The Long Shadow of Colonialism and the Geography of Corporate Investment*

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Abstract

This paper investigates the historical roots of geographic investment concentration using variation in British colonial governance in India: direct versus indirect rule. States with a higher share of direct-ruled districts exhibit greater investment concentration, with historical differences accounting for 13% of spatial variation. This concentration is driven by lower investment in direct-ruled areas. Using a border district-pair design and an IV strategy, we find that investment is 8–10% lower in direct-ruled areas. Focusing on the cotton industry, we show that direct rule disrupted well-established local industries and hindered human capital accumulation, leading labor-intensive firms to invest less.

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1 Introduction

What explains geographic concentration of investment? From Steel City to Silicon Valley, the formation of economic clusters has captured the attention of academics and policymakers alike, raising questions about the forces that shape and sustain these spatial patterns. Existing research has largely focused on geographic advantages and agglomerative forces – such as the availability and cost of production factors – as drivers of cluster formation and persistence (see Glaeser and Gottlieb (2009) for a survey). This paper departs from this tradition and attempts to examine the role of traumatic historical events in shaping current investment patterns. Specifically, this paper investigates a specific historical mechanism: the disruptive legacy of colonialism and its long-run impact on the business environment. Addressing this question is crucial because investment can drive economic development and long-term income levels (King and Levine, 1993; Levine, 1997; Rajan and Zingales, 1998). Therefore, understanding how historical disruptions shape the geography of investment can inform the micro-foundations of geographic inequality – specifically, why certain regions become wealthier than others and which policies can effectively address these disparities (Moretti, 2024).

We examine how colonial-era policies designed to protect industries in the colonial state contributed to the decline of well-established industries in the colonized regions. These policies have long-lasting effects by impeding human capital accumulation, thereby undermining the availability of skilled labor, one of the foundations necessary for investment. In doing so, we argue that historical events can shape investment outcomes, not merely through institutional channels, such as property rights (North, 1991) or endowments (Levine, 2005), but also by undermining the very agglomerative forces of Marshall (1920) and locational fundamentals of Davis and Weinstein (2002) that facilitate investment and growth. Therefore, this paper introduces a novel mechanism that links traumatic historical shocks to contemporary geographic disparities in investment.

We study how historical differences shape contemporary geographic variation in investment in the context of India, which provides an ideal setting for two key reasons. First, its colonial history offers a natural experiment to examine spatial differences in investment. Prior to British rule, which began in 1757, the Indian subcontinent was governed by local rulers. During the colonial period, the British assumed direct administrative and economic control over certain regions, referred to as "direct-ruled" areas, while others were governed indirectly through native rulers, known as "indirect-ruled" areas. Following independence in 1947, all regions, regardless of their colonial history, adopted a uniform legal and administrative framework. We verify that areas under direct and indirect rule were broadly similar across several observable dimensions before British colonization, creating a unique opportunity to study historical variation within the same present-day state. Second, we combine this historical variation with granular project-level data, allowing us to compare investment projects announced by the same firm across different regions during the same year. This approach allows us to examine relationship between historical differences and contemporary investment patterns.

Identifying the relationship between historical events and present-day investment presents two key challenges. First, the differences in present-day de-jure rule of law may be correlated with these historical events. We address this concern by conducting a within-state analysis that exploits variation in colonial administration – direct versus indirect rule – while holding constant state-level legal institutions, since all jurisdictions within a state are subject to the same legal framework. Second, regions under direct and indirect rule may differ systematically along unobservable dimensions. We address this concern using a border district-pair design, which compares neighboring areas to control for local investment shocks and mitigate omitted variable bias, such as geographic or endowment-based differences. Additionally, we implement an instrumental variable strategy to account for potential selection bias, i.e., systematic differences across regions that may be associated with areas being under direct or indirect rule. Together, these approaches strengthen our empirical strategy in addressing the identification challenges inherent in linking historical events to contemporary economic activity, particularly investment. Specifically, our empirical approach enables us to isolate the effects of traumatic historical disruptions from institutional factors and locational fundamentals on present-day investment.

We begin by documenting that investment is concentrated within Indian states. We compute a state-level measure of investment concentration, using the Herfindahl-Hirschman Index (HHI). We compare the investment HHI with three benchmark measures – (1) equal investment in all districts, $\frac{1}{N}$; (2) investment proportional to the geographical area of the 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. We confirm these findings using alternative measures of concentration, such as Zipf slope coefficient and Theil index. Specifically, the level of the within-state investment concentration is 20 percentage points higher relative to any of the benchmarks. Moreover, the proportion of districts under direct rule can explain 13% of total variation in within-state investment concentration.

We document that states with a greater 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 from 1996 through 2018 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 × year and state × 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 legal traditions, differences in the distances to the state capitals, the religion of pre-colonial rulers, and spatial trends. Moreover, we verify that the direct and indirect-ruled areas are similar across several observable characteristics, such as geography, distance from the state capital, and colonial population composition. Further, using a placebo analysis, we argue that our results are unlikely to be spurious.

While these tests are informative and increase confidence in our results, we cannot entirely rule out the possibility that direct- and indirect-ruled districts differ systematically along other unobservable dimensions. As a result, directly comparing all such districts risks introducing selection bias or unobserved confounding variables, potentially undermining the validity of the analysis. We address this concern by focusing on contiguous districts within the same state that are separated solely by administrative borders, where one district was historically under direct British rule and the neighboring district was under indirect British rule. This approach leverages the shared geographical characteristics and similar exposure to investment opportunity shocks of bordering districts. Importantly, these adjacent districts likely would have followed comparable trajectories absent colonization. The key advantage of this design is its ability to control for smoothly varying unobservable factors that could otherwise bias our estimates.

Our empirical strategy employs a border district-pair design, comparing investments by the same firm across such district pairs. Specifically, we include firm × district-pair × year fixed effects. The fixed effects structure ensures that we identify the differences in investment by the same firm, across adjacent districts within the same year, thereby abstracting away from the confounding factor of non-random matching of firms to districts. Overall, this approach ensures that our analysis more accurately captures the relationship between direct rule and firm investment without confounding influences. Our district-pair 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 the 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 employed previously by Iyer (2010). Between 1848 and 1856, the Doctrine of Lapse 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 squares 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 provide evidence that supports this assumption through two falsification tests. In the first 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 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. The first test addresses the concern that the death of the ruler may be attributable to other features, such as environmental factors or genetics, which may directly affect postcolonial investment. The second test addresses the concern that the death of the ruler between 1848 and 1856, regardless of the presence of a natural heir, may reflect geography-specific conditions during that period which can affect long-term outcomes through channels other than direct rule.

We propose a novel mechanism through which direct British rule influences investment in the present: the disruption of well-established local industries. We show that this channel has long-term persistent effects through disrupted patterns of human capital accumulation. This channel highlights how shocks to local industries can affect the geographic concentration of investment over time.

We document that the East India Company (EIC) – a colonial trading enterprise with administrative authority – consolidated economic power by annexing key cotton-producing territories in India. We find that a cotton-producing district was 40% more likely to be under direct British rule, than under indirect rule.

During the 18th century, Indian textiles dominated the global market, accounting for 25% of the world textile trade (Maddison, 1995). This supremacy posed a direct threat to the British textile industry.

By taking direct control of Indian cotton-producing regions, the British secured a strategic advantage in global trade. Direct rule enabled them to procure raw cotton at extremely low prices, often as the sole buyer in these areas, while simultaneously prohibiting exports of finished Indian cotton textiles to other countries (Alavi, 1982; Mukund, 1992; Sahoo, 2015; Nakatomi, 2008). This dual strategy allowed British textile mills to access cheap raw materials while eliminating a significant competitor in the global market (Mill, 1848). As a result, 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 (Bolts, 1775; Gandhi, 1921; Gopal, 1965; Avineri, 1969; Cypher, 2008; Sahoo, 2015). The disruption of flourishing and dominant economic industries resulted in significant economic losses, borne by the native population.

We use the invention of Whitney's cotton gin in 1794 as a natural experiment to study how the incentives of the British to dismantle the Indian cotton textile industry changed with the widespread adoption of the cotton gin in the United States. Whitney's cotton gin greatly improved the efficiency of separating cotton fibers from seeds, but it was only effective for short-staple cotton. Since the cotton grown in India was of the long-staple variety, the British textile industry switched its primary raw cotton supply from India to the U.S. Therefore, because the invention of the cotton gin made the U.S. the primary supplier of raw cotton, the British had weaker incentives to dismantle the Indian cotton textile industry.

Combining the timing of the widespread adoption of the cotton gin by 1800, with historical data on Indian domestic exports of agricultural commodities from 1884 until 1920, we document that the domestic cotton exports of provinces annexed before 1800 are lower than the domestic exports of provinces annexed after 1800. Moreover, we note that this effect is not driven by a lower likelihood of cotton-producing areas falling under direct British rule after 1800. This finding suggests that British incentives tied to the global textile trade played a central role in undermining the Indian cotton industry, and the removal of these incentives lessened the overall economic harm.

We show that these economic effects have persisted to the present. 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 in the present. Moreover, we find that this effect is largely driven by lower investment in direct-ruled districts that were annexed before 1800, when British incentives to dismantle the existing cotton industry were stronger.

We posit that the long-term consequences of the disruption of existing local industries can be attributed to its effect on human capital accumulation. 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 disruption 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. While one might expect such economic disruption to incentivize workers to develop new skills, the cotton textile workforce, predominantly from lower-caste communities, faced significant social and economic impediments that limited their ability to transition into new professions (Broadberry and Gupta, 2005).

We provide empirical evidence in support of this hypothesis. Using education outcomes and self-reported interest in education as proxies for human capital acquisition, we find that individuals from lower castes in direct-ruled districts that produced cotton during the pre-colonial period and were annexed *before* the invention of the Whitney gin experienced lower levels of human capital accumulation compared to their counterparts in similar districts annexed *after* its invention. Specifically, using survey data on necessity and interest in education, our results suggest that economic disruption can have long-term effects by diminishing the motivation to invest in human capital, particularly among marginalized groups that face structural barriers to mobility and adaptation.

To further explore this mechanism, we examine heterogeneity in firm-level investment, focusing on firms with varying degrees of labor intensity. The rationale is that if populations in regions formerly under direct British rule are less likely to acquire basic education or skills, then firms that are more dependent on labor are less inclined to invest in those areas. Consistent with this hypothesis, we find that firms operating in labor-intensive industries are significantly less likely to invest in districts subject to direct British rule, compared to contiguous districts that were not under direct rule. This result is robust to the inclusion of district $\times$ year fixed effects, in addition to firm $\times$ district-pair $\times$ year fixed effects. Overall, the evidence supports our hypothesis that the enduring legacy of British colonial rule continues to shape contemporary investment patterns, primarily through its impact on the availability of skilled labor, a key agglomerative force.

Lastly, we examine three other potential mechanisms. First, we consider whether persistent differences in legal regimes or their enforcement could explain our results (Porta et al., 1998; La Porta et al., 1999; Acemoglu, Johnson, and Robinson, 2001; Glaeser and Shleifer, 2002). This explanation is unlikely, as our estimation strategy compares direct and indirect-ruled districts within the same state, where contract enforcement and property rights laws are uniformly applied, regardless of historical governance. Moreover, corporate disputes are adjudicated by state-level high courts or debt recovery tribunals, allowing us to control for variation in law enforcement at the firm level. To further address this concern, we conduct tests within districts that were all under direct British rule, thereby fixing historical institutions. Additionally, data on the time taken by local district courts to resolve criminal cases reveal no significant differences in law enforcement across districts.

Second, we investigate whether our results may be driven by differences in the persistence of public investment, as suggested by previous research (Huillery, 2009; Dell and Olken, 2020; Lowes and Montero, 2021; Méndez and Van Patten, 2022). This explanation is unlikely, as postcolonial governments implemented equalization policies aimed at reducing disparities in public goods provision (Banerjee and Somanathan, 2007). Iyer (2010) documents substantial convergence in public goods provision across districts by 1991. We document no systematic differences in the provision of public goods, measured by the number of schools and access to electricity between direct- and indirect-ruled districts during our sample period. These findings indicate that disparities in public investment, particularly those related to education, are unlikely to explain our results.

Third, we study whether our results may be driven by differences in the persistence of trust due to traumatic historical events faced by direct ruled areas (Nunn and Wantchekon, 2011). Using survey data on trust in state agents, such as politicians, police, and panchayats (elected village governments), we find no evidence of systematic differences in trust across direct and indirect-ruled districts. This suggests that mistrust stemming from direct colonial rule is unlikely to explain our results.

Related Literature: The key contribution of this paper to the literature is examining the determinants of geographic concentration of economic activity. 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 the emergence of clusters.[1] We add to this literature by introducing a novel aspect that can explain the geographic variation of private investment: historical disruptions, stemming from colonial rule. Specifically, our analysis comparing directly ruled districts annexed at different points in time allows us to compare districts with a shared historical legacy but varying intensities of traumatic historical shocks, driven by shifts in British incentives to dismantle well-established local industries. Therefore, our empirical results complement the theoretical framework of Krugman (1991) which establishes the role of shocks to a "core" region's agglomeration advantages in producing persistent spatial divergence.

While a large literature since North (1981) focuses on the role of history in explaining present-day outcomes, this literature has focused on the role of colonial occupation in shaping present-day institutions or property rights that subsequently affect other economic outcomes.[2] We contribute to this literature

[1] See e.g., Ellison and Glaeser (1997); Duranton and Puga (2004); Duranton and Overman (2005); Glaeser and Gottlieb (2009); Ellison, Glaeser, and Kerr (2010); Glaeser, Kerr, and Kerr (2015).

[2] See Porta et al. (1998); La Porta et al. (1999); Acemoglu, Johnson, and Robinson (2001); Glaeser and Shleifer (2002); Banerjee and Iyer (2005); Nunn (2008); Berger (2009); Huillery (2009); Iyer (2010); Nunn and Wantchekon (2011); Dell (2010); Bobonis and Morrow (2014); Donaldson (2018); Méndez and Van Patten (2022); Dell and Olken (2020); and Lowes and Montero (2021), among others. See North (1991), Levine (2005), Spolaore and Wacziarg (2013), Acemoglu, Gallego, and Robinson (2014), and Nunn (2014) for a detailed review of this literature. Also, see Fenske, Gupta, and Mukhopadhyay (2025) and Guardado (2023) for recent reviews of the literature on the long-run economic legacies of colonialism.

by highlighting the impact of traumatic historical shocks on the disruption of well-established local industries. Specifically, we demonstrate that colonial disruptions can undermine the development of skills and knowledge. Importantly, this economic disruption channel can affect human capital accumulation and explain the geographic variation in investment today, even in the absence of differential institutional effects. Our findings resonate with the theoretical model of Gennaioli et al. (2013) and complement their cross-country analysis. Additionally, our results support the notion that human capital accumulation is influenced by the surrounding environment, as suggested by Bell et al. (2019). By focusing on cotton-producing regions within direct-ruled districts in the same state, we control for both historical and present-day institutional quality, addressing the challenge of disentangling the effects of institutions and human capital accumulation highlighted by Acemoglu, Gallego, and Robinson (2014) and Goldin (2024). Collectively, our results emphasize that colonialism itself can significantly shape agglomerative forces and regional investment patterns, independent of its effects on property rights and institutions.

