrative control of Indian princes, with indirect control by the British. Indirect ruled areas were present in all parts of British India, with a relatively higher concentration in the western and the central regions. Table A.1 shows the distribution of British acquisition of direct ruled districts over time and by reason.⁵ The majority of districts were acquired during the ring fence period primarily by active conquest. 38% of districts that were acquired during subordinate isolation were through the Doctrine of Lapse, wherein a territory came under direct British rule if the ruler of the territory died without a biological natural heir. Districts that were acquired under misrule, comprise 7% of the sample of districts under direct rule.

Additionally, we obtain data on road constructions from the *Pradhan Mantri Gram Sadak Yojana* (PMGSY) and the Census through the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG) (Asher and Novosad (2020a)).⁶ SHRUG reports the data on earliest completed roads for each village or town. The data provides comprehensive information on contract dates, projected completion dates, actual completion dates, road lengths, and construction costs. Our focus of the data is on construction delays measured as the difference in the natural logarithm of actual time taken to complete a road and the natural logarithm of estimated time to complete the road. At the village/town level, the median and the mean values of construction delays are 0.45 and 0.55, respectively. This measure exhibits large heterogeneity with a standard deviation of 0.71.

4 Results

This section describes the key results, and the associated empirical methodology. Our results show that investment is concentrated within states and this geographic concentration of investment can be explained by the proportion of districts under direct British rule.

4.1 Aggregate Analysis

We begin our empirical analysis by examining the concentration of corporate investment. We choose the state as the unit of analysis as the law is fixed at the state level.

⁵*District* refers to the modern day districts as of 2014, hereafter.

⁶We direct the readers to Asher et al. (2021) for a comprehensive description of the SHRUG dataset.

4.1.1 Geographic Concentration of Investment

We begin our empirical analysis by examining the geographic concentration of corporate investment. The HHI measures the geographic concentration of investment. Using project level announcement data, we identify the location and size of each project. We then compute the state-level investment Herfindahl-Hirschman Index (HHI) as a measure of within state investment concentration. It is computed by summing the squared share of investment in a district, relative to the state in a given year. We use state as the unit of analysis, as the legal framework under which investment occurs is fixed within a state. Figure 3 plots the time-series of aggregate investment HHI between 1996 and 2018. The weighted HHI, weighted by the share of investment in the state relative to the country, shows a stable trend throughout the period with an average (median) value of 0.266 (0.255). The level of geographic concentration increases in the HHI.

While the HHI measures the geographic concentration of investment, it does not imply concentration on its own. Therefore, we compare the observed HHIs to three benchmarks: number HHI (# HHI), aggregate area HHI (Area HHI), and population HHI (Pop HHI). First, number HHI is computed as the inverse of number of districts in the state - $\frac{1}{N}$, where N denotes the number of districts in the state. This measure assumes equal investment across all districts. Second, state-level area HHI is computed as the squared sum of the share of geographic area of districts within a state. The aggregate area HHI is computed by weighing each state-level area HHI with the area of the state relative to the country. This measure assumes that the investment in each district, relative to the state, is proportional to the area of the district, relative to the state. Third, state-level population HHI is computed as the squared sum of the share of 2001 district-level population within a state. The aggregate population HHI is computed by weighing each state-level population HHI with the population of the state relative to the country. This measure assumes that the investment in each district, relative to the state, is proportional to the population of the district, relative to the state. These benchmarks act as a baseline for the geographic concentration of investment in a frictionless spatial equilibrium. The geographic concentration of investment implied by the observed HHI is 20 percentage-point higher than either measure of the benchmark. Hence, the comparison of the observed HHI with either of the three benchmarks indicate that investment is spatially concentrated within states.

4.1.2 What explains the geographic concentration in investment?

This section documents the relationship between the geographic concentration of investment and direct British rule. Figure 4 presents a scatterplot of state-level geographic concentration of investment and the percent of districts under direct British rule in that state. State-level geographic concentration of investment is measured using the average state-level investment HHI between 1996 and 2018. The plot provides prima-facie evidence that the geographic concentration of investment increases with in the proportion of districts under direct British rule.

Table 2 extends the analysis presented in Figure 4 in a regression framework after controlling for other state-level covariates. Columns (1) to (6) present the estimate of the regression of state-level average HHI on the percent of districts under direct British rule, while sequentially adding state-level covariates. Across all columns the estimate of interest, the coefficient associated with % Direct Rule, is negative and statistically significant at the 10% level. While the statistical significance of the estimate is low, given the number of observations, the addition of other state-level covariates does not change the estimate or the precision of the estimate, despite the model R^2 increasing from 13% to 60%. This indicates that despite the limited statistical power of the regression, the relationship between state-level geographic concentration of investment and the percent of districts under direct British rule is likely to be robust. Economically the estimate indicates that a one standard deviation increase in the percent of districts under direct British rule, corresponding to a 28 percent-point increase, is associated with a 0.12-0.15 standard deviation increase in investment concentration, relative to the mean. This corresponds to a 8.5%-10.7% increase in investment concentration. Moreover, our key explanatory variable, percent of districts under direct British rule, can explain 13% of total variation in investment concentration. Overall, our results indicate that percent of districts under direct British rule can explain investment concentration, and this relationship is robust and economically meaningful.

4.2 Baseline Analysis

Thus far, we have established a correlation between geographic concentration in corporate investment and percent of direct ruled districts. In this section, we provide a systematic analysis of this relationship using micro-level data from project announcements, showing that smaller projects are announced in districts that were historically under direct British rule. The key advantage of

using granular data is to rule out potential biases that may create a wedge between the true and the observed relationship between investment and direct British rule.

4.2.1 Empirical Strategy

The objective of this section is to systematically analyze the relationship between project size and the historical origin of a district. We run a regression of the natural logarithm of the project size on a binary variable that takes a value of 1 if the district where the project is announced was historically under direct British rule, and 0 otherwise. Equation 1 displays the baseline specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct rule}_j + \theta_{i,y} + \theta_{s(j \in s),y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t} \quad (1)$$

$\ln(Y_{i,j,t})$ denotes the natural logarithm of project size announced by firm i in district j , where district j is in state s , at time t in fiscal year y . Direct rule_j is a binary variable taking a value of 1 if the district where the project is announced was historically under direct British rule. $\theta_{i,y}$ denotes firm \times year fixed effects controlling for all time-varying observed and unobserved heterogeneity at the firm level. $\theta_{s(j \in s),y}$ denote state \times year fixed effects controlling for all time-varying observed and unobserved heterogeneity at the state level. Specifically, $\theta_{s(j \in s),y}$ controls for the law at the state level. Additionally, θ_t denotes time fixed effects accounting for aggregate shocks and temporal trends in corporate investment. The primary source of heterogeneity in equation 1 relates to precolonial boundaries. Hence, we estimate the standard errors by two-way clustering at the precolonial boundaries and time level.⁷ Additionally, we report Conley (1999) standard errors to ensure that our inference is not contaminated by spatial noise. We also control for the latitude and the longitude of the district to capture spatial trends (Kelly (2019)), spatial correlation in business cycles coming from the relation between distance to equator and economic performance (Acemoglu, Johnson and Robinson (2001)), and social infrastructure (Hall and Jones (1999)).

We interpret β in equation 1 as a within state estimator, estimating the difference in investment size of projects between direct and indirect ruled districts within a state, while controlling for firm-specific time-varying policy. Alternatively, β may be interpreted as a within firm estimator, controlling for time-varying changes in the legal framework within a state. Under the second interpretation, the estimate gives the log difference in the project size announced by a firm in a

⁷The total number of clusters with precolonial boundaries are 95, and 92 with time in the sample.

direct ruled district relative to an indirect ruled district. The β estimate may be interpreted as causal under the assumption of randomness of Direct Rule $_j$. We discuss this assumption in detail in section 7.

4.2.2 Baseline Results

Table 3 reports the regression results from the estimation of equation 1. The coefficient of interest is the estimate β associated with Direct Rule $_j$. Columns (1) to (5) sequentially add fixed effects to estimate equation 1, as reported in column (5). The estimate of β is negative and statistically significant across all columns. The magnitude of the estimate in column (5) indicates that projects announced in direct ruled districts are 8.8% smaller in size than projects announced in indirect ruled districts by the same firm, while controlling for the legal framework. The estimate indicates that for every project worth ₹ 100 million in an indirect ruled district, the size of the project in a direct ruled district is ₹ 91.19 million. This amounts to an average within-state difference of ₹ 8.81 million per project across districts.⁸ Given the median project size of ₹ 1000 million, this corresponds to a within-state difference of ₹ 88.1 million (\approx \$1.2 million) per project on annual basis, corresponding to a difference of \$120 million per 100 projects. We further discuss and elaborate on the economic significance of the estimates in section 4.2.3.

Moreover, we validate the precision of the estimates through the Conley (1999) standard errors, adjusting for spatial dependence within 100 km, reported in square brackets. Overall, the results indicate the estimate is negative and statistically significant. The negative estimate of β indicates that districts that were historically under direct British rule exhibit lower investment today relative to districts under indirect rule.

4.2.3 Extensive and Intensive Margin

This section presents the results on the relation between investment and direct British rule, using district as the unit of analysis. The motivation to conduct the analysis at the aggregate level is threefold. First, aggregating data at the district-time level allows us to create a balanced district-time panel. This facilitates the quantification of the spatial differences in corporate investment via the extensive and the intensive margins. Second, this allows benchmarking the magnitude of the estimate, making it easier to interpret the estimates. Third, the key level of heterogeneity we exploit

⁸The effect is calculated using $\mathbb{E}[\ln(Y_{i,1,t})|\text{Direct Rule}_j = 1, j \in s] - \mathbb{E}[\ln(Y_{i,0,t})|\text{Direct Rule}_j = 0, j \in s] = \beta$

is at the district level whereas the key dependent variable is at the firm \times district level. Moulton (1990) argues that a regression estimating the effect of an aggregate variable on a micro unit can lead to standard errors that are downward biased resulting in type I error.

Table 4 reports the results on the relation between corporate investment and direct British rule at the district-level. The dependent variable in column (1) takes a value of 1 if a project is announced in that district-year and 0 otherwise. Column (2) uses total investment in district j as the dependent variable. Column (3) uses total investment in district j , conditional on positive investment as the dependent variable. Column (4) uses the total number of projects announced in district j . Column (5) uses the total number of projects announced, conditional on announcement in district j . Column (6) uses the share of investment in district j relative to investment across all districts in the state. Column (7) uses the share of the number of projects relative to the total number of projects announced in the state as the dependent variable. We control for state \times time fixed effects in all columns to account for observed and unobserved state-level time-varying heterogeneity. We interpret the estimate of direct rule (=1), as a within state estimator.

Across all specifications, the coefficient of direct British rule is negative and statistically significant in both the extensive and the intensive margins of corporate investment. Using a linear probability model, in column (1), we find that projects are 25% less likely to be announced in direct ruled districts relative to indirect ruled districts within a state. Column (2) shows that total investment is ₹16 billion lower in direct ruled districts relative to indirect ruled districts within a state. On the intensive margin, we find that conditional on announcement, project size is lower by ₹28 billion in direct ruled districts relative to indirect ruled districts within a state, as shown in column (3).⁹ In addition, on average, for five projects announced in indirect ruled districts, only one project is announced in direct ruled districts within the state, shown in column (4). Furthermore, conditional on announcement, the ratio of projects announced in indirect to direct ruled districts is 7:1, shown in column (5). Lastly, we find the share of investment and the share of number of projects are 7% lower in direct ruled districts compared to indirect ruled districts within a state, shown in columns (6) and (7). This estimate is economically large; on average, a district accounts for \approx 9% of the states' total investment and project count. Overall, the results indicate that the long-run

⁹The results are robust to using the natural logarithm of one plus total investment as the dependent variable for columns 2 and 3 as shown in Table C.1. We prefer the reported regressions over using logged values for ease of the interpretation of the estimate.

effect of direct British rule is negative, statistically significant, and economically large. Moreover, the effect shows up in both the extensive and the intensive margins of corporate investment.

5 Robustness

5.1 Omitted Variable Bias

While the estimate of β , reported in Table 3, is negative and statistically significant across all columns, the magnitude of the estimate decreases in comparing across columns, suggesting omitted variable bias may be a concern (Altonji, Elder and Taber (2005); Oster (2019)). From column (1) to column (5), the magnitude of the point estimate of β decreases from -0.17 to -0.09 with a simultaneous increase in the model R^2 from 3% in column (1) to 72% in column (5). Instability in the estimate may suggest the presence of omitted variable bias. We address this concern by conducting an Oster (2019) test. The Oster (2019) identified set based on the change in β and the model R^2 between column (1) and column (5) is $(-0.0893, -0.0549)$, which safely excludes 0. Furthermore, we do a step-wise Oster (2019) test. The identified sets on moving from column (1) to column (2), column (2) to column (3), and column (3) to column (4) are $(-0.113, -0.048)$, $(-0.133, -0.115)$, and $(-0.086, -0.036)$, respectively. All of the sets safely exclude 0. These results suggest that omitted variables are unlikely to drive the relationship between project size and historical origins of a district, under the assumption Oster (2019).

5.2 Controlling for Other Geographic Covariates

This section examines the robustness of the baseline estimate to other time-invariant district level covariates. The British were likely to take over strategic territories. This includes territories along the coast, territories with greater geographic area, or territories that have favorable climatic conditions. To control for this, we include geographic covariates, which may be correlated with the likelihood of a district being under direct rule.¹⁰ In addition to geographic covariates, we control for the distance of the district from the state capital, as a state's capacity to provide necessary infrastructure for investment may be restricted by the physical distance between administrators and citizens (Bardhan (2002); Asher, Nagpal and Novosad (2018)).

¹⁰Appendix Table C.2 presents the baseline results with the inclusion of covariates such as the historical ruling empire, religion of the precolonial ruler, and population demographics during colonial times. Our results are robust to the inclusion of these covariates, especially the religion of the ruler – a variable that was significant in Table 1.

Table 5 reports the estimation results of the baseline equation 1 after controlling for geographic covariates. The estimate of interest is the coefficient of Direct Rule_{*j*}, β . The estimate of β is negative, statistically significant at the 5% level and remains relatively stable in magnitude across columns (1)-(9). The magnitude of β in column (9) is quantitatively similar to the magnitude of β in column (5) of Table 3. The vast majority of geographic covariates cannot explain the within state heterogeneity in corporate investment. However, altitude, rainfall and distance to state capital are statistically significant. Higher altitude, rainfall, and greater distance to state capital are associated with lower investment. These results are consistent with the notion that unfavorable geographic conditions and distance from state headquarters may deter corporate investment. Overall, the results indicate that the estimate of β is not driven by other geographic characteristics that could potentially be correlated with the direct rule variable.

5.3 Placebo Test

We conduct a placebo test wherein we randomize whether a district was under direct British rule or not. This test validates that the results are neither spurious nor driven by spatial autocorrelation of investment or other spatial noise (Kelly (2019)).

We randomly assign a district to be under direct British rule or indirect British rule, irrespective of the colonial history of the district. Each district has a probability of 0.6370 to be assigned as directly ruled district. This probability level is chosen based on the observed empirical probability of being a direct ruled district in the data. The random assignment of districts ignores the spatial autocorrelation in the likelihood of a district being under direct or indirect British rule but retains the spatial autocorrelation in investment. Ignoring the spatial autocorrelation in the likelihood of direct rule is of little concern as the Moran I for direct rule is small indicating that the direct rule variable is relatively randomly distributed in the space.¹¹

We estimate the baseline specification, equation 1, using the random assignment of districts into direct and indirect ruled districts as the key explanatory variable. We refer to this as placebo direct rule. We estimate the coefficient associated with the placebo direct rule and repeat this exercise 10,000 times. To negate the validity of the baseline results, the null hypothesis that the point estimate associated with placebo direct rule is zero, must be rejected.

¹¹Section D discusses the calculation and the magnitude of spatial autocorrelation of direct British rule.

