

The Geography of Corporate Investment: Role of History Beyond Institutions *

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Abstract

This paper shows that colonial rule shaped contemporary spatial patterns of private investment through industrial disruption and its effects on human capital. Using variation between direct and indirect British rule in India, a border district–pair design, and an IV based on ruler succession, we find investment is 8–11% lower in historically direct-ruled districts. The British disproportionately annexed cotton-producing regions, dismantling local textile industries. Using adoption of Whitney cotton gin as an exogenous shift in incentives to suppress Indian textiles, we show earlier annexations caused greater industrial disruption, weaker transmission of industry skills, and persistent investment gaps, especially among labor-intensive firms.

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1 Introduction

What explains the geographic concentration of investment? From Steel City to Silicon Valley, the formation of economic clusters has captured the attention of academics and policymakers alike. While the geographic concentration of investment is well-documented, the underlying forces that shape and sustain these spatial clusters remain a subject of ongoing research. Understanding the forces behind these clusters is central to understanding the micro-foundations of geographic inequality – specifically, why certain regions become wealthier than others and which policies can effectively mitigate these disparities (Moretti, 2024). Existing research has largely focused on agglomerative forces – such as the availability and cost of production factors – and geographic advantages as drivers of cluster formation and persistence (see Glaeser and Gottlieb, 2009, for a survey). However, historical factors can also play a critical role in shaping present-day economic (Michalopoulos and Papaioannou, 2017a,b,c, 2020) and financial (D’Acunto, 2017, 2018, 2019; D’Acunto et al., 2019) outcomes.

In this paper, we examine how historical starting points influence firms’ investment decisions and contribute to geographic inequalities in the present. The extant literature postulates two channels through which history can affect current investment and economic growth via their effects on property rights. First, differences in legal traditions, shaped by historical circumstances, can lead to varying property rights frameworks (see North, 1991). Second, the endowment view posits that factors such as climate, crops, indigenous populations, and disease environments during European colonization shaped long-term property rights and institutional development (see Levine, 2005).

In contrast to these established frameworks, this paper explores a novel mechanism linking historical circumstances to current investment outcomes. We emphasize the role of the destruction of well-established economic organizations caused by historical events and their persistent effects on human capital accumulation. Specifically, we emphasize that such disruptions can undermine the development of skills and knowledge, i.e., human capital accumulation, and contribute to persistent geographic disparities in investment. Therefore, this paper argues that historical factors, beyond their impact on property rights or institutions, can significantly shape the geographic distribution of investment by weakening the very agglomerative forces of Marshall (1920) and locational fundamentals of Davis and Weinstein (2002) that facilitate investment and growth.

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. 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 in the same year. Finally, we introduce and test a new mechanism – colonial disruption of well-established local industries – by exploiting variation in precolonial cotton production as well as the timing of annexation relative to the invention of the Whitney cotton gin. By comparing regions that were all under direct rule, and thus subject to similar institutions, this test allows us to move beyond institutional persistence and examine how historical economic shocks continue to shape investment behavior today.

Our empirical strategy builds on the identification framework introduced in [Iyer \(2010\)](#) to address a distinct question using new data and methods. While [Iyer \(2010\)](#) examines how the persistence of colonial institutions affects long-term public goods provision at the district level, we focus on private investment decisions. Specifically, we investigate how colonial rule shapes the geography of corporate investment in the present, through the destruction of established industries, rather than through persistent local institutions. The granularity of our project-level data enables us to compare a firm’s investment choices across different districts within the same year. By including firm $\times$ year fixed effects, we mitigate concerns about non-random matching between firms and districts. To further strengthen identification, we implement a border district-pair design that compares investment by the same firm in contiguous districts within a state. Incorporating firm $\times$ district-pair $\times$ year fixed effects allows us to isolate historical variation from contemporaneous geographic or economic differences. We further build on these results by exploiting a previously unexamined historical event: the adoption of the Whitney cotton gin, which markedly changed British incentives to dismantle the local cotton industry. This variation allows us to compare outcomes across direct ruled districts that differed only in the timing of annexation. Together, these strategies allow us to examine how historical differences shape contemporary investment patterns and to identify the role of industrial disruption in the persistence of economic outcomes.

We begin our analysis by conducting a direct comparison of direct and indirect-ruled districts combined with project-level data from 1996 through 2018. We document that the size of investment projects in direct-ruled areas is 8.8% lower than the size of investment projects in indirect-ruled areas. This estimate is robust to the inclusion of firm $\times$ year and state $\times$ year fixed effects. Moreover, we characterize that the differences in investment occur at both the intensive and the extensive margins. On the extensive margin, we find that projects are 25% less likely to be announced in direct-ruled districts relative to indirect-ruled districts within a state. On average, total investment is ₹16 billion (\$ 0.2 billion) lower in direct-ruled districts relative to indirect-ruled districts within the same state. We

verify that these results are unlikely to be spurious or driven by differences in geographic conditions, legal traditions, distance to the state capital, demographic characteristics of the general population or elites during the colonial era, and spatial trends.

While these tests are informative, a direct comparison of investment in direct and indirect ruled 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 border district-pair design compares investments by the same firm across district pairs. Specifically, we include firm $\times$ district-pair $\times$ 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.

Furthermore, 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 previously employed in [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.

A key identifying assumption is the exclusion restriction, which requires that the death of a ruler without a natural heir affects contemporary corporate investment only through the imposition of direct British rule. We support this assumption with two falsification tests. First, we show that ruler deaths without heirs in indirectly ruled districts outside the Doctrine of Lapse period have no effect on

investment, addressing concerns that such deaths reflect other confounding factors. Second, we show that ruler deaths with heirs in indirectly ruled districts during the Doctrine of Lapse period also have no effect, ruling out geography-specific or period-specific shocks. Both tests yield null results, supporting the exclusion restriction.

Thus far, we have documented lower investment in areas that were historically under direct British rule. In the second part of our analysis, 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-run effects through disrupted patterns of human capital accumulation. Thus, this highlights a channel through which shocks to local industries can affect the geographic concentration of investment over time.

We begin by documenting that the East India Company – 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 the Whitney 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. The Whitney cotton gin greatly improved the efficiency of separating cotton fibers from seeds and was widely adopted in the US. Since the cotton grown in India could not be processed at comparable costs, 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. This context allows us to distinguish the effects of colonization across different periods to understand the effects of substantial shifts in European objectives over time, as proposed in [Michalopoulos and Papaioannou \(2020\)](#).

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 within a sample of direct ruled districts, 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. Our findings suggest 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 persist 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](#); [Munshi and Rosenzweig, 2006](#)).

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 cotton gin experience 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. We document that

the differences in physical education infrastructure and home-production needs cannot explain these differences.

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.

While multiple channels may mediate the long-run effects of changes in the objective of the British to acquire direct control of the areas, we provide systematic evidence that human capital accumulation plays an important role in shaping contemporary investment patterns. To better situate this channel within a broader set of possible mechanisms, we examine three alternative mechanisms. First, we consider whether persistent differences in legal regimes or their enforcement could drive our results (Porta et al., 1998; La Porta et al., 1999; Acemoglu et al., 2001; Glaeser and Shleifer, 2002). Second, we assess whether persistent differences in public investment explain our findings (Huillery, 2009; Dell and Olken, 2020; Lowes and Montero, 2021; Méndez and Van Patten, 2022). Third, we examine whether persistent mistrust arising from direct colonial rule could account for our results (Nunn and Wantchekon, 2011). Overall, we do not find any systematic evidence that these alternative mechanisms account for the patterns we document.

Related Literature: The key contribution of this paper 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: industrial 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 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

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

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 by highlighting the impact of 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 industrial disruption 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 view that human capital accumulation is shaped by the surrounding environment, as suggested by [Bell et al. \(2019\)](#). In our setting, these effects are particularly salient for low-caste workers.³ 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 et al. \(2014\)](#) 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.

A central contribution of this paper is to exploit variation in the timing of British annexation, combined with precolonial cotton production and the invention of Whitney’s cotton gin, to identify a specific economic mechanism through which colonial rule has persistent effects. While [Iyer \(2010\)](#) introduces the IV strategy based on the Doctrine of Lapse to study institutional persistence, we leverage within–direct-rule variation in annexation timing to isolate disruptions to a particular industry rather than differences in institutions. The invention and diffusion of the cotton gin fundamentally altered British incentives toward Indian cotton-producing regions, generating plausibly exogenous variation in the intensity of colonial economic disruption. This allows us to move beyond institutional channels and directly test whether the dismantling of a well-established local industry can shape long-run investment outcomes. Hence, our findings contribute to the research agenda proposed by [Michalopoulos and](#)

²See [North \(1991\)](#), [Levine \(2005\)](#), [Spolaore and Wacziarg \(2013\)](#), [Acemoglu et al. \(2014\)](#), [Nunn \(2014\)](#), [Michalopoulos and Papaioannou \(2017a,b,c\)](#), [Fenske et al. \(2025\)](#) and [Guardado \(2023\)](#) for a detailed review of the literature that discusses the role of historical factors in shaping present-day broad economic outcomes.

³[Chaudhuri \(1978\)](#) argues that India’s historical strength in cotton textiles rested on a large supply of skilled labor with specialized knowledge transmitted across generations. The disruption of the cotton industry reduced incentives for workers to pass down this knowledge and discouraged subsequent generations from acquiring these skills. While economic disruption might otherwise spur skill reallocation, cotton textile workers—predominantly from lower-caste communities—faced substantial social and economic barriers that limited their ability to transition into new occupations ([Broadberry and Gupta, 2005](#); [Munshi and Rosenzweig, 2006](#)).

[Papaioannou \(2020\)](#), which emphasizes the importance of distinguishing across phases of colonization to account for shifts in European objectives over time.