Our paper is closest to Iyer (2010), which examines the long-term consequences of direct colonial rule in India. However, we highlight a new channel not discussed in Iyer (2010) – colonial disruption of well-established local industries that can explain the long-term 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 and attributes these differences to the persistence of institutions. However, 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.

Our work contributes to the burgeoning literature on history and finance, which leverages natural experiments from the past to explain current financial outcomes (see D'Acunto (2017) and Klüppel, Pierce, and Snyder (2018) for a survey and D'Acunto (2018) for a list of other works). In particular, our study relates to D'Acunto (2019), which uses cross-country data and distance from Mainz, Germany – the birthplace of the Gutenberg printing press – as an instrument for historical literacy to show that European regions with higher literacy levels have more manufacturers filing patents. However, we differ from D'Acunto (2019) in two key ways. First, our unique project-level data allows for better identification of the effect from within-firm variation across neighboring districts. Second, our findings shed light on a different channel through which colonial regimes influence contemporary economic outcomes. Specifically, our economic disruption channel adds to other well-documented mechanisms such as mistrust (Nunn and Wantchekon, 2011), infrastructure development (Berger, 2009; Donaldson, 2018;

Dell and Olken, 2020), health and education infrastructure (Huillery, 2009; Méndez and Van Patten, 2022), legal regimes affecting property rights (Porta et al., 1998; La Porta et al., 1999; Acemoglu, Johnson, and Robinson, 2001; Glaeser and Shleifer, 2002; Banerjee and Iyer, 2005; Iyer, 2010), and civic capital (Guiso, Sapienza, and Zingales, 2004, 2008, 2011, 2013).

Lastly, our work is related to D'Acunto, Prokopczuk, and Weber (2019) indicating the effect of historical differences on social attitudes. That paper shows that households in German counties with higher historical antisemitism exhibit lower trust in the financial sector in the present. Similarly, Pierce and Snyder (2018) finds that present-day firms in African countries with higher historical slave extraction face lower access to formal and informal credit. Distinct from this mistrust in institutions channel, we contribute to this literature by documenting that lower-caste individuals are less likely to value human capital accumulation today in regions where their traditional occupations were disrupted. We show that this mechanism, in turn, helps explain the spatial distribution of private investment, especially investment by labor-intensive firms.

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 evidence from bordering districts. Section 6 presents the results from the instrumental variable regressions. Section 7 discusses the mechanism. Section 8 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 describe the history 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 and lasted 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.[3] 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 the active interference of the British colonial power in the internal affairs of princely states. For example, during his tenure as governor-general of India from 1899 to 1905, Lord Curzon forced fifteen rulers to abdicate on the grounds 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 constituted approximately 45% of the total geographic area of present-day India, and 23% of the 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.

[3] This is referred to 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.

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 under Misrule* incentivized princely states to provide better governance and institutions. Princely states had between four and 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 the development of better early institutions and governance policy in indirect-ruled areas, resulting in greater state efficiency. Iyer (2010) documents persistence of the differences in public goods provision between direct and indirect-ruled districts post-independence. However, she notes that these differences diminish substantially over time, exhibiting convergence. Using data from 1971 to 1991, Banerjee and Somanathan (2007) also documents convergence in public goods provision across regions, consistent with policies of equalization followed by postcolonial governments.[4] We verify that the difference in public goods provision is muted for our sample period.

Economically, areas under direct colonial rule faced the brunt of protectionist policies, restrictive contracts, and exploitative practices, often leading to the disruption of well-established local industries. These factors are discussed in the context of cotton production in section 7, although the mechanism may apply in other contexts as well.[5]

2.3 What Determines Direct Rule?

In this section, we explore whether direct rule can be explained by a multitude of factors, such as geographic features, the distance to the state capital, the religion of the precolonial ruler, and the 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 for the presence of a Muslim ruler. The likelihood of a territory being under direct rule increases if the area was ruled by a Muslim

[4] Specifically, Banerjee and Somanathan (2007) document that in the two decades after 1971, there was a dramatic expansion in rural infrastructure. By 1991, primary schools were available in nearly 74% of all villages. Over the two decades, the share of villages with electricity connections increased from 18% to 70% and there was a nine-fold increase in access to piped water.

[5] The EIC became the "de facto ruler of Bengal, where it established a monopoly in grain trading and prohibited local traders and 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.

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. This data has been previously employed in Alok and Ayyagari (2020) to study the association between the electoral cycle and the corporate investment cycle. Naaraayanan and Wolfenzon (2024) uses this database to study the relationship between investment and affiliation.

The CapEx database serves as the source of the 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 (≈ $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 identity, project location (district and state), project size, and date of project announcement. This dataset also allows for tracking of the project outcomes.

The data employed in this paper covers project announcements across all states for the universe of all firms in India spanning between fiscal years 1996 and 2018.[6] 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.[7] 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 C presents an overview of the sample in terms of its distribution over time, amount, and project type.

[6] A 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.

[7] The 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.

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.[8] 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 the 2014 administrative state boundaries. Direct-ruled areas consist of regions under the direct administrative rule of the British until 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 central regions. Appendix Table A.1 shows the distribution of British acquisition of direct-ruled districts over time and by reason. 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 bilateral trade from Donaldson (2018). This data reports the disaggregated physical quantities of commodities shipped between two trade blocks. A trade block spans three to five districts. Four of the trade blocks are major ports of colonial India: Bombay, Calcutta, Karachi, and Madras. The trade flow data presents the final shipments between two regions. We use this data to study how the total volume of cotton trade changed through the turn of the 19th century.

[8]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.

3.3 Other Datasets

We utilize data on human capital accumulation from the first wave of the India National Demographic and Health Survey (DHS) (1998-99). Specifically, we use data on education outcomes and self-reported interest in education, including highest educational attainment, years of schooling, literacy status, and the main reason for never attending school.

To rule out alternative explanations, we make use of (1) data on electrification from the Population Census of 1991, 2001, and 2011 (Asher et al., 2021); (2) data on criminal court records between 2010 and 2018 from the Data Development Lab Judicial Data Portal (Ash et al., 2023); and (3) data on trust in state agents from the 2011 Indian Human Development Survey (IHDS).

4 Results

This section describes the empirical methodology and key results. 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

This section presents evidence on the concentration of corporate investment. We choose the state as the unit of analysis as the law is fixed at the state level.

4.1.1 Geographic Concentration of Investment

We begin our empirical analysis by examining the geographic concentration of corporate investment. The Herfindahl-Hirschman Index (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 HHI as a measure of within-state investment concentration. It is calculated by summing the squared share of each district's investment as a proportion of the state's total investment in a given year. We use the 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 the 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 calculated by weighting each state-level area HHI by the area of the state relative to the country. This measure assumes that a district's share of state-level investment is proportional to its share of the state's total area. Third, state-level population HHI is computed as the squared sum of the share of the 2001 district-level population within a state. The aggregate population HHI is calculated by weighting each state-level population HHI by the population of the state relative to the country. This measure assumes that a district's share of state-level investment is proportional to its share of the state's total population. 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 points higher than any of the benchmark measures.

Alternative Measures of Concentration: We complement this analysis by examining the geographic concentration of investment using two other measures prominently employed in urban economics: the Zipf slope coefficient and the Theil index. To compute the Zipf slope coefficient, we follow Gabaix (1999a,b). We first rank districts within each state-year based on their investment levels. We then perform a regression of the natural logarithm of these ranks on the natural logarithm of investment amounts. The resulting regression coefficient, the slope (usually a negative estimate), serves as the Zipf slope coefficient, capturing the intensity of concentration in investment within a state-year.

The magnitude of the Zipf slope provides insight into the degree of geographic variation in investment. Specifically, a smaller magnitude (less negative values) of the Zipf slope indicates greater concentration. For instance, if investment were nearly uniform across districts, the Zipf slope would approach negative infinity; if all investment were concentrated in a single district, the slope would be closer to zero (Davis and Weinstein, 2002). Figure 4a presents the time series of the average and weighted Zipf slope coefficients from 1996 to 2018. Throughout this period, the Zipf slope remains relatively stable, with an average (median) value of -0.380 (-0.384), suggesting high levels of geographic concentration in investment over time. For comparison, we also estimate the Zipf slope coefficient using population data instead of investment. The resulting median value is -1.17, which is close to the typical expected value of -1 under the assumptions of Gibrat's Law (Eeckhout, 2004). This indicates that investment is considerably more concentrated than what would be implied by the distribution of population alone.

The Theil index is computed annually at the state level using the ratio of each district's total investment to its state's average investment. Specifically, the Theil index is calculated as $T = \sum_i \frac{x_i}{\mu} ln(\frac{x_i}{\mu})$, where x_i is the total investment in district i and μ is the average investment across districts in the state year. For ease of interpretation, we transform the Theil index into the Atkinson Index, which has a range between 0 and 1, where 0 indicates uniform distribution and 1 indicates the highest level of concentration. Figure 4b presents the time series of the average and weighted Theil index from 1996 to 2018. The index has an average (median) value of 0.765 (0.793), suggesting high levels of geographic concentration in investment.

Overall, the two alternative measures echo the results from the analysis based on HHI measures, i.e., investment is geographically 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 5 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 the proportion of districts under direct British rule.

Table 2 extends the analysis presented in Figure 5 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 positive 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 robust.

Economically, the estimate indicates that a one standard deviation increase in the percent of districts under direct British rule, corresponding to a 28 percentage points increase, is associated with a 0.12-0.15 standard deviation increase in investment concentration, relative to the mean. This corresponds to a 2.8-3.6 percentage points increase in investment concentration which is equivalent to 10.6-13.6% increase relative to the mean. Moreover, our key explanatory variable, the percent of districts under direct British rule, can explain 13% of the total variation in investment concentration.

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 primary advantage of using project-level data is the ability to control for all time-varying firm-level heterogeneity by including firm × year fixed effects, allowing for a comparison of the same firm's investment across direct and indirect-ruled districts in the same year.

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 presents the baseline specification:

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct rule}_j + \theta_{i,y} + \theta_{s(j \in s),y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t} \quad (1)$$

$ln(Y_{i,j,t})$ 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 × year fixed effects controlling for all time-varying observed and unobserved heterogeneity at the firm level. $\theta_{s(j \in s),y}$ denotes state × 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. We also control for the latitude and the longitude of the district to capture spatial trends (Dell, 2010; Michalopoulos and Papaioannou, 2016), spatial correlation in business cycles coming from the relation between distance to the equator and economic performance (Acemoglu, Johnson, and Robinson, 2001), and social infrastructure (Hall and Jones, 1999).

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 (95 clusters) and time (92 clusters) level. Additionally, we report Conley (1999) standard errors that are adjusted for spatial dependence within 100 km to ensure that our inference is not contaminated by spatial noise.

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 direct-ruled district relative to an indirect-ruled district.

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. The estimate of β is negative and statistically significant across all columns. 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.

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. Economically, this 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. 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 annually, 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.

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 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 × 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 approximately 9% of the state's total investment and project count. Overall, the results indicate that the long-term 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.

4.2.4 Robustness – 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 when 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

[9] The results are robust to using alternative measures of the dependent variables. Appendix Table D.1 presents the results. In columns 1 and 3 we use one plus the natural logarithm of district-level investment, and one plus the natural logarithm of the number of projects as the dependent variables, respectively. In columns 2 and 4, we use the natural logarithm of district-level investment, and the natural logarithm of the number of projects as the dependent variable, respectively. Further, in columns 5 and 6 we demonstrate robustness using a Poisson pseudo-maximum likelihood model as advised by Chen and Roth (2024) and Cohn, Liu, and Wardlaw (2022), with the dependent variable in level form. We prefer the reported regressions over using other transformations or estimators for ease of the interpretation of the estimate.

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 when 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 assumptions of Oster (2019).

4.2.5 Robustness – Controlling for Geographic, Historical & Other Covariates

This section examines the robustness of the baseline estimates to other time-invariant district-level covariates. The British were likely to take over strategic territories. This includes territories such as those along the coast, those with larger geographic areas, and those with favorable climatic conditions. Moreover, they could selectively choose territories based on the demographic characteristics of the rulers, elites, and populace in those regions. To account for this, we include geographic and historical covariates, which may be correlated with the likelihood of a district being under direct rule.

Appendix Table D.2 reports the estimation results of the baseline equation 1 after controlling for geographic covariates. Appendix Table D.3 presents the baseline results with the inclusion of historical covariates such as the ruling empire, religion of the precolonial ruler, and historical population demographics such as the proportion of Muslims, Sikhs, lower caste individuals, and elites. Our estimate of interest is the coefficient of Direct Rule$_j$, which remains negative, statistically significant, and stable in magnitude. Overall, the results indicate that the estimate of β is unlikely to be driven by other geographic and historical characteristics that could potentially be correlated with the direct rule variable.

4.2.6 Robustness – 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. We randomly assign a district to be under direct British rule or indirect British rule, irrespective of the colonial history of the district.[10] 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

[10]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. Appendix E discusses the calculation and the magnitude of spatial autocorrelation of direct British rule.

the placebo direct rule and repeat this exercise 10,000 times. To invalidate the baseline results, the null hypothesis that the point estimate associated with placebo direct rule is zero must be rejected.

Appendix Figure D.1 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, as 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 of investment or other spatial noise.

5 Evidence from Bordering Districts

Our approach thus far, comparing the differences in investment between direct and indirect-ruled districts, faces two primary challenges. First, the British did not randomly assign districts to direct or indirect rule. Second, direct and indirect-ruled districts may systematically differ across several unobservable dimensions. As a result, directly comparing all such districts risks introducing selection bias or unobserved confounding variables, potentially undermining the validity of the analysis.

This section addresses this concern by focusing on contiguous districts within the same state that are separated solely by administrative borders, where one district was historically under direct British rule and the neighboring district was under indirect British rule. This sample of contiguous districts is shown in Figure 6a. Our empirical strategy employs a border district-pair design, comparing investments by the same firm across such district pairs. We focus on bordering district pairs because these districts likely share similar geographical characteristics, face similar investment opportunity shocks, and would likely have followed similar trajectories in the absence of colonization. The key advantage of this design is its ability to control for smoothly varying unobservable factors that could otherwise bias our estimates (Dube, Lester, and Reich, 2010). Moreover, the focus on contiguous districts within the same state allows us to control for variation in the legal and policy frameworks across states.

5.1 Empirical Strategy

We combine the sample of contiguous direct and indirect-ruled district pairs 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:

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,p(j\in p),y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t} \quad (2)$$

where, $ln(Y_{i,j,t})$ denotes the natural logarithm of the size of the project announced by firm i in district j at time t during 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, and 0 otherwise. As in the baseline specification 1, we also control for the latitude and longitude of the district.