Figure 5 presents a visual assessment of the kernel density of β , coefficient associated with placebo direct rule, estimated using 10,000 simulations. The distribution of β is centered around 0, varying from -0.106 to 0.087 with a standard deviation of 0.026. We fail to reject the null hypothesis – the average point estimate from the placebo analysis is equal to zero. The dashed, red line denotes the location of the coefficient of the interaction term from column 5 of Table 3. 0.1% of estimates, among the 10,000 simulated placebo β , lie to the left of the dashed red line. The results of the placebo test corroborate the argument that the baseline results are neither spurious nor driven by spatial noise or spatial autocorrelation in investment.

6 Evidence from Bordering Districts

The traditional approach used to compare the differences in investment level across direct and indirect ruled district implicitly assumes that one district in India is as good a control as any other district within a state. Such a comparison can bias our inference in the presence of systematic differences between direct and indirect ruled districts. This section addresses this concern by focusing on contiguous districts that are separated only by administrative borders in cases where one district was under direct British rule and another under indirect rule within a state. This sample of contiguous districts is shown in Figure 6a. The identifying assumption of using this subsample is that these districts are immediately adjacent and are expected to follow similar paths had India not been colonized. Moreover, whether a district within a contiguous direct-indirect ruled pair was under direct British rule or not is likely to be a matter of chance. Hence, the indirect ruled districts provide a valid counterfactual to the adjacent direct ruled districts. More importantly focusing on this subsample of data allows us to implicitly control for the costs of moving goods, people, and ideas and geography – the four important determinants of investment choice (Marshall (1920), Ellison and Glaeser (1997), Ellison, Glaeser and Kerr (2010)) and investment opportunities, as these parameters are likely to be similar across contiguous districts.

6.1 Empirical Strategy

We combine the sample of contiguous direct and indirect ruled districts with a simple OLS regression framework to compare the difference in the size of investment projects across the

two districts. We estimate the following regression specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,p(j \in p),y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t} \quad (2)$$

where, $\ln(Y_{i,j,t})$ denotes the natural logarithm of the size of the project announced by firm i in district j . Direct Rule_j is a binary variable taking a value of 1 if the district where the project is announced was historically under direct British rule, and 0 otherwise. As in the baseline specification 1, we also control for the latitude and longitude of the district. $\theta_{i,p(j \in p),y}$ denotes district-pair \times firm \times year fixed effects. District-pair refers to the adjoining pair of a indirect ruled district and a direct ruled district, within a state. This definition of district-pair can result in the presence of a single district in multiple pairs inducing a mechanical correlation across district-pairs. We account for multiple sources of correlations in the error term by multi-way clustering at the precolonial state (64 clusters), district-pair (174 clusters) and time (92 clusters) levels. The estimate of β is identified through within district-pair variation of the same firm, i.e., we effectively compare the size of investment projects announced by a firm in a direct ruled district with the size of investment projects announced by the same firm in bordering indirect ruled districts. This estimate pools all local comparisons and allows for spatial autocorrelation, addressing the dual problems of omitted variable bias and bias in the estimated standard errors.

6.2 Results

Table 6 reports the regression results from the estimation of equation 2 using the subsample of contiguous direct and indirect ruled districts. The coefficient of interest is the estimate of β associated with Direct Rule_j . Columns (1) to (3) sequentially add fixed effects to estimate equation 2, as reported in column (3). The estimate of β across all columns is negative and statistically significant. The point estimate in column (3) is marginally greater in magnitude relative to the baseline estimate reported in column (5) of Table 3. This indicates that any systematic differences across districts that are likely to affect the estimate of β in the traditional approach employed in equation 1 are likely to understate the true effect. The magnitude of the estimate in column (3) indicates that projects announced in direct ruled districts are 10.8% smaller in size than projects announced in indirect ruled districts by the same firm within a district-pair.

An important assumption of this local identification approach is that contiguous districts are likely to be similar to each other. While a simple balance test on some observed characteristics can provide reasonable confidence (see, Table 1), it is likely to be insufficient as it cannot rule out differences across the entire set of observed and, importantly, unobserved characteristics. We verify this assumption using a different approach that accounts for all observed and unobserved characteristics. We compare a new set of indirect ruled districts that are directly adjacent to the sample of indirect ruled districts, shown in Figure 6a. We refer to this new set of indirect ruled districts as hinterland indirect districts as these districts are separated from the sample of direct ruled districts in Figure 6a by a strip of indirect ruled districts. The rationale of this test is that if our original test comparing contiguous direct and indirect ruled districts is plagued by systematic differences across contiguous districts, then a comparison of the set of contiguous indirect ruled districts should also generate an effect of similar size. Figure 6b presents the sample. The indirect ruled districts, referred as border indirect districts, used in Figure 6a are marked in blue and the contiguous indirect ruled districts, the hinterland indirect districts, are marked in gray. Table 7 reports the estimation results of comparing the size of investment projects within the sample of contiguous indirect ruled districts. The estimate of *Hinterland*(= 1) is economically small and statistically insignificant. This indicates that the results reported in Table 6 are unlikely to be driven by systematic differences across contiguous districts.

6.2.1 Project Abandonment

The ideal empirical design to test for lower investment in direct ruled districts should indicate whether a firm forgoes positive net present value (NPV) projects in direct ruled areas. However, the data for such an analysis is unavailable. Instead, we focus on the status of a project after announcement. Announced projects are positive NPV projects. The abandonment of these projects can reflect that a firm forgoes a positive NPV opportunities. This section examines the relationship between direct rule and the status of announced projects. Specifically, we compare the likelihood that a project is abandoned in direct ruled districts relative to indirect ruled districts.

We examine the differences in project post-announcement status by comparing the outcomes of a cohort of projects announced in a year. Specifically, we analyze the abandonment status within one, two, three, four, and five years of an announcement. This cohort analysis allows us to

circumvent econometric issues related to the right-censoring bias. Another concern with a direct comparison of abandonment status of announced projects in direct and indirect ruled districts is that the ex-post project abandonment could reflect the effect of subsequent geographic or firm specific events rather than capturing firms' tendency to forgo ex-ante positive NPV opportunities. We address this concern through our local identification approach, by including firm \times district-pair \times year fixed effects. The identifying assumption is that a firm is likely to have identical exposure to shocks within a pair of adjacent districts.

Table 8 reports the results from the linear probability model, combining the cohort analysis with the local identification approach. The table examines the probability that a given firm abandons a project in a direct ruled district relative to a contiguous indirect ruled district for five years after announcement. Columns (1)-(5) compare the likelihood of project abandonment within one, two, three, four, and five years of announcement, respectively. Our results show that the same firm is 2.5 percentage-points more likely to abandon its projects in a direct ruled district relative to a contiguous indirect ruled district. This effect is economically significant, given that the unconditional probability of project abandonment is 13%. These results indicate that firms are more likely to forgo economically sound investment opportunities in direct ruled districts.

7 Instrumental Variable Regression

This section reports and discusses the results from an instrumental variable (IV) strategy to address the selection issue. Thus far, we have compared the corporate investment between districts that are under direct and indirect rule within a state. A concern with such a comparison is the problem of selection which hinders interpreting the estimate of β in equation 1 as causal. While the low value of the Moran I statistic implies spatial randomness under a parametric setup, it does not completely rule out selection. We address the issue of selection by exploiting a unique feature of British annexation policy in India – the Doctrine of Lapse.

We use the British policy of the Doctrine of Lapse as an instrument for direct British rule. Under the Doctrine of Lapse, the governor-general of British India annexed the Indian princely states where the ruler died without a natural heir. This policy was in effect between 1848 and 1856. The Doctrine of Lapse allows us to exploit the death of a ruler without a natural heir, during 1848 to 1856, as an instrument for a territory coming under the direct British rule.

The instrument relies on two crucial assumptions: relevance and exclusion. The relevance restriction posits that the death of a ruler without a natural heir can explain the likelihood of a territory coming under direct British rule. The relevance condition is a mechanical byproduct of the policy, which we verify in the first stage.¹² The exclusion restriction posits that the death of a ruler without a natural heir affects current corporate investment only through the territory being under direct British rule. The identifying assumption required for the exclusion restriction to hold is that the death of the ruler without a natural heir during the period of 1848 to 1856 is likely a matter of chance and unlikely to have a direct impact on corporate investment in the present. We address issues related to the exclusion restriction in section 7.1.

Table 9 reports the results from the 2SLS regression and compares the estimate with the IV and the OLS estimate. The sample is restricted to all indirect ruled districts and districts under direct rule that were annexed after 1847. We restrict the direct ruled districts sample, because we are unable to construct the instrument for periods before 1847 as the policy was not applicable during other periods. Column (1) reports the OLS estimate for the sample under consideration. The estimate of β is negative and statistically significant. Column (2) presents the IV specification regressing the natural logarithm of project size on the instrument. The IV estimate is negative and statistically significant. Columns (3) and (4) report the second stage and the first stage of the 2SLS estimation strategy, respectively. The instrument is positively related to the likelihood that a district fell under direct British rule. A district is 66% more likely to be under direct British rule if its ruler died without a natural heir between 1848 and 1856. The f-statistic of the first stage is 28.94 implying that the instrument is not a weak instrument and unlikely to overestimate the effect in the second stage. The second stage estimate of direct rule is negative and statistically significant at the 5% level. The magnitude of the 2SLS estimate in column (3) is comparable to the size of the OLS estimate in column (1), indicating that selection is likely to be of little concern. The closeness in the magnitudes of the OLS and the 2SLS estimates is consistent with very small values of the Moran I statistic, indicating that direct and indirect ruled districts are randomly distributed in space. Overall, this analysis indicates that the results discussed so far are unlikely to be driven by selection

¹²Lord Dalhousie was the governor-general of India between 1848-56. During his reign, the British gained direct control of seven native states wherein four were occupied by exercising the Doctrine of Lapse and three were annexed due to other reasons. In total, rulers of eight native states died during this period without a natural heir. This accounts for a total of 48 current districts being brought under direct British rule with 54.17% of districts annexed under the policy of Doctrine of Lapse.

bias.

7.1 Discussion on the Exclusion Restriction

This section discusses the validity of the exclusion restriction of the instrument. The exclusion restriction states that the death of a ruler without a natural heir affects current corporate investment only through the territory being under direct British rule. The identifying assumption required for the exclusion restriction to hold is that the death of the ruler without a natural heir between 1848 and 1856 is likely a matter of chance, and does not directly impact present-day investment.

The identification strategy is invalid if the Doctrine of Lapse was strategically implemented to acquire certain areas, or rulers could choose to opt out of this policy. The Doctrine of Lapse did not allow the British to take over their most desired states of Oudh and Hyderabad ([Rahim \(1963\)](#)). The British could resort to other modes of annexation to acquire their most desired states. Indeed, the British acquired Oudh and Hyderabad through other means. Another threat to identification is the possibility that the British were responsible for the deaths of the rulers whose kingdoms they wanted to acquire. [Iyer \(2010\)](#) reviews historical records and argues that neither case is likely. Moreover, the policy of lapse was completely unexpected, ruling out any self-selection into or out of the policy ([Iyer \(2010\)](#)). Additionally, all states were administered identically under direct British rule, irrespective of the mode of annexation.

Exclusion requires that the death of a ruler without a natural heir affect current investment by only influencing the likelihood of direct rule during colonial times. We consider several threats to the validity of the exclusion restriction. The death of a ruler without a natural heir may be attributable to several other factors, e.g., environment, genetics, etc., which may affect postcolonial corporate investment through other channels. Alternatively, the death of a ruler without a natural heir may usher in a period of political turmoil, resulting in adverse long-run consequences captured by our estimation. We address these two claims via a falsification test. We directly regress the natural logarithm of project size on a binary variable that takes a value of 1 if the ruler died without a natural heir in the periods before 1858 and after 1858, in a sample of indirect ruled districts. The Doctrine of Lapse was not applicable in these periods and indirect districts were not subject to direct British rule. Hence, if the death of a ruler without a natural heir affects present day outcomes directly through channels other than the direct rule, we should find significant, comparable results

in this sample. Column 5 of Table 9 reports the results. The coefficient of interest is close to zero and statistically insignificant. This indicates that the death of a ruler without a natural heir is unlikely to directly affect long-term outcomes.

Another concern with regard to our exclusion assumption is that the death of the ruler between 1847 and 1858, regardless of presence of natural heir, may reflect geography-specific conditions during the period that can affect long-term outcomes through channels other than direct rule. We test this proposition by directly regressing the natural logarithm of project size on a binary variable that takes a value of 1 if the ruler died *with* a natural heir in the periods after 1847 and before 1858, in a sample of indirect ruled districts. As before, these territories were not under direct rule, hence, we should find significant, comparable results in this sample if the death of a ruler during this period affects present day outcomes directly through other channels. Column 6 of Table 9 reports the results. The estimate of interest is both small and insignificant. This indicates that the death of a ruler is unlikely to directly affect long-term outcomes.

8 Mechanism

This section discusses the underlying mechanisms through which direct British rule affects investment. First, we show that cotton-producing districts are more likely to be under direct British rule, and subject to adverse economic policies, resulting in the destruction of existing economic organizations with long-run detrimental effects. Second, using data on public road constructions, we show that the state takes longer than estimated to finish its projects in direct ruled districts. This systematic delay in the construction of roads reflects inefficiency, indicating lower state capacity. Moreover, we rule out alternative hypotheses that may explain our results, such as differences in law and enforcement, provision of public goods, and trust.

8.1 Cotton Production and Direct Ruled Areas

England began with driving the Indian cottons from the European market; it then introduced twist into Hindostan, and in the end inundated the very mother country of cotton with cottons.

-Marx (1853)

This section discusses a mechanism through which direct British rule affects corporate investment in the present: destruction of existing economic organizations. We show this in the context of cotton

production in India, a well-established industry that posed a significant threat to the British textile industry, before the onset of British colonial rule. We begin by documenting precolonial cotton production as a strong determinant of direct British rule. Then, we describe several ways through which the British destroyed the cotton industry in India, and the associated long-run consequences thereof.

8.1.1 Capture of Cotton Industry

India produced about 25% of the world's manufacturing output in 1750, of which, textiles constituted a significant share (Marks (2019)). Indian textiles dominated the world textile market in the 18th century, accounting for 25% of the global textile trade (Maddison et al. (1995)). The Indian cotton textiles were the most important manufactured goods in the 18th century (Parthasarathi (2011)) with India being home to the world's most important cotton textile industry (Robson (1957)). Cypher (2008) notes that the Indian textile production was marked by the presence of skilled laborers and large factory towns, which threatened the British textile industry – a leading sector of the British economy.

Politically, the British, through the EIC, consolidated economic power in India by annexing cotton producing territories (Schoen (2009)). The direct control of cotton producing areas allowed the British to control the supply of cotton, a valued commodity with a rising market share in the textile industry. This allowed the British to meet two objectives – protect the interests of the domestic textile industry in Britain, and increase Britain's share of global trade, by directly controlling India's largest industry. This is also reflected in the historical narrative that the objective of the East India Company was to to make India an agricultural colony of British capitalism by securing a monopoly on the supply of Indian goods and products, specifically cotton (Sahoo (2015)).

We begin by documenting empirical evidence, consistent with the historical narrative that the British took direct control of cotton-producing areas. Figure 7 shows that 80% of all districts under direct British rule were cotton producing districts. A cotton producing district was 40% more likely to be under direct British rule, relative to indirect rule. The likelihood that a cotton producing district is under direct British rule is economically large and statistically significant.

8.1.2 Destruction of Cotton Industry

The negative effects of the destruction of India's textile industry is evidenced by its fall in global textile exports, from 25% in the 17th century to just 2% at the end of British rule in 1947 ((Das (1946)). The destabilization of the well-flourishing textile industry resulted from the policies and practices of the British. In this section, we emphasize five ways through which the British destroyed the textile industry, specifically cotton industry in India: (1) protectionism of the British textile industry through tariffs, (2) price fixing through monopsony power, (3) violence against textile producers, (4) deprivation of new technological innovations in the local industries, and (5) neglect of local institutions.