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 ([Banerjee and Somanathan, 2007](#); [Iyer, 2010](#); [Iyer and Weir, 2025](#)). 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](#); [Klüppel et al., 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 ethnic partitioning ([Michalopoulos and Papaioannou, 2013, 2014, 2016](#); [Alesina et al., 2016](#)), mistrust ([Nunn and Wantchekon, 2011](#)), and infrastructure development ([Berger, 2009](#); [Donaldson, 2018](#); [Dell and Olken, 2020](#)), such as health facilities and schools ([Huillery, 2009](#); [Méndez and Van Patten, 2022](#)), legal regimes affecting property rights ([Porta et al., 1998](#); [La Porta et al., 1999](#); [Acemoglu et al., 2001](#); [Glaeser and Shleifer, 2002](#); [Banerjee and Iyer, 2005](#); [Iyer, 2010](#)), and civic capital ([Guiso et al., 2004, 2008, 2011, 2013](#)).

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)

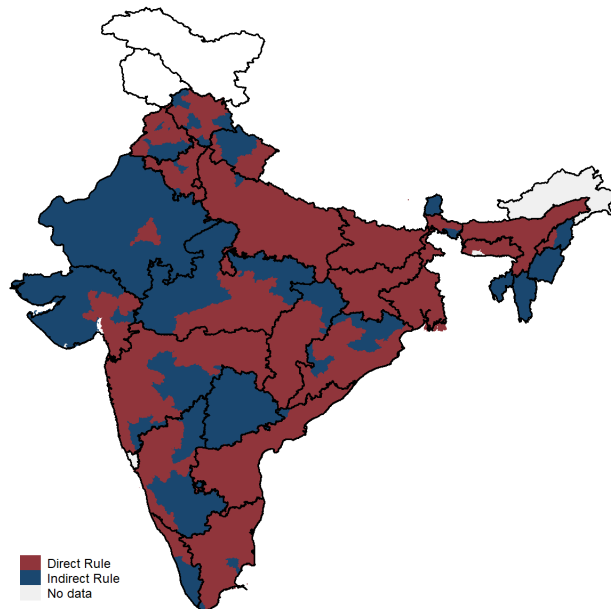
British colonial rule over the Indian subcontinent began in 1757 and lasted until 1947. After the decline of the Mughals and the Maratha Empire, India was divided into several small states ruled by royal families, referred to as *princely states*. The British Empire consolidated its grip on political 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 the Subsidiary Alliance, the East India Company (EIC) entered contracts with princely states, providing the princely states with subsidiary militia for protection and necessary defense in exchange 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, a princely state would automatically lapse to the EIC if the ruler died without a natural heir. After the Sepoy Mutiny of 1857 power was transferred from the EIC to the British Crown, putting a formal end to active British annexation of India.⁴ 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.

⁴This is referred to as the Government of India Act 1858 – an Act of the British Parliament, which nationalized the EIC and granted the British monarch supreme authority over India, as well as power and possessions of the EIC.

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.

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.

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, 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 and joined the Indian Union by 1950. Thereafter, princely rulers no longer maintained sovereignty, and the indirect-ruled districts faced similar political and legal structures as the direct-ruled districts within modern-day states.

2.2 Colonial Differences

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

Politically, rulers of princely states were under constant threat of being annexed in case of a misrule. Annexation 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 long-lasting differences in public goods provision between direct and indirect-ruled districts post-independence. However, she notes that these differences diminished substantially over time, exhibiting convergence. Using data from 1971 to 1991, Banerjee and Somanathan (2007) also document convergence in public goods provision across regions, consistent with policies of equalization followed by postcolonial governments.⁵ 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.⁶

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, the population distribution during colonial times, and precolonial cotton and silk production. Table 1 presents results from a linear probability model, regressing the likelihood of an area being under direct rule on these characteristics. Columns (1) through (5) consider each group of covariates separately, and column (6) includes all covariates jointly, including precolonial cotton and silk production. Table 1 indicates that the majority of these characteristics cannot predict direct rule. With the exception of the religion of the precolonial ruler and, most importantly, precolonial cotton production, direct and indirect-ruled areas seem evenly balanced on these observable characteristics, even when we include all covariates simultaneously in column (6). Precolonial silk production, by contrast, is not a significant predictor of direct rule in any specification, which helps rule out the possibility that the cotton result simply reflects a general tendency of the British to annex agriculturally valuable districts.

⁵Specifically, 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.

⁶The 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.

Table 1: What Predicts Direct British Rule?

Dep Var: Direct Rule (=1)	(1)	(2)	(3)	(4)	(5)	(6)
Altitude (MSL)	0.0002 (0.0003)					0.0001 (0.0002)
Coast (=1)	0.1820 (0.1176)					0.1501 (0.1055)
ln(Area)	-0.0637 (0.0816)					-0.0082 (0.0779)
Slope	-1.0837 (3.6432)					3.9455** (1.7975)
Rain (cm)	0.0015 (0.0010)					0.0012 (0.0009)
Max-Temp	0.0061 (0.0113)					-0.0064 (0.0112)
Min-Temp	0.0126 (0.0104)					-0.0023 (0.0090)
ln(Distance)		0.0396 (0.0611)				0.0378 (0.0539)
Maratha Ruler			0.2279 (0.1550)			0.0323 (0.1618)
Muslim Ruler			0.3853*** (0.1276)			0.1809 (0.1488)
Prop Muslim				0.2663 (0.3447)		-0.0824 (0.2508)
Prop Sikhs				0.6377 (1.0841)		0.0645 (0.9541)
Prop Lower Caste				0.5613 (0.3940)		0.2505 (0.3316)
Prop Elites				-0.3153 (0.6895)		-0.1117 (0.6223)
Cotton					0.4494*** (0.1185)	0.3794*** (0.1371)
Silk					0.0034 (0.1438)	0.0327 (0.1604)
Constant	0.5330 (0.8825)	0.4275 (0.3253)	0.4336*** (0.0933)	0.5111*** (0.1445)	0.3298*** (0.0939)	0.1876 (0.8527)
# Obs	294	294	294	294	294	294
R ²	0.0814	0.0042	0.1257	0.0293	0.1905	0.2803

The table reports the differences in key observables between direct and indirect-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. Column (5) uses precolonial cotton and silk production. Cotton takes a value of 1 if the district produced cotton before British rule, and 0 otherwise; Silk is defined analogously for silk production. Column (6) includes all covariates from columns (1)-(5) jointly. Standard errors reported in parentheses are clustered by native states. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.4 Discussion: Cotton Production and the Assignment of Direct Rule

The balance results in Table 1 point to precolonial cotton production as the single most important predictor of direct British rule among the characteristics we examine. The magnitude of this relationship is large: column (6) implies that a district with precolonial cotton production is 37.9 percentage points more likely to have come under direct rule, holding fixed geography, distance to the state capital, the religion and identity of the precolonial ruler, and 1911 demographic composition. This estimate is close to, and reinforces, the unconditional comparison reported later in Section 7, where we show that a cotton-producing district was 40% more likely to be under direct rule than an otherwise similar

indirect-ruled district (see Figure 3). The similarity between the conditional estimate in Table 1 and the unconditional comparison in Figure 3 indicates that the relationship between cotton production and direct rule is not an artifact of some other correlated characteristic, such as coastal access or precolonial political structure, that happens to also predict where cotton was grown.

This finding does two things for our identification strategy. First, it substantiates the historical narrative, discussed in detail in Section 7, that the East India Company targeted cotton-producing territories for direct administrative and economic control in order to secure access to raw cotton and to suppress a competing textile industry. Second, and more importantly for our empirical design, it establishes precolonial cotton production as a plausible source of variation in direct rule that operates independently of the other observable characteristics of a district. Because Table 1 shows that direct and indirect-ruled districts are otherwise balanced on geography, distance, precolonial political structure, and demographic composition, we can attribute the residual association between cotton and direct rule to the commercial motivations described in Section 7, rather than to a general British preference for annexing agriculturally advantaged or otherwise strategically valuable territory.

We note that precolonial silk production, which we include in columns 5 and 6 as a natural comparison, shows no such relationship with direct rule. Silk-producing districts were not part of the same globally competitive export industry that cotton represented for the British textile sector, and the absence of a similar pattern for silk supports our interpretation that the cotton result reflects a targeted commercial motive specific to cotton, rather than a general tendency of the British to annex regions with valuable agricultural production of any kind. Taken together, these results motivate our use of precolonial cotton production as an instrument for direct British rule in Section 6, and as the basis for our exploration of industrial disruption as a mechanism in Section 7.

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 for India-based academic research. For instance, this data has been previously employed in [Alok and Ayyagari \(2020\)](#) to study the association between the electoral cycle and the corporate investment cycle.

The CapEx database serves as the source of 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 million ($\approx$ \$0.2 million) 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 2018.⁷ 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.⁸ Appendix Figure C.1 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 across 427 districts in the sample. Appendix C presents an overview of the sample in terms of its distribution over time, amount, and project type.

3.2 Historical Geography of British Rule

The historical data on the boundaries of British rule, precolonial states, dates of annexation, mode of annexation by the British, and details of the death of kings in precolonial states comes from Iyer (2010). We verify the historical data through primary data sources including Baden-Powell (1892), Chakrabarti (1896), Hunter and Bartholomew (1908), and Lee-Warner (1910). Iyer (2010) provides a crosswalk between precolonial boundaries and districts of India, as per the 1961 Census. We manually match the 1961 Indian districts to the districts of India in 2014, using the mapping discussed in Kumar and Somanathan (2009) and current district websites.⁹ The 2014 district names are used to identify the districts where projects are announced in the CapEx database. Additionally, we collect data on precolonial cotton production, religion of the ruler, and 1911 census demographics at the district-level from Lee (2017).

⁷ 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.

⁸ 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.

