Economic Opportunity Costs: Banking Access and Educational (Dis)Investment

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Abstract

Financial development is widely credited with promoting growth. However, its effect on human capital is less clear. When bank branches enter previously unbanked Indian villages, secondary enrollment falls substantially: credit expansion stimulates local economic activity and raises the opportunity cost of adolescent time. From pre-policy wage surveys we estimate each district's wage premium to secondary completion and find that the decline is concentrated where that premium is lowest. Primary-age children, too young to work, show no decline; their enrollment rises modestly. Declines are largest where credit funds labor-intensive agriculture and where households are financially vulnerable, but strong educational culture partially attenuates it. Overall, financial development may draw adolescents out of school among the poor, for whom schooling is often the principal route upward, raising questions about intergenerational mobility.

Keywords: financial development, access to banking, economic opportunity, human capital investment, school enrollment

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1 Introduction

Financial development has long been heralded as a catalyst for economic growth and poverty reduction in developing countries (Jeremy Greenwood and Boyan Jovanovic, 1990; Robert G King and Ross Levine, 1993; Thorsten Beck, Ross Levine and Norman Loayza, 2000; Robin Burgess and Rohini Pande, 2005; Miriam Bruhn and Inessa Love, 2014). Whether it also builds human capital is far less clear (Manisha Shah and Bryce Millett Steinberg, 2017; Patrick Agte, Arielle Bernhardt, Erica Field, Rohini Pande and Natalia Rigol, 2024). For households near subsistence, access to finance changes not only what a family can afford, but also what its children's time is worth, leaving the net effect on schooling ambiguous a priori. The answer matters because schooling is the primary channel through which a household's circumstances today are passed on to the next generation (Gary S. Becker and Nigel Tomes, 1979; Gary Solon, 1999; Raj Chetty, John N. Friedman, Emmanuel Saez, Nicholas Turner and Danny Yagan, 2020). Establishing where, and for whom, financial inclusion crowds schooling in rather than out therefore speaks directly to intergenerational mobility: whether income gains compound into the next generation or are partly financed by it.

As banking services expand into rural communities, they typically stimulate local economic activity, increase household income, and reduce financial constraints. These factors could enable greater investment in children's education. However, these same economic improvements can simultaneously raise the opportunity cost of education, particularly for older children who become valuable labor resources as credit flows to labor-intensive sectors and local enterprises expand. When banks enter previously unbanked areas, this creates both an income effect (increased household resources for education) and a substitution effect (higher returns to immediate labor participation versus schooling). Previous theoretical work has posited an inverse U-shaped relationship between household wealth and child labor, where modest economic improvements may actually pull children from education into the workforce (Basu, Das and Dutta, 2010)[1]. Our study examines this tension empirically, exploring how the expansion of formal banking services in previously unbanked rural villages in India affects educational trajectories across different age groups, family backgrounds, and school types.

[1] Basu and Van (1998) establish the "luxury axiom" that families send children to work only when household income from non-child labor sources drops below a critical subsistence level; Baland and Robinson (2000) and Ranjan (2001) show that credit-market frictions force families into using child labor; Bhalotra and Heady (2003) document the empirical "wealth paradox" where land-rich households use more child labor; Basu, Das and Dutta (2010) first presents the theoretical inverse U-shaped relationship between land wealth and child labor. They further test it using Indian household survey data.

Our research explores the multifaceted relationship between financial access and educational outcomes through a quasi-experimental framework in rural India. Utilizing comprehensive data from the District Information System of Education (DISE) covering over 305,000 schools across nearly 160,000 unbanked rural villages, we examine detailed enrollment patterns by grade level, gender, caste, and school type between 2011 and 2017. We exploit a unique policy change implemented by the Reserve Bank of India (RBI) in July 2013 that required Regional Rural Banks (RRBs) to allocate at least 25 percent of their newly opened branches to unbanked rural villages—specifically tier 5 and tier 6 villages without any existing bank branch. The phased implementation of this policy across villages between 2014 and 2017 provides temporal variation that allows us to examine the dynamic effects of financial access on educational outcomes. Our setting offers two key advantages for identifying causal effects. First, unlike previous banking policies that targeted districts with limited financial access,[2] this initiative focused exclusively on completely unbanked villages,[3] providing a cleaner identification of first-order effects from introducing formal financial services. Second, RRBs are mandated to serve rural populations with branch expansion driven primarily by policy priorities rather than market incentives, mitigating endogenous selection concerns. We address remaining identification challenges through village-specific time trends, baseline characteristic interactions, and alternative control & treatment group specifications.