In an ideal scenario where firms randomly chose districts for investment, we could directly attribute differences in aggregate investment in the border district-pair design to the rule type. However, firms' decision to invest in a district is unlikely to be random. In the presence of non-random matching, the observed average difference in investment between district types may not capture the true effect of the direct rule. Instead, it may reflect differences in the characteristics of districts that firms choose to invest in.

We address this issue by including district-pair × firm × year ($\theta_{i,p(j\in p),y}$) fixed effects in our analysis. The inclusion of district-pair × firm × year fixed effects ensures that we identify the differences in investment by the same firm across adjacent districts within the same year, thereby abstracting away from the confounding factor of non-random matching of firms to districts. Such an identification strategy has been employed previously in Fracassi, Petry, and Tate (2016) and Kempf and Tsoutsoura (2021) to address the non-random matching between credit rating analysts and the firms they cover. Overall, this approach ensures that our analysis more accurately captures the relationship between direct rule and firm investment, on the intensive margin, without confounding influences.

Lastly, the definition of district-pair can result in the presence of a single district in multiple pairs inducing a mechanical correlation across district-pairs. Following Dube, Lester, and Reich (2010), 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. Therefore, 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.

5.2 Results from Bordering Districts

Table 5 presents the regression results from estimating equation 2 using the subsample of contiguous districts under direct and indirect British rule. The coefficient of interest is β, associated with Direct Rule$_j$. Columns (1) to (4) incrementally add fixed effects to refine the estimation, with the full specification reported in column (4). Across all columns, the estimate of β is negative and statistically significant. The point estimate in column (4) is slightly larger in magnitude compared to the baseline estimate in column (5) of Table 3, suggesting that systematic differences across districts in the baseline approach likely understate the true effect. Specifically, the estimate in column (4) implies that projects announced in directly ruled districts are 10.8% smaller than those in indirectly ruled districts by the same firm within a district-pair.

An important assumption of this approach is that contiguous districts are similar to each other. To test this, we introduce a new set of indirectly ruled districts, referred to as "hinterland" indirect districts. These districts are adjacent to the previously analyzed indirect-ruled districts but are separated from the directly ruled districts by a strip of indirect-ruled district. The rationale is that if systematic differences across contiguous districts are driving the observed results, a comparison between contiguous indirect-ruled districts should yield a similar effect. Such a validation of border district-pair design has been previously employed in Huang (2008) to evaluate the effects of bank branch deregulation in the US.

Figure 6b presents this sample. The previously analyzed indirect-ruled districts ("border" indirect districts) are marked in navy blue, while the contiguous indirect-ruled districts ("hinterland" indirect districts) are marked in gray. Table 6 reports the results of comparing investment project sizes within this sample of contiguous indirect-ruled districts. The estimate of $Hinterland(=1)$ is economically small and statistically insignificant. This finding suggests that the results in Table 5 are unlikely to be driven by systematic differences across contiguous districts.

5.2.1 Project Abandonment

The ideal empirical design to test for lower investment in direct-ruled districts would 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. Assuming that announced projects are positive NPV projects, the abandonment of these projects can reflect that a firm forgoes 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 compared to indirect-ruled districts.

We analyze the differences in post-announcement project outcomes by comparing the abandonment status of projects announced in the same year. Specifically, we track whether a project is abandoned within one, two, three, four, and five years of its announcement. This cohort-based analysis helps address right-censoring bias. Moreover, we employ a border district-pair design, incorporating firm × district-pair × year fixed effects. This strategy allows us to tackle a key concern: that ex-post project abandonment might reflect subsequent geographic or firm-specific events rather than a firm's tendency to forgo ex-ante positive NPV opportunities. The identifying assumption underlying this strategy is that a firm is exposed to similar shocks within pairs of adjacent districts in the same state.

Table 7 presents the results from a linear probability model that combines the cohort analysis with the border district-pair design. The table examines the likelihood that a firm abandons a project in a directly ruled district compared to a contiguous indirectly ruled district within five years of announcement. Columns (1) through (5) report the probability of abandonment within one, two, three, four, and five years, respectively. The findings indicate that a firm is 2.5 percentage points more likely to abandon its projects in a directly ruled district than in a contiguous indirectly ruled district. This effect is economically significant, given that the unconditional probability of project abandonment is 13%. These results suggest that firms may be more likely to forgo economically sound investment opportunities in direct-ruled districts.

5.2.2 Discussion on Internal Validity

The inclusion of district-pair × firm × year fixed effects allows us to compare each firm's investment decisions across adjacent districts within the same year, thereby avoiding confounding from non-random firm-to-district matching. However, this approach raises concerns about internal validity due to potential selection bias. Since firms make their present-day investment decisions long after the distinction between direct and indirect rule was established, their decision to invest in both types of adjacent districts may stem from unobserved preferences or characteristics that systematically differ from firms investing in either of the district types. This selection could bias our estimates, undermining the internal validity of our analysis.

We address this concern by estimating our border district-pair specification without district-pair × firm × year fixed effects and using the full sample of investment projects in bordering districts. Specifically, unlike the analysis in Table 5 we do not restrict the sample to only include firms that invest in adjacent districts. Column (1) of Appendix Table D.4 presents the results using the full sample of investment projects in bordering districts. We include district-pair × year fixed effects to ensure that we are identifying the estimate of interest using variation across adjacent districts. The estimate of interest associated with Direct Rule is negative and statistically significant. Moreover, for comparison, column (2) replicates the results with the same set of fixed effects but restricting the sample to the one employed in Table 5. Our results show that the estimate of interest remains stable in magnitude, despite this sample adjustment. This suggests that our estimate from the border district-pair design reported in Table 5 is likely to be internally valid.

5.2.3 Robustness – Firm Ownership & Project Type

We show that our results are unlikely to be driven by firm ownership or project type. Appendix Table D.5 indicates that our findings are unlikely to be influenced by ownership structures of government or business groups – legally independent entities characterized by a large ownership stake and common control by a single entity (Khanna and Yafeh, 2007). Lastly, Appendix Table D.6 indicates that our results are not driven by projects aimed at establishing new units or investment projects intended to improve or expand existing investments.

6 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 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. 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 is 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.[11] The exclusion restriction is 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 6.1.

Table 8 reports the results from the 2SLS regression and compares the estimate with the IV and

[11] Lord 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.

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 1848 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. Overall, this analysis indicates that the results discussed so far are unlikely to be driven by selection bias.

6.1 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. In this section, we examine various challenges to the exclusion restriction, such as the strategic acquisition of territory by the British, voluntary withdrawal from the policy by incumbent rulers, and confounding variables that result in a ruler's demise without a natural heir. These factors could potentially impact the long-term consequences of a region through channels other than direct rule.

First, we argue that the Doctrine of Lapse was not strategically implemented to acquire certain territories, nor could rulers voluntarily opt out of this policy. Rahim (1963) notes that the Doctrine of Lapse did not allow the British to take over their most desired states of Oudh and Hyderabad. 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 Doctrine 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.

Second, we rule out alternative hypotheses that may explain our findings. One possibility is that the death of a ruler without a natural heir reflects factors unrelated to colonial rule, such as environmental or genetic conditions, which might influence postcolonial corporate investment through other channels. Another possibility is that such deaths triggered political instability, leading to adverse long-term consequences captured by our estimation. To address these claims, we conduct a falsification test by regressing the natural logarithm of project size on a binary variable indicating whether a ruler died without a natural heir during periods before 1847 or after 1856, in a sample of indirectly ruled districts. These periods predate or postdate the application of the Doctrine of Lapse, and indirect districts were not subjected to direct British rule. If the death of a ruler without a natural heir affects present-day outcomes through non-colonial channels, we would expect to see significant results. Column 5 of Table 8 shows that the coefficient is close to zero and statistically insignificant, suggesting that these deaths without an heir do not directly influence long-term outcomes.

Third, we demonstrate that the timing of a ruler's death is unlikely to directly affect long-term outcomes. A potential concern is that ruler deaths during 1848–1856 – during the Doctrine of Lapse – regardless of the presence of a natural heir, may reflect region-specific conditions or events that could influence long-term outcomes independently of direct rule. To test this, we regress the natural logarithm of project size on a binary variable indicating whether a ruler died with a natural heir during this period, again focusing on a sample of indirectly ruled districts. Since these territories were not under direct rule, any significant results would imply that factors other than direct rule were at play. Column 6 of Table 8 reports that the coefficient is economically small and statistically insignificant, reinforcing the view that the timing of a ruler's death does not independently affect long-term outcomes.

7 Mechanism

This section discusses a mechanism through which direct British rule affects corporate investment in the present: disruption of existing local industries. 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. We begin by examining the economic motivations for British control of the cotton industry and demonstrating that areas with precolonial cotton production were more likely to come under direct British rule. Further, we use the adoption of the Whitney cotton gin as a natural experiment to examine the impact of shifts in

British incentives on their pursuit of cotton in India. We show that there are long-term effects associated with the disruption of the cotton industry and explain the ways through which colonial policies and practices have persisted to the present.

7.1 Capture of the Cotton Industry

> *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)

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, 1995). The Indian cotton textiles were the most important manufactured goods in the 18th century with India being home to the world's most important cotton textile industry (Parthasarathi, 2011; 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. The control of cotton allowed the British to meet two objectives: (1) protect the interests of the British textile industry, and (2) 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 make India an agricultural colony of British capitalism by securing a monopoly on the supply of Indian goods and products, specifically cotton (Sahoo, 2015).

The negative effects of the disruption of India's textile industry are 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. We emphasize four ways through which the textile industry, specifically the cotton industry, was destroyed in India: (1) protectionism of the British textile industry through tariffs, (2) price fixing through monopsony power, (3) violence against textile producers, and (4) deprivation of new technological innovations in the local industries. We direct readers to Appendix B for a detailed discussion on the ways through which the Indian textile industry was destroyed, specifically the cotton industry. Overall the attitude of the British towards the Indian cotton textile production is best described by H.H. 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, 1848).

We document 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, than under indirect rule. The likelihood that a cotton-producing district is under direct British rule is economically large and statistically significant.

7.2 Disruption of Cotton Industry and Adoption of Whitney Cotton Gin

This section examines how British incentives to control the Indian cotton industry evolved in response to a significant shift in global cotton supply – the rise of the American colonies as the primary cotton supplier. This shift acted as a shock, reducing the incentives of the British to control and dismantle India's cotton sector, ultimately leading to reduced economic disruption within the domestic cotton industry for areas annexed later.

The emergence of the American colonies as a low-cost supplier of cotton by 1801 reduced the incentives of the British to continue their exploits of cotton in India.[12] This transformation began with Eli Whitney's patenting of the cotton gin in 1794 and its subsequent adoption in the US by 1800. The Whitney cotton gin effectively and efficiently removed the seeds from cotton plants, thereby making the process of extracting seeds from fibers for the short-staple type cotton very cheap. In contrast, the cotton farmed in India was of the long-staple variety and did not benefit from this invention. Logan (1965) notes a widespread adoption of the superior and cheap American short staple among Lancashire textile manufacturers. As a result, after the adoption of the Whitney cotton gin, Indian cotton fell out of

[12] In 1801, the US's annual cotton production exceeded 22 million kilograms. By the early 1830s, the US had become the world's leading cotton producer (Oyangen, 2014).

favor with British cotton mills. After 1800, the British had less motive to dismantle the Indian cotton textile sector.

We combine the timing of the widespread adoption of the Whitney cotton gin with data on Indian domestic exports of agricultural commodities from 1884 to 1920 to examine whether patterns in cotton trade flows differ systematically across provinces annexed before 1800 ("early") and after 1800 ("late"). The goal of this test is to assess whether changes in British economic incentives tied to the global textile trade contributed to long-term economic disruption. If other channels were driving the observed outcomes, we would expect no systematic differences in cotton exports between early and late-annexed regions. In contrast, if British incentives to capture the cotton industry were central, these differences should be reflected in divergent trade patterns.

Table 9 presents this result. The table examines the relationship between the natural logarithm of trade flows for each commodity between trade blocks on the interaction term of early annexation and cotton commodity. Early annexation takes a value of one for trade blocks that were under direct British rule before 1800, and zero, otherwise. Cotton commodity takes a value of one for trade flows of cotton and zero for all other commodities. Our preferred specification includes exporter province × importer, province × commodity fixed effects, commodity × year fixed effects, and exporter block × importer block × year effects. Columns 1-6 sequentially add these fixed effects. The coefficient on the interaction term is negative and statistically significant across all columns. The negative coefficient indicates that the cotton exports of trade blocks that were annexed before 1800 were lower than the cotton exports of trade blocks annexed after 1800.

This result indicates that the negative economic effects of the British capture of the Indian cotton industry were concentrated among districts that were annexed before the widespread US adoption of the cotton gin. This finding is consistent with our hypothesis that, with the rise of American cotton after 1800, the British had weaker incentives to dismantle the Indian cotton textile sector. We also note that this effect is not driven by a lower likelihood of cotton-producing areas being annexed under direct British rule after 1800. Appendix Figure F.1 shows that among districts under direct rule, 80% of districts were cotton-producing districts regardless of the period in which they were annexed.

Taken together, the two results indicate that there was no shift in the annexation policy, rather a shift in how the local cotton industry was treated after annexation.

7.3 Long-Term Effects

This section examines the long-term consequences of colonial disruption of the cotton industry. In particular, we study how direct British rule affects corporate investment in the present through the disruption 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 × district-pair × year fixed effects.[13] Table 10 presents the long-term effects of the disruption 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 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 22% 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 × district-pair × 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-day corporate investment directly or indirectly through channels other than direct British rule.

Next, we examine the impact on present-day corporate investment among direct-ruled areas that were annexed before 1800 relative to areas annexed after 1800. The objective of this test is to complement the analysis in section 7.2 in documenting that the reduced investment in direct-ruled areas

[13] We also report similar results from the full sample in Appendix Table D.7 for completeness.

is primarily driven by cotton-producing areas that were annexed before 1800, when the British may have had greater incentives to dismantle the existing cotton industry.

Table 11 presents this analysis, restricting the sample to direct-ruled areas. Column 1 reports a negative coefficient on precolonial cotton production. Column 2 adds an indicator variable for early annexation to the specification in column 1. The estimate of precolonial cotton reduces in magnitude and becomes statistically insignificant, whereas the coefficient of early annexation is negative and statistically significant. Column 3 supplements the specification reported in column 2 by adding the interaction term of early annexation and precolonial cotton production. The estimates of precolonial cotton production and early annexation attenuate in magnitude and statistical significance, whereas the interaction term is negative and statistically significant. This indicates that the present-day lower investment in historically direct-ruled areas is driven by cotton-producing districts that were annexed before 1800.

7.3.1 Human Capital Accumulation as a Channel for Long-Term Effects

The long-term consequences of the disruption of existing local industries can be attributed to its effect on human capital accumulation. 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 disruption 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. While one might expect such economic disruption to incentivize workers to develop new skills, the cotton textile workforce, predominantly from lower-caste communities, faced significant social and economic impediments that limited their ability to transition into new professions (Broadberry and Gupta, 2005).