⁹ District websites often include a webpage that details the history of a district, and accounts for administrative splits and name changes. If the district website does not have a history page, we refer to the Wikipedia page and verify information about splits or name changes through government notices. If a 2014 district matches with more than one 1961 district, we restrict the match to the 2014 district that reports the greatest geographic area. The detailed list of 2014 Indian districts is obtained from the Environmental Systems Research Institute (ESRI), Inc.

Lastly, 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.

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 natural heir. Districts that were acquired under misrule comprise 7% of the sample of districts under direct rule.

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

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 $\times$ year fixed effects, allowing for a comparison of the same firm's investment across direct and indirect-ruled districts in the same year.

4.1 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 $\times$ year fixed effects controlling for all time-varying observed and unobserved heterogeneity at the firm level. $\theta_{s(j \in s),y}$ denotes state $\times$ year fixed effects controlling for all time-varying observed and unobserved heterogeneity at the state level. Specifically, $\theta_{s(j \in s),y}$ controls for the law at the state level. Additionally, θ_t denotes time fixed effects accounting for aggregate shocks and temporal trends in corporate investment. 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 et al., 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 Baseline Results

Table 2 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.

Table 2: Investment and Direct British Rule

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)	(5)
Direct Rule (=1)	-0.1755** (0.0836) [0.0548]***	-0.1130*** (0.0416) [0.0356]***	-0.1146** (0.0523) [0.0371]***	-0.0864** (0.0346) [0.0332]***	-0.0881*** (0.0324) [0.0331]***
State FE	Yes	Yes	Yes	Yes	
Firm FE		Yes	Yes		
Qtr $\times$ Year FE			Yes	Yes	Yes
Firm $\times$ Year FE				Yes	Yes
State $\times$ 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 $\times$ year and state $\times$ year fixed effects respectively. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. Conley (1999) standard errors adjusted for spatial dependence within 100 km are reported in square brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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.¹⁰ 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 4.3.

¹⁰While the estimate of β , reported in Table 2, 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 et al., 2005; Oster, 2019). From column (1) to column (5), the magnitude of the point estimate of β decreases from -0.17 to -0.09 with a simultaneous increase in the model R^2 from 3% in column (1) to 72% in column (5). Instability in the estimate may suggest the presence of omitted variable bias. We address this concern by conducting an Oster (2019) test. The Oster (2019) identified set based on the change in β and the model R^2 between column (1) and column (5) is $(-0.0893, -0.0549)$, which safely excludes 0. Furthermore, we do a step-wise Oster (2019) test. The identified sets 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).

4.3 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 3 reports the results on the relation between corporate investment and direct British rule at the district level. The dependent variable in column (1) takes a value of 1 if a project is announced in that district-year and 0 otherwise. Column (2) uses total investment in district j as the dependent variable. Column (3) uses total investment in district j , conditional on positive investment, as the dependent variable. Column (4) uses the total number of projects announced in district j . Column (5) uses the total number of projects announced, conditional on announcement in district j . Column (6) uses the share of investment in district j relative to investment across all districts in the state. Column (7) uses the share of the number of projects relative to the total number of projects announced in the state as the dependent variable. We control for state $\times$ time fixed effects in all columns to account for observed and unobserved state-level time-varying heterogeneity. We interpret the estimate of direct rule (=1) as a within-state estimator.

Across all specifications, the coefficient of direct British rule is negative and statistically significant in both the extensive and the intensive margins of corporate investment. Using a linear probability model, in column (1), we find that projects are 25% less likely to be announced in direct-ruled districts relative to indirect-ruled districts within a state. Column (2) shows that total investment is ₹16 billion lower in direct-ruled districts relative to indirect-ruled districts within a state. On the intensive margin, we find that conditional on announcement, project size is lower by ₹28 billion in direct-ruled districts relative to indirect-ruled districts within a state, as shown in column (3).¹¹ In addition, on average, for five projects announced in indirect-ruled districts, only one project is announced in direct-ruled districts

¹¹The 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 et al. \(2022\)](#), 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.

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.

Table 3: Balanced Panel Analysis: Investment and Direct British Rule

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Announce=1	$I_{j,t}$	$I_{j,t} I_{j,t} > 0$	# Projects $_{j,t}$	# Projects $_{j,t} \# > 0$	$\frac{I_{j,t}}{\sum_{j \in s} I_{j,t}}$	$\frac{\text{Projects}_{j,t}}{\sum_{j \in s} \text{Projects}_{j,t}}$
Direct Rule (=1)	-0.2534* (0.1346)	-16174.5813** (7910.9896)	-28350.1337** (11777.9807)	-4.1791** (2.0257)	-6.8549** (2.7350)	-7.0724** (2.9954)	-7.1912* (3.7651)
State $\times$ Qtr $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Average	0.6453 (0.0931)	19861.4928 (5367.5878)	37851.9045 (7901.7155)	4.4693 (1.3584)	7.7296 (1.8328)	8.7459 (2.0333)	8.8529 (2.5316)
# Obs	35,256	35,256	17,052	35,256	19,050	35,256	35,256
R^2	0.1854	0.2363	0.3115	0.1800	0.1621	0.0500	0.1070

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

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

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 $\times$ quarter $\times$ year fixed effects. We aggregate the firm-location-time investment dataset at location-time level, using total investment and total number of projects announced. We create a balanced panel by assigning the value of 0 to a location-time if no projects are announced in the location-time. The dependent variable in column (1) takes a value of 0 if no project is announced in that location-time and 1 otherwise. Column (2) uses total investment in district j as the dependent variable. Column (3) uses total investment in district j , conditional on any investment as the dependent variable. Column (4) uses total number of projects announced in district j . Column (5) uses total number of projects announced, conditional on any announcements in district j . Column (6) uses the share of investment in district j relative to investment across all districts in the state. Column (7) uses the share of the number of projects relative to the total number of projects announced in the state as the dependent variable. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.4 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.

4.5 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.¹² We estimate the baseline specification, equation 1, using the random assignment of districts into direct and indirect-ruled districts as the key explanatory variable. We refer to this as placebo direct rule. We estimate the coefficient associated with the placebo direct rule and repeat this exercise 10,000 times. To 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 associated with Direct Rule from column 5 of Table 2. 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

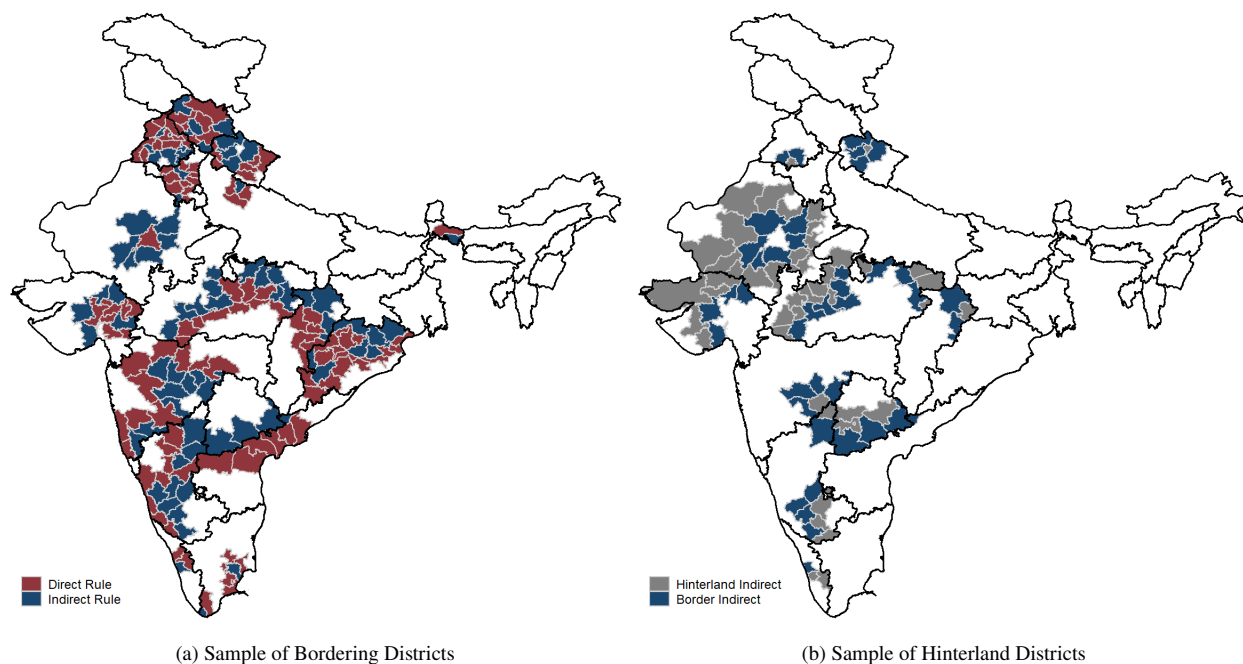
Our approach thus far, comparing the differences in investment between direct and indirect-ruled districts, faces two primary challenges. First, the British did not randomly assign districts to direct or indirect rule. Second, direct and indirect-ruled districts may systematically differ across several 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

¹²Each 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.

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 2a. 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 geographic characteristics, face similar investment opportunity shocks, and would have likely 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 et al., 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.

Figure 2: Sample for Border District-Pair Design



The Figure 2a 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 2b 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 2b are chosen based on indirect-ruled districts in Figure 2a and the indirect-ruled districts immediately bordering them. The exact international geographic boundaries have not been verified.

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 $\times$ firm $\times$ year ($\theta_{i,p(j \in p),y}$) fixed effects in our analysis. The inclusion of district-pair $\times$ firm $\times$ 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 previously employed in [Fracassi et al. \(2016\)](#) and [Kempf and Tsoutsoura \(2021\)](#) to address the non-random matching between credit rating analysts and the firms they cover and in [Ghosh et al. \(2025\)](#) to address the non-random matching between depositors and their stock investment. 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 et al. \(2010\)](#), 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 4 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 2, 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.