Our empirical strategy leverages this staggered implementation of bank branch expansion across unbanked rural villages to identify causal effects on educational outcomes. Following the policy design, our sample consists exclusively of tier 5 and tier 6 villages (populations below 10,000) that had no existing bank branches before the 2013 policy change. Our baseline specification includes several critical fixed effects and controls to address potential confounders. School fixed effects remove time-invariant school characteristics that might influence enrollment trends, ensuring identification comes from within-school changes over time. District-by-year fixed effects compare schools within the same district in each year, controlling for broader district-specific economic cycles, policy changes, and other time-varying shocks that might affect all schools in a district similarly. We further include village-specific linear time trends to account for unobserved village-level characteristics that might evolve gradually over time (Burlig and Preonas, 2024). This comprehensive approach isolates the impact of bank branch entry.

[2]See recent studies such as Chakraborty, Javadekar and Ramcharan (2021), Cramer (2025), Chang and Ravindran (2023), Kulkarni, Mahajan and Ritadhi (2023), and Jiao and Mo (2023).

[3]See "RRBs asked to open at least 25 pc branches in unbanked areas", The Economic Times, November 6, 2012. Accessed August 30, 2025. https://economictimes.indiatimes.com/news/economy/finance/rrbs-asked-to-open-at-least-25-pc-branches-in-unbanked-areas/articleshow/17119134.cms

To validate our identification strategy, we conduct several robustness checks, including tests for parallel pre-trends, alternative control group definitions, and recently developed methodologies that address potential biases in staggered difference-in-differences designs (De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021).

A necessary condition for studying how banking access affects adolescent education is to first establish that branch entry had a real effect on the local economy. Before turning to our main results, we therefore confirm that the RBI's branch mandate translated into genuine economic activity in previously unbanked villages. Firm entry increases by 7.7% and firm exits fall by 9.1%, and nightlight intensity rises as well, both pointing to real gains in local economic activity. Household income moves in the same direction: in districts most intensely exposed to branch entry, labor income rises by 13.3% and business income by 19.8%, confirming that the credit expansion translated into real income gains for households, not just aggregate activity. Having established that branch entry generated a meaningful economic shock, we next turn to how it shaped household schooling decisions

We begin with adolescents old enough to participate in the labor market it created. We document that bank branch entry reduces secondary enrollment by 13.3%. The potential contribution of secondary students to household income rises substantially, especially when local economic opportunities expand through banking access. This creates a powerful substitution effect—the opportunity cost of schooling increases as adolescents become valuable labor resources in the expanding local economy[4]. The magnitude of this effect—with secondary enrollment declining by 13.3%, representing approximately 21 fewer students per treated school—underscores the significant opportunity cost that expanding economic activity imposes on human capital investment decisions for adolescents.

This substitution mechanism should not apply to primary-age children. Being below working age, they cannot meaningfully participate in the expanding local economy, so banking access should not raise their opportunity cost of schooling the way it does for adolescents. Instead, we find that bank branch entry increases primary school enrollment by 3.2%. This is most plausibly explained by an income effect operating in the opposite direction: as financial constraints are relaxed, households can afford to send younger children to school, particularly given that primary education is subsidized under India's

[4]Shah and Steinberg (2017) find that in positive rainfall years, school-aged children perform worse on tests and report a lower attendance rate. Our results are consistent with theirs, but focus on long-term local economic growth through access to finance, which is beyond the short-term rainfall shock.

Right to Education Act. In addition, as adults intensify their own participation in expanded economic opportunities, schools may serve a valuable supervisory function for children too young to work or be left unsupervised. The improved household finances from banking access thus translate into greater educational access for primary-age children, for whom schooling carries an income benefit rather than a competing opportunity cost.