First, the enactment of protectionist policies and tariffs enabled the British to monopolize trade.¹³ By 1813, British textiles faced 5% import tax in India, compared to up to 85% for Indian textiles in Britain (Alavi (1982); Nakatomi (2008)). Moreover, Indian goods had to bear much higher taxes within India (Govil (1950)) and pay higher freight charges for inland transportation of its goods through railways (Bagchi (1972)). As a result, the cotton industry in India not only lost its foreign markets, but also its home market (Sahoo (2015)). The British exports accounted for 52% of Indian cotton consumption in the 1890s, as opposed to 0.1% in 1810s (Broadberry and Gupta (2009)). Hence, the protectionist policies tilted trading in favor of the British.

Second, the British forced the weavers to exclusively sell their goods to them at fixed prices, establishing themselves as a monopsony. Prices were often fixed below costs, causing weavers to fall into poverty (Mukund (1992)). Moreover, local agents, *gomasthas*, monitored production, ensuring that weavers could not privately sell to other buyers. As a result, the textile industry in India deteriorated. The real wages of Indian laborers in 1820 reached 45% of their wages in 1600, substantially reducing their purchasing power (Cypher (2008)). The economic abuse of Indian cotton producers is best portrayed in the March 1921 Young India magazine essay by Mahatma Gandhi:

¹³The British have a long history of taking actions to protect its domestic textile industry and eliminate the competitiveness of the Indian textile industry, before the onset of colonization. This included the enactment of several bills. In 1685, a 10% tariff was imposed on the import of Indian goods. In 1690, this tariff was increased to 20%. In 1700, the First Calico Act was passed which banned the import of painted, dyed, stained, and printed fabric. In the same year, the British abolished the export duty on English woolen products, to improve the competitiveness of the British textile industry. In 1707, the tariff on Indian goods was increased to 50%. These acts of protectionism culminated in the Second Calico Act of 1721, which banned the sale of most cotton textiles from India.

“The labour of these artisans was so cruelly suppressed that they were obliged to cut off their own thumbs in order to avoid imprisonment...In my opinion, such cutting off would be less cruel than the terrorism which resulted in self-mutilation” ([Gandhi \(1921\)](#)).

Third, in addition to economic abuse, there is evidence that laborers experienced physical abuse. Bolts, a British merchant and judge of the mayor’s court of Calcutta recorded,

“With every species of monopoly, therefore every kind of oppression to manufactures of denominations throughout the country has daily increased; insomuch that weavers for daring to sell their goods, and Dallals and Pykars, for having contributed to or connived at such sales, have, by the Company’s agents, been frequently seized and imprisoned, confined in irons, fined considerable sums of money, flogged, and deprived, in the most ignominious manner. . . Weavers also, upon their inability to perform such agreements as have been forced from them by the Company’s agents ... have had their goods seized, and sold on the spot, to make good the deficiency: and the winders of raw silk, called Nagaads, have been treated also with such injustice, that instances have been known of their cutting off their thumbs, to prevent their being forced to wind silk” ([Bolts \(1775\)](#)).

Fourth, there is scant evidence that the British encouraged industrialization of cotton mills. Charles Wood, the Secretary for State in India, discouraged the British Government from promoting the use of cotton mills in India to produce equivalent goods that Britain could produce ([Gopal \(1965\)](#)). The British attitude towards the domestic industrial expansion in India was laissez-faire ([Avineri \(1969\)](#)). The government neither supplied capital nor exported the industrial innovations of Britain to India, preventing the development of the Indian cotton mills in the same way the mills of Lancashire had developed – “the financier had not entered the field.” ([Sahoo \(2015\)](#)).

Fifth, there is insufficient evidence that the British developed institutions to promote the indigenous cotton industry. The emergence of American colonies as a cheap supplier of cotton further reduced British incentives to improve institutions in India. Lord Canning, The Governor General of India from 1856-1857, believed that market forces would improve the institutional quality in India and that there was no need for the state to intervene. Even during the Lancashire cotton famine, caused by the interruption in supplies of raw cotton from the United States, the Governor General reluctantly issued a resolution in India in 1861, announcing the offer of prizes in each presidency to the grower of the largest quantity of cotton combined with the best quality.¹⁴ The pressure from the British Parliament forced the erstwhile Indian government to pass some measures to improve the institutions to ensure quality production and exports. However, the enforcers of

¹⁴“Canning had an uneasy conscience about the propriety and equity of his action, for he suspected that it transgressed the ‘limits of Government’s activity” [Tripathi \(1967\)](#).

these laws felt these measures were half-hearted. Sir Bartle Frere, Governor of Bombay – the key signatory in the 1862 the Cotton Frauds Act which stipulated checks for adulteration in cotton exports – noted that the reform “did not go far enough” (Tripathi (1967)).¹⁵ The tepid response of the British government in India to the Lancashire cotton famine reflects the lack of interest to invest in institutions to promote cotton cultivation in India. Hence, precolonial cotton-producing areas did not experience improvements in institutional quality during colonization.

Overall the attitude of the British towards the Indian cotton textile production is best described by HH Wilson, in his continuation of James Mill’s *The History of British India*.

“... had not such prohibitory duties and decrees existed, the mills of Paisley and of Manchester would have been stopped in their outset, and could hardly have been again set in motion, even by the powers of steam. They were created by the sacrifice of the Indian manufactures. Had India been independent, she would have retaliated; would have imposed preventive duties upon British goods, and would thus have preserved her own productive industry from annihilation. This act of self-defence was not permitted her; she was at the mercy of the stranger. British goods were forced upon her without paying any duty; and the foreign manufacturer employed the arm of political injustice to keep down and ultimately strangle a competitor with whom he could not contend on equal terms” (Mill and Wilson (1848)).

8.1.3 Long-Run Effect

Thus far, we have described several ways through which the British destroyed existing economic organizations centered around cotton production in India. This section examines the long-run consequences of these actions. In particular, we study how direct British rule affects corporate investment in the present through the destruction of the cotton industry during the colonial era.

The analysis uses cotton cultivation during precolonial times as an instrument for direct British rule in a sample of bordering districts, which include firm \times district-pair \times year fixed effects.¹⁶ Table 10 presents the long-run effect of the destruction of the Indian cotton industry on corporate investment in the present, through its impact on direct British rule. Column 1 reports the IV estimate. The IV estimate is negative and statistically significant, indicating that areas with greater cotton cultivation during the precolonial period have lower corporate investment in the present. Columns 2 and 3 report the second and first stage from the 2SLS estimation, respectively. The first stage estimate in column 3 is positive and statistically significant with an f-statistic of

¹⁵Moreover, the law was largely ineffective as noted by W.W. Ravenscroft, Chief Cotton Inspector in Bombay in his letter to A.D. Robertson, April 23 and June 1, 1864 (Secretariat Records Office). See also letter of H. Brooke, Secretary, Bombay Chamber of Commerce, to Ravenscroft, April 26, 1864 (Secretariat Records Office).

¹⁶We also report similar results from the full sample in appendix Table C.3 for completeness.

47.4. This implies the relevance of the instrument and indicates that the second stage estimate is unlikely to be upward biased due to weak instrument concerns. Column 2 reports the results from the second stage. The second stage estimate is negative and statistically significant, indicating that a firm reduces its project size by 20% in direct ruled areas relative to indirect ruled areas, within a pair of adjacent districts.

The key identifying assumption of this analysis is that cotton cultivation during the precolonial period does not directly affect corporate investment in the present through other channels. The usage of adjacent district pairs and firm \times district-pair \times year fixed effects helps alleviate several threats to our identification strategy. Adjacent district pairs are likely to have similar geographic and climatic conditions, conducive to cotton cultivation. Hence, whether one district grew cotton and its bordering district did not is likely to be a matter of chance. We further strengthen our claim for the exclusion assumption by conducting a falsification exercise. The falsification exercise focuses on a sample of adjacent indirect ruled district pairs, in which one district grew cotton during the precolonial period and the other did not. If cotton cultivation during the precolonial period affects present corporate investment either directly or indirectly through channels other than direct British rule, we should find differences across these adjacent district pairs that were never under direct British rule. Column 4 of Table 10 reports the results from this falsification test. The estimate of interest is close to zero and statistically insignificant. This indicates that precolonial cotton cultivation is unlikely to explain differences in present corporate investment directly or indirectly through channels other than direct British rule.

The long-run consequences of the destruction of existing economic organizations can be attributed to two mechanisms. First, the destruction of strong economic organizations hampers the intergenerational transfer of skills and knowledge. Chaudhuri (1978) argues that India's stronghold in the cotton industry, at its peak, can be explained by a large supply of skilled labor with specialized knowledge that was transferred over generations. The destruction of the cotton industry reduced the incentives of the cotton laborers to pass down their knowledge to future generations and disincentivized the subsequent generation from acquiring these skills. One may expect that the destruction of the cotton industry may incentivize the workers in that industry to acquire new skills. However, the communities working in the cotton textile industry belonged to specific caste groups (Broadberry and Gupta (2005)) and faced social and economic impediments in entering

new professions. Second, an industry achieves dominance in an area over a long period of time. The traditional Marshallian or agglomerative forces – ideas, people and goods, combined with geographic factors typically explain the agglomeration of industries in specific areas. The destruction of such a dominant industry that developed over a long period of time disrupts this natural process and renders the comparative advantage of the area futile with long-run negative consequences for future firm activity.

8.2 State Efficiency

Another key difference between indirect and direct ruled districts during the colonial period was the quality of governance. Rulers of indirect ruled districts faced the threat of British annexation under the pretext of misrule. This incentivized princely states to provide better governance. Moreover, rulers of indirect ruled districts exhibited longer tenure than their direct ruled counterparts, incentivizing long-term planning and development. We hypothesize that these two forces facilitated better governance and higher state efficiency in indirect ruled areas relative to direct ruled areas which have persisted to the present. This section validates this hypothesis by examining a key facet of state efficiency – the ability of the state to provide public goods in a timely fashion.

8.2.1 Delays in Public Road Construction

An important function of the state is to provide public goods that may otherwise be susceptible to private market failures. The efficiency of the state in the provision of public goods hinges on several aspects including the quality, cost, and timely delivery. In this section, we focus on timely delivery to identify state inefficiency. Projects that consistently overrun their estimated timelines reflect state inefficiency. State efficiency or state capacity is an important indicator of economic growth, as the state can expand the productive capacity of an area by increasing resources and enhancing the productivity of existing resources through timely public capital investment (Munnell (1992)).

We focus on road construction under the national road building program, *Pradhan Mantri Gram Sadak Yojana* (PMGSY), to identify state (in)efficiency. We focus on the road building program for four main reasons. First, infrastructure spending is increasingly seen as a key policy lever for governments to drive economic growth. Roads are an important public capital investment, especially in emerging markets like India which face severe constraints on its transportation infras-

structure (Das et al. (2019)). Second, the implementation of this program was the responsibility of the state and local bureaucrats. Third, PMGSY has been linked to development outcomes. The construction of roads under PMGSY has been associated with increased mobility of workers from agriculture into waged labor (Asher and Novosad (2020b)), increased school enrollment through improved access to schools (Mukherjee (2012), Adukia, Asher and Novosad (2020)), and higher lending (Agarwal, Mukherjee and Naaraayanan (2021)). Fourth, the richness of the data for roads constructed under PMGSY allow us to construct a measure of state efficiency which is otherwise difficult to construct.

We measure state efficiency using time overruns for road construction. This is measured as the difference in the natural logarithm of actual time taken to complete a road and the natural logarithm of estimated time to complete the road. We refer to this measure as construction delay. Our measure of construction delay may be driven by four factors. First, a state may consistently underestimate the completion time of a project, suggesting that the state may be unaware of its capacity for on-the-ground implementation. Second, state agents may shirk their responsibilities, contributing to delays. Both of these factors are indicators of state inefficiency. Third, idiosyncratic geographic shocks can cause delays in the construction of roads. We control for such shocks in our estimation strategy using a local identification approach. Fourth, roads constructed in direct ruled areas may be systematically different from roads in indirect ruled areas. We address this issue by comparing the differences in construction delays for roads that are in the same percentile value of cost and length of roads across the two district types.

Table 11 reports the results of regressing construction delays measured at the village-year level on the Direct Rule variable, using a sample of contiguous direct and indirect ruled districts. The estimation procedure allows us to control for District-Pair \times Year fixed effects. Hence, the difference in construction delays between direct and indirect ruled districts is estimated by comparing the average construction delay in a direct ruled district with an indirect ruled district within a contiguous district pair. This strategy of comparing adjacent districts implicitly controls for all geography specific shocks which could explain differences in construction delays. Additionally, we control for the geographic distances from each village to town centers with different population thresholds. The geographic distances control for the supply-chain networks to each village and the administrative relevance of each village, which can be crucial for the timely completion of roads.

Column (1) reports this result. The estimate of interest is positive and statistically significant at the 1% level. The estimate indicates that road construction projects are delayed by 10.2% more in direct ruled districts relative to a contiguous indirect ruled districts. This estimate is economically large and represents a 18.4% increase relative to the sample mean. Additionally, columns (2)-(4) supplement the results in column (1) by including district-pair \times cost percentile \times year, district-pair \times road length percentile \times year, and district-pair \times cost percentile \times road length percentile \times year fixed effects, respectively. These estimates compare the delay in road construction for roads constructed across the two district types within a contiguous district-pair that are likely to be similar to each other across two important dimensions – cost of construction and road length. The results from columns (2)-(4) reinforce the results reported in column (1). Overall, these results imply that the state machinery is weaker in direct ruled districts, impeding private firms' access to investment opportunities, especially for more time-sensitive opportunities.

8.3 Alternative Explanations: Public Goods, Trust & Conflict

This section discusses three alternative explanations that can potentially explain our results – differences in law and enforcement, provision of public goods, and trust. We argue that none of these forces can account for our results.

Law and Enforcement: The British faced a high mortality rate in India, discouraging them from active investments in improving the quality of institutions ([Acemoglu, Johnson and Robinson \(2001\)](#)). Hence, the differences in the rule of law across direct and indirect ruled districts could potentially explain the differences in investment. However, this hypothesis is unlikely to explain our results as our estimation strategy compares direct and indirect ruled districts within a state. The law governing contract enforcement, property rights, and dispute resolution is fixed within a state regardless of the historical origins of the district.¹⁷ Alternatively, while the law may be fixed within a state, the historical origins can affect the enforcement of the law across districts. [Ponticelli and](#)

¹⁷Moreover, it is to be noted that India is an outlier to the [Acemoglu, Johnson and Robinson \(2001\)](#) hypothesis. See appendix Figure E.1. Figure E.1a presents a scatter plot of the average protection against expropriation and settler mortality, after standardizing the value for both the variables to zero for India for the [Acemoglu, Johnson and Robinson \(2001\)](#) sample. The plot shows that only five countries - Australia, Canada, New Zealand, Singapore, and United States have average protection against expropriation superior to India. Additionally, the three neighboring countries, Bangladesh, Sri Lanka (direct ruled) and Pakistan (partly direct ruled) have similar settler mortality to India but inferior protection against expropriation. The regression of economic prosperity, measured as the natural logarithm of GDP per capita (PPP) in 1995, on average protection against expropriation in Figure E.1b reinforces that India is an outlier. Hence, the theory of origins of formal institutions as posited in [Acemoglu, Johnson and Robinson \(2001\)](#) is unlikely to explain the institutions and the relationship between institutions and economic growth in India.

[Alencar \(2016\)](#) document a negative effect of weak enforcement on investment. This hypothesis is also unlikely to explain our results as all corporate cases are adjudicated by the respective high courts or debt recovery tribunals of the state rather than the judicial machinery at the district level. Therefore, our within-state estimation strategy ensures that all firms are not only operating in an identical legal framework but are also exposed to identical enforcement of the legal framework. A third concern is that differences in enforcement at the district level can affect projects indirectly through other channels. We rule out this hypothesis by examining differences in court delays, the average time taken by local courts to deliver a verdict for criminal cases, across district types. Appendix Table [E.1](#) shows that the differences in court delays across district types are insignificant for our sample. Hence, our results cannot be explained by differences in local enforcement across district types.