Table 4: Border District-Pair Analysis Using Sample of Bordering Districts

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)
Direct Rule (=1)	-0.0877* (0.0452)	-0.0974** (0.0452)	-0.1090** (0.0473)	-0.1084** (0.0443)
Qtr $\times$ Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes			
Firm $\times$ Year FE		Yes	Yes	
District-Pair $\times$ Year FE	Yes	Yes	Yes	
Firm $\times$ District-Pair FE			Yes	
Firm $\times$ District-Pair $\times$ 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 2a, 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 $\times$ district-pair $\times$ 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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.

Figure 2b 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. Appendix Table D.4 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 4 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 $\times$ district-pair $\times$ 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 5 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 $\times$ firm $\times$ 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

Table 5: Likelihood of Project Abandonment

Dep Var: Project Abandonment	(1)	(2)	(3)	(4)	(5)
	Year 1	Year 2	Year 3	Year 4	Year 5
Direct Rule (=1)	0.0094 (0.0118)	0.0284** (0.0125)	0.0256* (0.0145)	0.0257* (0.0150)	0.0248* (0.0134)
Qtr $\times$ Year FE	Yes	Yes	Yes	Yes	Yes
Firm $\times$ District-Pair $\times$ Year FE	Yes	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes
# Obs	10,938	10,938	10,938	10,938	10,938
R^2	0.5881	0.6125	0.6117	0.6153	0.6231

The table reports the probability of abandonment of project announced in direct-ruled districts relative to the projects announced in indirect-ruled districts. We identify the status of the project each year, until five years 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 $\times$ district-pair $\times$ year fixed effects, and quarter $\times$ year fixed effects. Lat/Long indicates controls for latitude and longitude of the district. Standard errors are estimated by multiway clustering at the native states, district-pair, and time of project announcement level.

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 $\times$ firm $\times$ year fixed effects and using the full sample of investment projects in bordering districts. Specifically, unlike the analysis in Table 4 we do not restrict the sample to only include firms that invest in adjacent districts. Column (1) of Appendix Table D.5 presents the results using the full sample of investment projects in bordering districts. We include district-pair $\times$ 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 4. 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 4 is likely to be internally valid.

5.2.3 Robustness – Firm Ownership & Project Type

Further, we show that our results are unlikely to be driven by firm ownership or project type. Appendix Table D.6 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). Appendix Table D.7 also 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 further address the selection issue. While the low value of the Moran I statistic, presented in Appendix E, 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, following Iyer (2010). 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 key 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. We verify the relevance assumption in the first stage.¹³ 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 6 reports the results from the 2SLS regression and compares the estimate with the IV and the OLS estimate. The sample is restricted to all indirect-ruled districts and districts under direct rule that were annexed after 1847.¹⁴ 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

¹³Lord 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.

¹⁴We do so because we are unable to construct the instrument for periods before 1848 as the policy was not applicable during other periods.

Table 6: 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*** (0.0596)		-0.2239** (0.0960)			
Rule Death, No Heir, Lapse (=1)		-0.1475* (0.0759)		0.6589*** (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:

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

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

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 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 $\times$ year and state $\times$ year fixed effects, respectively. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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, indicating that the instrument is not a weak instrument. 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 assesses the validity of the exclusion restriction, which requires that the death of a ruler without a natural heir affects contemporary corporate investment only through the imposition of direct British rule. For this restriction to hold, such deaths between 1848 and 1856 must be plausibly exogenous

and have no direct effect on present-day investment.

We first address concerns that the Doctrine of Lapse was strategically used to acquire particular territories or that rulers could self-select into or out of the policy. Historical evidence suggests neither is likely. The British did not use the Doctrine of Lapse to annex their most desired states, such as Oudh and Hyderabad, which were instead acquired through other means (Rahim, 1963). Moreover, the policy was unexpected, and there is no evidence that the British engineered ruler deaths to facilitate annexation (Iyer, 2010). Importantly, all annexed territories were administered identically under direct rule, regardless of the mode of annexation.

We next rule out alternative channels through which the death of a ruler without a natural heir might directly affect long-term outcomes, such as environmental factors, genetic conditions, or political instability. We conduct a falsification test by examining ruler deaths without heirs occurring before 1847 or after 1856 in indirectly ruled districts, where the Doctrine of Lapse did not apply. If such deaths affected present-day investment through non-colonial channels, we would expect similar effects in this sample. As shown in Column 5 of Table 6, the estimated effect is close to zero and statistically insignificant.

Finally, we test whether the timing of a ruler's death during the Doctrine of Lapse period could itself influence long-term outcomes, independent of direct rule.¹⁵ Focusing again on indirectly ruled districts, we examine deaths of rulers who had natural heirs during 1848–1856. Column 6 of Table 6 shows that the estimated effect is economically small and statistically insignificant, indicating that ruler deaths during this period do not directly 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.

¹⁵A 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.

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 played a central role in global manufacturing and textile production in the eighteenth century. In 1750, it accounted for about 25% of world manufacturing output, with textiles constituting a substantial share of this production (Marks, 2019). Specifically, Indian textiles dominated international markets, representing roughly a quarter of global textile trade during the 18th century (Maddison, 1995) and hosting the world's leading cotton textile industry (Parthasarathi, 2011; Robson, 1957). Cypher (2008) notes that this industry was characterized by skilled labor and large textile-producing towns, which posed a serious competitive threat to the British textile sector, then a leading industry in the British economy.

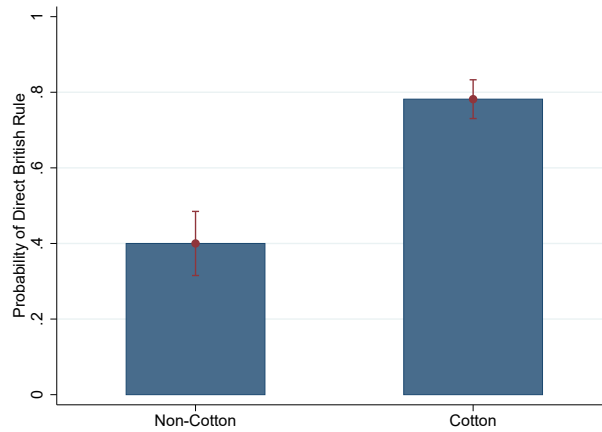
Politically, the British, through the EIC, consolidated economic power in India by annexing cotton-producing territories (Schoen, 2009). The direct control of cotton-producing areas allowed the British to control the supply of cotton, a valued commodity with a rising market share in the textile industry. 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 EIC 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-established textile industry in India 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).

Figure 3: 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.

We document empirical evidence, consistent with the historical narrative that the British took direct control of cotton-producing areas. Figure 3 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. This 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 lower economic disruption of the cotton industry in 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.¹⁶ This transformation began

¹⁶In 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).

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. The technological advance was particularly consequential for U.S. upland cotton: Whitney-style saw gins substantially lowered the cost of processing American cotton, whereas Indian producers continued to rely primarily on lower-capacity roller-ginning technologies (Lakwete, 2005; Estur and Gergely, 2010). The resulting expansion in the supply of American cotton improved Britain’s outside option for raw-cotton procurement. As a result, after the adoption of the Whitney cotton gin, Indian cotton fell out of favor with British cotton mills and the Lancashire textile manufacturers adopted the cheaper American cotton (Logan, 1965). Hence, the British had less motive to dismantle the Indian cotton textile sector after the widespread adoption of the Whitney cotton gin in 1800.

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 within a sample of direct-ruled districts 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 7 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 $\times$ importer, province $\times$ commodity fixed effects, commodity $\times$ year fixed effects, and exporter block $\times$ importer block $\times$ 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.

Table 7: Trade Flows, Cotton, and Timing of Annexation

Dep Var: Trade Flows	(1)	(2)	(3)	(4)	(5)	(6)
Cotton Commodity & Early Annexation	-0.4985*** (0.0974)	-0.6209*** (0.1009)	-0.6173*** (0.0923)	-0.4649*** (0.0359)	-0.4417*** (0.0287)	-0.4384*** (0.0433)
Early Annexation	0.2020 (0.1566)	0.0800 (0.0657)	0.0805 (0.0649)	0.0985 (0.0583)		
Cotton Commodity	0.1365** (0.0518)					
Year FE		Yes				
Commodity FE		Yes				
Export. Prov. $\times$ Import. Prov. FE		Yes				
Export. Prov. $\times$ Import. Prov. $\times$ Year FE			Yes	Yes	Yes	
Commodity $\times$ Year FE			Yes	Yes	Yes	Yes
Export. Prov. $\times$ Import. Prov. $\times$ Commodity FE				Yes	Yes	Yes
Export. Block $\times$ Year FE					Yes	
Import. Block $\times$ Year FE					Yes	
Export. Block $\times$ Import. Block $\times$ 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 $\times$ importer province. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

7.3 Long-Term Effect on Investment

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 $\times$ district-pair $\times$ year fixed effects.¹⁷ Table 8 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, indicating the relevance of the instrument. Column 2 reports the results from the second stage. The second stage estimate is negative

¹⁷We also report similar results from the full sample in Appendix Table D.8 for completeness.

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.

Table 8: 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* (0.1166)		0.9120*** (0.1326)	0.0040 (0.0838)
Qtr $\times$ Year FE	Yes	Yes	Yes	Yes
Firm $\times$ District-Pair $\times$ 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

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

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

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 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 $\times$ district-pair $\times$ 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The key identifying assumption of this analysis is that cotton cultivation during the precolonial period does not directly affect corporate investment in the present through other channels. The usage of adjacent district pairs and firm $\times$ district-pair $\times$ year fixed effects helps alleviate several threats to our identification strategy. Adjacent district pairs are likely to have similar geographic and climatic conditions, conducive to cotton cultivation. Hence, whether one district grew cotton and its bordering district did not is likely to be a matter of chance.