These divergent responses across age groups point to a common underlying mechanism. At its core, the decision facing a household is a trade-off between short-term income and the long-term returns to education. Bank branch entry expands local economic opportunity and raises the income an adolescent can earn immediately, while continuing in school delivers a payoff only years later. Whether a household withdraws a child from school should therefore depend on how large that deferred payoff is: where completing secondary school yields little in the local labor market, the sacrifice involved in leaving is small, and households should more readily trade schooling for immediate earnings. To test this, we construct a district-level measure of the returns to secondary education using a Mincerian wage regression on NSSO data, estimating the wage premium associated with completing secondary school separately for each district, and split treated districts into terciles of this measure. The results align closely with the proposed mechanism: the secondary enrollment decline is concentrated entirely in districts with the lowest baseline returns to secondary schooling, with no discernible effect in medium- or high-return districts. Households pull adolescents out of school precisely where the long-term payoff to finishing is smallest, and keep them enrolled where it is largest, even as short-term local labor demand rises.

Having shown that the response depends on the long-run returns to schooling, we next examine the other side of the trade-off: the size of the short-term income opportunity itself. If adolescent labor is being substituted toward newly available income-earning activity, the effect should be larger wherever credit expansion generates more of that activity. We find that the intensity and composition of credit disbursement matter substantially. In villages with high credit intensity—those receiving above-median total credit disbursement—secondary enrollment declines by 15.9%, while primary enrollment increases by 4.2%. In contrast, low credit intensity villages show statistically insignificant effects on both primary and secondary enrollment. This pattern confirms that the substitution effect on older children only becomes pronounced when credit expansion substantially stimulates local economic opportunities.

The sector allocation of credit reveals additional insight into the same trade-off. In villages where agricultural credit comprises a high share of total lending, secondary enroll-

ment drops dramatically by 23.0%, while primary enrollment increases modestly by 3.9%. Villages with low agricultural credit share experience virtually no change in secondary enrollment and similar primary enrollment gains (3.5%). This agricultural sector heterogeneity is particularly revealing: farming activities in rural India are labor-intensive and seasonal, creating immediate demand for adolescent workers who can contribute meaningfully during planting and harvest periods. The concentration of credit in agriculture thus amplifies the substitution effect by creating concrete short-term income opportunities for older children, sharpening the trade-off against the long-term returns to staying in school.[5]

Beyond these credit-driven mechanisms, we identify village-level characteristics that either amplify or mitigate the negative effects on secondary enrollment. Local educational culture, proxied by village literacy rates, serves as a powerful moderating force. In villages with high literacy rates, primary enrollment increases significantly (7.0%) while secondary enrollment shows a smaller, non-significant decline (7.6%). In contrast, villages with low literacy rates experience dramatic secondary enrollment decreases (22.5%). This pattern suggests that stronger educational norms and values—reflected in higher baseline literacy—can partially counterbalance the pull of economic opportunities for older students, making families more willing to forego immediate income in favor of continued schooling.

Conversely, household economic vulnerability amplifies the substitution effect. In villages with high proportions of marginal workers—those working fewer than six months per year and facing chronic income insecurity—secondary enrollment drops substantially (22.6%), nearly twice the decline observed in villages with medium proportions of marginal workers. This pronounced effect indicates that financially precarious households face more acute trade-offs between immediate income and long-term human capital investment. When banking access expands local economic opportunities, these vulnerable families prioritize the immediate income older children can generate over uncertain future returns from secondary education. The combination of weak educational culture (low literacy) and high economic vulnerability (high marginal worker share) creates conditions where financial development most severely undermines adolescent schooling, highlighting the importance of complementary policies to protect human capital accumulation in economically fragile communities.

[5]The informal sector represents a complementary pathway: using an informal labor absorption capacity measure from the 2013 Indian Economic Census, Table A.10 shows that secondary enrollment declines by 10.9% in villages dominated by family-operated non-farm businesses, where such capacity is highest, compared to no significant effect in low family enterprise villages.