Provision of Public Goods: We examine the differences in the provision of public goods across direct and indirect ruled districts. This analysis differs from the analysis of section [8.2.1](#), which compares delays in the public goods projects across direct and indirect districts – not the amount of public goods. Using data on public goods provision across Indian districts from 1961 until 1991, [Iyer \(2010\)](#) documents the differences across direct and indirect ruled districts. The differences in the provision of public goods diminish substantially over time, exhibiting convergence, consistent with policies of equalization followed by postcolonial governments ([Iyer \(2010\)](#), [Banerjee and Somanathan \(2007\)](#)). Using the Census data for 1991, 2001 and 2011, we verify that the differences in public goods provision, measured by number of schools and electricity supply, are insignificant for our sample (see appendix Table [E.2](#)). Hence, the differences in the provision of public goods are unlikely to drive our results.

Trust: We have argued that direct ruled districts were under the direct administration of the British, hence, more prone to exploitation. This is likely to have fomented greater mistrust in the state during colonial rule in direct ruled districts relative to indirect ruled districts. This mistrust in the state can persist over the long-term ([Nunn and Wantchekon \(2011\)](#)). The lack of trust in the state and its machinery can impede the ability of the state from investing in basic infrastructure required for corporate investment. Therefore, an alternative hypothesis is that the current difference in investment reflect differences in trust in the state, attributable to the colonial origins of districts. Using the 2011 survey data from the Indian Human Development Survey (IHDS) on trust in state

agents, such as politicians, police, panchayats, and banks, we do not find any evidence of systematic differences in trust in state actors across direct and indirect ruled districts (see, appendix Figure E.2). Hence, we argue that differences in trust, due to colonial rule, are unlikely to drive our results.

9 Conclusion

In this paper, we show that history can explain geographic concentration of investment over and above the traditional agglomerative forces, geographic differences and expectations. This paper uses within country variation in historical circumstances, combined with a local identification approach and instrumental variable strategy to explain the spatial differences in investment. We use spatial variation in direct and indirect British rule to identify differences in historical circumstances. Our aggregate analysis shows that the differences in historical origins can explain 13% of total geographic variation in investment. Our micro-level estimate suggests that investment is 8-10% lower in direct ruled areas, relative to indirect ruled areas. We further explore two channels through which history affects investment – economic organizations and state capacity. First, we show that cotton-producing districts are more likely to be under direct British rule, and subject to adverse economic policies, resulting in the destruction of existing economic organizations with long-run detrimental effects. Second, using data on public road constructions, we show that the state takes longer than estimated to finish its projects in direct ruled districts. This systematic delay in the construction of roads reflects inefficiency, indicating lower state capacity.

Our work has three distinct contributions. First, we focus on a novel aspect that can explain the geographic concentration of investment: history. Second, our empirical strategy is better adept at solving the difficult identification problems associated with identifying the relationship between history and economic activity. Third, we explain how differences in colonial rule can produce long-run consequences, through two distinct mechanisms, namely, the destruction of well-established economic organizations and state capacity.

Our findings demonstrate that history can have enduring influence on the trajectory of economic development within a country. More broadly, this work informs discussions on the root causes of inequality, aiding our understanding of how the vestiges of history can create cleavages within a nation. Future study on how historical processes can perpetuate inequality may be a fruitful area of work, to further the discussion on economic disparities.

References

- Acemoglu, Daron, Ali Cheema, Asim I Khwaja, and James A Robinson.** 2020. "Trust in state and nonstate actors: Evidence from dispute resolution in Pakistan." *Journal of Political Economy*, 128: 3090–3147.
- Acemoglu, Daron, Simon Johnson, and James A Robinson.** 2001. "The colonial origins of comparative development: An empirical investigation." *American Economic Review*, 91(5): 1369–1401.
- Adukia, Anjali, Sam Asher, and Paul Novosad.** 2020. "Educational investment responses to economic opportunity: evidence from Indian road construction." *American Economic Journal: Applied Economics*, 12(1): 348–76.
- Agarwal, Sumit, Abhiroop Mukherjee, and S Lakshmi Naaraayanan.** 2021. "Roads and Loans." *Available at SSRN 3103001*.
- Alavi, Hamza.** 1982. *Capitalism and colonial production*. Routledge.
- Allen, Charles, and Sharada Dwivedi.** 1984. *Lives of the Indian princes*. Century.
- Alok, Shashwat, and Meghana Ayyagari.** 2020. "Politics, state ownership, and corporate investments." *The Review of Financial Studies*, 33(7): 3031–3087.
- Altonji, Joseph G, Todd E Elder, and Christopher R Taber.** 2005. "Selection on observed and unobserved variables: Assessing the effectiveness of Catholic schools." *Journal of Political Economy*, 113(1): 151–184.
- Arthur, W Brian.** 1986. *Industry location patterns and the importance of history*. Center for Economic Policy Research, Stanford University.
- Ash, Elliot, Sam Asher, Aditi Bhowmick, Daniel Chen, Tanaya Devi, Christoph Goessmann, Paul Novosad, and Bilal Siddiqi.** 2021. "Measuring Gender and Religious Bias in the Indian Judiciary." Working Paper.
- Asher, Sam, and Paul Novosad.** 2020a. "Rural Roads and Local Economic Development." *American Economic Review*.
- Asher, Sam, and Paul Novosad.** 2020b. "Rural roads and local economic development." *American Economic Review*, 110(3): 797–823.
- Asher, Sam, Karan Nagpal, and Paul Novosad.** 2018. "The Cost of Distance: Geography and Governance in Rural India." *World Bank Working Paper*.
- Asher, Sam, Tobias Lunt, Ryu Matsuura, and Paul Novosad.** 2021. "Development Research at High Geographic Resolution: An Analysis of Night Lights, Firms, and Poverty in India using the SHRUG Open Data Platform." *World Bank Economic Review*.
- Ashton, Stephen Richard.** 1977. "British Policy Towards the Indian States, 1905-1939." PhD diss. School of Oriental and African Studies (University of London).

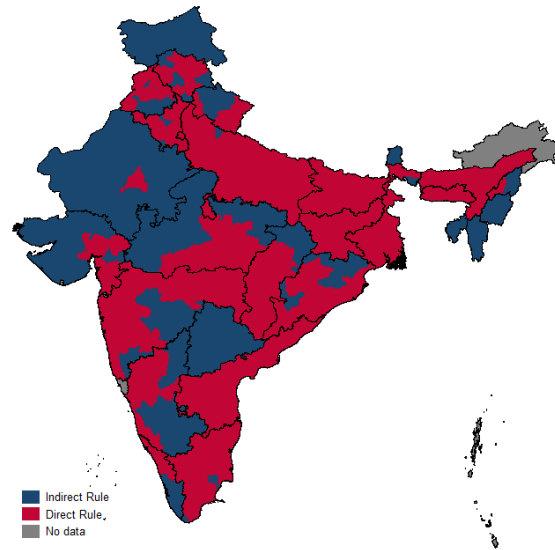
- Avineri, Shlomo.** 1969. *Karl Marx on Colonialism and Modernization: his despatches and other writings on China, India, Mexico, the Middle East and North Africa.* Anchor Books.
- Baden-Powell, Baden Henry.** 1892. *The land-systems of British India.* Vol. 2, Clarendon Press.
- Bagchi, Amiya Kumar.** 1972. *Private investment in India, 1900-1939.* Cambridge University Press.
- Banerjee, Abhijit, and Lakshmi Iyer.** 2005. "History, institutions, and economic performance: The legacy of colonial land tenure systems in India." *American Economic Review*, 95(4): 1190–1213.
- Banerjee, Abhijit, and Rohini Somanathan.** 2007. "The political economy of public goods: Some evidence from India." *Journal of development Economics*, 82(2): 287–314.
- Bardhan, Pranab.** 2002. "Decentralization of governance and development." *Journal of Economic Perspectives*, 16(4): 185–205.
- Berger, Daniel.** 2009. "Taxes, Institutions and Local Governance: Evidence from a Natural Experiment in Colonial Nigeria."
- Bobonis, Gustavo J, and Peter M Morrow.** 2014. "Labor coercion and the accumulation of human capital." *Journal of Development Economics*, 108: 32–53.
- Bolts, William.** 1775. *Considerations on India Affairs: Particularly Respecting the Present State of Bengal and Its Dependencies...* Vol. 3, J. Almon.
- Broadberry, Stephen, and Bishnupriya Gupta.** 2009. "Lancashire, India, and shifting competitive advantage in cotton textiles, 1700–1850: The neglected role of factor prices 1." *The Economic History Review*, 62(2): 279–305.
- Broadberry, Stephen N, and Bishnupriya Gupta.** 2005. "Cotton textiles and the great divergence: Lancashire, India and shifting competitive advantage, 1600-1850."
- Chakrabarti, Jadab Chandra.** 1896. *The native states of India.* Luzac & Company.
- Chaudhuri, Kirti N.** 1978. *The Trading World of Asia and the English East India Company: 1660-1760.* Cambridge University Press.
- Conley, Timothy G.** 1999. "GMM estimation with cross sectional dependence." *The Journal of Econometrics*, 92(1): 1–45.
- Cypher, James.** 2008. *The process of economic development.* Routledge.
- Das, Abhiman, Syed Ejaz Ghani, Arti Grover Goswami, William Kerr, and Ramana Nanda.** 2019. "Infrastructure and Finance: Evidence from India's GQ Highway Network." *Harvard Business School Entrepreneurial Management Working Paper*, , (19-121).
- Das, Taraknath.** 1946. "The Economic History of India: 1600-1800."

- Dell, Melissa.** 2010. "The persistent effects of Peru's mining mita." *Econometrica*, 78(6): 1863–1903.
- Dell, Melissa, and Benjamin A Olken.** 2020. "The development effects of the extractive colonial economy: The dutch cultivation system in java." *The Review of Economic Studies*, 87(1): 164–203.
- Donaldson, Dave.** 2018. "Railroads of the Raj: Estimating the impact of transportation infrastructure." *American Economic Review*, 108(4-5): 899–934.
- Duranton, Gilles, and Henry G Overman.** 2005. "Testing for localization using micro-geographic data." *The Review of Economic Studies*, 72(4): 1077–1106.
- Ellison, Glenn, and Edward L Glaeser.** 1997. "Geographic concentration in US manufacturing industries: a dartboard approach." *Journal of Political Economy*, 105(5): 889–927.
- Ellison, Glenn, Edward L Glaeser, and William R Kerr.** 2010. "What causes industry agglomeration? Evidence from coagglomeration patterns." *American Economic Review*, 100(3): 1195–1213.
- Ethier, Wilfred J.** 1982. "Decreasing costs in international trade and Frank Graham's argument for protection." *Econometrica: Journal of the Econometric Society*, 1243–1268.
- Gandhi, Mohandas Karamchand.** 1921. "Notes." *Young India*, 19: 487.
- Gopal, Sarvepalli.** 1965. "British Policy in India, 1858-1905." *Cambridge*.
- Govil, Kanhaiya Lal.** 1950. "The Cotton Industry of India: Prospect and Retrospect."
- Hall, Robert E, and Charles I Jones.** 1999. "Why do some countries produce so much more output per worker than others?" *The Quarterly Journal of Economics*, 114(1): 83–116.
- Huillery, Elise.** 2009. "History matters: The long-term impact of colonial public investments in French West Africa." *American Economic Journal: Applied Economics*, 1(2): 176–215.
- Hunter, William Wilson, and John George Bartholomew.** 1908. *The Imperial Gazetteer of India*. Vol. 22, Clarendon Press.
- Iyer, Lakshmi.** 2010. "Direct versus indirect colonial rule in India: Long-term consequences." *The Review of Economics and Statistics*, 92(4): 693–713.
- Kelly, Morgan.** 2019. "The standard errors of persistence."
- Krugman, Paul.** 1987. "The narrow moving band, the Dutch disease, and the competitive consequences of Mrs. Thatcher: Notes on trade in the presence of dynamic scale economies." *Journal of development Economics*, 27(1-2): 41–55.
- Krugman, Paul.** 1991. "History versus expectations." *The Quarterly Journal of Economics*, 106(2): 651–667.

- Kumar, Hemanshu, and Rohini Somanathan.** 2009. "Mapping Indian districts across census years, 1971-2001." *Economic and Political Weekly*, 69–73.
- Lee, Alexander.** 2017. "Redistributive colonialism: The long term legacy of international conflict in India." *Politics & Society*, 45(2): 173–224.
- Lee-Warner, William.** 1910. *The Native States of India*. Macmillan and Company, limited.
- Lowes, Sara, and Eduardo Montero.** 2020. "Concessions, violence, and indirect rule: Evidence from the congo free state." National Bureau of Economic Research.
- Maddison, Angus, et al.** 1995. *Monitoring the world economy, 1820-1992*. Development Centre of the Organisation for Economic Co-operation and . . .
- Marks, Robert B.** 2019. *The origins of the modern world: A global and environmental narrative from the fifteenth to the twenty-first century*. Rowman & Littlefield.
- Marshall, Alfred.** 1920. "Principles of Economics." *London: MacMillan*, 621.
- Marx, Karl.** 1853. *The british rule in India*. Electric Book.
- Méndez-Chacón, Esteban, and Diana Van Patten.** 2019. "Multinationals, monopsony and local development: Evidence from the united fruit company." *Unpublished Manuscript*.
- Michalopoulos, Stelios, and Elias Papaioannou.** 2013. "Pre-colonial ethnic institutions and contemporary African development." *Econometrica*, 81(1): 113–152.
- Mill, James, and Horace Hayman Wilson.** 1848. "History of British India. Vol. I."
- Moulton, Brent R.** 1990. "An illustration of a pitfall in estimating the effects of aggregate variables on micro units." *The Review of Economics and Statistics*, 334–338.
- Mukherjee, Mukta.** 2012. "Do better roads increase school enrollment? Evidence from a unique road policy in India." *Working Paper*.
- Mukund, Kanakalatha.** 1992. "Indian textile industry in 17th and 18th Centuries: Structure, organisation and responses." *Economic and political weekly*, 2057–2065.
- Munnell, Alicia H.** 1992. "Policy watch: infrastructure investment and economic growth." *Journal of economic perspectives*, 6(4): 189–198.
- Nakatomi, K.** 2008. *History of Indian Textiles*. K. Hatanaka, ed., Textile Arts of India. San Francisco: Chronicle Books.
- Nunn, Nathan.** 2008. "The long-term effects of Africa's slave trades." *The Quarterly Journal of Economics*, 123(1): 139–176.
- Nunn, Nathan, and Leonard Wantchekon.** 2011. "The slave trade and the origins of mistrust in Africa." *American Economic Review*, 101(7): 3221–52.