This section provides empirical evidence in support of this hypothesis. We show that human capital acquisition is lower among lower-caste individuals in direct-ruled districts that produced cotton during the pre-colonial period and were annexed before the invention of the Whitney gin. We utilize data from the National Demographic and Health Survey (DHS) for households in direct-ruled districts, using education outcomes and self-reported interest in education as proxies for human capital acquisition. Specifically, we compare the education outcomes of lower-caste individuals in early-annexed cotton-producing districts with those in later-annexed districts. The analysis focuses on household heads, with the key coefficient derived from the triple interaction of low caste, early annexation, and precolonial cotton production.

Table 12 presents the results on educational attainment. The key dependent variable is a binary indicator equal to 1 if the household head has received higher education (post-secondary education) and

0 otherwise. Columns 1–5 estimate the coefficient of interest associated with the triple interaction term of low caste, early annexation, and precolonial cotton across various combinations of fixed effects. We successively add granular levels of fixed effects along with latitude and longitude controls, to account for regional and temporal variation across district-ruled districts. Particularly, column 5 includes district fixed effects to control for all factors that may have influenced certain districts' ability to grow cotton during the precolonial period and their likelihood of annexation at specific times. Additionally, we include native state × caste and present-day state × caste fixed effects in this column to control for caste-level characteristics at the state level.

The estimate for the triple interaction term is negative, remains stable and statistically significant across all columns. Our most conservative estimate suggests that lower-caste households in direct-ruled areas that produced cotton during the precolonial period and were annexed before the invention of the Whitney gin are 11.3% less likely to attain higher education compared to their counterparts in direct-ruled cotton-producing areas annexed after the invention of the Whitney gin in the present. This finding indicates that human capital accumulation remains persistently weaker in these regions relative to other areas with similar historical economic conditions, but later annexation.

To further support these findings, we extend our analysis to additional education outcomes, including the likelihood of having no formal education, attaining primary or secondary education as the highest level, literacy, and total years of schooling, using the same sample. The results, presented in Appendix Table D.8, suggest significant disparities. Lower-caste households in early-annexed, cotton-producing districts are 69% more likely to have no formal education, 26% less likely to complete primary school, 32% less likely to complete secondary school, 69% less likely to be literate, and have an average of 5.1 fewer years of schooling compared to their counterparts in later-annexed cotton-producing districts in the present. These findings highlight the persistent effect of economic disruption on educational attainment.

Our hypothesis on human capital acquisition posits that lower-caste households lose the community knowledge accumulated over generations due to the disruption of key economic structures. Furthermore, because these households belong to marginalized castes, they face significant barriers to entering new professions, which may ultimately erode their interest in acquiring education. To test this conjecture, we leverage survey data, specifically examining the variable "main reason never went to school." The dependent variable is a binary variable that takes a value of 1 if the primary reason a household member did not attend school was a perceived lack of necessity or interest in education, and 0 otherwise.

Table 13 presents the results. As in prior analyses, the coefficient of interest is the interaction term for low caste, early annexation, and precolonial cotton. This captures differences in the stated interest in acquiring education between low-caste households in early-annexed cotton-producing districts and those in later-annexed cotton-producing districts. The estimate of interest is positive, statistically significant, and robust across various fixed effects specifications in columns 1-5. Our most conservative estimate indicates that lower-caste households in directly ruled areas that produced cotton during the pre-colonial period but were annexed before the invention of the Whitney gin are 18.5% more likely to report a lack of interest in acquiring education compared to their counterparts in later-annexed districts.

Overall, these results shed light on the underlying mechanism behind the long-term effects of economic disruption. Specifically, we document that economic disruption can have long-term effects by diminishing the motivation to invest in human capital, particularly among marginalized groups that face structural barriers to mobility and adaptation.

7.3.2 Labor Intensity & Firm Investment

So far, we have argued that individuals in regions under direct rule exhibit lower incentives to invest in education and human capital accumulation. To further explore this mechanism, we examine heterogeneity in firm-level investment, focusing on firms with varying levels of labor intensity. The rationale is that if populations in direct-ruled regions are less inclined to acquire basic education or skills, firms that rely more heavily on labor are less likely to invest in those areas.

To this end, we measure an industry's labor intensity using the ratio of employees to net property, plant, and equipment, using data from U.S. firms. Under the assumption that labor and capital markets in the US – particularly for large, publicly listed firms – are relatively frictionless, this approach enables us to more accurately classify industries by their labor intensity. This assumption is similar to the one employed in the seminal work of Rajan and Zingales (1998) to measure reliance on external finance. We then compare investment of Indian firms operating in industries with higher or lower labor intensity across bordering districts.[14] This comparison helps us examine whether firms in more labor-intensive industries are less likely to invest in direct ruled areas.

Table 14 presents the results. Column 1 reports the baseline effect of direct rule, controlling for district-pair × firm × year fixed effects. Column 2 extends this model by including an interaction term between direct rule and high labor intensity. The coefficient on the interaction is negative and statistically significant, while the coefficient on direct rule alone becomes insignificant. In Column 3, we further include district × year fixed effects to account for any district-level selection into direct

[14] Specifically, we calculate the labor intensity for all firms as the ratio of employees to net property, plant, and equipment, using annual Compustat data from 1991 to 1995. We classify firms into 48 Fama-French industries based on their historical SIC codes. For each industry, we determine the median labor intensity across all sampled firms. We then manually match each Indian firm's industry to the corresponding Fama-French industry. Firms are classified as high labor intensity if their employee-to-property, plant, and equipment ratio exceeds the sample median.

rule. The estimate for the interaction between direct rule and high labor intensity remains negative and statistically significant, reinforcing our main finding.

Overall, the results suggest that firms in labor-intensive industries are significantly less likely to invest in districts formerly under direct British rule compared to adjacent districts that were not under direct rule. This finding supports our hypothesis that the long-term effects of British colonial rule continue to influence investment patterns today, primarily through their impact on human capital accumulation.

7.3.3 Alternative Explanations: Enforcement, 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 these channels are unlikely to account for our results.

Law and Enforcement: An alternative mechanism is that there were pre-existing differences in the rule of law across direct and indirect-ruled districts that persist to date and can explain the differences in present-day investment. There are three specific concerns, related to law and enforcement. We discuss and rule out all three.

The first potential concern may be that 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). Under this hypothesis, the differences in the rule of law across direct and indirect-ruled districts could potentially explain 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.[15]

The second potential concern may be that although the law may be fixed within a state, the historical origins can affect the enforcement of the law across districts. This weak enforcement can negatively affect firm investment and productivity (Ponticelli and Alencar, 2016; Boehm and Oberfield, 2020). This channel is also unlikely to explain our results as all corporate cases are adjudicated by the

[15] Moreover, it is worth noting that India is an outlier to the Acemoglu, Johnson, and Robinson (2001) hypothesis. See Appendix Figure F.2. Appendix Figure F.2a presents a scatterplot 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 the 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 Appendix Figure F.2b 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.

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 potential concern may be that differences in enforcement of local cases 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 F.1 shows that the differences in court delays across district types are economically small and statistically insignificant for our sample. Hence, our results are unlikely to 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. 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 the number of schools and electricity supply, are insignificant for our sample (see Appendix Table F.2). Hence, the differences in the provision of public goods are unlikely to drive our results. This result also suggests that our results are unlikely to be driven by differences in the persistence of public investment, as discussed in Huillery (2009) and Méndez and Van Patten (2022).

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 compared 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 to invest in basic infrastructure required for corporate investment. Therefore, an alternative hypothesis is that the differences in investment in the present 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, and panchayats (elected village governments), we do not find any evidence of systematic differences in trust in state actors across direct and indirect-ruled districts (see Appendix Figure F.3). Hence, we argue that differences in trust, due to colonial rule, are unlikely to drive our results.

This observation of lack of differences in trust between direct and indirect-ruled areas is consistent with Iyer (2010). Using data on state-level elections from the 1960s and from the 1980s, Iyer (2010) finds that there are no significant differences in voter turnout or the vote margin of the winning candidates, a proxy for the competitiveness of elections and trust in the incumbent state leader.

8 Conclusion

This paper presents three empirical findings. First, we show that differences in colonial rule can explain present-day geographic concentration in investment. We use within-country variation in historical circumstances – direct and indirect British rule – combined with a border district-pair design and instrumental variable strategy to explain the spatial differences in investment. 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.

Second, we propose a novel mechanism through which direct British rule influences investment in the present: the disruption of well-established local industries. We document that the East India Company (EIC) consolidated economic power through the annexation of cotton-producing territories. We argue that direct control of cotton-producing territories allowed the British to meet their objectives of protecting the interests of the British textile industry and increasing Britain's share of global trade. As a result, areas under direct colonial rule were subject to economic policies and practices that dismantled the well-established economic organizations centered around the cotton industry. Using the invention of the cotton gin in 1794 as a natural experiment, we present evidence indicating that the British incentives to consolidate their monopoly of the global cotton textile industry were the primary driver behind the economic decline of Indian textile industry. Our findings indicate that lower levels of present-day investment in historically direct-ruled areas are concentrated in cotton-producing districts. Furthermore, we show that this effect is particularly pronounced in cotton-producing districts annexed before the widespread adoption of the cotton gin around 1800.

Third, we show that the disruption of local industries has long-term persistent effects through disrupted patterns of human capital accumulation. We argue that the disruption of the cotton industry reduced the incentives of the cotton laborers to transmit their specialized knowledge to future generations and discouraged subsequent generations from acquiring these skills. Moreover, the cotton textile workforce, predominantly from lower-caste communities, faced significant social and economic impediments that limited their ability to transition into new professions. Using education outcomes and self-reported interest in education as proxies for human capital acquisition, we find that individuals from lower castes in direct-ruled districts that produced cotton during the pre-colonial period and were annexed before the invention of the Whitney gin experienced lower levels of human capital accumulation. The long-term effects of colonial disruptions on human capital accumulation are reflected in lower investment by firms that rely more heavily on labor, as populations in direct-rule regions are less likely to acquire basic education or skills.

Overall, this paper makes two key contributions to the literature. First, we present a novel mechanism linking historical disruptions due to colonial rule to current investment outcomes. Second, we argue that historical disruptions due to colonial rule can significantly shape the geographic distribution of investment by influencing the development of the agglomerative forces and locational fundamentals, discussed in Marshall (1920) and Davis and Weinstein (2002) – beyond their effects on property rights or institutions. Therefore, our empirical results complement the theoretical framework of Krugman (1991) which establishes the role of shocks to a "core" region's agglomeration advantages in producing persistent spatial divergence. More broadly, our 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.

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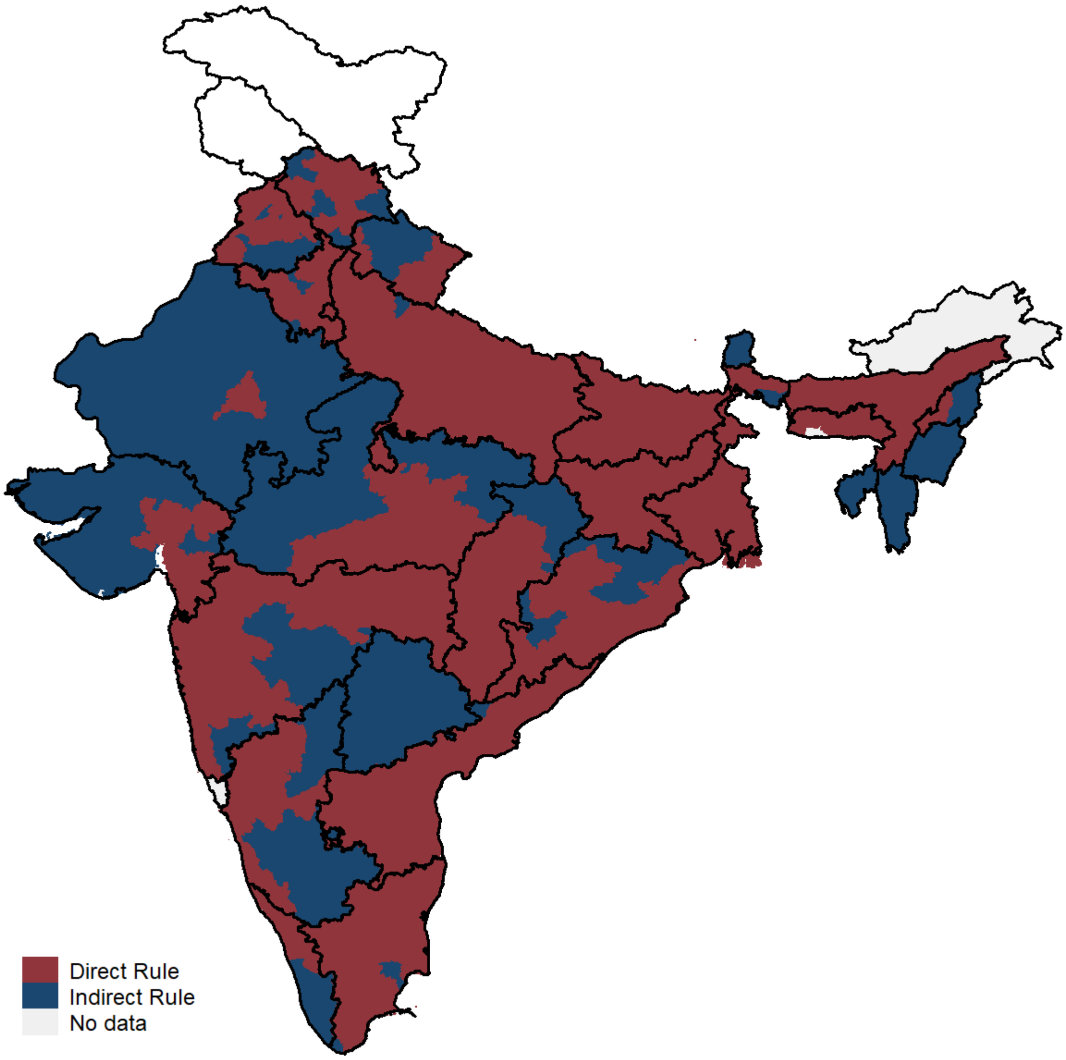
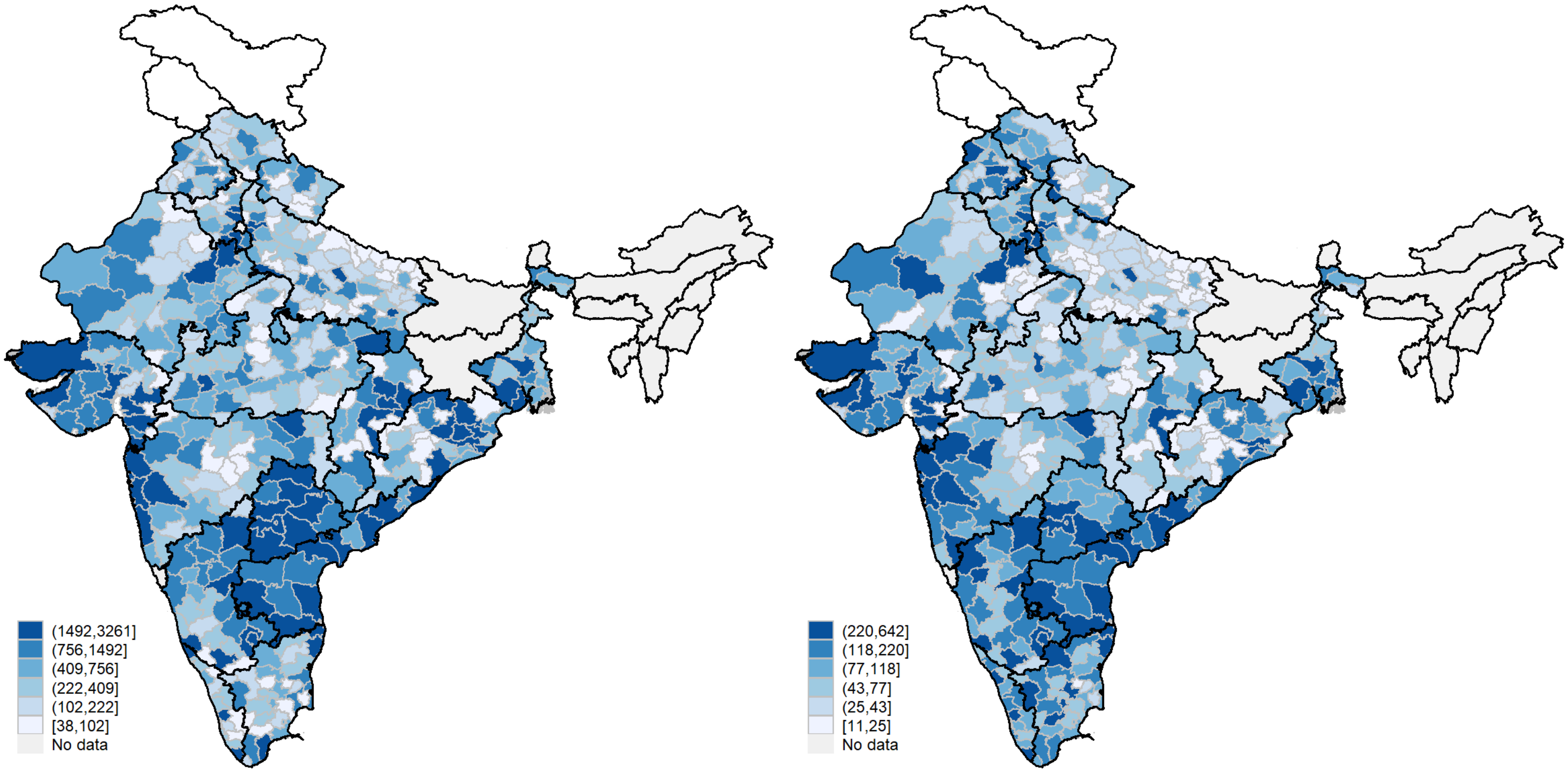
Figure 1: Current and Colonial Boundaries