We validate our findings through several robustness checks. First, event studies confirm parallel pre-trends between treatment and control villages before branch entry, with divergence occurring precisely after banking access expands. Second, the opposing directional effects we observe across age groups—increases in primary enrollment alongside decreases in secondary enrollment—alleviate concerns about village-specific confounders or selection driving our results, as any such factor would likely affect all education levels similarly rather than producing these sharply contrasting patterns. Third, alternative specifications addressing potential selection in branch placement—including village-characteristic interactions with year fixed effects and matched control groups—yield consistently similar estimates. Fourth, applying newer econometric methods designed for staggered difference-in-differences settings (De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021) produces results aligned with our baseline estimates. Fifth, placebo tests randomly assigning treatment years to unbanked villages reveal null effects, supporting our causal interpretation. Finally, redefining control groups to mitigate potential spillovers and using block-level fixed effects instead of district-year fixed effects does not materially change our conclusions. These consistent results across multiple specifications and methodological approaches strengthen our confidence that financial development indeed causes the observed divergent effects on educational investment across different age groups.

Our study contributes to several strands of the literature. First, this paper provides novel evidence to the emerging literature on financial development and human capital formation[6]. Few prior studies document only the positive effect of increased financial access on households' educational investment for their children[7]. Studies by Sun and Yannelis (2016), Stein and Yannelis (2020), and Chang and Ravindran (2023) find that bank branch entry significantly improve children's enrollment, graduation rates, and test scores[8]. Additionally, providing households with free savings accounts, via randomized controlled trials, increases educational spending and school attendance for girls (Prina,

[6]While prior studies typically rely on household surveys or samples of school-age children to measure educational outcomes, we use a comprehensive administrative dataset covering all registered schools across both rural and urban areas, spanning all grades and detailed facility characteristics. See Burlig and Preonas (2024) for recent DISE study and data descriptions.

[7]See Ajefu and Massacky (2022) for a recent example of the fintech adoption.

[8]Sun and Yannelis (2016) examine the impact of increased credit supply on college enrollment, leveraging the staggered state-level bank branching deregulation in the U.S. from the 1970s to the early 1990s (See also Reilly (2020*a*,*b*), which implements the same policy shock and documents positive effects on high school graduation). Similarly, Stein and Yannelis (2020) study the impact of Freedman's Savings Bank branches (1865-1870), which primarily served recently freed African Americans, finding that families with bank accounts were significantly more likely to have children enrolled in school, though they do not account for children's age. Chang and Ravindran (2023) utilize the Reserve Bank of India's 2005 branch authorization policy to provide evidence of bank branch expansion in underbanked districts.

2015; Chiapa, Prina and Parker, 2016)[9]. Utilizing staggered bank branch expansion in rural India, we show that access to finance leads to a significantly and economically large negative impact on students' enrollment. This is consistent with the theoretical inverse-U-shaped relationship between household income and child labor by focusing on the left-hand (ascending) side of the curve, where families are at the bottom of the income distribution.

Second, we contribute directly to the literature on the determinants of human capital investment. In Becker (1962) human capital framework, household income constraints and opportunity costs create a fundamental trade-off in schooling decisions. Recent studies exploit variation from infrastructure construction (e.g., Adukia, Asher and Novosad, 2020), rainfall shocks (e.g., Shah and Steinberg, 2017; Bau et al., 2020), and agricultural input prices (e.g., Sviatschi, 2022) to evaluate income-versus-substitution effects. However, these shocks on schooling decisions could be limited or partially identified due to transitory fluctuations and the indirect effect on local families. Instead, we complement this literature by exploiting the bank branch expansion, a long-run access to formal finance that directly reduces households' financial constraints and stimulates local economic growth.

Third, we contribute to the important literature that links the impact of bank branch expansion to the real economic outcomes in emerging countries. Prior studies show that access to formal financial services reduces wage inequality (Fonseca and Matray, 2024), improves income and employment (Ji, Teng and Townsend, 2023), boosts household savings (Agarwal et al., 2017), and alleviates poverty (Burgess and Pande, 2005; Burgess, Pande and Wong, 2005). To the best of our knowledge, our paper is the first to identify the intergenerational human-capital choices, documenting a "dual effect" of financial access on enrollment: modest positive impact for younger children, but large negative effect for older cohorts, highlighting an age-specific trade-off overlooked in earlier studies.