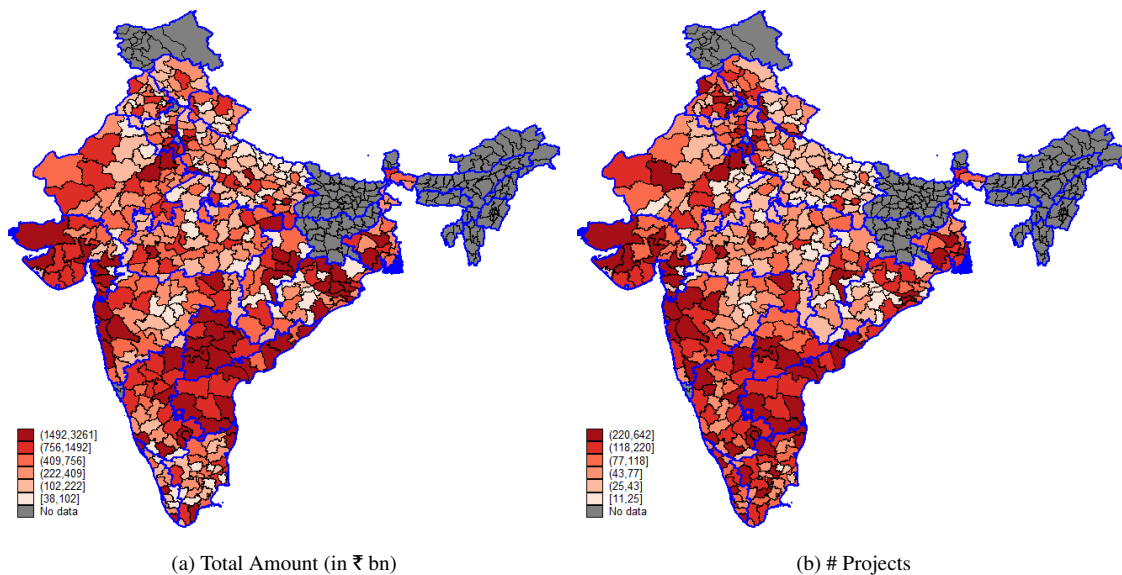
- Oster, Emily.** 2019. “Unobservable selection and coefficient stability: Theory and evidence.” *Journal of Business & Economic Statistics*, 37(2): 187–204.
- Panagariya, Arvind.** 1986. “Increasing returns, dynamic stability, and international trade.” *Journal of International Economics*, 20(1-2): 43–63.
- Parthasarathi, Prasannan.** 2011. *Why Europe grew rich and Asia did not: Global economic divergence, 1600–1850*. Cambridge University Press.
- Ponticelli, Jacopo, and Leonardo S Alencar.** 2016. “Court enforcement, bank loans, and firm investment: evidence from a bankruptcy reform in Brazil.” *The Quarterly Journal of Economics*, 131(3): 1365–1413.
- Porta, Rafael La, Florencio Lopez-de Silanes, Andrei Shleifer, and Robert W Vishny.** 1998. “Law and finance.” *Journal of Political Economy*, 106(6): 1113–1155.
- Rahim, MA.** 1963. “Lord Dalhousie’s Administration of the Conquered and Annexed States (New Delhi: S. Chand).”
- Robson, Robert.** 1957. “cotton industry in Britain.”
- Sahoo, Rajib Lochan.** 2015. “Indian Cotton Mills and the British Economic Policy, 1854-1894.” *Proceedings of the Indian History Congress*, 76: 356–367.
- Schoen, Brian.** 2009. *The Fragile Fabric of Union: Cotton, Federal Politics, and the Global Origins of the Civil War*. JHU Press.
- Tripathi, Dwijendra.** 1967. “Opportunism of Free Trade: Lancashire Cotton Famine and Indian Cotton Cultivation.” *The Indian Economic & Social History Review*, 4(3): 255–263.
- Zingales, Luigi.** 2017. “Towards a political theory of the firm.” *Journal of Economic Perspectives*, 31(3): 113–30.

Figure 1: Current and Colonial Boundaries



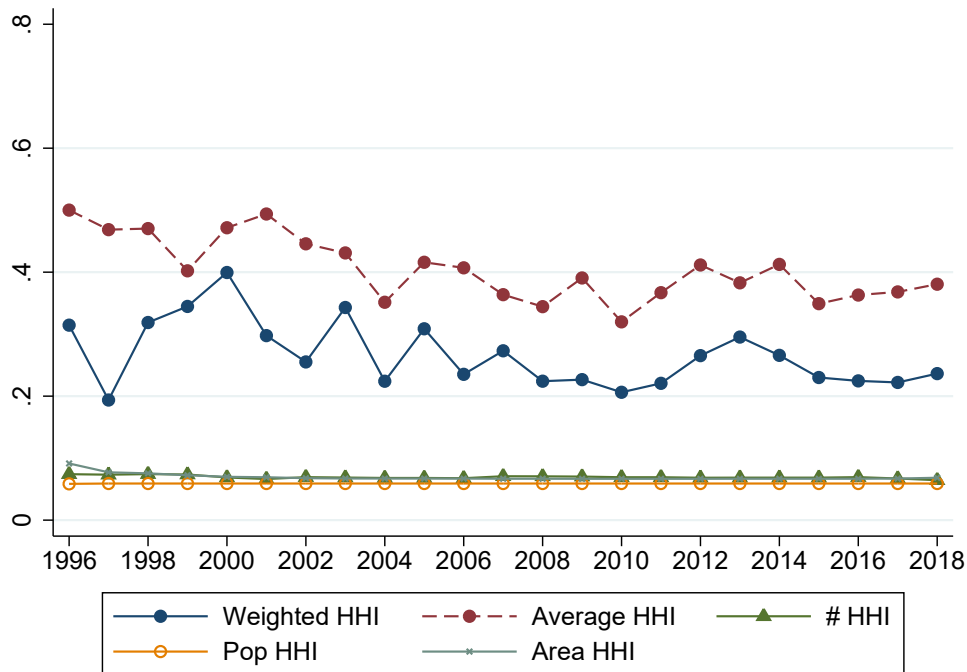
The figure plots the historical boundaries of direct and indirect British rule, along with the state administrative boundaries, as of 2014. The exact international geographic boundaries have not been verified.

Figure 2: Geography of Project Announcements



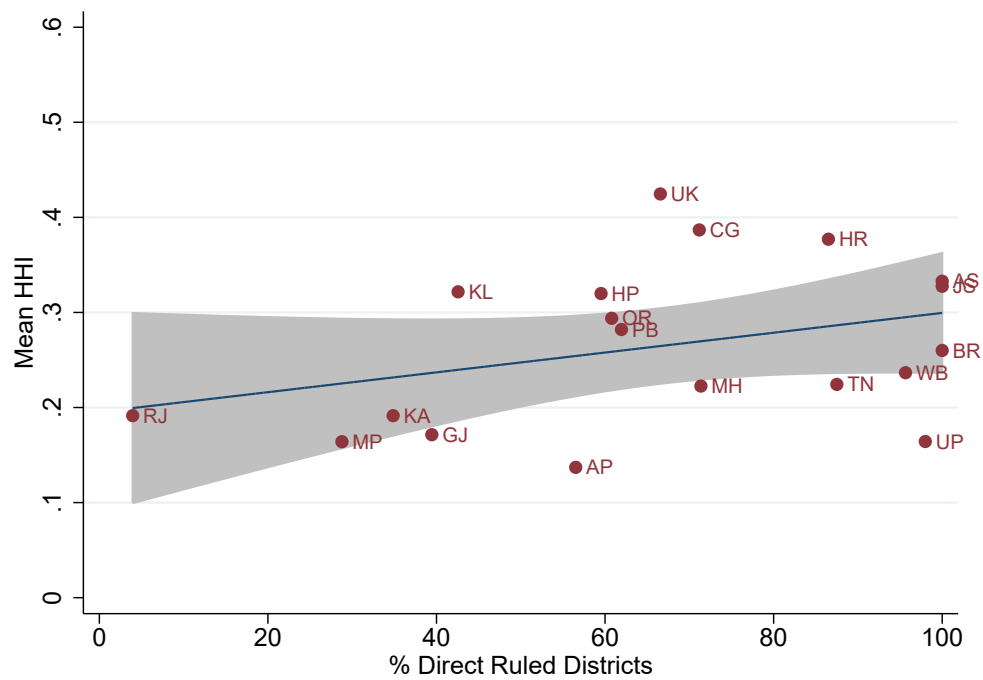
This figure plots the heatmap of total amount (in ₹ billions) and the number of projects announced in the 19 states in our sample between fiscal year 1996 and 2018. Total amount and number of projects are calculated as the total amount and total number of projects announced in each district. The exact international geographic boundaries have not been verified.

Figure 3: Geographic Concentration of Investment



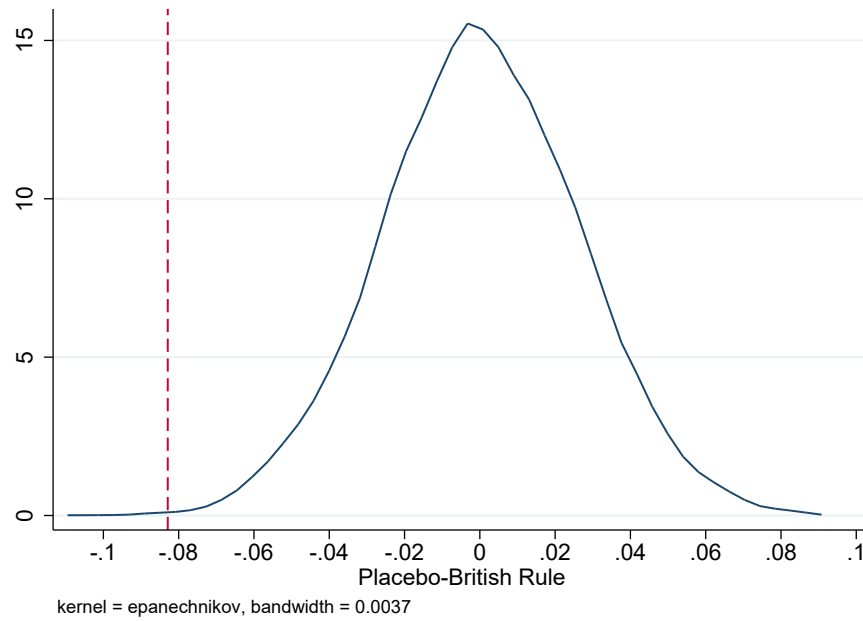
The figure plots a time-varying measure of state-level average and weighted Herfindahl-Hirschman Index (HHI). HHI is computed adding the squared share of investment in a district relative to the state in a year. The red line plots the simple average HHI for a year across all states. The blue line plots the weighted HHI where the HHI of each state is weighted by the share of investment in the state relative to the country. Alongside are three measures of benchmark HHI. # HHI assumes equal investment across all districts and is the simple average of $\frac{1}{N}$, where N denotes the number of districts in a state. State-level Area HHI is computed assuming the investment in each district relative to the state is proportional to the area of the district relative to the state. The aggregate Area HHI is computed by weighing each state by the share of total area in the state. Pop HHI is computed assuming the investment in each district relative to the state is proportional to the population of the district relative to the state population. The aggregate Pop HHI is computed by weighing each state by the share of total population in the state.

Figure 4: Geographic Concentration of Investment and Direct Rule



The figure plots the scatterplot of average state level investment HHI and the percent of direct ruled districts within a state and the best fit line between the two measures. The unit of observation is state. State-level average HHI is computed using data on project announcements from fiscal year 1996 till 2018.

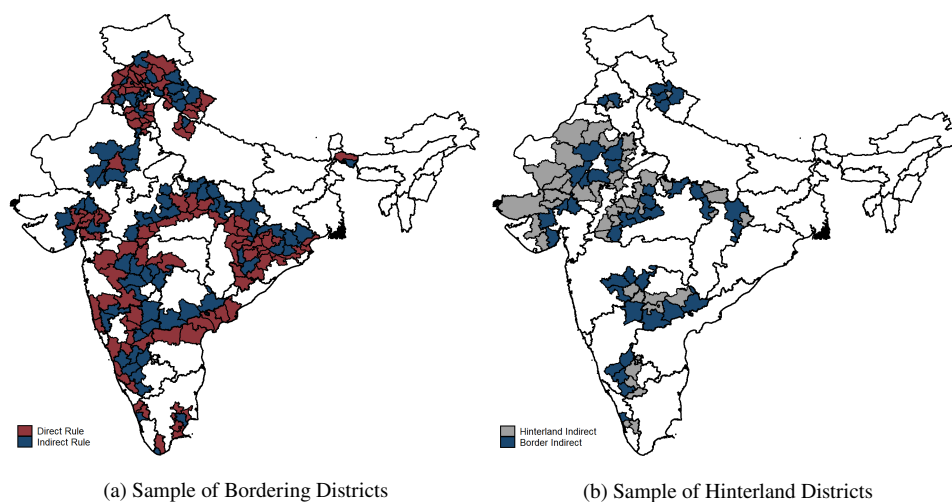
Figure 5: Placebo Test: Randomization of the Direct British Rule



Min	p1	p5	p25	p50	p75	p95	p99	Max	Mean	SD
-0.1058	-0.0604	-0.0432	-0.0174	0.0001	0.0180	0.0436	0.0620	0.0870	0.0003	0.0262

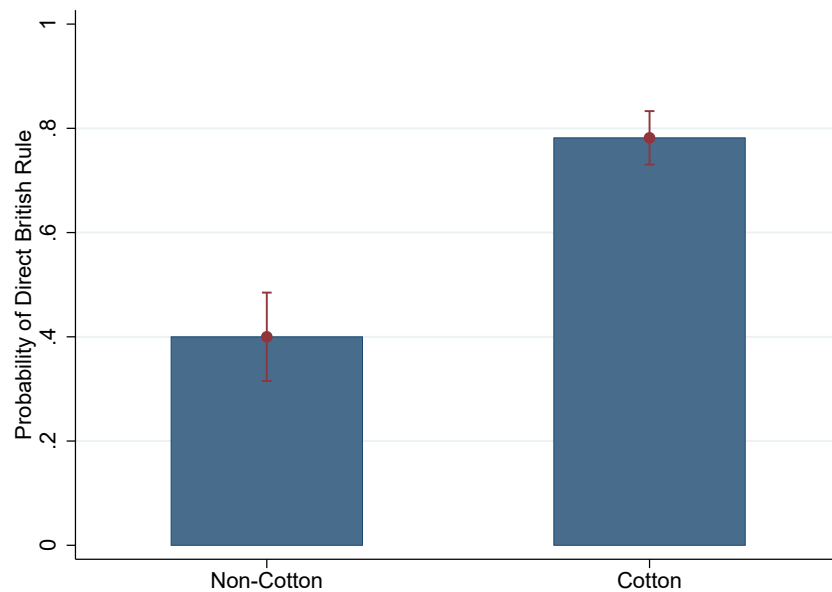
The figure plots the kernel density of the point estimates of $Placebo - Direct - Rule_j$ obtained from the 10,000 Monte-Carlo simulations. A placebo direct rule binary variable is generated for each district. Every district has a probability of 0.6370 to be assigned as directly ruled district. We generate the $Placebo - Direct - Rule_j$ variable 10,000 times and estimate the baseline specification, equation 1, using the generated placebo direct rule variable each time. The table gives the numbers associated with the distribution of the estimates plotted in the figure. The dashed red line shows the point estimate from column (5) of Table 3. There are 0.1% of points to the left of the red-dashed line.

Figure 6: Sample for Local Identification Approach



The Figure 6a plots the sample of direct and indirect ruled districts bordering each other within a state, along with the state administrative boundaries, as of 2014. The Figure 6b plots the sample of indirect ruled districts bordering each other within a state, along with the state administrative boundaries, as of 2014. The districts in Figure 6b are chosen based on indirect ruled districts in Figure 6a and the indirect ruled districts immediately bordering them. The exact international geographic boundaries have not been verified.

Figure 7: Probability of Direct British Rule Based on Cotton Production



The figure plots the probability that a district is under direct British rule. The bar graph denotes the likelihood that a district is under direct British rule for non-cotton producing and cotton producing districts. Standard errors are denoted by the red error-bars.

Table 1: What Predicts Direct British Rule?