We further strengthen our claim for the exclusion assumption by conducting a falsification exercise. The falsification exercise focuses on a sample of adjacent indirect-ruled district pairs, in which one district grew cotton during the precolonial period and the other did not. If cotton cultivation during the

precolonial period affects present corporate investment either directly or indirectly through channels other than direct British rule, we should find differences across these adjacent district pairs that were never under direct British rule. Column 4 of Table 8 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.

Table 9: Investment and Precolonial Cotton Production

Dep Var: ln(Project Size)	(1)	(2)	(3)
Precolonial Cotton (=1)	-0.3838* (0.1879)	-0.1160 (0.1740)	-0.0564 (0.1944)
Early Annexation (=1)		-0.5800*** (0.1938)	0.0341 (0.1601)
Precolonial Cotton $\times$ Early Annexation			-0.6448** (0.3073)
Qtr $\times$ Year FE	Yes	Yes	Yes
Firm $\times$ Year FE	Yes	Yes	Yes
State $\times$ Year FE	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes
# Obs	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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 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. This comparison controls for differences in institutions, as both districts experienced direct British rule and are now part of the same state, therefore, facing similar historical and current institutional environments.

Table 9 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.

One potential concern is that our results could reflect differences in technology adoption rather than economic disruption. [Bhattacharjee \(2022a\)](#) shows that structural barriers, such as high replacement costs and resistance from British engineers unfamiliar with new technologies, slowed the diffusion of modern machinery in Indian cotton mills. If direct-ruled regions employed more British engineers, slower adoption could mechanically depress productivity and investment. While we cannot fully rule out this channel, our identification relies on early annexation before 1800, predating the period of technological stagnation documented in [Bhattacharjee \(2022a\)](#).

Another related explanation could be that our results reflect British protectionist policies favoring domestic industry rather than localized industrial dismantling. [Bhattacharjee \(2022b\)](#) shows that tariff protections introduced during the colonial period fostered limited industrial growth. However, two considerations suggest that tariffs are unlikely to be the primary driver of our results. First, tariffs were imposed at international trading ports and varied by industry, not by the geographic location of production within India. Our empirical strategy, by contrast, compares bordering districts that faced the same port-based tariff regimes but differed in their annexation status. If cotton tariffs were driving the findings, we would expect to see differential effects within the bordering-districts sample used in our falsification exercise, where instead we find no such effects. Second, protective tariffs were introduced only in 1919, long after annexation under direct rule ended in 1847, making them an unlikely explanation for the persistent divergence we document.

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](#); [Munshi and Rosenzweig, 2006](#)).

This section presents empirical evidence supporting this hypothesis. We document that lower-caste individuals exhibit reduced human capital accumulation in districts under direct British rule that historically produced cotton during the pre-colonial period and were annexed before the invention of the Whitney cotton gin. Specifically, we compare two groups of direct-ruled, precolonial cotton-producing districts: those annexed before the invention of the gin and those annexed afterward. This comparison controls for differences in institutions, as both groups experienced direct British rule and are now part of the same state, therefore, facing similar historical and current institutional environments. Moreover, since our analysis compares individuals belonging to different castes within the same district, we can also control for any differences in geography or endowments.

Using data from the National Demographic and Health Survey (DHS) for households in direct-ruled districts, 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 10 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 $\times$ caste and present-day state $\times$ 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 cotton 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 cotton 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.

Table 10: 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** (0.0448)	-0.1235*** (0.0391)	-0.1122*** (0.0200)	-0.1145*** (0.0194)	-0.1132*** (0.0220)
Low caste × Early Annexation	0.1254*** (0.0262)	0.1337*** (0.0192)			
Low caste × Precolonial Cotton	0.0254 (0.0254)	-0.0013 (0.0242)	-0.0482** (0.0200)	-0.0460** (0.0194)	-0.0429* (0.0220)
Precolonial Cotton × Early Annexation	0.1026* (0.0506)	0.1092*** (0.0312)	0.1189*** (0.0213)	0.1278*** (0.0215)	
Early Annexation (=1)	-0.1425*** (0.0227)				
Low caste (=1)	-0.1622*** (0.0172)	-0.1501*** (0.0191)			
Precolonial Cotton (=1)	-0.0192 (0.0265)	0.0272 (0.0190)	0.0436* (0.0213)	0.0347 (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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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.9, 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 11: 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*** (0.0940)	0.2720*** (0.0695)	0.2643*** (0.0567)	0.2714*** (0.0524)	0.1855*** (0.0512)
Low caste × Early Annexation	-0.3303*** (0.0656)	-0.2610*** (0.0632)			
Low caste × Precolonial Cotton	0.0351 (0.0476)	0.0211 (0.0300)	0.0409 (0.0567)	0.0338 (0.0524)	-0.0367 (0.0512)
Precolonial Cotton × Early Annexation	-0.3323*** (0.0779)	0.1340* (0.0695)	0.1309** (0.0508)	0.1311** (0.0512)	
Early Annexation (=1)	0.3067*** (0.0330)				
Low caste (=1)	-0.0306 (0.0260)	-0.0346 (0.0224)			
Precolonial Cotton (=1)	-0.0044 (0.0256)	-0.0740** (0.0343)	-0.0825 (0.0508)	-0.0827 (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
<i>N</i>	14,322	14,322	14,322	14,322	14,322
<i>R</i> 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 11 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 cotton 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. Moreover, we verify in Appendix Table D.10 that

this lack of interest in acquiring education is not driven by lack of adequate schooling infrastructure or home production needs.

By examining variation across individuals within the same entity, our analysis isolates the impact of historical events on human capital, separately from their effects on broader institutional development – a key challenge in the literature since the seminal work of [Gennaioli et al. \(2013\)](#) and [Acemoglu et al. \(2014\)](#).

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 labor intensity for each industry 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.¹⁸ This comparison helps us examine whether firms in more labor-intensive industries are less likely to invest in direct ruled areas.

Table 12 presents the results. Column 1 reports the baseline effect of direct rule, controlling for district-pair $\times$ firm $\times$ 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 $\times$ year fixed effects to account for any district-level selection into direct rule. The estimate for the interaction between direct rule and high labor intensity remains negative and statistically significant, reinforcing our main finding.

To strengthen our argument that the primary mechanism behind the long-term persistence of British rule operates through the disruption of economic institutions – specifically, that this leads to a sustained decline in human capital accumulation – we focus on a subset of districts historically under

¹⁸Specifically, 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.

Table 12: 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** (0.0443)	0.0175 (0.0529)	
Direct Rule $\times$ High L/K Ratio		-0.2075** (0.0892)	-0.2875** (0.1177)
Qtr $\times$ Year FE	Yes	Yes	Yes
Firm $\times$ District-Pair $\times$ Year FE	Yes	Yes	Yes
District $\times$ Year FE			Yes
Lat/Long	Yes	Yes	Yes
# 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 2a, 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

direct British rule, previously analyzed in Table 9. This restriction ensures comparability by holding the institutional context constant, as all districts in this sample share the same colonial experience and are now part of the same state, facing similar historical and current institutional environments.

Table 13 presents the results of comparing investment levels across two groups of directly ruled cotton districts: those annexed before the invention of the cotton gin and those annexed afterward. We further examine the within-district variation in investment by distinguishing firms based on labor intensity. This approach allows us to control for district $\times$ year fixed effects, accounting for unobserved, time-varying heterogeneity at the district level that could influence our findings. Our results indicate that in districts annexed before the invention of the cotton gin, labor-intensive firms are significantly less likely to undertake investment activities. This suggests that earlier annexation, likely associated with greater disruption of economic institutions, has long-lasting negative effects on firm investment, particularly for labor-intensive firms.

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

We discuss three alternative explanations that can potentially explain our results – differences in law and enforcement, provision of public goods, and trust. We show that these channels are unlikely to account for our results. Appendix sections F.2, F.3, and F.4 present a detailed discussion of these alternative explanations.

Table 13: Investment and Precolonial Cotton Production Based on Labor Intensity

Dep Var: ln(Project Size)	(1)	(2)
Precolonial Cotton $\times$ Early Annexation	-0.6448** (0.3073)	
Precolonial Cotton (=1)	-0.0564 (0.1944)	
Early Annexation (=1)	0.0341 (0.1601)	
High L/K Ratio $\times$ Precolonial Cotton $\times$ Early Annexation		-1.6420* (0.9168)
High L/K Ratio $\times$ Early Annexation		1.4500 (0.9165)
High L/K Ratio $\times$ Early Annexation		1.4500 (0.9165)
High L/K Ratio $\times$ Precolonial Cotton		1.0665** (0.4636)
District $\times$ Year FE		Yes
Qtr $\times$ Year FE	Yes	Yes
Firm $\times$ Year FE	Yes	Yes
State $\times$ Year FE	Yes	Yes
Lat/Long	Yes	Yes
# Obs	9,465	9,325
R^2	0.8070	0.8562

The table reports the results of regressing the natural logarithm of project cost on the interactions of the early annexation dummy, precolonial cotton production dummy and a High L/K Ratio dummy using a sample of direct-ruled, according to the following specification:

$$\ln(Y_{i,j,t}) = \beta_1 \cdot \text{Early}_j \times \text{Cotton}_j \times \text{High L/K}_i + \beta_2 \cdot \text{Early}_j \times \text{Cotton}_j + \beta_3 \cdot \text{Early}_j \times \text{High L/K}_i + \beta_4 \cdot \text{Cotton}_j \times \text{High L/K}_i + \theta_{iP(t)} + \theta_{jt} + \delta_1 \cdot \text{Latitude}_j + \delta_2 \cdot \text{Longitude}_j + \varepsilon_{i,j,t}$$

In this specification, the dependent variable $\ln(Y_{i,j,t})$ denotes the project size for firm i in district j at time t . The triple interaction term $\text{Early}_j \cdot \text{Cotton}_j \cdot \text{High L/K}_i$ captures the effect of being located in a directly ruled district that produced cotton, was annexed early, and had a high labor-to-capital ratio. 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. 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 are clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 IV strategy to explain the spatial differences in investment. Our micro-level estimate suggests that investment is 8-10% lower in direct-ruled areas, relative to indirect-ruled areas.