The rest of the paper proceeds as follows. Section 2 describes the branch licensing policy. Section 3 introduces the data and sets out the empirical specification. Section 4 presents the enrollment results, Section 5 tests the mechanism behind them, and Section 6 asks which households respond and why. Section 7 shows that the adolescents leaving school enter the labor force. Section 8 concludes.

[9]Prina (2015) and Chiapa, Prina and Parker (2016) utilize the same randomized controlled trial from 2010 to 2011 in Nepal that provided no-fee savings accounts to female-headed households in largely unbanked communities, and find increased educational expenditures and schooling level for girls aged 11-16 years.

2 Background of the Branch Licensing Policy

To understand the impact of access to finance on children's educational opportunities, we focus on a new bank branching policy introduced by the Reserve Bank of India (RBI) in 2013. This policy targets regional rural banks (RRBs), a major and understudied government-owned institution expanding into previously unbanked areas.[10] RRBs were created under the RRB Act of 1976, following a recommendation of the Narasimham Working Group, as institutions that would combine a cooperative bank's local knowledge with a commercial bank's professional management, with an explicit mandate to serve "small and marginal farmers, agricultural labourers, artisans and small entrepreneurs."[11] Unlike government-owned public sector banks, which operate under a broader commercial orientation and can expand in more profitable markets, RRBs are jointly owned by the central government, a public-sector sponsor bank, and the state government, and are mandated to allocate 75% of their total loans to priority sectors,[12] under their regulatory mission to support rural economies. This focus is crucial given the significant financial exclusion in India, where, as in 2011, only 35% of adults held a bank account[13], leaving approximately 557 million people unbanked. Taken together, examining a less-studied yet critical financial institution expanding in bank desert regions highlights the unique contribution of our paper.

From a Fragmented, Loss-Making Sector to a Consolidated One

For much of their history, RRBs were financially fragile. By the 1980s, 152 of 188 RRBs had accumulated losses, and government-mandated agricultural debt waivers and wage-parity awards through the 1990s pushed losses higher still, prompting a recapitalization of roughly INR 2,188 crore between 1994 and 2000.[14] The RBI responded with a two-phase amalgamation of RRBs: a first phase (2005–2010) merging RRBs sponsored by the same

[10] Prior studies examine branch expansion by private banks (e.g., retail-integrated banks in Bruhn and Love (2014)), state-owned banks (Agarwal et al., 2017), general government-owned banks (e.g., Fonseca and Matray (2024)), and mixed commercial banks (e.g., scheduled commercial banks in Burgess and Pande (2005) and Cramer (2025)), however, rural branch expansion (e.g., India's Regional Rural Banks) receives little attention.

[11] See the Regional Rural Banks Act, 1976, and the Report of the Working Group on Rural Banks (Narasimham Working Group, 1975).

[12] RRBs are required to allocate 75% of their total loans to priority sectors. See RBI details here.

[13] See "India has second largest unbanked population in the world", The Times of India, Jun 15, 2018. Accessed August 31, 2025. https://timesofindia.indiatimes.com/business/india-has-second-largest-unbanked-population-in-the-world/articleshow/64570254.cms

[14] See the Report of the Committee on the Financial System (Narasimham Committee II, 1998) and subsequent RBI annual reports on RRB recapitalization.

bank within a state, and a second phase (2012 onward) merging across sponsor banks into single state-level entities. This consolidation reduced the number of RRBs from 196 to 57 by March 2014, and coincided with the completion of core banking solution (CBS) migration across all RRBs by 2013–14. The resulting institutions were larger, better capitalized, and administratively equipped to expand their branch networks quickly and uniformly across a state, rather than branch-by-branch decisions made by many small, financially precarious entities. This institutional consolidation is what made a coordinated, RBI-mandated branch expansion into unbanked villages administratively feasible by 2013, and it also means that the branches we study were opened by a small number of large, government-controlled entities operating under a common national mandate, rather than by many independent banks each responding to local market conditions.