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

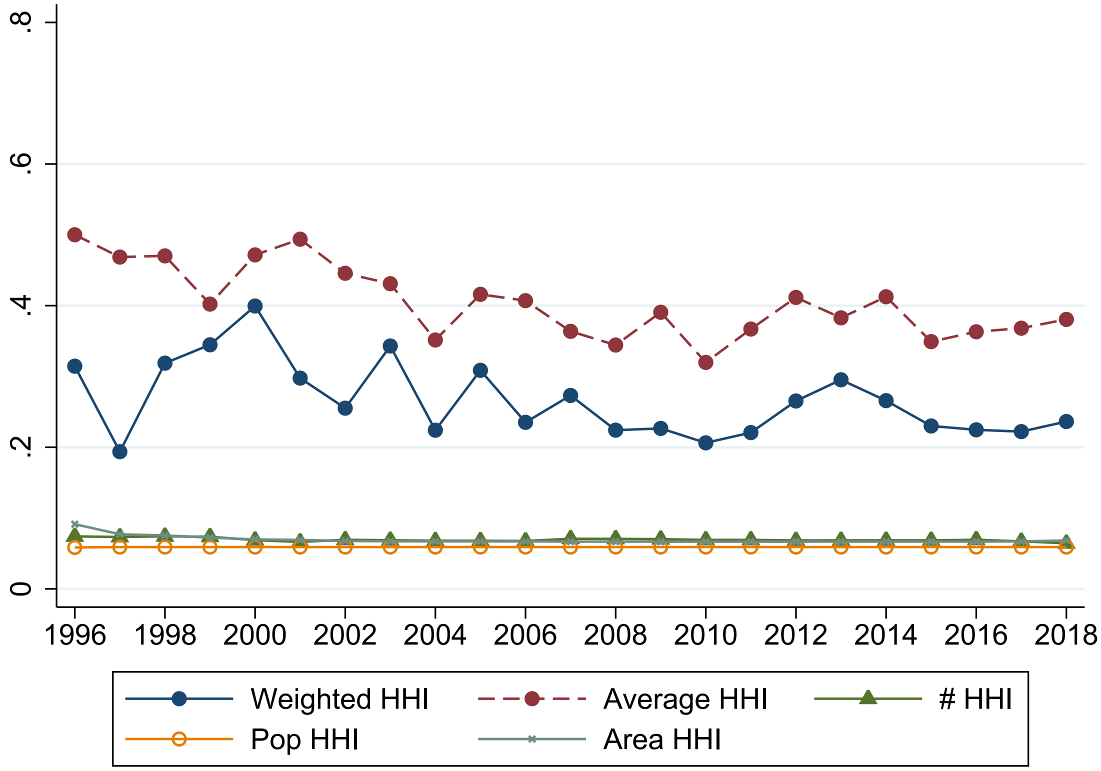


(a) Total Amount (in ₹bn)

(b) # Projects

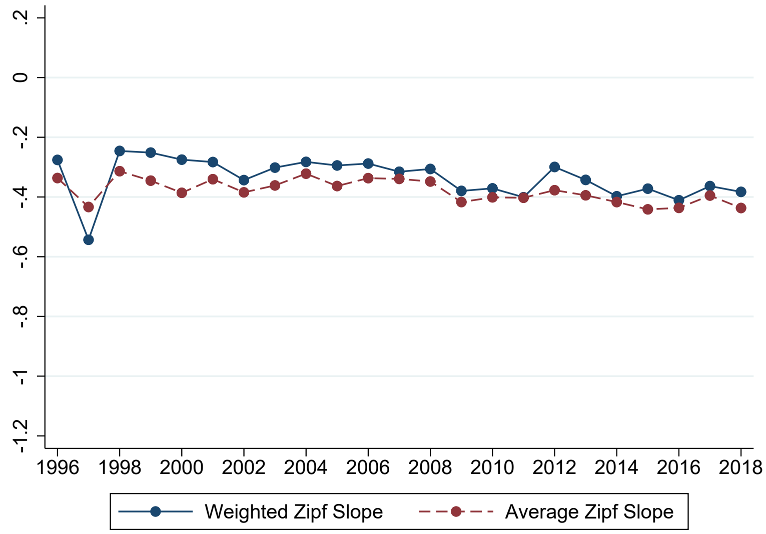
This figure plots the heatmap of total amount (in ₹billions) and the number of projects announced in the 17 states in our sample between fiscal years 1996 and 2018. Total amount and number of projects are calculated as the total amount and total number of projects announced in each district. The exact 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 by 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: Alternative Measures of Geographic Concentration of Investment

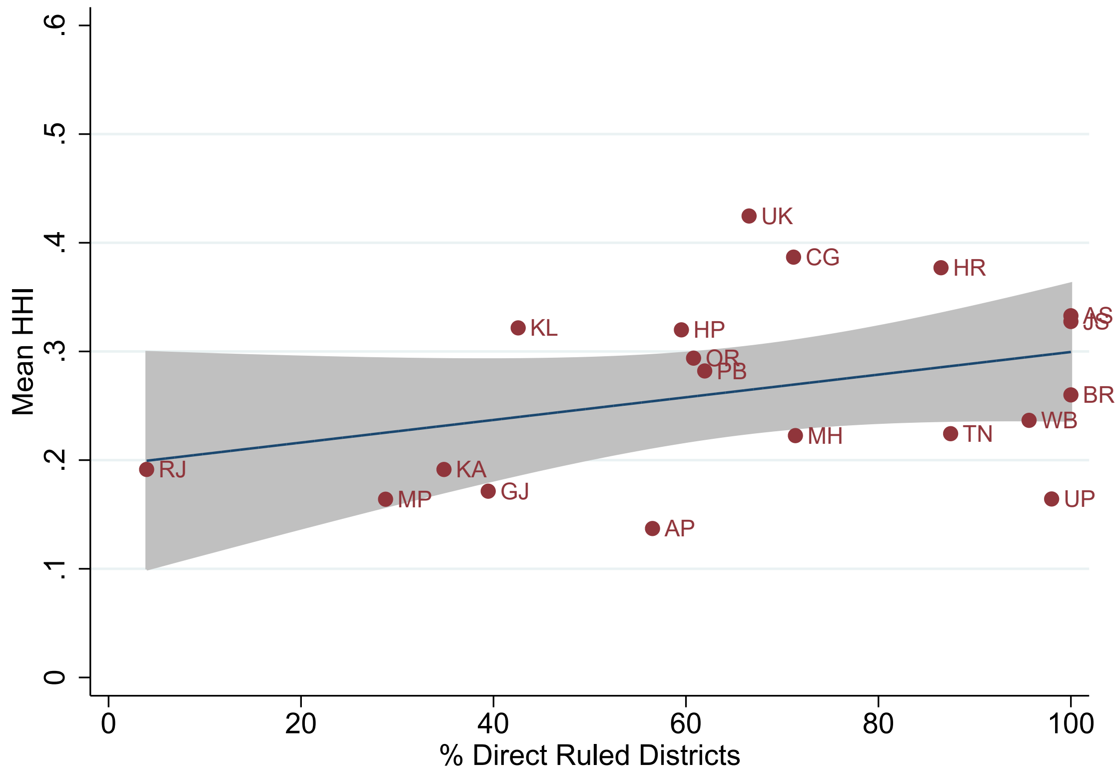


(a) Zipf Slope Coefficient

(b) Theil index

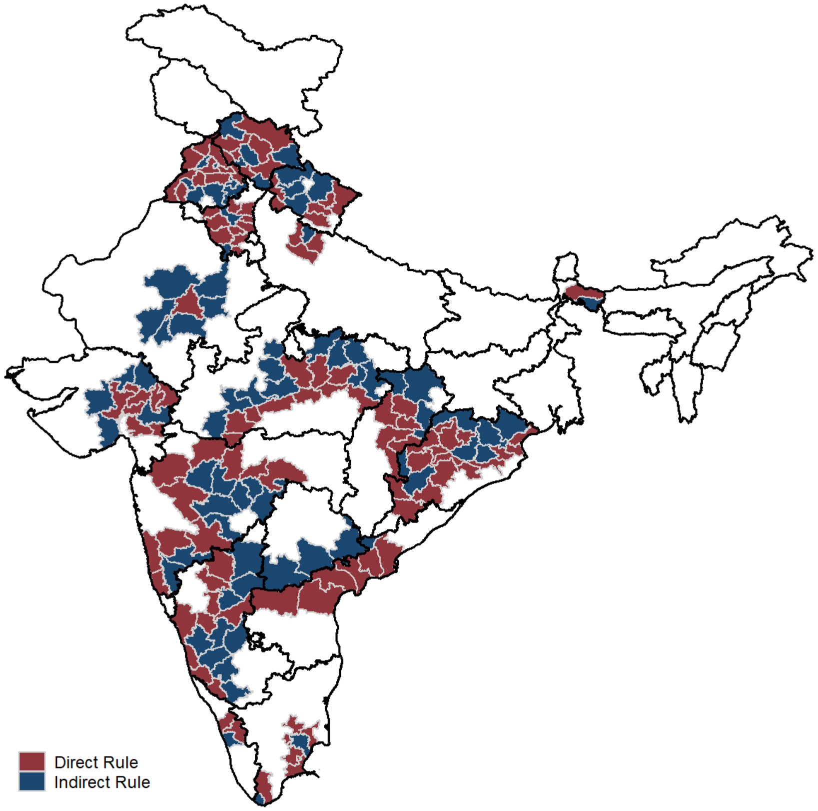
The figure plots two alternative measures of geographic concentration of investment. Figure 4a presents a time-varying measure of state-level average and weighted Zipf slope coefficients. To estimate the Zipf slope coefficient, firms are ranked by total cost within each state-announcement year, with higher costs receiving lower (higher) ranks. The natural logs of both project size (*ln(Project Size)*) and rank (*ln(Rank)*) are then computed. We run the following regression: $ln(\text{Rank})_{s,y} = \alpha + \beta ln(\text{Project Size})_{s,y} + \varepsilon_{s,y}$ where s denotes the state, and y denotes the announcement year. Observations with too few firms (fewer than 8) are excluded to ensure meaningful estimation. The red line plots the simple average Zipf slope coefficient. The blue line plots the weighted Zipf slope coefficient, where the Zipf coefficient of each state is weighted by the share of investment in the state relative to the country. Figure 4b plots a time-varying measure of state-level average and weighted Theil indices. The red line plots the simple average Theil index. The Theil index is computed annually at the state level using the ratio of each district's total project cost to its state's average project cost. Specifically, the Theil index is calculated as $T = \sum_i \frac{x_i}{\mu} ln(\frac{x_i}{\mu})$, where x_i is the project size in district i and μ is the average cost across districts in the state-year. The blue line plots the weighted Theil index coefficient where the Theil index coefficient of each state is weighted by the share of investment in the state relative to the country. A rescaling transformation, $1 - e^{T \times -1}$, is applied for interpretability on a bounded [0,1] scale.