Second, we identify a novel mechanism through which direct British rule affects contemporary investment: the disruption of well-established local industries. We document that the EIC systematically annexed cotton-producing territories to protect British textile interests and expand Britain's share of global trade. 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 the Indian textile industry. Our findings indicate that lower levels of present-day investment in historically direct-ruled areas are concentrated in cotton-producing districts, particularly in cotton-producing districts annexed before the widespread adoption of the cotton gin around 1800.

Third, we show that the disruption of local industries generated persistent effects through human capital accumulation. The collapse of the cotton industry weakened incentives to transmit specialized skills across generations, and workers – predominantly from lower-caste communities – faced significant barriers to transitioning to new professions. Using education outcomes and self-reported interest in education as proxies for human capital, we find lower accumulation among lower-caste individuals in historically cotton-producing, direct-ruled districts annexed before 1800. These human capital deficits are reflected in lower contemporary investment, especially by labor-intensive firms.

Overall, we identify a novel mechanism linking colonial-era disruptions to contemporary investment outcomes and show that such disruptions can shape the geographic distribution of investment by weakening agglomerative forces and locational fundamentals, beyond their effects on property rights or institutions. In doing so, we contribute to broader discussions on the historical roots of spatial inequality.

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Online Appendix for:

**“The Geography of Corporate Investment: Role of History Beyond
Institutions”**

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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 (EIC) 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 EIC, 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 EIC. Pitt's India Act of 1774 provided for the appointment of a Board of Control and gave the government ultimate authority. EIC's autonomous control of the subcontinent was successively reduced over time.¹⁹ Further, the Charters of 1813 asserted the Crown's undoubted sovereignty over all EIC territories, while the 1833 Charter Act invested the Board of Control with full authority over EIC and increased the power of the governor-general. While the subcontinent was initially under the control of the EIC, 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 EIC 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 EIC entered contracts with princely states, providing the latter with the subsidiary militia for protection and necessary defense for payment. In the event of default by the Indian ruler, a part of their territory was surrendered to the British. The states of Hyderabad (1798), Mysore (1799), Awadh (1801), Peshwa (1802), Bhonsle and Scindia (1803), Udaipur, Jodhpur, and Jaipur (1818) were annexed under the Subsidiary Alliance. The second wave of British annexation, *subordinate isolation*, began in 1818, lasting till the Indian Mutiny of 1857. This phase was marked by the policy of Doctrine of Lapse, under the leadership of its chief architect, Lord Dalhousie, the British governor-general of India between 1848 and 1856. Under the Doctrine of Lapse, any subordinate princely state would automatically lapse to the British EIC if the ruler died without a natural heir. The

¹⁹Parliament controlled the EIC by extending its charter 20 years at a time. Those granted in 1793, 1813, 1833, and 1853 successively whittled away the EIC's commercial rights and trading monopolies.

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 EIC to the British Crown after the Sepoy Mutiny of 1857.²⁰ 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.²¹ 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.

²⁰This is referred to as the Government of India Act 1858 – an Act of the British Parliament, which nationalized the EIC and granted the British monarch supreme authority over India, as well as power and possessions of the EIC.

²¹Princely 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.²² 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 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).

²²The 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.

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

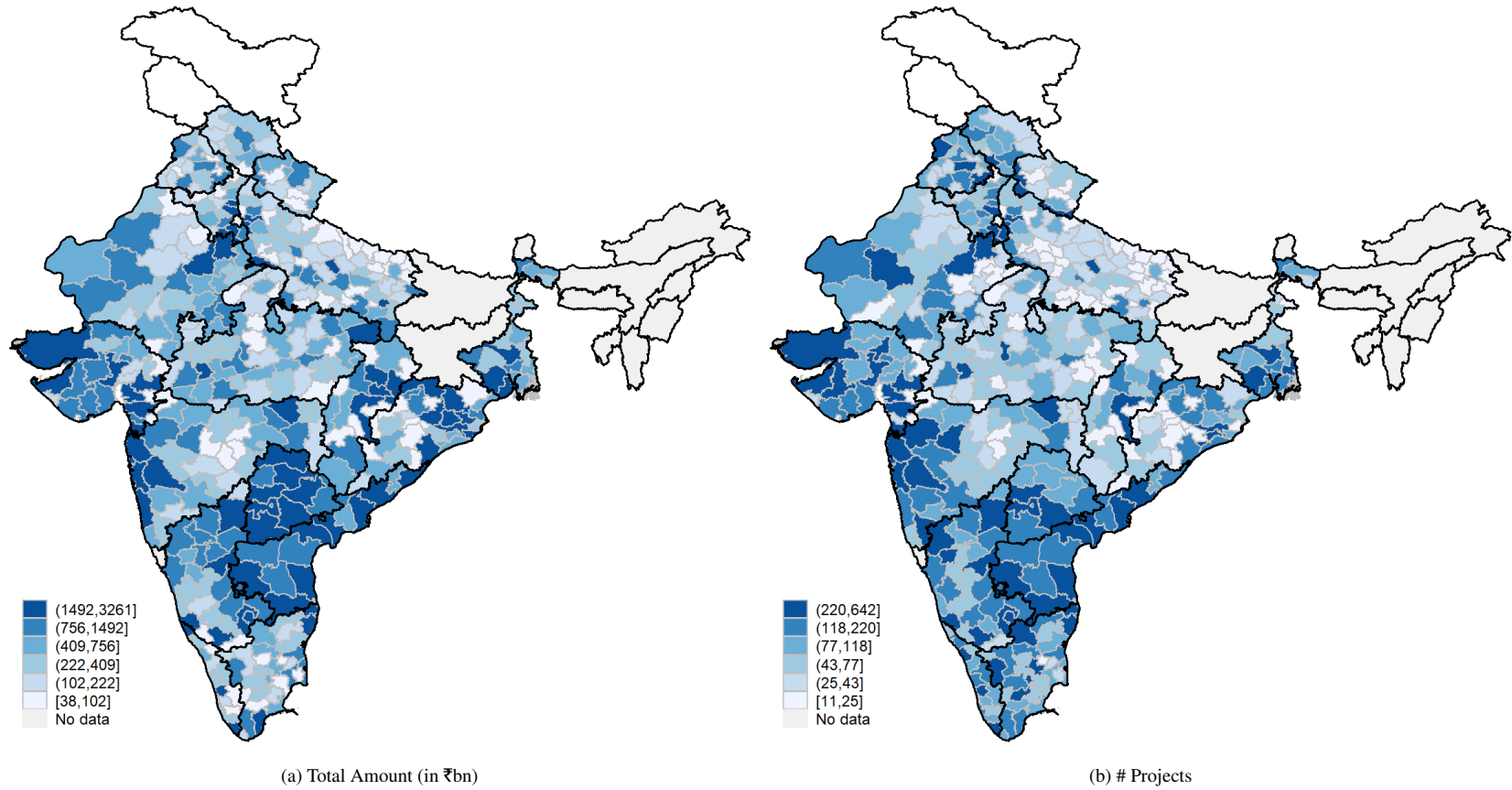
The table reports the total number of projects, the mean, the median value of projects (in ₹billion), and the total value of projects (in ₹trillion) announced across 17 states by project size between fiscal years 1996 and 2018.

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.

Figure C.1: Geography of Project Announcements



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.

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)
	$\ln(1+I_{j,t})$	$\ln(I_{j,t})$	$\ln(1+\# \text{ Projects}_{j,t})$	$\ln(\# \text{ Projects}_{j,t})$	$I_{j,t}$	$\# \text{ Projects}_{j,t}$
Direct Rule (=1)	-2.6273* (1.3827)	-1.6590** (0.6903)	-0.7431** (0.3693)	-1.1878** (0.4745)	-1.7940** (0.7804)	-2.6497*** (0.8618)
State $\times$ Qtr $\times$ 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 $\times$ quarter $\times$ year fixed effects. We aggregate the firm-location-time investment dataset at location-time level, using total investment announced. We create a balanced panel by assigning the value of 0 to a location-time if no projects are announced in the location-time. Column (1) includes all district-year observations and column (2) includes a district-year observation only if there was at least one project announced there. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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** (0.0316)	-0.0916*** (0.0342)	-0.1045*** (0.0340)	-0.1025*** (0.0346)	-0.0782** (0.0345)	-0.0926** (0.0355)	-0.0846** (0.0322)	-0.0856** (0.0376)
Altitude (MSL)	-0.0001 (0.0001)							-0.0001 (0.0001)
Coast (=1)		0.0210 (0.0470)						0.0050 (0.0493)
ln(Area)			0.0296 (0.0274)					0.0423 (0.0293)
Slope				0.6204 (0.7147)				0.7599 (1.1256)
Rain (cm)					-0.0007** (0.0003)			-0.0006* (0.0004)
Max-Temp						0.0042 (0.0031)		-0.0004 (0.0033)
Min-Temp						0.0026 (0.0041)		-0.0008 (0.0038)
ln(Distance)							-0.0217*** (0.0064)	-0.0437** (0.0186)
Qtr × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Obs	27,223	28,820	24,470	24,470	28,820	23,499	28,820	21,181
R ²	0.7195	0.7160	0.7318	0.7317	0.7161	0.7302	0.7161	0.7380