The 2013 Branch Authorization Mandate

RRB branches operate under Section 23 of the Banking Regulation Act, 1949, which requires RRBs to seek RBI approval for any new branch opening, conditional on fulfilling financial stability criteria.[15] In 2011, RBI first relaxed branch licensing requirements for RRBs, permitting them to open new branches without prior case-by-case approval if they met performance-based thresholds in their most recent inspection report: a capital to risk weighted assets ratio (CRAR) of at least 9%, net non-performing assets (NPA) below 5%, no default in CRAR or statutory liquidity ratio (SLR) over the preceding year, a net profit in the last financial year, and CBS compliance.[16]

More importantly, to meet the objective of banking penetration and financial inclusion rapidly in unbanked rural villages, the RBI announced a separate mandate in 2013 requiring every RRB to open branches in underbanked areas.[17] With most RRBs now meeting the 2011 performance thresholds following consolidation and recapitalization, this 2013 mandate moved the RBI from relaxing *how* RRBs could open branches to directing *where*. Specifically, this policy[18] (still in force now) requires each RRB allocate at least 25 percent

[15]Details are in Master Circular on Branch Licensing - Regional Rural Banks (RRBs). Accessed July 3, 2025. On November 28, 2025, this circular was consolidated into Reserve Bank of India (Regional Rural Banks – Branch Authorisation) Directions, 2025. The underlying rules remain in force, and the consolidation preserves the legal effect of prior actions (Reserve Bank of India Press Release, November 28, 2025).

[16]See RBI Circular: Branch Licensing Relaxation for further details. Accessed July 3, 2025. On November 28, 2025, this circular was consolidated into Reserve Bank of India (Regional Rural Banks – Branch Authorisation) Directions, 2025. The underlying rules remain in force, and the consolidation preserves the legal effect of prior actions (Reserve Bank of India Press Release, November 28, 2025).

[17]This requirement is independent of previous financial or eligibility flexibility regarding branch openings with or without RBI approval.

[18]More policy details are here. Accessed July 3, 2025. This rule remains in force and was refined in the

of their newly opened branches each year to unbanked rural villages — tier 5 or tier 6[19] villages that do not have a physical branch of any scheduled commercial bank.[20] The RBI explicitly framed the requirement as addressing "existing asymmetries in achieving financial inclusion," and structured it as a rolling three-year target (2013–16): banks were encouraged to front-load openings in unbanked centres early in the cycle, and branches opened beyond the 25% floor in a given year could be carried forward against future-year targets.[21] Because the quota is defined at the level of each bank's national annual branch-opening plan rather than targeted at specific villages, which village within the pool of eligible unbanked centres received a branch in a given year was substantially a function of internal administrative rollout planning within each state-level RRB rather than a village-specific assessment of local growth prospects.

The Political Economy of the Push into Unbanked Villages

The timing of the mandate was not incidental. It followed a decade-long policy trajectory toward universal financial access, beginning with the Rangarajan Committee on Financial Inclusion (2008), which set a target of 100% financial inclusion and prompted the RBI's first Financial Inclusion Plans (2010–13). The 2013 branch mandate extended this push into a second FIP cycle (2013–16). It also arrived at a moment of acute political urgency: the central government launched the Direct Benefit Transfer (DBT) scheme on January 1, 2013, routing cash-based welfare payments — LPG subsidies, NREGA wages, scholarships, and pensions — directly into beneficiaries' bank accounts, with the LPG subsidy component (PAHAL) rolled out in an initial 20 districts from June 2013 before scaling nationally. DBT could only reach rural beneficiaries where a bank account, and some banking access point, already existed, which put direct pressure on the RBI and on

Rationalisation of Branch Authorisation Policy (May 31, 2019): Core Banking Solution-enabled Business Correspondent outlets now count toward the "banked" condition, and non-compliance triggers a lockout from Tier 1 to Tier 4 openings. See RBI Circular: Rationalisation of Branch Authorisation Policy. On November 28, 2025, these circulars were consolidated into Reserve Bank of India (Regional Rural Banks – Branch Authorisation) Directions, 2025. The underlying rules are still in force, and the consolidation preserves the legal effect of prior actions (Reserve Bank of India Press Release, November 28, 2025).