Figure 5: Geographic Concentration of Investment and Direct Rule

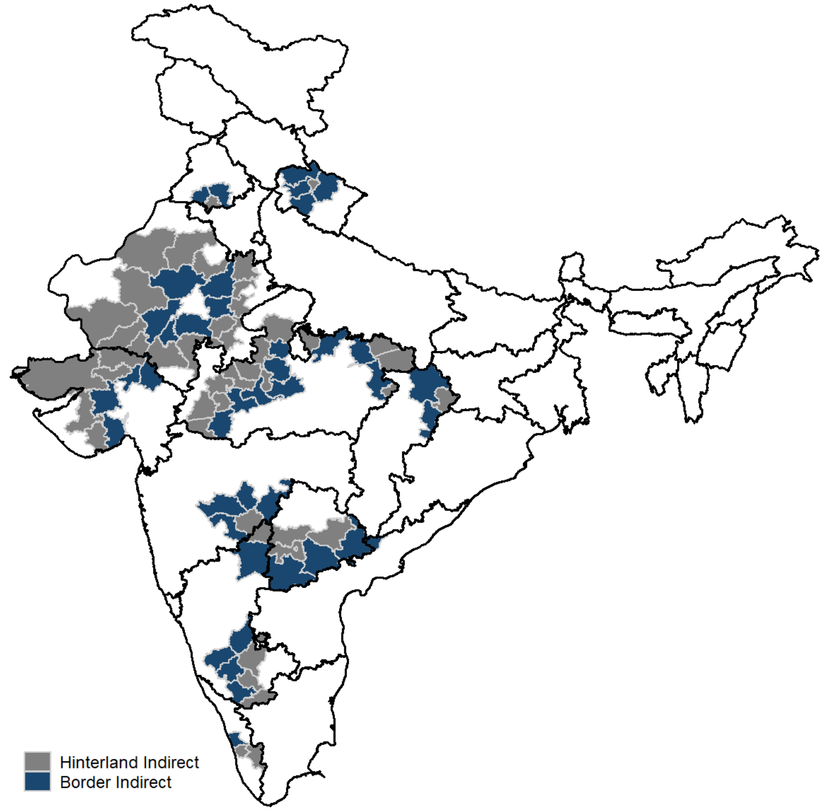


The figure plots the scatterplot of average state level investment HHI and the percent of direct-ruled districts within a state and the best fit line between the two measures. The unit of observation is the state. State-level average HHI is computed using data on project announcements from fiscal years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

Figure 6: Sample for Border District-Pair design



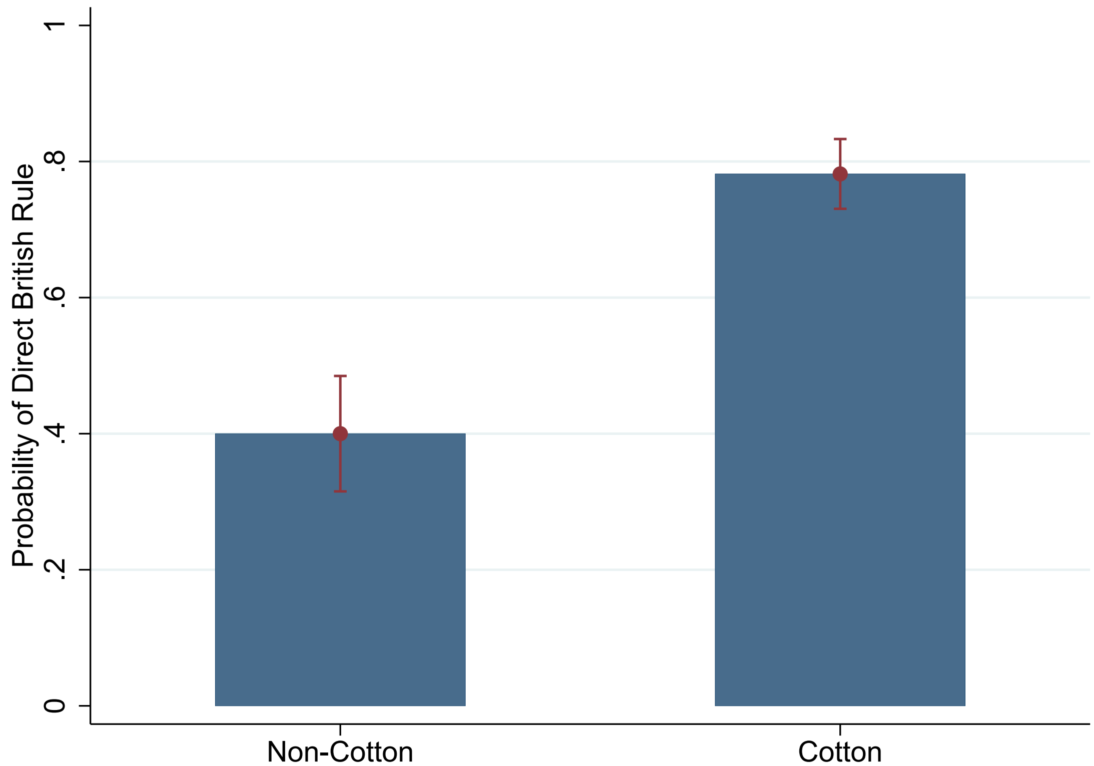
(a) Sample of Bordering Districts



(b) Sample of Hinterland Districts

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				0.0002
	(0.0003)				(0.0002)
Coast (=1)	0.1820				0.1720
	(0.1176)				(0.1179)
ln(Area)	-0.0637				-0.0692
	(0.0816)				(0.0799)
Slope	-1.0837				2.6706
	(3.6432)				(2.3314)
Rain (cm)	0.0015				0.0012
	(0.0010)				(0.0009)
Max-Temp	0.0061				-0.0010
	(0.0113)				(0.0113)
Min-Temp	0.0126				0.0028
	(0.0104)				(0.0090)
ln(Distance)		0.0396			0.0707
		(0.0611)			(0.0577)
Maratha Ruler			0.2279		0.2449
			(0.1550)		(0.1524)
Muslim Ruler			0.3853***		0.3319**
			(0.1276)		(0.1420)
Prop Muslim				0.2663	-0.1818
				(0.3447)	(0.2848)
Prop Sikhs				0.6377	-0.2291
				(1.0841)	(0.9907)
Prop Lower Caste				0.5613	0.5439
				(0.3940)	(0.3518)
Prop Elites				-0.3153	-0.1544
				(0.6895)	(0.6948)
Constant	0.5330	0.4275	0.4336***	0.5111***	0.3777
	(0.8825)	(0.3253)	(0.0933)	(0.1445)	(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 the 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*	0.1463**	0.1227*	0.1522*	0.1514*	0.1369*
	(0.0661)	(0.0531)	(0.0685)	(0.0793)	(0.0813)	(0.0778)
# Districts		-0.0182***	-0.0188***	-0.0180***	-0.0194***	-0.0183***
		(0.0038)	(0.0039)	(0.0042)	(0.0047)	(0.0053)
Area per District			-0.0582	-0.1081	-0.1354	-0.1090
			(0.1128)	(0.1402)	(0.1391)	(0.1541)
Population Density				-0.8775	-1.2064	-0.8094
				(0.8750)	(0.9068)	(1.0235)
GDP per capita					-0.1159	0.0302
					(0.1124)	(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. This regression is weighted by the total investment in each state. 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**	-0.1130***	-0.1146**	-0.0864**	-0.0881***
	(0.0836)	(0.0416)	(0.0523)	(0.0346)	(0.0324)
	[0.0548]***	[0.0356]***	[0.0371]***	[0.0332]***	[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:

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,y} + \theta_{s(j \in s),y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

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	$I_{j,t}$	$I_{j,t} \mid I_{j,t} > 0$	# 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*	-16174.5813**	-28350.1337**	-4.1791**	-6.8549**	-7.0724**	-7.1912*
	(0.1346)	(7910.9896)	(11777.9807)	(2.0257)	(2.7350)	(2.9954)	(3.7651)
State × Qtr × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Average	0.6453	19861.4928	37851.9045	4.4693	7.7296	8.7459	8.8529
	(0.0931)	(5367.5878)	(7901.7155)	(1.3584)	(1.8328)	(2.0333)	(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:

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

where $Y_{j,t}$ 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 × quarter × 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: Border District-Pair Analysis Using Sample of Bordering Districts

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)
Direct Rule (=1)	-0.0877*	-0.0974**	-0.1090**	-0.1084**
	(0.0452)	(0.0452)	(0.0473)	(0.0443)
Qtr × Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes			
Firm × Year FE		Yes	Yes	
District-Pair × Year FE	Yes	Yes	Yes	
Firm × District-Pair FE			Yes	
Firm × District-Pair × Year FE				Yes
Lat/Long	Yes	Yes	Yes	Yes
# Obs	11,947	11,947	11,947	11,947
R^2	0.7315	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:

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,p(j\in p)y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

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 effects. 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 6: Border District-Pair design: Sample of Hinterland Districts

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)
Hinterland (=1)	-0.0407	0.0382	0.0353	0.0355
	(0.0554)	(0.0519)	(0.0357)	(0.0323)
Qtr × Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes			
Firm × Year FE		Yes	Yes	
District-Pair × Year FE	Yes	Yes	Yes	
Firm × District-Pair FE			Yes	
Firm × District-Pair × Year FE				Yes
Lat/Long	Yes	Yes	Yes	Yes
# Obs	4,953	4,953	4,953	4,953
R^2	0.7816	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:

$$ln(Y_{i,j,t}) = \beta \cdot \text{Hinterland}_j + \theta_{i,p(j\in p)y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

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 × district-pair × year fixed effects. 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 7: 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	0.0284**	0.0256*	0.0257*	0.0248*
	(0.0118)	(0.0125)	(0.0145)	(0.0150)	(0.0134)
Qtr × Year FE	Yes	Yes	Yes	Yes	Yes
Firm × District-Pair × 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 after 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 border district-pair design includes firm × district-pair × year fixed effects, and quarter × 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 8: Instrumental Variable Regression: Death of Ruler without Natural Heir

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	2SLS		Falsification	
			Second Stage	First Stage		
Direct Rule (=1)	-0.2236***		-0.2239**			
	(0.0596)		(0.0960)			
Rule Death, No Heir, Lapse (=1)		-0.1475*		0.6589***		
		(0.0759)		(0.1225)		
Rule Death, No Heir, No Lapse (=1)					-0.0098	
					(0.0580)	
Rule Death, Yes Heir, Lapse (=1)						0.0072
						(0.0758)
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	8,034	8,129
R^2	0.763	0.7628	0.0016	0.8646	0.763	0.7626
KP LM Statistic			5.9527**			
KP Wald F-Statistic			28.9431			

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

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{s(j \in s),y} + \theta_{i,y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

$$\text{Direct Rule}_j = \gamma \cdot \text{Ruler Death-No Heir}_j + \theta_{s(j \in s),y} + \theta_{i,y} + \delta_3 \cdot \text{Latitude}_j + \delta_4 \cdot \text{Longitude}_j + \theta_t + \epsilon_{i,j,t}$$

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 1857. 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 1848 and after 1858. Ruler Death, Yes Heir, Lapse (=1) takes a value of 1 if the incumbent ruler dies after 1847 and before 1857 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 9: Trade Flows, Cotton, and Timing of Annexation

Dep Var: Trade Flows	(1)	(2)	(3)	(4)	(5)	(6)
Cotton Commodity & Early Annexation	-0.4985***	-0.6209***	-0.6173***	-0.4649***	-0.4417***	-0.4384***
	(0.0974)	(0.1009)	(0.0923)	(0.0359)	(0.0287)	(0.0433)
Early Annexation	0.2020	0.0800	0.0805	0.0985		
	(0.1566)	(0.0657)	(0.0649)	(0.0583)		
Cotton Commodity	0.1365**					
	(0.0518)					
Year FE		Yes				
Commodity FE		Yes				
Export. Prov. × Import. Prov. FE		Yes				
Export. Prov. × Import. Prov. × Year FE			Yes	Yes	Yes	
Commodity × Year FE			Yes	Yes	Yes	Yes
Export. Prov. × Import. Prov. × Commodity FE				Yes	Yes	Yes
Export. Block × Year FE					Yes	
Import. Block × Year FE					Yes	
Export. Block × Import. Block × Year FE						Yes
# Obs	47,447	47,447	47,447	47,447	47,447	47,447
R^2	0.0117	0.2723	0.3125	0.4188	0.5458	0.6569

The table reports the results from regressing the natural logarithm of trade flows for each commodity between trade blocks on the interaction term of early annexation and cotton commodity. Early annexation (=1) takes a value of one for trade blocks that were under direct British rule before 1800, and zero, otherwise. Cotton commodity (=1) takes a value of one for trade flows of cotton and zero for all other commodities. Standard errors reported in parentheses are clustered by exporter province × importer province. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Instrumental Variable Regression: Precolonial Cotton Production

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)
	IV	2SLS		Falsification
		Second Stage	First Stage	
Direct Rule (=1)		-0.2272*		
		(0.1270)		
Precolonial Cotton (=1)	-0.2073*		0.9120***	0.0040
	(0.1166)		(0.1326)	(0.0838)
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	1,395
R^2	0.7901		0.7786	0.8897
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

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,p(j\in p),y} + \theta_{i,y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$
$$\text{Direct Rule}_j = \gamma \cdot \text{Cotton}_j + \theta_{i,p(j\in p),y} + \theta_{i,y} + \theta_t + \delta_3 \cdot \text{Latitude}_j + \delta_4 \cdot \text{Longitude}_j + \epsilon_{i,j,t}$$

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: Investment and Precolonial Cotton Production

Dep Var: ln(Project Size)	(1)	(2)	(3)
Precolonial Cotton (=1)	-0.3838*	-0.1160	-0.0564
	(0.1879)	(0.1740)	(0.1944)
Early Annexation (=1)		-0.5800***	0.0341
		(0.1938)	(0.1601)
Precolonial Cotton × Early Annexation			-0.6448**
			(0.3073)
Qtr × Year FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes
# Obs	9,465	9,465	9,465
R^2	0.8058	0.8069	0.8070

The table reports the results of regressing present-day corporate investment on direct-ruled areas based on the timing of annexation. Early annexation (=1) takes a value of one for areas under direct British rule before 1800, and zero, otherwise. Precolonial cotton (=1) for areas that produced cotton. Standard errors are clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Human Capital Accumulation and Precolonial Cotton Production

Dep Var: 1[Higher Education]	(1)	(2)	(3)	(4)	(5)
Low caste × Early Annexation × Precolonial Cotton	-0.1161**	-0.1235***	-0.1122***	-0.1145***	-0.1132***
	(0.0448)	(0.0391)	(0.0200)	(0.0194)	(0.0220)
Low caste × Early Annexation	0.1254***	0.1337***			
	(0.0262)	(0.0192)			
Low caste × Precolonial Cotton	0.0254	-0.0013	-0.0482**	-0.0460**	-0.0429*
	(0.0254)	(0.0242)	(0.0200)	(0.0194)	(0.0220)
Precolonial Cotton × Early Annexation	0.1026*	0.1092***	0.1189***	0.1278***	
	(0.0506)	(0.0312)	(0.0213)	(0.0215)	
Early Annexation (=1)	-0.1425***				
	(0.0227)				
Low caste (=1)	-0.1622***	-0.1501***			
	(0.0172)	(0.0191)			
Precolonial Cotton (=1)	-0.0192	0.0272	0.0436*	0.0347	
	(0.0265)	(0.0190)	(0.0213)	(0.0215)	
Native State FE		Yes			
Present State FE		Yes			
Native State × Caste FE			Yes	Yes	Yes
Present State × Caste FE			Yes	Yes	Yes
Annexation Year FE				Yes	
District FE					Yes
Lat/Long					Yes
# Obs	41,278	41,278	41,278	41,278	41,278
R^2	0.0422	0.0604	0.0751	0.0767	0.0993