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

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

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 $\times$ year and state $\times$ year fixed effects respectively. Lat/Long indicates controls for latitude and longitude of the district. Standard errors reported in parentheses are two-way clustered by native states and time of project announcement. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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** (0.0366)	-0.1021*** (0.0339)	-0.0932** (0.0356)
Maratha Ruler	-0.0330 (0.0397)		-0.0330 (0.0415)
Muslim Ruler	-0.0212 (0.0147)		-0.0375 (0.0302)
Prop Muslim		-0.0249 (0.2006)	-0.0083 (0.2024)
Prop Sikhs		-0.3338 (0.2109)	-0.3783* (0.2225)
Prop Lower Caste		0.0646 (0.1188)	0.0634 (0.1189)
Prop Elites		-0.0742 (0.2140)	-0.0541 (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

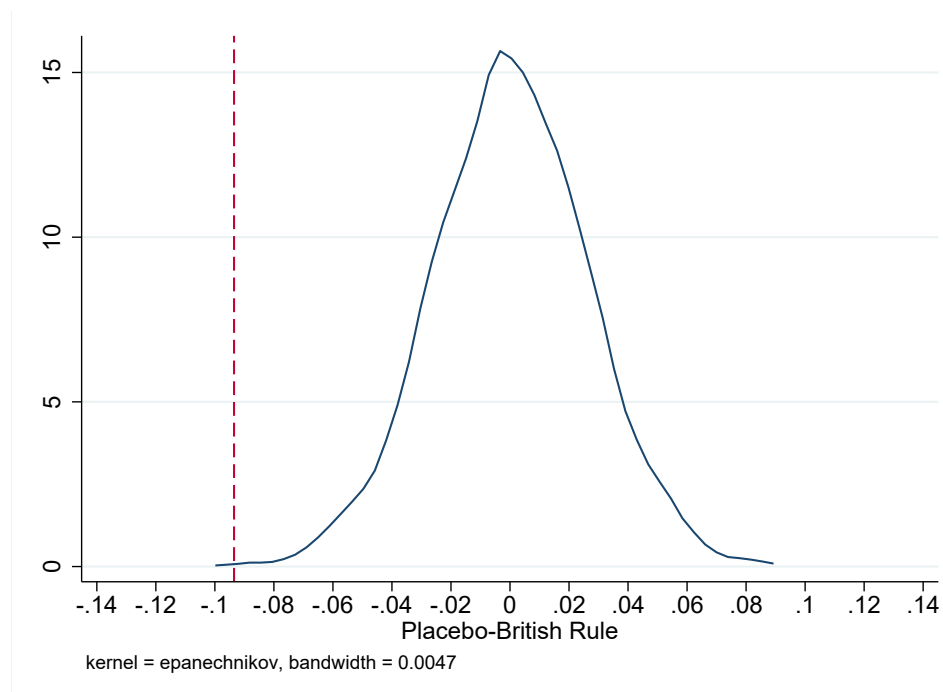
The table reports the results of regressing the natural logarithm of project cost on the direct British rule dummy after controlling for district level characteristics, per the following specification:

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

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 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 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 2. There are 0.1% of points to the left of the red-dashed line.

D.5 Border District-Pair Design using Sample of Hinterland Districts

Table D.4: Border District-Pair Design: Sample of Hinterland Districts

Dep Var: ln(Project Size)	(1)	(2)	(3)	(4)
Hinterland (=1)	-0.0407 (0.0554)	0.0382 (0.0519)	0.0353 (0.0357)	0.0355 (0.0323)
Qtr $\times$ Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes			
Firm $\times$ Year FE		Yes	Yes	
District-Pair $\times$ Year FE	Yes	Yes	Yes	
Firm $\times$ District-Pair FE			Yes	
Firm $\times$ District-Pair $\times$ 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 2b, 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 2a, and 0 otherwise. $\theta_{i,p(j \in p)y}$ denotes the firm $\times$ district-pair $\times$ 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.6 Internal Validity of Border District-Pair Design

Table D.5: Border District-Pair Design: Internal Validity

Dep Var: ln(Project Size)	(1)	(2)
Direct Rule (=1)	-0.0775** (0.0355)	-0.0877* (0.0452)
Qtr $\times$ Year FE	Yes	Yes
Firm FE	Yes	Yes
District-Pair $\times$ 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 2a, per the following specification:

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

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 $\times$ 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 4, 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.7 Heterogeneity by Business Group and Government Firms

Table D.6: Robustness: Heterogeneity by Business Group and Government Firms

Dep Var: ln(Project Size)	(1)	(2)
Direct Rule (=1)	-0.1151** (0.0529)	-0.1098** (0.0467)
Govt Ownership $\times$ Direct Rule	0.0319 (0.0961)	
Business Group Affiliation $\times$ Direct Rule		0.0388 (0.1932)
Qtr $\times$ Year FE	Yes	Yes
Firm $\times$ District-Pair $\times$ 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 2a, 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 $\times$ district-pair $\times$ 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.8 Heterogeneity by Project Type

Table D.7: Robustness: Heterogeneity by Project Type

Dep Var: ln(Project Size)	(1)	(2)
Direct Rule (=1)	-0.1084** (0.0443)	-0.1274** (0.0550)
Project Type = Improvement $\times$ Direct Rule		0.0543 (0.0924)
Project Type = Improvement		0.0530 (0.1227)
Qtr $\times$ Year FE	Yes	Yes
Firm $\times$ District-Pair $\times$ 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 2a, 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 $\times$ district-pair $\times$ 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.9 Instrumental Variable Regression: Precolonial Cotton Production

Table D.8: 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*** (0.0369)		0.4144*** (0.1119)
Qtr × Year FE	Yes	Yes	Yes
Firm × Year FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes
# Obs	19,800	19,800	19,800
R^2	0.7305		0.6738
KP LM Statistic			10.0881***
KP Wald F Statistic			13.7121

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

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

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.10 Alternative Measures of Human Capital Accumulation

Table D.9: Alternative Measures of Human Capital Accumulation and Precolonial Cotton Production

Dep Var:	(1) 1[No Education]	(2) 1[Primary Edu.]	(3) 1[Secondary Edu.]	(4) 1[Literate]	(5) # Educ. Year
Low caste × Early Annexation × Precolonial Cotton	0.6912*** (0.0518)	-0.2613*** (0.0038)	-0.3174*** (0.0596)	-0.6927*** (0.0442)	-5.0703*** (0.3787)
Low caste × Precolonial Cotton	0.0701 (0.0518)	-0.0323*** (0.0038)	0.0057 (0.0596)	-0.0810* (0.0442)	-0.9284** (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.11 Human Capital Acquisition: Ruling Out School Supply and Household Production Channels

Table D.10: School Non-Attendance: Evidence Against Infrastructure and Household Production Constraints

Dep Var:	(1)	(2)
	1[Lack of Schools]	1[Home Production]
Low caste $\times$ Early Annexation $\times$ Precolonial Cotton	-0.2349*** (0.0278)	-0.3938*** (0.0120)
Low caste $\times$ Precolonial Cotton	0.0028 (0.0278)	0.0455*** (0.0120)
Native State $\times$ Caste FE	Yes	Yes
Present State $\times$ Caste FE	Yes	Yes
District FE	Yes	Yes
Lat/Long	Yes	Yes
# Obs	14,320	14,320
R^2	0.1796	0.1736

The table reports regression results where the dependent variable is an indicator for whether the primary reason a household member did not attend school was inadequate schooling facilities (column 1) or household production needs, such as household chores, farm or family business work, outside employment, or caregiving responsibilities (column 2). 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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:

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

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).

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

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.