[19] RBI classifies villages into 6 categories based on 2001 census data: Tier 1 villages have a population above 100,000; tier 2 villages have 99,999–50,000 people; tier 3 villages have 49,999–20,000 people; Tier 4 villages have 19,999–10,000 people; tier 5 villages have 9,999–5,000; tier 6 villages have less than 5,000 people.

[20] According to the RBI, scheduled commercial banks are commercial banks listed in the Second Schedule of the RBI Act, 1934. It includes public sector banks, private sector banks, foreign banks, regional rural banks, scheduled payments banks, scheduled small finance banks, and scheduled co-operative banks. As of end-March 2024, India's commercial banking sector has 141 institutions, of which 137 are scheduled banks under the Second Schedule of the Reserve Bank of India Act.

[21] See "RBI asks banks to expand branches in unbanked rural areas," Business Standard, May 28, 2013.

public-sector-controlled RRBs to close the physical banking gap in unbanked villages on a compressed timetable, independent of any single village's commercial prospects. This same financial-inclusion agenda continued past our sample period: the RBI's Committee on Comprehensive Financial Services for Small Businesses and Low-Income Households (the Nachiket Mor Committee), constituted in September 2013, recommended universal access to a bank account and new categories of payments and small finance banks, and the incoming government's Pradhan Mantri Jan Dhan Yojana (launched August 2014) subsequently made RRBs central to a nationwide account-opening drive.[22] The 2013 branch mandate we exploit therefore sits inside a broader, bipartisan push to expand physical banking access that was driven by welfare-delivery logistics and financial-inclusion targets set in Mumbai and Delhi, rather than by the anticipated economic trajectory of any particular unbanked village.

Figure 1 illustrates the expansion of RRB branches in our sample of 159,628 underbanked rural villages from 2013 to 2017. Following the policy, there is a sharp increase in new branch openings. As shown in Panel B of Figure 1, 292 of these villages received new branches after 2013. This expansion in the number of branches is associated with an increase in credit allocation in the villages (Figure 2).

To our knowledge, this is the first paper to exploit a branching policy targeted explicitly at rural villages that previously do not have any bank branches. A few prior studies use other branch authorization policies to investigate the effects of banking expansion on credit allocation (Chakraborty, Javadekar and Ramcharan, 2021), health outcomes (Cramer, 2025), economic growth (Young, 2018), firm investment (Kulkarni, Mahajan and Ritadhi, 2023), and firm-level employment shifts (Jiao and Mo, 2023). The work most closely related to this paper is Chang and Ravindran (2023), which, like Young (2018), Cramer (2025), Kulkarni, Mahajan and Ritadhi (2023), and Jiao and Mo (2023), employs the 2005 policy reform that pushed scheduled commercial banks into "underbanked" districts with a population-to-branch ratio above the national average. That policy operated on aggregate district-level variation, applied mainly to scheduled commercial banks excluding RRBs, and targeted areas with limited — but not absent — financial access.

Our setting differs on both dimensions. First, we examine branch expansion at a more granular village level, among villages with literally no prior banking presence, rather than at the district level among already-served areas. Second, RRBs are mandated to serve rural populations under a fixed lending mandate and are majority government-owned, so branch placement decisions were shaped by the administrative rollout of a national quota

[22] RRBs opened 3.02 crore Jan Dhan accounts by July 2015, over 25% of all Jan Dhan accounts opened in rural areas.

and a welfare-delivery deadline rather than by each bank's own commercial assessment of a village's prospects. We return to how these institutional features inform our empirical design in Section 3.3, and provide supporting robustness tests in Section 4.2.1.

3 Data and Empirical Design

3.1 Data Sources

School Data: We obtain large and comprehensive educational statistics from the District Information System of Education (DISE). DISE includes information on both elementary and secondary schools, covering 28 states and 8 union territories in India for the years between 2011 and 2017.[23] The data provides a wide range of school-level variables, including school characteristics (e.g., location, or school type (public or private schools)), enrollment[24] (across gender and caste), facility conditions, teacher information, and students' exam performances. With the unique school identifiers, we are able to observe the changes over time and across dimensions for each school. The location of each school is crucial for us to identify schools in villages with new bank branches opening and those without.