Dep Var: Direct Rule (=1)	(1)	(2)	(3)	(4)	(5)
Altitude (MSL)	0.0002 (0.0003)				0.0002 (0.0002)
Coast (=1)	0.1820 (0.1176)				0.1720 (0.1179)
ln(Area)	-0.0637 (0.0816)				-0.0692 (0.0799)
Slope	-1.0837 (3.6432)				2.6706 (2.3314)
Rain (cm)	0.0015 (0.0010)				0.0012 (0.0009)
Max-Temp	0.0061 (0.0113)				-0.0010 (0.0113)
Min-Temp	0.0126 (0.0104)				0.0028 (0.0090)
ln(Distance)		0.0396 (0.0611)			0.0707 (0.0577)
Maratha Ruler			0.2279 (0.1550)		0.2449 (0.1524)
Muslim Ruler			0.3853*** (0.1276)		0.3319** (0.1420)
Prop Muslim				0.2663 (0.3447)	-0.1818 (0.2848)
Prop Sikhs				0.6377 (1.0841)	-0.2291 (0.9907)
Prop Lower Caste				0.5613 (0.3940)	0.5439 (0.3518)
Prop Elites				-0.3153 (0.6895)	-0.1544 (0.6948)
Constant	0.5330 (0.8825)	0.4275 (0.3253)	0.4336*** (0.0933)	0.5111*** (0.1445)	0.3777 (0.9042)
# Obs	294	294	294	294	294
R ²	0.0814	0.0042	0.1257	0.0293	0.1939

The table reports the differences in key observables between direct and direct ruled districts. Column (1) uses geographic covariates such as altitude of the district, coastal dummy indicating if the district is along the coast or not, natural logarithm of district area, average slope, average rainfall, maximum and minimum temperature. Column (2) uses an administrative covariate of the natural logarithm of the distance of the district from the state capital. Column (3) uses two binary variables. Maratha ruler takes a value of 1 if the precolonial ruler belonged to the Maratha empire, the largest empire before the entry of British in India. Muslim ruler takes a value of 1 if the precolonial ruler was of Muslim faith and 0 otherwise. Column (4) includes proportion of Muslim, Sikh, lower caste and elites as per 1911 Indian survey. Elites are defined based on castes that were coded as politically dominant, landowning, or military in the provincial volumes of the 1911 census. Standard errors reported in parentheses are clustered by native states. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Investment Concentration and State Characteristics

Dep Var: HHI	(1)	(2)	(3)	(4)	(5)	(6)
% Direct Rule	0.1213* (0.0661)	0.1463** (0.0531)	0.1227* (0.0685)	0.1522* (0.0793)	0.1514* (0.0813)	0.1369* (0.0778)
# Districts		-0.0182*** (0.0038)	-0.0188*** (0.0039)	-0.0180*** (0.0042)	-0.0194*** (0.0047)	-0.0183*** (0.0053)
Area per District			-0.0582 (0.1128)	-0.1081 (0.1402)	-0.1354 (0.1391)	-0.1090 (0.1541)
Population Density				-0.8775 (0.8750)	-1.2064 (0.9068)	-0.8094 (1.0235)
GDP per capita					-0.1159 (0.1124)	0.0302 (0.2965)
% Urban						-0.0087 (0.0144)
# Obs	19	19	19	19	19	19
R^2	0.1269	0.5086	0.5227	0.5422	0.5753	0.5933

This table presents the estimates of regressions of the average state-level HHI on key state-level explanatory variables. The key explanatory variables include percent of British districts in a state, number of districts in a state, area per district, population density, GDP per capita, and urban population percent. Average values of these variables between 1996 and 2018 are used as explanatory variables. Robust standard errors are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Investment and Direct British Rule

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)	(5)
Direct Rule (=1)	-0.1755** (0.0836) [0.0548]***	-0.1130*** (0.0416) [0.0356]***	-0.1146** (0.0526) [0.0371]***	-0.0864** (0.0348) [0.0332]***	-0.0881*** (0.0326) [0.0331]***
State FE	Yes	Yes	Yes	Yes	
Firm FE		Yes	Yes		
Qtr × Year FE			Yes	Yes	Yes
Firm × Year FE				Yes	Yes
State × Year FE					Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes
# Obs	28,820	28,820	28,820	28,820	28,820
R ²	0.0303	0.5067	0.5465	0.7088	0.7160

The table reports the results of regressing the natural logarithm of project cost on the direct British rule dummy, per the following specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,y} + \theta_{s(j \in s),y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

where i denotes firm announcing a project of size $Y_{i,j,t}$ in district j , located in state s ($j \in s$) at time t during year y . Direct Rule (=1) is a binary variable that takes a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. $\theta_{i,y}$ and $\theta_{s(j \in s),y}$ denote firm × year and state × year fixed effects respectively. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. Conley (1999) standard errors adjusted for spatial dependence within 100 km are reported in square brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Balanced Panel Analysis: Investment and Direct British Rule

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Announce=1	$I_{j,t}$	$I_{j,t} I_{j,t} > 0$	Projects $_{j,t}$	Projects $_{j,t} \# > 0$	$\frac{I_{j,t}}{\sum_{j \in s} I_{j,t}}$	$\frac{\text{Projects}_{j,t}}{\sum_{j \in s} \text{Projects}_{j,t}}$
Direct Rule (=1)	-0.2534* (0.1346)	-16174.5813** (7910.8774)	-28350.1337** (11777.6353)	-4.1791** (2.0257)	-6.8549** (2.7350)	-7.0724** (2.9954)	-7.1912* (3.7650)
State \times Qtr \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Average	0.6453 (0.0931)	19861.4928 (5367.5117)	37851.9045 (7901.4838)	4.4693 (1.3584)	7.7296 (1.8328)	8.7459 (2.0333)	8.8529 (2.5316)
# Obs	35,256	35,256	17,052	35,256	19,050	35,256	35,256
R^2	0.1854	0.2363	0.3115	0.1800	0.1621	0.0500	0.1070

The table reports the results of regressing total project cost and total number of projects and their shares on the direct British rule dummy as per the following specification:

$$I_{j,t} = \beta \cdot \text{Direct Rule}_j + \theta_{s(j \in s),t} + \varepsilon_{j,t}$$

where $I_{j,t}$ denotes total investment or number of projects in district j , located in state s ($j \in s$) at time t . Direct Rule (=1) is a binary variable taking a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. $\theta_{s(j \in s),t}$ denotes state \times quarter \times year fixed effects. We aggregate the firm-location-time investment dataset at location-time level, using total investment and total number of projects announced. We create a balanced panel by assigning the value of 0 to a location-time if no projects are announced in the location-time. The dependent variable in column (1) takes a value of 0 if no project is announced in that location-time and 1 otherwise. Column (2) uses total investment in district j as the dependent variable. Column (3) uses total investment in district j , conditional on any investment as the dependent variable. Column (4) uses total number of projects announced in district j . Column (5) uses total number of projects announced, conditional on any announcements in district j . Column (6) uses the share of investment in district j relative to investment across all districts in the state. Column (7) uses the share of the number of projects relative to the total number of projects announced in the state as the dependent variable. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Investment and Direct British Rule: Controlling for District Level Geographic Characteristics

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Direct Rule (=1)	-0.0692** (0.0317)	-0.0916*** (0.0344)	-0.1045*** (0.0340)	-0.1025*** (0.0348)	-0.0782** (0.0346)	-0.0926** (0.0355)	-0.0846** (0.0323)	-0.0856** (0.0376)
Altitude (MSL)	-0.0001 (0.0001)							-0.0001 (0.0001)
Coast (=1)		0.0210 (0.0470)						0.0050 (0.0493)
ln(Area)			0.0296 (0.0274)					0.0423 (0.0293)
Slope				0.6204 (0.7148)				0.7599 (1.1256)
Rain (cm)					-0.0007** (0.0003)			-0.0006* (0.0004)
Max-Temp						0.0042 (0.0031)		-0.0004 (0.0033)
Min-Temp						0.0026 (0.0041)		-0.0008 (0.0038)
ln(Distance)							-0.0217*** (0.0065)	-0.0437** (0.0186)
Qtr × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Obs	27,223	28,820	24,470	24,470	28,820	23,499	28,820	21,181
R ²	0.7195	0.7160	0.7318	0.7317	0.7161	0.7302	0.7161	0.7380

The table reports the results of regressing the natural logarithm of project cost on the direct British rule dummy after controlling for district level characteristics, per the following specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \Theta X_j + \theta_{i,y} + \theta_{s(j \in s),y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

where i denotes firm announcing a project of size $Y_{i,j,t}$ in district j , located in state s ($j \in s$) at time t during year y . Direct Rule (=1) is a binary variable that takes a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. X_j refers to a vector of non-time varying geographic characteristics including altitude (MSL), coastal dummy, natural logarithm of geographic area, slope of land, rainfall (in cm), maximum and minimum temperature, and the natural logarithm of the distance of the district from state capital. $\theta_{i,y}$ and $\theta_{s(j \in s),y}$ denote firm \times year and state \times year fixed effects respectively. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Local Identification Approach: Sample of Bordering Districts

Dep Var: ln(Project Size)	(1)	(2)	(3)
Direct Rule (=1)	-0.0974** (0.0469)	-0.1090** (0.0488)	-0.1084** (0.0457)
Qtr × Year FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	
District-Pair × Year FE	Yes	Yes	
Firm × District-Pair FE		Yes	
Firm × District-Pair × Year FE			Yes
Lat/Long	Yes	Yes	Yes
# Obs	11,947	11,947	11,947
R^2	0.7856	0.7940	0.7944

The table reports the results of regressing the natural logarithm of project cost on the direct British rule dummy using a sample of bordering districts within a state as shown in Figure 6a, per the following specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,p(j \in p)y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

where i denotes firm announcing a project of size $Y_{i,j,t}$ in district j , located in state s ($j \in s$) at time t during year y . Direct Rule (=1) is a binary variable that takes a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. $\theta_{i,p(j \in p)y}$ denotes the firm × district-pair × year fixed effect. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are multiway clustered by native states, district-pair, and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Local Identification Approach: Sample of Hinterland Districts

Dep Var: ln(Project Size)	(1)	(2)	(3)
Hinterland (=1)	0.0382 (0.0549)	0.0353 (0.0391)	0.0355 (0.0353)
Qtr \times Year FE	Yes	Yes	Yes
Firm \times Year FE	Yes	Yes	
District-Pair \times Year FE	Yes	Yes	
Firm \times District-Pair FE		Yes	
Firm \times District-Pair \times Year FE			Yes
Lat/Long	Yes	Yes	Yes
# Obs	4,953	4,953	4,953
R^2	0.8340	0.8431	0.8432

The table reports the results of regressing the natural logarithm of project cost on the hinterland dummy using a sample of bordering districts, both indirect ruled, within states as shown in Figure 6b, per the following specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Hinterland}_j + \theta_{i,p(j \in p)y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

where i denotes firm announcing a project of size $Y_{i,j,t}$ in district j , located in state s ($j \in s$) at time t during year y . Hinterland (=1) is a binary variable that takes a value of 1 if the indirect ruled district is in the hinterland of the sample of direct ruled districts shown in Figure 6a, and 0 otherwise. $\theta_{i,p(j \in p)y}$ denotes the firm \times district-pair \times year fixed effect. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are multi-way clustered by native states, district-pair, and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Likelihood of Project Abandonment

Dep Var: Project Abandonment	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
Direct Rule (=1)	0.0094 (0.0121)	0.0284** (0.0128)	0.0256* (0.0148)	0.0257* (0.0153)	0.0248* (0.0137)
Qtr \times Year FE	Yes	Yes	Yes	Yes	Yes
Firm \times District-Pair \times Year FE	Yes	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes
# Obs	10,938	10,938	10,938	10,938	10,938
R^2	0.5881	0.6125	0.6117	0.6153	0.6231

The table reports the probability of abandonment of project announced in direct ruled districts relative to the projects announced in indirect ruled districts. We identify the status of the project each year, until five years of its announcement, i.e., for a project announced in year x we examine the probability of its abandonment from year $x + 1$ until year $x + 5$. Our local identification approach includes firm \times district-pair \times year fixed effects, and quarter \times year fixed effects. Lat/Long indicates controls for latitude and longitude of the district. Standard errors are estimated by multiway clustering at the native states, district-pair, and time of project announcement level.

Table 9: Instrumental Variable Regression: Death of Ruler without Natural Heir

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var: ln(Project Size)	OLS	IV	2SLS		Falsification	
			Second Stage	First Stage		
Direct Rule (=1)	-0.2236*** (0.0604)		-0.2239** (0.0960)			
Ruler Death, No Heir, Lapse (=1)		-0.1475* (0.0766)		0.6589*** (0.1225)		
Ruler Death, No Heir, No Lapse (=1)					0.0037 (0.0568)	
Ruler Death, Yes Heir, Lapse (=1)						0.0072 (0.0764)
Qtr × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes	Yes
# Obs	10,293	10,293	10,293	10,293	8,129	8,129
R ²	0.5692	0.5693	-0.0563	0.6691		
KP LM Statistic				5.9527**		
KP Wald F Statistic				28.9393		

The table reports the results of regressing the natural logarithm of project cost on Direct Rule (=1) as per the following 2SLS specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{s(j \in s),y} + \theta_{i,y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

$$\text{Direct Rule}_j = \gamma \cdot \text{Ruler Death-No Heir}_j + \theta_{s(j \in s),y} + \theta_{i,y} + \text{Latitude}_j + \text{Longitude}_j + \theta_t + \epsilon_{i,j,t}$$

where i denotes firm announcing a project of size $Y_{i,j,t}$ in district j , located in state s ($j \in s$) at time t during year y . Direct Rule (=1) is a binary variable taking a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. Ruler Death, No Heir (=1) takes a value of 1 if the incumbent Indian ruler dies without a natural heir after 1847 and before 1858. Ruler Death, No Heir, No Lapse (=1) takes a value of 1 if the incumbent Indian ruler dies without a natural heir when the Doctrine of Lapse was not valid – before 1847 and after 1858. Ruler Death, Yes Heir, Lapse (=1) takes a value of 1 if the incumbent ruler dies after 1847 and before 1858 and has a natural heir. The sample for columns 1-4 consists of all districts that were consolidated under direct British rule after 1847 and districts that remained under indirect rule. The sample for columns 5 and 6 include districts that remained under indirect rule. $\theta_{i,y}$ and $\theta_{s(j \in s),y}$ denote firm × year and state × year fixed effects, respectively. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Instrumental Variable Regression: Precolonial Cotton Production

	(1)	(2)	(3)	(4)
Dep Var: ln(Project Size)	IV	2SLS		Falsification
		Second Stage	First Stage	
Direct Rule (=1)		-0.2272* (0.1270)		
Precolonial Cotton (=1)	-0.2073* (0.1166)		0.9120*** (0.1326)	0.0344 (0.0876)
Qtr × Year FE	Yes	Yes	Yes	Yes
Firm × District-Pair × Year FE	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes
# Obs	9,491	9,491	9,491	1,871
R ²	0.7901		0.7786	0.8077
KP LM Statistic			7.3567***	
KP Wald F Statistic			47.3955	

The table reports the results of regressing the natural logarithm of project cost on Direct Rule (=1) as per the following 2SLS specification

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,p(j \in p),y} + \theta_{i,y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

$$\text{Direct Rule}_j = \gamma \cdot \text{Cotton}_j + \theta_{i,p(j \in p),y} + \theta_{i,y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \epsilon_{i,j,t}$$

where i denotes firm announcing a project of size $Y_{i,j,t}$ in district j , located in state s ($j \in s$) at time t during year y . Direct Rule (=1) is a binary variable taking a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. Precolonial cotton (=1) takes a value of 1 if the district produced cotton before annexation. $\theta_{i,p(j \in p),y}$ and θ_t denote firm × district-pair × year and time fixed effects, respectively. Lat/Long indicates controls for latitude and longitude of the district. The falsification test in column 4 uses a sample of bordering indirect ruled district-pairs where one district produced cotton during the precolonial period and other did not. Standard errors reported in parentheses are multiway clustered by native states, district-pair and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Road Construction Delays and Direct British Rule

Construction Delay	(1)	(2)	(3)	(4)
Direct Rule (=1)	0.1016*** (0.0353)	0.1218** (0.0554)	0.0855* (0.0507)	0.1335** (0.0644)
District-Pair × Year FE	Yes			
District-Pair × Cost Pct. × Year FE		Yes		
District-Pair × Length Pct. × Year FE			Yes	
District-Pair × Cost Pct. × Length Pct. × Year FE				Yes
Lat/Long	Yes	Yes	Yes	Yes
Distance Controls	Yes	Yes	Yes	Yes
# Obs	35,656	35,656	35,656	35,656
R^2	0.3226	0.8677	0.8536	0.9687
Mean	0.5533			
Median	0.4483			
Std. Dev.	0.7161			

The table reports the results of regressing variables related to road construction on the direct British rule dummy, per the following specification.

$$Y_{i,j,t} = \beta \cdot \text{Direct Rule}_j + \theta_{p(j \in p),c,l,t} + \text{Latitude}_j + \text{Longitude}_j + X_i + \epsilon_{i,j,t}$$

where $Y_{j,t}$ denotes the outcome variables observed for a village i in district j at time t . $Y_{i,j,t}$ is the road construction delay for the earliest completed new road (natural logarithm of actual construction time of the earliest completed new road minus the natural logarithm of estimated construction time of the earliest completed new road) in column 1, natural logarithm of road construction cost of the earliest completed new road in column 2, and the natural logarithm of the length of earliest completed new road in column 3. Direct Rule (=1) is a binary variable that takes a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. $\theta_{p(j \in p),c,l,t}$ denotes the district-pair × road cost percentile × road length percentile × year fixed effect. Year refers to the year of the sanction date of the earliest completed road. Lat/Long indicates controls for latitude and longitude of the district. X_i refers to distance controls and include distances to the nearest urban areas, i.e., the geographic distance to the nearest town with populations greater than 10,000, 50,000, 100,000 and 500,000 people, respectively. Standard errors reported in parentheses are two-way clustered by native states and district-pair. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Internet Appendix for:

“What Explains Geographic Variation in Corporate Investment?”