The table reports the results of regressing an indicator for whether the household head has received higher education on direct-ruled areas based on the timing of annexation, cotton production, and caste. Low caste (=1) takes a value of one if the household head belongs to a scheduled tribe, scheduled caste, or other backward caste. Early annexation (=1) takes a value of one for areas under direct British rule before 1800, and zero, otherwise. Precolonial cotton (=1) for areas that produced cotton. Our sample only includes districts that were under direct British rule. Education data comes from the National Demographic and Health Survey 1998/1999 wave. Standard errors are clustered by native states. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Interest in Acquiring Education and Precolonial Cotton Production

Dep Var: 1[No Interest]	(1)	(2)	(3)	(4)	(5)
Low caste × Early Annexation × Precolonial Cotton	0.3846***	0.2720***	0.2643***	0.2714***	0.1855***
	(0.0940)	(0.0695)	(0.0567)	(0.0524)	(0.0512)
Low caste × Early Annexation	-0.3303***	-0.2610***			
	(0.0656)	(0.0632)			
Low caste × Precolonial Cotton	0.0351	0.0211	0.0409	0.0338	-0.0367
	(0.0476)	(0.0300)	(0.0567)	(0.0524)	(0.0512)
Precolonial Cotton × Early Annexation	-0.3323***	0.1340*	0.1309**	0.1311**	
	(0.0779)	(0.0695)	(0.0508)	(0.0512)	
Early Annexation (=1)	0.3067***				
	(0.0330)				
Low caste (=1)	-0.0306	-0.0346			
	(0.0260)	(0.0224)			
Precolonial Cotton (=1)	-0.0044	-0.0740**	-0.0825	-0.0827	
	(0.0256)	(0.0343)	(0.0508)	(0.0512)	
Native State FE		Yes			
Present State FE		Yes			
Native State × Caste FE			Yes	Yes	Yes
Present State × Caste FE			Yes	Yes	Yes
Annexation Year FE				Yes	
District FE					Yes
Lat/Long					Yes
N	14,322	14,322	14,322	14,322	14,322
R^2	0.0027	0.0756	0.0869	0.0882	0.1319

The table presents regression results where the dependent variable is an indicator for whether the primary reason a household member did not attend school was either that education was not considered necessary or there was a lack of interest in studies. Key independent variables include indicators for directly ruled areas based on annexation timing, cotton production, and caste. Alternative reasons for not attending school include factors such as distance from school, the need to work on the family farm or in a family business, high costs, inadequate school facilities for girls, and caregiving responsibilities for siblings. Low caste (=1) takes a value of one if the household head belongs to a scheduled tribe, scheduled caste, or other backwards caste. Early annexation (=1) takes a value of one for areas under direct British rule before 1800, and zero, otherwise. Precolonial cotton (=1) for areas that produced cotton. Education data comes from the National Demographic and Health Survey 1998/1999 wave. Our sample only includes districts that were under direct British rule. Standard errors are clustered by native states. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: Border District-Pair Analysis Based on Labor Intensity Using Sample of Bordering Districts

Dep Var: ln(Project Size)	(1)	(2)	(3)
Direct Rule (=1)	-0.1084**	0.0175	
	(0.0443)	(0.0529)	
Direct Rule × High L/K Ratio		-0.2075**	-0.2875**
		(0.0892)	(0.1177)
Qtr × Year FE	Yes	Yes	Yes
Firm × District-Pair × Year FE	Yes	Yes	Yes
District × Year FE			Yes
Lat/Long	Yes	Yes	Yes
height# Observations	11,947	11,947	11,374
R^2	0.7944	0.7946	0.8179

The table reports the results of regressing the natural logarithm of project cost on the interaction of the direct British rule dummy with a High L/K Ratio dummy using a sample of bordering districts within a state, as shown in Figure 6a, according to the following specification:

$$\ln(Y_{i,j,t}) = \beta \cdot \text{DirectRule}_j \times \text{High L/K}_i + \theta_{i,p(j\in p)y} + \theta_{j,y} + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

where $Y_{i,j,t}$ is project size for firm i in district j at time t, and θ denotes various combinations of fixed effects, as specified per column. We measure an industry's labor intensity using the ratio of employees to net property, plant, and equipment, based on data from U.S. firms. Specifically, we calculate the labor intensity for all firms as the ratio of employees to net property, plant, and equipment, using annual Compustat data from 1991 to 1995. We classify firms into 48 Fama-French industries based on their historical SIC codes. For each industry, we determine the median labor intensity across all sampled firms. We then manually match each Indian firm's industry to the corresponding Fama-French industry. Firms are classified as high labor intensity if their employee-to-property, plant, and equipment ratio exceeds the sample median. Standard errors (in parentheses) are clustered by native state, district-pair, and quarter-year. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Internet Appendix for:

"The Long Shadow of Colonialism and the Geography of Corporate Investment"

Contents

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.[16] 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 the 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 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

[16] Parliament 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.

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.[17] 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.[18] 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 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.

[17] This is referred to 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.

[18] 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.

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 Disruption of Cotton Industry

In this section, we emphasize four ways through which the textile industry, specifically the cotton industry, was destroyed in India: (1) protectionism of the British textile industry through tariffs, (2) price fixing through monopsony power, (3) violence against textile producers, and (4) deprivation of new technological innovations in the local industries.

First, the enactment of protectionist policies and tariffs enabled the British to monopolize trade.[19] By 1813, British textiles faced 5% import tax in India, compared to up to 85% for Indian textiles in Britain (Alavi (1982); Nakatomi (2008)).

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 to prevent weavers from privately selling 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 – "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

[19] The British have a long history of taking actions to protect their 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.

> 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)). Sahoo (2015) writes "the financier had not entered the field" indicating lack of supply of capital to India, preventing the development of the Indian cotton mills in the same way the mills of Lancashire had developed.

Overall the attitude of the British towards the Indian cotton textile production is best described by H.H. 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 (1848)).

Appendix C Details of Project Announcement Data

Table C.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 years 1996 and 2018.

Table C.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 years 1996 and 2018.

Table C.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 years 1996 and 2018.

Appendix D Robustness

D.1 Balanced Panel Analysis

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

	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln(1+I_{j,t})$	$\ln(I_{j,t})$	ln(1+# Projects$_{j,t}$)	ln(# Projects$_{j,t}$)	$I_{j,t}$	# Projects$_{j,t}$
Direct Rule (=1)	-2.6273*	-1.6590**	-0.7431**	-1.1878**	-1.7940**	-2.6497***
	(1.3827)	(0.6903)	(0.3693)	(0.4745)	(0.7804)	(0.8618)
State × Qtr × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# Obs	35,256	17,052	35,256	19,050	35,256	35,256
R^2	0.2008	0.2655	0.2399	0.1894		
Model	OLS	OLS	OLS	OLS	Poisson	Poisson

The table reports the results from the following regression specification:

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

where $y_{j,t}$ denotes the natural logarithm of 1+total investment in district j, located in state s ($j \in s$) at time t in column 1, the natural logarithm of total investment in column 2, the natural logarithm of 1+number of projects in column 3, and the natural logarithm of the number of projects in column 4. Columns 5 and 6 use the total investment and number of projects as the dependent variable. Columns 1-4 report estimates using an OLS model. Columns 5 and 6 report estimates using a Poisson pseudo-maximum likelihood model. 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 × quarter × 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.2 Controlling for District Level Geographic Characteristics

Table D.2: 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**	-0.0916***	-0.1045***	-0.1025***	-0.0782**	-0.0926**	-0.0846**	-0.0856**
	(0.0316)	(0.0342)	(0.0340)	(0.0346)	(0.0345)	(0.0355)	(0.0322)	(0.0376)
Altitude (MSL)	-0.0001							-0.0001
	(0.0001)							(0.0001)
Coast (=1)		0.0210						0.0050
		(0.0470)						(0.0493)
ln(Area)			0.0296					0.0423
			(0.0274)					(0.0293)
Slope				0.6204				0.7599
				(0.7147)				(1.1256)
Rain (cm)					-0.0007**			-0.0006*
					(0.0003)			(0.0004)
Max-Temp						0.0042		-0.0004
						(0.0031)		(0.0033)
Min-Temp						0.0026		-0.0008
						(0.0041)		(0.0038)
ln(Distance)							-0.0217***	-0.0437**
							(0.0064)	(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:

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \Theta X_j + \theta_{i,y} + \theta_{s(j\in s),y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

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 × 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.3 Controlling for Precolonial Characteristics

Table D.3: Investment and Direct British Rule: Controlling for Precolonial Characteristics

Dep Var: ln(Project Size)	(1)	(2)	(3)
Direct Rule (=1)	-0.0831**	-0.1021***	-0.0932**
	(0.0366)	(0.0339)	(0.0356)
Maratha Ruler	-0.0330		-0.0330
	(0.0397)		(0.0415)
Muslim Ruler	-0.0212		-0.0375
	(0.0147)		(0.0302)
Prop Muslim		-0.0249	-0.0083
		(0.2006)	(0.2024)
Prop Sikhs		-0.3338	-0.3783*
		(0.2109)	(0.2225)
Prop Lower Caste		0.0646	0.0634
		(0.1188)	(0.1189)
Prop Elites		-0.0742	-0.0541
		(0.2140)	(0.2144)
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	0.7305	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:

$$
\begin{aligned}
ln(Y_{i,j,t}) =& \beta \cdot \text{Direct Rule}_j + \Theta X_j + \theta_{i,y} + \theta_{s(j\in s),y} + \theta_t \\
&+ \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}
\end{aligned}
$$

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 the 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 × 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.4 Internal Validity of Border District-Pair design

Table D.4: Border District-Pair design: Internal Validity

Dep Var: ln(Project Size)	(1)	(2)
Direct Rule (=1)	-0.0775**	-0.0877*
	(0.0355)	(0.0452)
Qtr × Year FE	Yes	Yes
Firm FE	Yes	Yes
District-Pair × Year FE	Yes	Yes
Lat/Long	Yes	Yes
# Obs	21,180	11,947
R^2	0.6876	0.7315

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:

$$\begin{aligned} ln(Y_{i,j,t}) =& \beta \cdot \text{Direct Rule}_j + \theta_i + \theta_{p(j \in p)y} + \theta_t \\ &+ \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t} \end{aligned}$$

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. $\theta_{p(j \in p)y}$ denotes the district-pair × year fixed effects. Lat/Long indicates controls for latitude and longitude of the district. Column 1 presents estimates based on the full sample of firms, without imposing the restriction that the same firm must invest in adjacent districts. Column 2 replicates column 1 from Table 5, restricting the sample to firms that invest in both the adjacent districts. Standard errors reported in parentheses are multi-way clustered by native states, district-pair, and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.5 Heterogeneity by Business Group and Government Firms

Table D.5: Robustness: Heterogeneity by Business Group and Government Firms

Dep Var: ln(Project Size)	(1)	(2)
Direct Rule (=1)	-0.1151**	-0.1098**
	(0.0529)	(0.0467)
Govt Ownership × Direct Rule	0.0319	
	(0.0961)	
Business Group Affiliation × Direct Rule		0.0388
		(0.1932)
Qtr × Year FE	Yes	Yes
Firm × District-Pair × Year FE	Yes	Yes
Lat/Long	Yes	Yes
# Obs	11,947	11,947
R^2	0.7944	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:

$$ln(Y_{i,j,t}) = \beta_1 \cdot \text{Direct Rule}_j + \beta_2 \cdot \text{Direct Rule}_j \times \text{Ownership Type}_i \\ + \theta_{i,p(j \in p)y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

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. Ownership Type (=1) is a binary variable that takes a value of 1 based on firm ownership. Specifically, it takes a value of one for all firms that are owned by the federal government in column 1. Business Group Affiliation takes a values of one if the firm belongs to a business group in column 2. $\theta_{i,p(j \in p)y}$ denotes the firm × district-pair × year fixed effects. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.6 Heterogeneity by Project Type

Table D.6: Robustness: Heterogeneity by Project Type

Dep Var: ln(Project Size)	(1)	(2)
Direct Rule (=1)	-0.1084**	-0.1274**
	(0.0443)	(0.0550)
Project Type = Improvement × Direct Rule		0.0543
		(0.0924)
Project Type = Improvement		0.0530
		(0.1227)
Qtr × Year FE	Yes	Yes
Firm × District-Pair × Year FE	Yes	Yes
Lat/Long	Yes	Yes
# Obs	11,947	11,947
R^2	0.7944	0.7945

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:

$$ln(Y_{i,j,t}) = \beta_1 \cdot \text{Direct Rule}_j + \beta_2 \cdot \text{Direct Rule}_j \times \text{Project Type}_i + \beta_3 \cdot \text{Project Type}_i \\ + \theta_{i,p(j\in p)y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

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. Project Type = Improvement is a binary variable that takes a value of one if the investment project intended to improve or expand existing investments. $\theta_{i,p(j\in p)y}$ denotes the firm × district-pair × year fixed effects. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.7 Instrumental Variable Regression: Precolonial Cotton Production

Table D.7: Instrumental Variable Regression: Precolonial Cotton Production

Dep Var: ln(Project Size)	(1)	(2)	(3)
	IV	2SLS	
		Second Stage	First Stage
Direct Rule (=1)		-0.3119***	
		(0.1078)	
Precolonial Cotton (=1)	-0.1293***		0.4144***
	(0.0369)		(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			13.7121

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

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_j + \theta_{i,y} + \theta_{s,y} + \theta_t + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

$$\text{Direct Rule}_j = \gamma \cdot \text{Cotton}_j + \theta_{i,p(j\in p),y} + \theta_{i,y} + \theta_{s,y} + \theta_t + \delta_3 \cdot \text{Latitude}_j + \delta_4 \cdot \text{Longitude}_j + \epsilon_{i,j,t}$$

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). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.8 Alternative Measures of Human Capital Accumulation

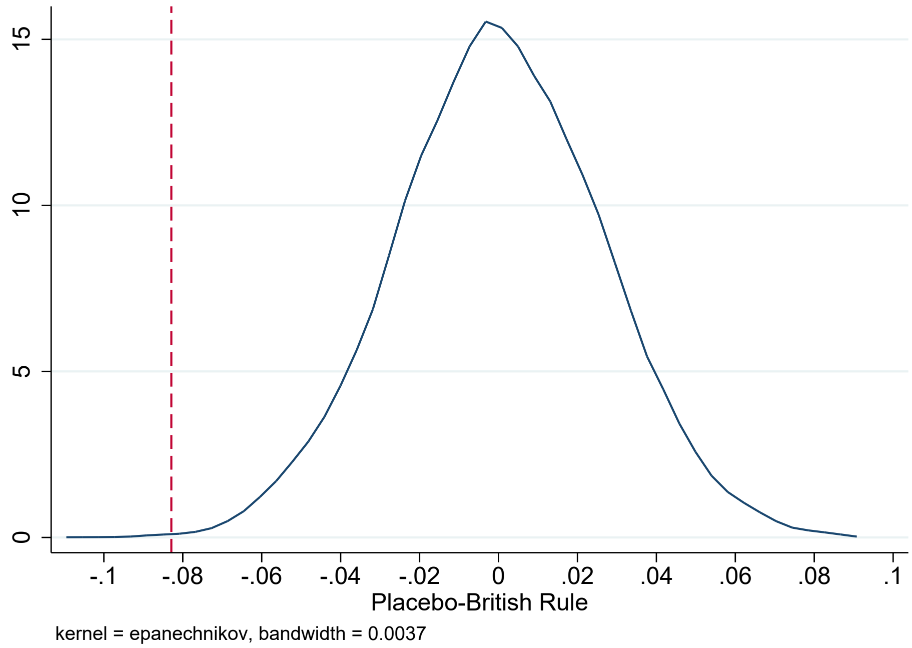
Table D.8: Alternative Measures of Human Capital Accumulation and Precolonial Cotton Production