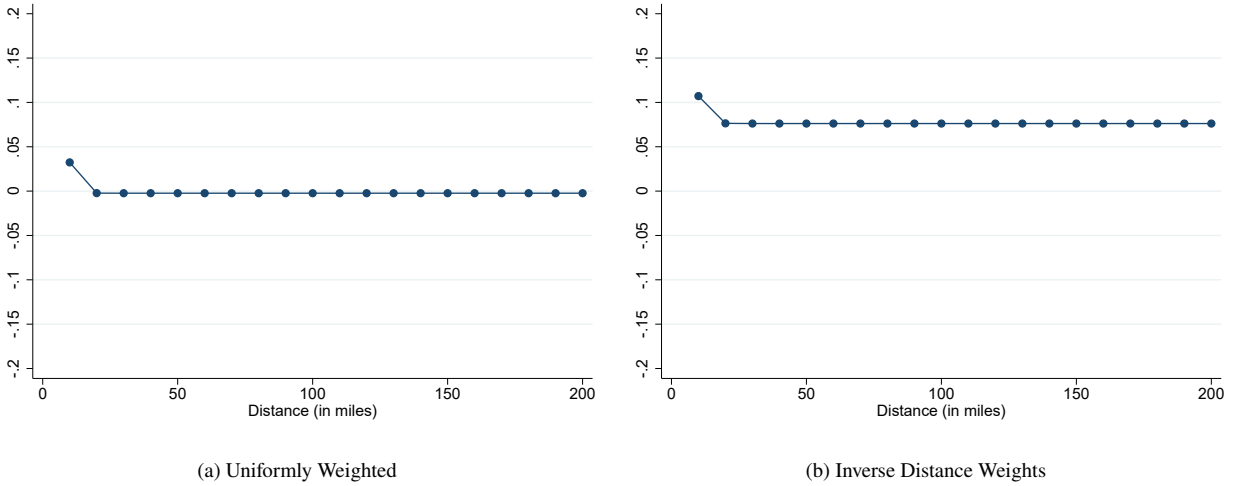
E.1.2 Inverse Weights

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

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

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

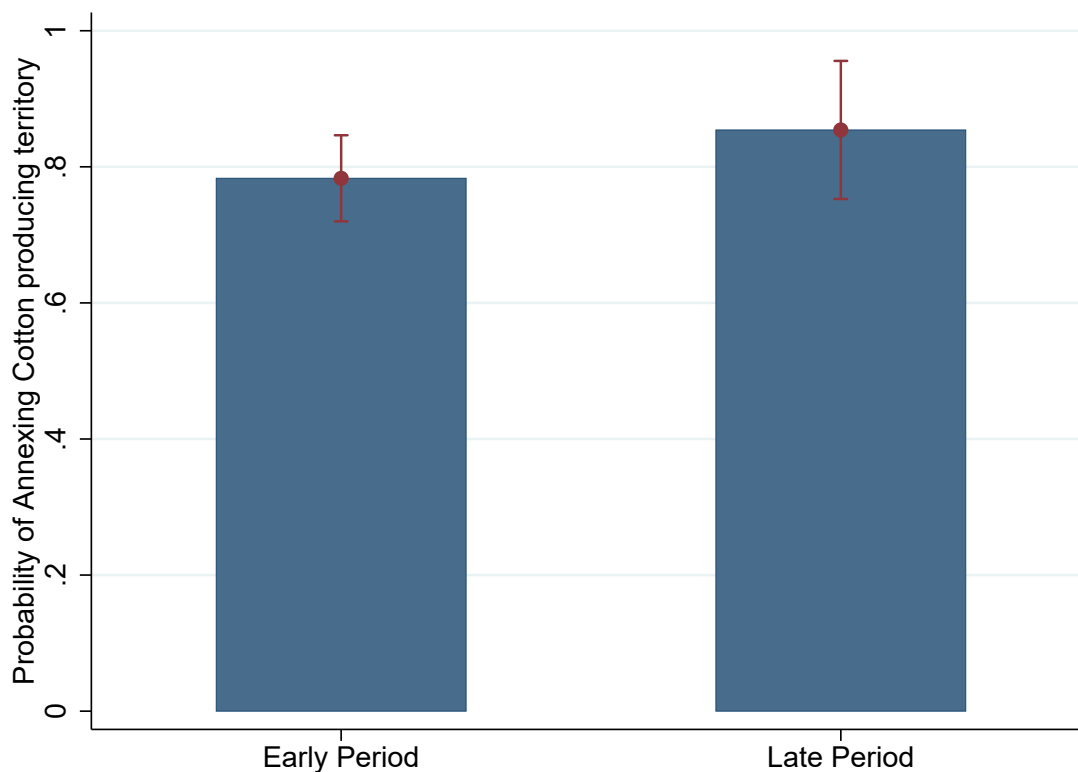


The figure plots the Moran I statistic for the Direct British Rule variable. The Moran I is computed for different distance thresholds from 10 miles to 200 miles. Figure E.1a uses the uniform weight between two points if they are within the distance limit and 0 otherwise. The weight takes a value of 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

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.

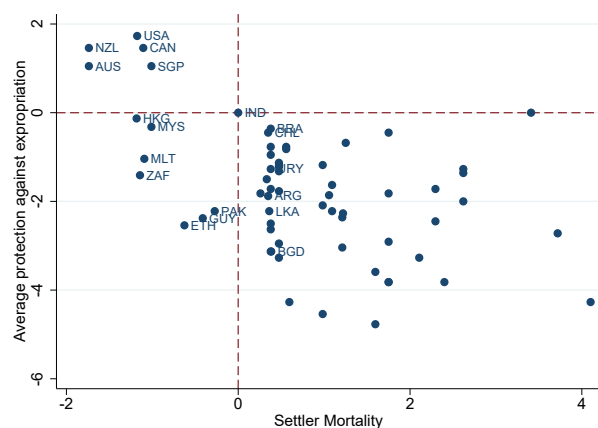
The first potential concern may be that the British faced a high mortality rate in India, discouraging them from active investments in improving the quality of institutions ([Acemoglu et al., 2001](#)). Under this hypothesis, the differences in the rule of law across direct and indirect-ruled districts 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.²³

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 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.

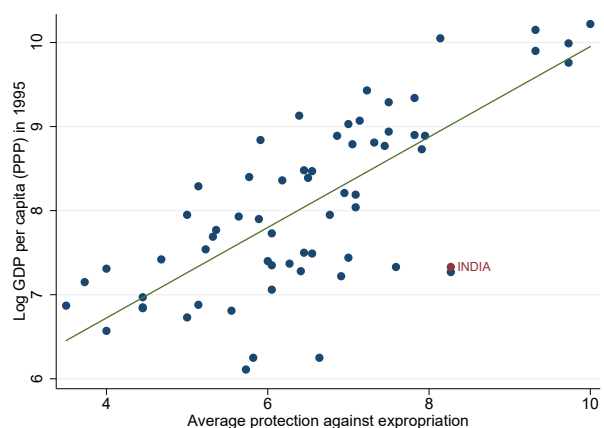
A third potential concern may be that differences in enforcement of local cases at the district level can affect projects indirectly through other channels. We rule out this hypothesis by examining differences in court delays, the average time taken by local courts to deliver a verdict for criminal cases, 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.

²³Moreover, it is worth noting that India is an outlier to the [Acemoglu et al. \(2001\)](#) 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 et al. \(2001\)](#) 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 et al. \(2001\)](#) is unlikely to explain the institutions and the relationship between institutions and economic growth in India.

Figure F.2: Protection Against Expropriation - [Acemoglu et al. \(2001\)](#)



(a) Protection Against Expropriation & Settler Mortality



(b) Protection Against Expropriation & Economic Prosperity

The figure replicates the two key results presented in [Acemoglu et al. \(2001\)](#) 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 (0.0245)	-0.0025 (0.0245)	-0.0025 (0.0243)
Statute $\times$ Year FE	Yes	Yes	
District-Pair $\times$ Year FE	Yes	Yes	
Statute $\times$ District-Pair FE		Yes	
Statute $\times$ District-Pair $\times$ Year FE			Yes
Lat/Long	Yes	Yes	Yes
# Obs	180,580	180,580	180,580
R^2	0.6155	0.7204	0.8077

The table reports the results of regressing the natural logarithm of district level mean delay in cases by statutes on the direct British rule dummy using a sample of bordering districts within states as shown in Figure 2a, 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 $\times$ district-pair $\times$ 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 25th and 75th 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 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 ([Banerjee and Somanathan, 2007](#); [Iyer, 2010](#)). 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 \(2022\)](#).

Another concern is that, while there may have been convergence in the quantity of public goods provision, differences in quality may still persist. For example, [Iyer and Weir \(2025\)](#) shows that despite convergence in access, the quality of public services remains uneven across directly and indirectly ruled areas. Consequently, the variation in colonial institutions may continue to influence contemporary investment outcomes through differences in the quality of public goods. We address this concern in two ways. First, in Table [9](#), we compare two groups of direct-ruled, pre-colonial cotton-producing districts: those annexed before the invention of the gin and those annexed afterward. This comparison controls for persistent differences in colonial institutions – whether in the quality or quantity of public goods – since both groups experienced direct British rule and share similar present-day institutional environments. Second, in Tables [12](#) and [13](#), we include district $\times$ year fixed effects, which account for all time-varying differences across districts. Therefore, our empirical methodology ensures that the effect is driven by the colonial destruction of economic organizations and its subsequent effect on human capital accumulation, rather than the persistence of colonial institutions.

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 (0.0888)	-0.0536 (0.0601)	-0.0248 (0.0187)	-0.0059 (0.0128)	0.0114 (0.0115)
District-Pair $\times$ Year FE	Yes	Yes	Yes	Yes	Yes
Lat/Long	Yes	Yes	Yes	Yes	Yes
# Obs	1,026	1,026	1,026	1,026	1,026
R^2	0.7800	0.7568	0.8272	0.8429	0.9317
Mean	1.7380	1.0845	0.3978	0.2557	0.7443
Median	1.5170	0.9248	0.3412	0.2133	0.9763
Std. Dev.	1.0615	0.6872	0.2714	0.1955	0.3278

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

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

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. We obtain data on 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 from the Population Census of 1991, 2001 and 2011, as reported in the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG). 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. Direct Rule (=1) is a binary variable that takes a value of 1 if the district was under direct British rule before independence in 1947, and 0 otherwise. $\theta_{p(j \in p)t}$ denotes the district-pair $\times$ year fixed 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

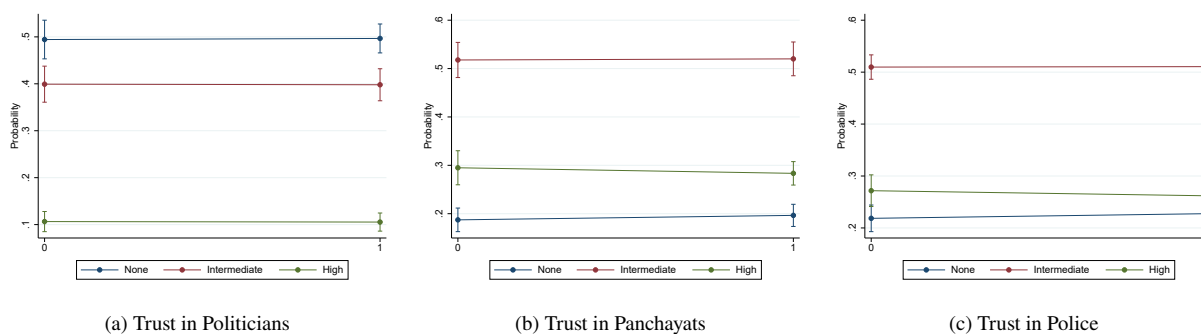
F.4 Trust

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.

Figure F.3: Trust in State Apparatus



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.