Appendix A British Rule in India

British colonial rule over the Indian subcontinent began in 1757 and lasted until 1947. After the decline of the Mughals and the Maratha Empire, India was divided into several small states ruled by royal families, referred to as *princely states*. The British East India Company (East India Company) started its rule in India after defeating the erstwhile Nawab of Bengal, in the Battle of Plassey, in 1757. By 1773, the Company was involved in the direct administration of the areas of present day West Bengal, Bangladesh, and Bihar, with the appointment of the first Governor-General, Warren Hastings. Despite that annexation of India was spearheaded by the East India Company, the crown controlled its activities through various means. The Regulating Act of 1773 set up a system whereby the government supervised and regulated the work of East India Company. Pitt’s India Act of 1774 provided for the appointment of a Board of Control and gave the government ultimate authority. East India Company’s autonomous control of the subcontinent was successively reduced over time.¹⁸ Further, the Charters of 1813 asserted the Crown’s undoubted sovereignty over all East India Company territories, while the 1833 Charter Act invested the Board of Control with full authority over East India Company and increased the power of the governor-general. While the subcontinent was initially under the control of the East India Company, there was a formal transfer of power from the Company to the British Crown after the Sepoy Mutiny of 1857, which marked the end of British expansion in India.

The British Empire consolidated its grip on political control across present-day India, Bangladesh, Burma, and Pakistan in “British India” under two broad policies of annexation namely Subsidiary Alliance and the Doctrine of Lapse. The *ring-fence* period between 1765 and 1818, was the first wave of British annexation, under the leadership of Lord Wellesley, the British Governor-General of India between 1798 and 1805. In this period, the East India Company sought to maintain a ring fence between its territories and the empires of the Marathas and Sikhs. One method of achieving this was the Subsidiary Alliance. Under Subsidiary Alliance, the East India Company

¹⁸Parliament controlled the East India Company by extending its charter 20 years at a time. Those granted in 1793, 1813, 1833, and 1853 successively whittled away the East India Company’s commercial rights and trading monopolies.

entered contracts with princely states, providing the latter with the subsidiary militia for protection and necessary defense for payment. In the event of default by the Indian ruler, a part of their territory was surrendered to the British. The states of Hyderabad (1798), Mysore (1799), Awadh (1801), Peshwa (1802), Bhonsle and Scindia (1803), Udaipur, Jodhpur, and Jaipur (1818) were annexed under the Subsidiary Alliance. The second wave of British annexation, *subordinate isolation*, began in 1818, lasting till the Indian Mutiny of 1857. This phase was marked by the policy of Doctrine of Lapse, under the leadership of its chief architect, Lord Dalhousie, the British Governor-General of India between 1848 and 1856. Under the Doctrine of Lapse, any subordinate princely state would automatically lapse to the British East India Company if the ruler died without a natural heir. The states of Satara (1848), Sambalpur (1849), Baghat (1850), Jaipur of Bundelkhand (1849), Udaipur of Rajputana (1852), Jhansi (1853) and Nagpur (1854) were annexed under Doctrine of Lapse.

Power was transferred from the East India Company to the British Crown after the Sepoy Mutiny of 1857.¹⁹ Certain areas that had been annexed by the British were integrated under direct rule, while remaining areas, were granted indirect rule in which authority was delegated to local princes (princely states). Despite the Queen's proclamation of 1858 ruled out any future annexation, the Governor-General did retain the power to interfere in the internal matters of the princely states in case of a "misrule." Ashton (1977) documents active interference by the British colonial power in the internal affairs of princely states, e.g., Lord Curzon, the Governor-General of India from 1899 to 1905 forced fifteen rulers to abdicate, during his tenure on account of misrule.

Princely states were heterogeneous – some consisted of a few villages, while others, over thousands of square miles. Some princely states had maximum administrative powers while other princely states had minimal administrative powers.²⁰ Iyer (2010) notes that the princely states constituted approximately 45% of the total geographic area of present day India, and 23% of total population in 1911. Additionally, she notes the presence of princely states across India with a high concentration in central and western India. The Foreign Office recognized about 680 Indian princely states in the year 1910.

After Indian Independence in 1947, areas under direct British rule federated. The princely states, however, could choose to join the Indian Union or remain independent. Then Prime Minister

¹⁹This is referred as the Government of India Act 1858 – an Act of the British Parliament, which nationalized the East India Company and granted the British monarch supreme authority over India, as well as power and possessions of the East India Company.

²⁰Princely states had varying degrees of legal autonomy, ranging from first-class, wherein the state could try criminal cases to third-class states whereby only small civil cases could be adjudicated by the ruler.

Jawaharlal Nehru and the Home Minister Sardar Vallabhbhai Patel, integrated all princely states into the Indian Union by 1950. Thereafter, princely rulers no longer maintained sovereignty, but played a major role in post-independence politics. [Allen and Dwivedi \(1984\)](#) document election of several former princes to federal and state-level political offices.

Table A.1: British Annexation - Time and Reason

	Ceded	Conquest	Grant	Lapse	Misrule	Total
Initial Settlement	0	6	3	0	0	9
Ring Fence (1765-1818)	58	114	15	0	3	190
Subordinate Isolation (1819-1856)	5	22	0	27	17	71
Post 1857 Revolt	2	0	0	0	0	2
Direct Ruled	65	142	18	27	20	272
Indirect Ruled						152
Total						424

The table reports the distribution of the number of districts that were invaded in the CapEx sample, over time and by method of annexation. These districts span a total of 17 states in the sample. The modern day districts are defined as of 2014. Time refers to the four periods marked by different policies - (1) the initial settlement period before 1765, (2) the ring fence period from 1765 until 1818, (3) subordinate isolation from 1819 until 1856, and (4) period of no annexation after the Indian Revolt of 1857.

Appendix B Data

B.1 Project Announcement - CapEx Database

Table B.1: Project Announcements by Fiscal Year

Year	# Projects	Total Amt (in tn)	Mean Amt (in bn)	Median Amt (in bn)
1996	588	3.71	6.31	0.53
1997	596	4.02	6.75	1.06
1998	367	4.85	13.20	1.22
1999	392	1.52	3.88	0.60
2000	456	1.83	4.01	0.48
2001	1,089	2.49	2.28	0.20
2002	1,033	2.89	2.80	0.22
2003	1,200	2.68	2.23	0.15
2004	1,478	8.13	5.50	0.40
2005	839	6.11	7.29	0.65
2006	1,154	15.10	13.11	0.84
2007	1,677	24.20	14.43	1.00
2008	1,608	22.00	13.69	1.39
2009	1,682	25.00	14.88	2.39
2010	2,040	26.00	12.73	1.65
2011	2,378	23.00	9.68	1.04
2012	1,902	17.50	9.18	1.00
2013	1,218	10.50	8.60	1.08
2014	1,060	6.66	6.29	1.40
2015	1,374	22.30	16.26	2.23
2016	1,605	13.20	8.19	1.45
2017	1,648	19.80	12.00	0.98
2018	1,436	8.93	6.22	1.24
Total	28,820	272.41	9.45	1.00

The table reports the total number of projects, the mean, the median value of projects (in ₹ billion), and the total value of projects (in ₹ trillion) announced across 17 states by all firms between fiscal year 1996 and 2018.

Table B.2: Project Announcements by Project Size

Project Size	# Projects	Total Amt (in tn)	Mean Amt (in bn)	Median Amt (in bn)
<0.3 bn	8,808	0.97	0.11	0.09
0.3-3 bn	11,088	12.60	1.14	0.98
3-10 bn	4,560	24.40	5.35	5.00
>10 bn	4,364	234.00	53.72	22.74
Total	28,820	272.41	9.45	1.00

The table reports the total number of projects, the mean, the median value of projects (in ₹ billion), and the total value of projects (in ₹ trillion) announced across 17 states by project size between fiscal year 1996 and 2018.

Table B.3: Project Announcements by Project Type

Project Type	# Projects	Total Amt (in tn)	Mean Amt (in bn)	Median Amt (in bn)
New Unit	20,510	208.00	10.16	0.91
Renovation & Modernisation	3,318	12.50	3.76	0.43
Substantial Expansion	4,992	51.50	10.32	2.07
Total	28,820	272.41	9.45	1.00

The table reports the total number of projects, the mean, the median value of projects (in ₹ billion), and the total value of projects (in ₹ trillion) announced across 17 states by project type between fiscal year 1996 and 2018.

Appendix C Robustness

Table C.1: Balanced Panel Analysis: Investment and Direct British Rule

Dep Var: $\ln(1 + I_{j,t})$	(1)	(2)
Direct Rule (=1)	-2.6273* (1.3827)	-1.6556** (0.6892)
State-Qtr-Year FE	Yes	Yes
Sample	All Districts	$I_{j,t} > 0$
# Obs	35,256	17,052
R^2	0.2008	0.2656

The table reports the results of regressing the natural logarithm of one plus the total project cost on the direct British rule dummy as per the following specification:

$$\ln(1 + I_{j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{s(j \in s),t} + \varepsilon_{j,t}$$

where $\ln(1 + I_{j,t})$ denotes the natural log of 1+total investment in district j , located in state s ($j \in s$) at time t . Direct Rule (=1) is a binary variable taking a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. $\theta_{s(j \in s),t}$ denotes state \times quarter \times year fixed effects. We aggregate the firm-location-time investment dataset at location-time level, using total investment announced. We create a balanced panel by assigning the value of 0 to a location-time if no projects are announced in the location-time. Column (1) includes all district-year observations and column (2) includes a district-year observation only if there was at least one project announced there. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Investment and Direct British Rule: Controlling for Religion of Ruler

Dep Var: ln(Project Size)	(1)	(2)	(3)
Direct Rule (=1)	-0.0831** (0.0368)	-0.1021*** (0.0339)	-0.0932** (0.0356)
Maratha Ruler	-0.0330 (0.0398)		-0.0330 (0.0415)
Muslim Ruler	-0.0212 (0.0151)		-0.0375 (0.0302)
Prop Muslim		-0.0249 (0.2007)	-0.0083 (0.2025)
Prop Sikhs		-0.3338 (0.2109)	-0.3783* (0.2225)
Prop Lower Caste		0.0646 (0.1195)	0.0634 (0.1195)
Prop Elites		-0.0742 (0.2140)	-0.0541 (0.2145)
Qtr \times Year FE	Yes	Yes	Yes
Firm \times Year FE	Yes	Yes	Yes
State \times Year FE	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes
# Obs	19,800	19,800	19,800
R^2	0.7305	0.7305	0.7305

The table reports the results of regressing the natural logarithm of project cost on the direct British rule dummy after controlling for district level characteristics, per the following specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \Theta X_j + \theta_{i,y} + \theta_{s(j \in s),y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

where i denotes firm announcing a project of size $Y_{i,j,t}$ in district j , located in state s ($j \in s$) at time t during year y . Direct Rule (=1) is a binary variable that takes a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. X_j refers to a vector of non-time varying characteristics associated with the demographics of the district. Maratha ruler takes a value of 1 if the precolonial ruler belonged to the Maratha empire, the largest empire before the entry of British in India. Muslim ruler takes a value of 1 if the precolonial ruler was of Muslim faith and 0 otherwise. We include the proportion of Muslim, Sikh, lower caste and elites as per 1911 Indian survey. Elites are defined based on castes coded as politically dominant, landowning, or military in the provincial volumes of the 1911 census. $\theta_{i,y}$ and $\theta_{s(j \in s),y}$ denote firm \times year and state \times year fixed effects respectively. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Instrumental Variable Regression: Precolonial Cotton Production

	(1)	(2)	(3)
Dep Var: ln(Project Size)	IV	2SLS	
		Second Stage	First Stage
Direct Rule (=1)		-0.3119*** (0.1078)	
Precolonial Cotton (=1)	-0.1293*** (0.0372)		0.4144*** (0.1119)
Qtr × Year FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes
# Obs	19,800	19,800	19,800
R^2	0.7305		0.6738
KP LM Statistic			10.0881***
KP Wald F Statistic			13.7112

The table reports the results of regressing the natural logarithm of project cost on Direct Rule (=1) as per the following 2SLS specification

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,y} + \theta_{s,y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

$$\text{Direct Rule}_j = \gamma \cdot \text{Cotton}_j + \theta_{i,p(j \in p),y} + \theta_{i,y} + \theta_{s,y} + \theta_t + \text{Latitude}_j + \text{Longitude}_j + \epsilon_{i,j,t}$$

where i denotes firm announcing a project of size $Y_{i,j,t}$ in district j , located in state s ($j \in s$) at time t during year y . Direct Rule (=1) is a binary variable taking a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. Precolonial cotton (=1) takes a value of 1 if the district produced cotton before annexation. $\theta_{i,y}$, $\theta_{s,y}$ and θ_t denote firm × year, state × year, and time fixed effects, respectively. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement for columns (1)-(3) and multiway clustered by native states, district-pair and time of project announcement for columns (4)-(6). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix D Spatial Autocorrelation

We calculate Moran's I statistic to test for global spatial autocorrelation. This provides a gauge of how randomly distributed the direct rule variable is in our analysis. The Moran I statistic measures the interdependency between different regions and ranges from -1 to 1. Under random distribution, the statistic approaches zero, asymptotically. A statistic value above zero reflects positive spatial autocorrelation between districts i and j . A statistic value below 0 reflects negative spatial autocorrelation between districts i and j .

Moran's I statistic is computed as follows:

$$I = \frac{\sum_{i=1}^n \sum_{j=1}^n w_{ij} z_i z_j}{\sum_{i=1}^n z_i^2} \quad (\text{D.1})$$

where n is the number of districts, z_i is an indicator for direct rule in district i – z_i is standardized, w_{ij} denotes the ij^{th} element of a row-standardized weight matrix which uses distance between i and j as weights.

D.1 Construction of Weight Matrix

In the weight matrix, diagonal elements are 0 (the distance between a region and itself is 0).

$$W = \begin{bmatrix} 0 & w_{1,2} & \cdots & w_{1,m-1} & w_{1,m} \\ w_{2,1} & 0 & \cdots & w_{2,m-1} & w_{2,m} \\ \vdots & \vdots & \ddots & \vdots & \vdots \\ w_{m-1,1} & w_{m-1,2} & \cdots & 0 & w_{m-1,m} \\ w_{m,1} & w_{m,2} & \cdots & w_{m,m-1} & 0 \end{bmatrix} \quad (\text{D.2})$$

In our analysis, we use two types of spatial weight matrices: inverse weighting and uniform weighting.