	(1)	(2)	(3)	(4)	(5)
Dep Var:	1[No Education]	1[Primary Edu.]	1[Secondary Edu.]	1[Literate]	# Educ. Year
Low caste × Early Annexation × Precolonial Cotton	0.6912***	-0.2613***	-0.3174***	-0.6927***	-5.0703***
	(0.0518)	(0.0038)	(0.0596)	(0.0442)	(0.3787)
Low caste × Precolonial Cotton	0.0701	-0.0323***	0.0057	-0.0810*	-0.9284**
	(0.0518)	(0.0038)	(0.0596)	(0.0442)	(0.3787)
Native State × Caste FE	Yes	Yes	Yes	Yes	Yes
Present State × Caste FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes
# Obs	41,175	41,175	41,175	41,175	41,175
R^2	0.1172	0.0439	0.0558	0.1212	0.1710

The table reports regression results for five dependent variables: an indicator for whether the household head has not received any formal education (column 1), whether the highest educational level attained for the household head is primary education (column 2), whether the highest educational level attained for the household head is secondary education (column 3), an indicator for whether the household head can read and write (column 4), and the number of years of education completed by the household head (column 5). The key independent variables include indicators for directly ruled areas based on annexation timing, cotton production, and caste. Low caste (=1) takes a value of one if the household head belongs to a scheduled tribe, scheduled caste, or other backwards caste. Early annexation (=1) takes a value of one for areas under direct British rule before 1800, and zero, otherwise. Precolonial cotton (=1) for areas that produced cotton. Education data comes from the National Demographic and Health Survey 1998/1999 wave. Our sample only includes districts that were under direct British rule. Standard errors are clustered by native states. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.9 Placebo Test

Figure D.1: 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.

Appendix E Spatial Autocorrelation

We calculate Moran's I statistic to test for global spatial autocorrelation. This provides a measure 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:

$$I = \frac{\sum_{i=1}^{n} \sum_{j=1}^{n} w_{ij} z_i z_j}{\sum_{i=1}^{n} z_i^2} \tag{E.1}$$

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.

E.1 Construction of Weight Matrix

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

$$W = \begin{bmatrix} 0 & w_{1,2} & \cdots & w_{1,m-1} & w_{1,m} \\ w_{2,1} & 0 & \cdots & w_{2,m-1} & w_{2,m} \\ \vdots & \vdots & \ddots & \vdots & \vdots \\ w_{m-1,1} & w_{m-1,2} & \cdots & 0 & w_{m-1,m} \\ w_{m,1} & w_{m,2} & \cdots & w_{m,m-1} & 0 \end{bmatrix} \tag{E.2}$$

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

E.1.1 Uniform Weights

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

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

where $1_{d_{ij}<d}$ 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.

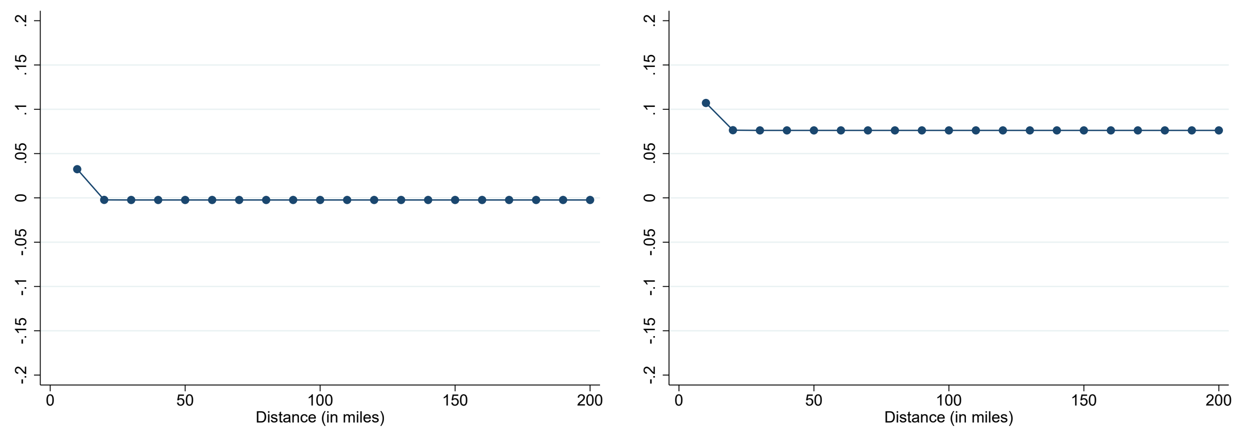
E.1.2 Inverse Weights

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

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

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.

Figure E.1: Spatial Autocorrelation - Moran I statistic



(a) Uniformly Weighted

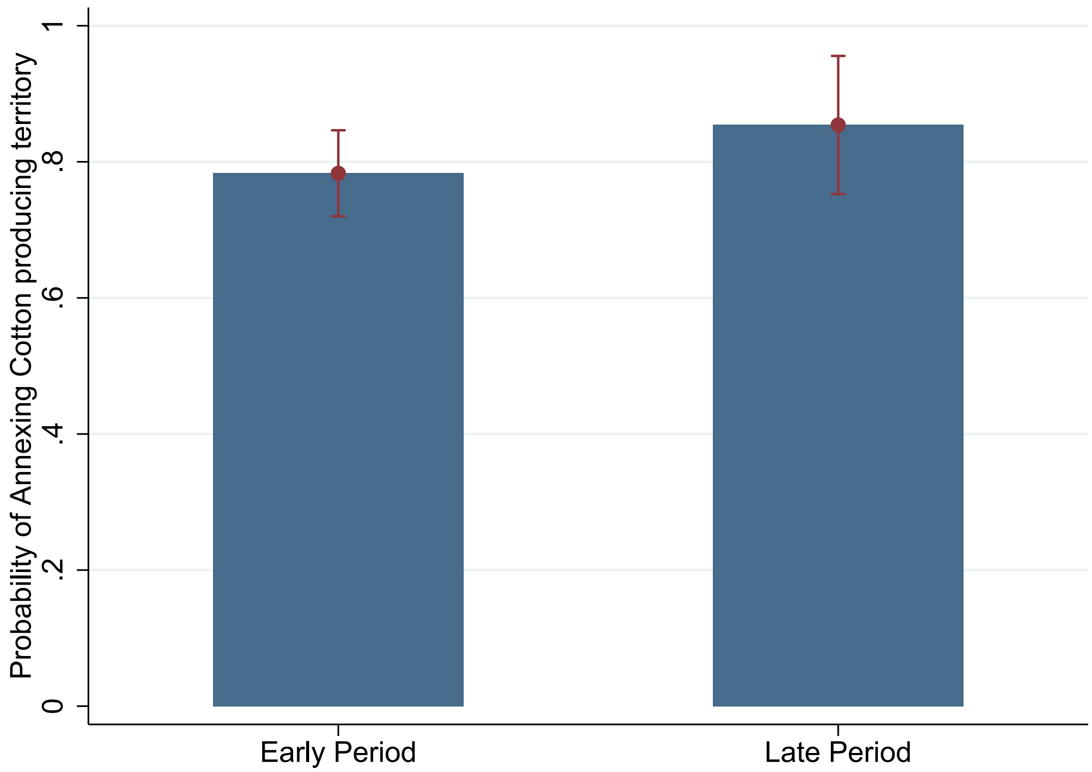
(b) Inverse Distance Weights

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 E.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 E.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 F Alternative Explanations

F.1 Annexation Timing, Cotton-Producing Area & Direct Rule

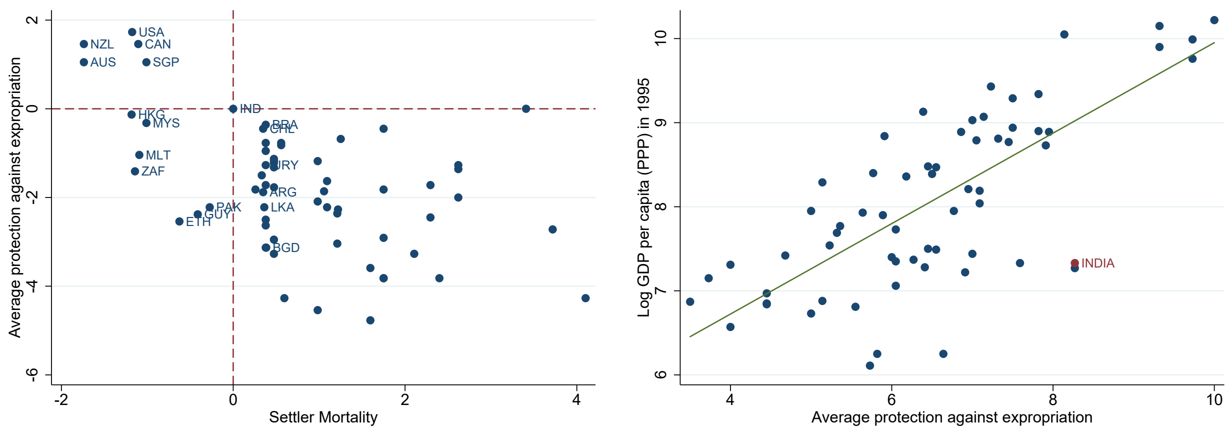
Figure F.1: Probability of Cotton-Producing Area under Direct Rule by Timing of Annexation



The figure plots the probability that a district under direct British rule is a cotton-producing district. The bar graph denotes the likelihood that a district under direct British rule is a cotton-producing territory for early and late annexation periods. Early period refers to annexation before 1800, and late period refers to annexation after 1800. Standard errors are denoted by the red error-bars.

F.2 Legal Framework & Institutions

Figure F.2: Protection Against Expropriation - Acemoglu, Johnson, and Robinson (2001)



(a) Protection Against Expropriation & Settler Mortality

(b) Protection Against Expropriation & Economic Prosperity

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 F.1: Court Delays and Direct British Rule

Dep Var: ln(Mean Delay)	(1)	(2)	(3)
Direct Rule (=1)	-0.0025	-0.0025	-0.0025
	(0.0245)	(0.0245)	(0.0243)
Statute × Year FE	Yes	Yes	
District-Pair × Year FE	Yes	Yes	
Statute × District-Pair FE		Yes	
Statute × District-Pair × 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:

$$ln(Y_{i,j,t}) = \beta \cdot \text{Direct Rule}_i + \theta_{j,p(i \in p),t} + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

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 × district-pair × year fixed effects. Lat/Long indicates controls for latitude and longitude of the district. Data on court records comes from Ash et al. (2023). 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 25$^{\text{th}}$ and 75$^{\text{th}}$ percentiles of case duration are 28 days and 746 days, respectively. Moreover, the total number of cases also exhibits a large heterogeneity. Standard errors reported in parentheses are two-way clustered by native states and district-pair. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

F.3 Public Good Provision

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 F.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	-0.0536	-0.0248	-0.0059	0.0114
	(0.0888)	(0.0601)	(0.0187)	(0.0128)	(0.0115)
District-Pair × 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

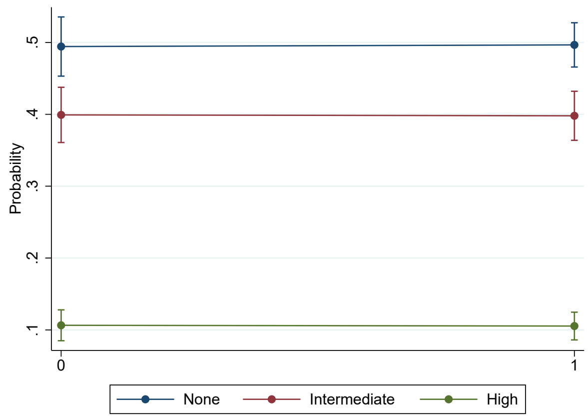
The table reports the results of regressing measures of public goods on the direct British rule dummy, per the following specification.

$$Y_{j,t} = \beta \cdot \text{Direct Rule}_j + \theta_{p(j \in p)t} + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \epsilon_{j,t}$$

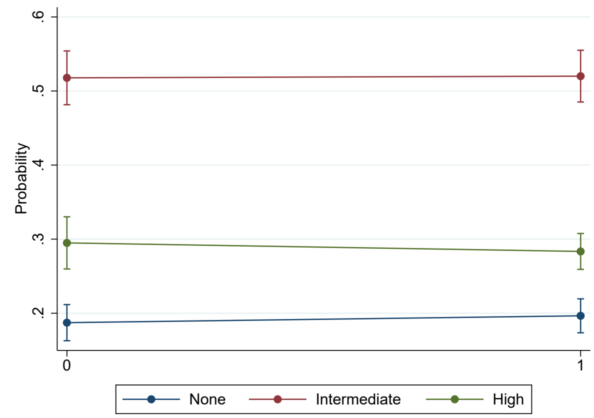
where $Y_{j,t}$ denotes the outcome variables observed for a district j at time t. $Y_{j,t}$ is the number of schools per 1,000 people in column 1, number of primary schools per 1,000 people in column 2, number of middle schools per 1,000 people in column 3, number of secondary and senior secondary schools 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 × year fixed effects. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

F.4 Trust

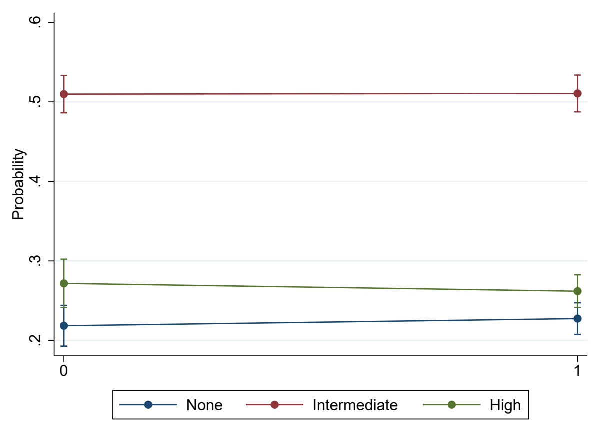
Figure F.3: Trust in State Apparatus



(a) Trust in Politicians



(b) Trust in Panchayats



(c) Trust in Police

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, and 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 politicians, police, and panchayat. 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.