D.1.1 Inverse Weights

Inverse weights between districts i and j are constructed in the following way.

$$w_{ij} = \begin{cases} \frac{d_{ij}^{-\delta}}{\sum_{j=1}^n d_{ij}^{-\delta}} & d_{ij} < d, i \neq j, \delta > 0 \\ 0 & otherwise \end{cases} \quad (D.3)$$

where d is a distance threshold, which we vary from 10 miles to 200 miles to study the robustness of the statistic. δ is a decay parameter for distance. It takes the value 1.

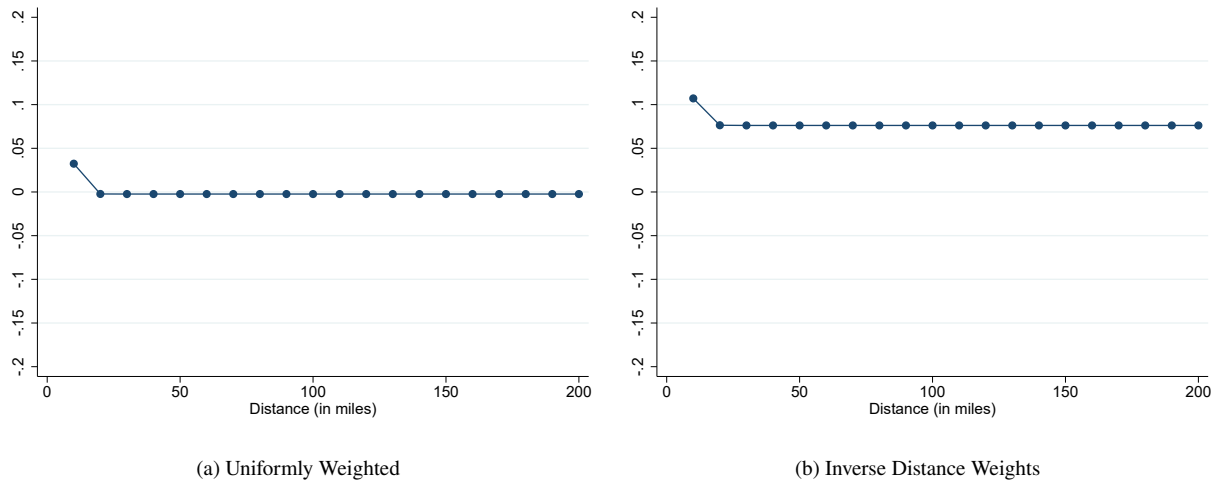
D.1.2 Uniform Weights

Uniform weights between i and j are constructed in the following way: $w_{ij} =$

$$\begin{cases} \frac{\mathbb{1}_{d_{ij} < d}}{\sum_{j=1}^n \mathbb{1}_{d_{ij} < d}} & d_{ij} < d, i \neq j \\ 0 & otherwise \end{cases}$$

where $\mathbb{1}_{d_{ij} < d}$ is an indicator function that takes the value 1 if the distance between districts i and j is below the threshold distance d and 0, otherwise.

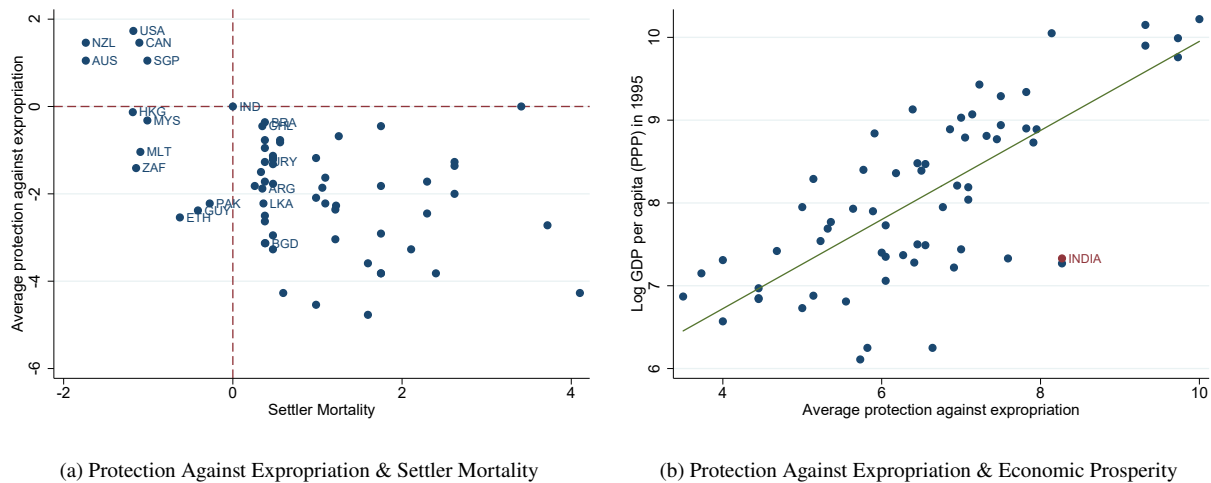
Figure D.1: Spatial Autocorrelation - Moran I statistic



The figure plots the Moran I statistic for the Direct British Rule variable. The Moran I is computed for different distance thresholds from 10 miles to 200 miles. Figure D.1a uses the uniform weight between two points if they are within the distance limit and 0 otherwise. The weight takes a value of 1 if the two points are within the threshold and 0 otherwise. Figure D.1b uses the weights calculated as the inverse of the distance between two points if they are within the distance limit and 0 otherwise. Weights are standardized such that they add up to 1 within a row.

Appendix E Alternative Explanations:

Figure E.1: Protection Against Expropriation - [Acemoglu, Johnson and Robinson \(2001\)](#)



The figure replicates the two key results presented in [Acemoglu, Johnson and Robinson \(2001\)](#) for their sample. Panel a shows the scatterplot of average protection against expropriation with settler mortality. We standardize the data such that the two measures take a value of zero for India. Panel b shows the scatterplot of log GDP per capita (PPP) in 1995 against the average protection against expropriation with the best-fit line. The red scatter dot in panel b denotes India.

Table E.1: Court Delays and Direct British Rule

Dep Var: ln(Mean Delay)	(1)	(2)	(3)
Direct Rule (=1)	-0.0025 (0.0245)	-0.0025 (0.0245)	-0.0025 (0.0243)
Statute \times Year FE	Yes	Yes	
District-Pair \times Year FE	Yes	Yes	
Statute \times District-Pair FE		Yes	
Statute \times District-Pair \times Year FE			Yes
Lat/Long	Yes	Yes	Yes
# Obs	180,580	180,580	180,580
R^2	0.6155	0.7204	0.8077

The table reports the results of regressing the natural logarithm of district level mean delay in cases by statutes on the direct British rule dummy using a sample of bordering districts within states as shown in Figure 6a, per the following specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_i + \theta_{j,p(i \in p),t} + \text{Latitude}_j + \text{Longitude}_j + \varepsilon_{i,j,t}$$

where i denotes a district with a mean duration of $Y_{i,j,t}$ for statute j during year t . Direct Rule (=1) is a binary variable that takes a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. $\theta_{j,p(i \in p),t}$ denotes the statute \times district-pair \times year fixed effect. Lat/Long indicates controls for latitude and longitude of the district. Data on court records comes from Ash et al. (2021). The dataset draws from 80 million court records spanning 2010-2018, sourced from the Indian e-Courts platform – an online database implemented by the Government of India, detailing information on the universe of India's 7,000+ district and subordinate trial courts and over 80,000 judges. The Indian e-Courts database provides comprehensive coverage of India's lower judiciary, consisting of District and Session courts. Our focus of the data is on the delay of a case. Delay is the time that a court takes to resolve a case. It is measured as the number of days between the initial filing date and the decision date. At the district-level, the median duration of a case is 241 days. There is a large heterogeneity in case delay. The 25th and 75th percentiles of case duration are 28 days and 746 days, respectively. Moreover, the total number of cases also exhibit a large heterogeneity. Standard errors reported in parentheses are two-way clustered by native states and district-pair. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Public Goods: We obtain data on public goods from the Population Census of 1991, 2001 and 2011, as reported in the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG). This dataset provides detailed information on public goods. It reports the number of schools at various levels, including primary, middle, lower secondary, and senior secondary at the village and town level, as well as the prevalence of rural electrification. We aggregate this data at the district-year level to compute the number of schools per 1,000 people and the fraction of villages with electricity connection.

Table E.2: Provision of Public Goods: Schools & Electricity

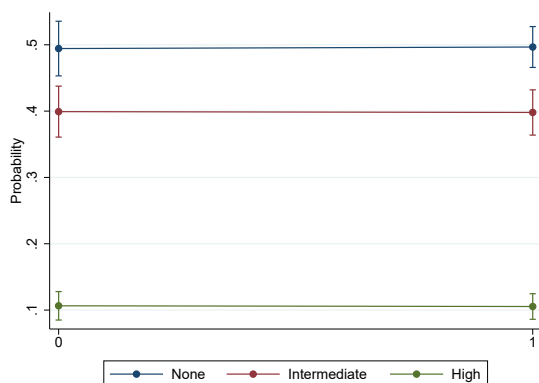
	(1)	(2)	(3)	(4)	(5)
	Total School	Primary School	Middle School	High School	Electricity
Direct Rule (=1)	-0.0844 (0.0887)	-0.0536 (0.0601)	-0.0248 (0.0187)	-0.0059 (0.0128)	0.0114 (0.0115)
District-Pair \times Year FE	Yes	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes
# Obs	1,026	1,026	1,026	1,026	1,026
R^2	0.7800	0.7568	0.8272	0.8429	0.9317
Mean	1.7380	1.0845	0.3978	0.2557	0.7443
Median	1.5170	0.9248	0.3412	0.2133	0.9763
Std. Dev.	1.0615	0.6872	0.2714	0.1955	0.3278

The table reports the results of regressing measures of public goods on the direct British rule dummy, per the following specification.

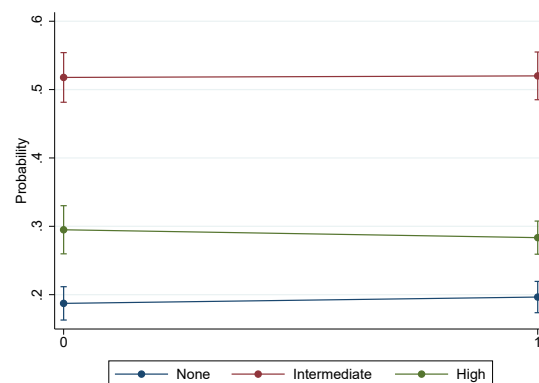
$$Y_{j,t} = \beta \cdot \text{Direct Rule}_j + \theta_{p(j \in p)t} + \text{Latitude}_j + \text{Longitude}_j + \epsilon_{j,t}$$

where $Y_{j,t}$ denotes the outcome variables observed for a district j at time t . $Y_{j,t}$ is the number of schools per 1,000 people in column 1, number of primary schools per 1,000 people in column 2, number of middle schools per 1,000 people in column 3, number of secondary and senior secondary schools per 1,000 people in column 4, and the fraction of villages with electricity connection in column 5. Direct Rule (=1) is a binary variable that takes a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. $\theta_{p(j \in p)t}$ denotes the district-pair \times year fixed effect. Lat/Long indicates controls for latitude and longitude of the district. The unit of observation is district year and the data on schools and electricity connection comes from the Census data for 1991, 2001 and 2011. Standard errors reported in parentheses are two-way clustered by native states and district-pair. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

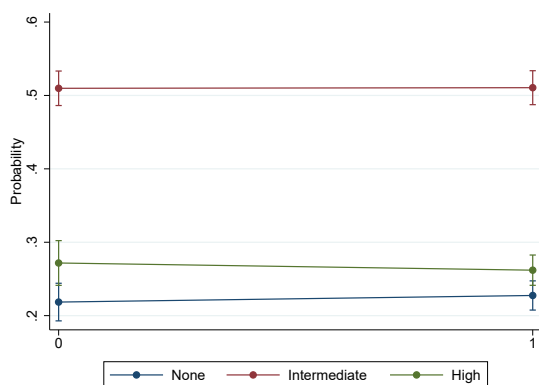
Figure E.2: Trust in State Apparatus



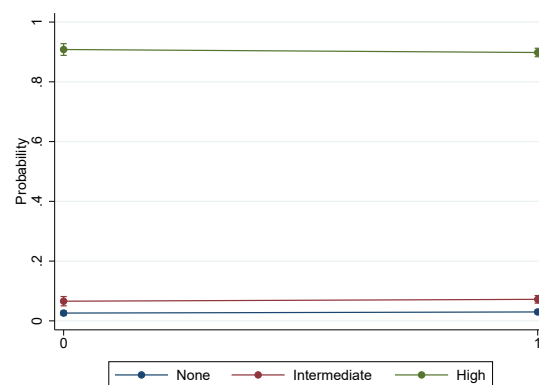
(a) Trust in Politicians



(b) Trust in Panchayats



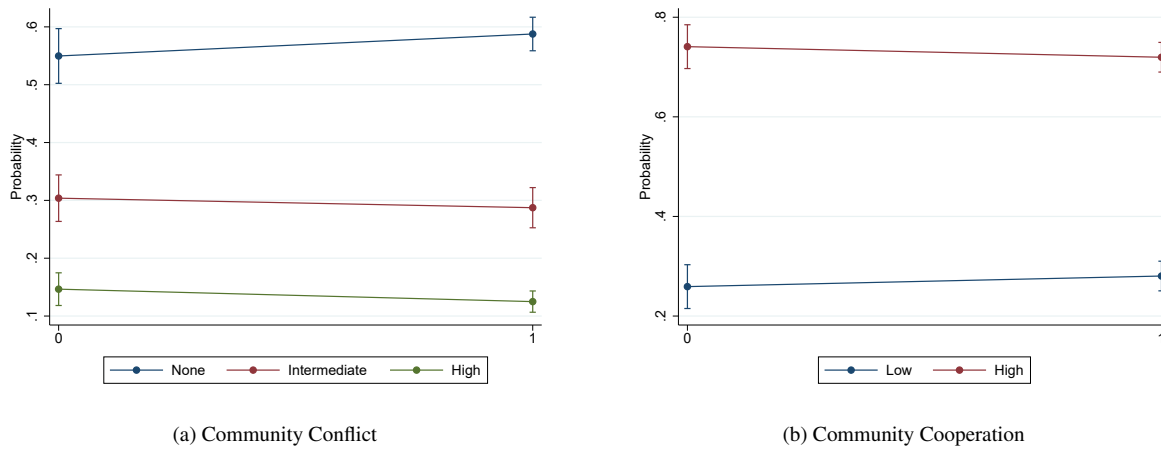
(c) Trust in Police



(d) Trust in Banks

The figure plots the average probability of response for trust in different segments of the state apparatus. The average probabilities are computed using an ordered probit regression. We control for the gender, age, religion/caste of the respondent and account for the location being urban or metro. In addition, we include state fixed effects. 0 denotes indirect rule and 1 denotes direct rule. The respondents are asked - "How much confidence do you have in X?" X denotes politician, panchayats, police and banks. A respondent can either say - (1) *No Confidence* coded as "None", (2) *Intermediate level of confidence* coded as "Confidence," or (3) *High level of confidence* coded as "High." The sum of the average three probabilities within a location add up to 1.

Figure E.3: Community Cooperation & Conflict



The figure plots the average probability of response for general conflict and community cooperation. The average probabilities are computed using an ordered probit regression. 1 denotes direct rule and 0 denotes indirect rule. We control for the gender, age, religion and caste of the household head and account for the location being urban or metro. In addition, we include state fixed effects. The respondents are asked - “How much general conflict is their in your neighbourhood or village?” for panel E.3a. A respondent can either say - (1) *No Confidence* coded as “None”, (2) *Intermediate level of confidence* coded as “Intermediate,” or (3) *High level of confidence* coded as “High.” The sum of the average three probabilities within a location add up to 1. The respondents are asked - “How much general cooperation is their in your neighbourhood or village?” for panel E.3b. A respondent can either say - (1) *High* coded as “High” or (3) *Low* coded as “Low.” The sum of the average the two probabilities within a location add up to 1.