Bank Branch Data: Bank branch entry data is from the Directory of Bank Officials published by the Reserve Bank of India (RBI). The data is used as a directory for all banks in India. For each bank branch, we observe its location, open date, and bank type (e.g., RRBs in our setting).

Bank Branch Financial Performance: Branch level financial performance is from RBI and is widely used in the literature (e.g., Sen and De, 2018). The data consists of two parts: Basic Statistical Return (BSR) 1 for outstanding credits and BSR 2 for savings.

Indian 2001 Census: Village-level population data (2001 Census) is available on the official Indian census website. Using the Village Directory series, we are able to identify our sample of rural villages — tier 5 and 6 villages. We combine this with the Bank Branch Data to identify the unbanked rural villages, as defined by the RBI's 2013 financial inclusion policy.

Indian 2011 Census: For village baseline demographic data and information on local amenities, we rely on the 2011 Census of India. We obtain data from the Socioeconomic

[23] Data reflects the statistics of September 30th (Jagnani and Khanna (2020)) for each school year, which starts in June or July.

[24] Enrollment data of grades nine and above is available only from 2013 onward.

High-resolution Rural-Urban Geographic Platform for India (SHRUG) (Asher et al., 2021). SHRUG's identifiers of location allow us to track and link villages across 2001 and 2011 censuses. Table 1 and Table A.5 show the village characteristics used in our study.

Firm Activity: We obtain firm registration data from the Ministry of Corporate Affairs (MCA), which includes all registered private sector firms from 2011 to 2017. The data covers firm registration code, incorporation date, and location (Pincodes). It also provides information on firm status, allowing us to classify firms into two categories: entry and exit.[25] This data helps us to evaluate changes in the local business environment before and after the opening of new bank branches.

Nightlight: We use nightlight intensity data from Visible Infrared Imaging Radiometer Suite (VIIRS) for 2012-2017, which measures night light detected at the pixel level. We obtain village-year level aggregated nightlight data from SHRUG. This data helps us evaluate overall changes in economic activity and infrastructure development in villages before and after the entry of bank branches.

Child Labor: We obtain individual-level employment data from two nationally representative household surveys: National Sample Survey Office (NSSO) 68th round Employment & Unemployment Survey (2011/12) and the Periodic Labour Force Survey (PLFS) (2017/18). We restrict the sample to children aged 14–18 in rural households and construct an indicator for whether the child's principal usual activity is working[26]. The repeated cross-sections survey data allow us to compare child labor outcomes in districts with differ exposure to new bank branches before and after the branching expansion policy.

Household Income: We obtain household monnthly income data from the Consumer Pyramids Household Survey (CPHS) from 2014 to 2017[27]. CPHS reports income disaggregated by source. We focus on two components: labor income (earnings from salaried employment including wages, overtime, and bonuses), and business income (profits from self-employment and informal enterprises including farming, retail, and other own-account activities). This data allows us to assess whether the opening of new bank branches translates into measurable changes in household earnings, and whether effects differ across formal and informal sources of income.

[25]Firm entry includes entities classified as "Active." Firm exit includes those with statuses such as "Dissolved," "Strike Off," "Under Process of Striking Off," "Under Liquidation," "Liquidated," and "Converted to LLP and Dissolved".

[26]We exclude own-account workers (self-employed individuals operating without hired workers) and employers (self-employed individuals who hire workers on a regular basis), as these categories are not plausible substitutes for schooling at this age.

[27]The sample is restricted to rural households to align with the main analysis on unbanked rural villages.

3.2 Sample

Data is at the school-village-district-year level from 2011 to 2017. Following the policy design, we focus on unbanked rural villages — tier 5 and 6 villages (as per 2001 census) without any existing bank branches. Within this sample, some villages received the phased entry of new RRBs branches after 2013.

Our final sample includes 305,531 schools in 159,628 rural villages,[28] of which 292 villages receive new bank branches between 2014 to 2017. The geographical distribution of the treated and control villages, and the distribution of treatment years is in Figure 1.

Table 1 presents the summary statistics for variables used in our main analysis. In panel A, the average enrollment for Primary students (Grades 1-8, age 6-14) is 104 for the years 2011-2017, while the average enrollment for Secondary students (Grades 9-12, age 14-18) is 158 from 2013 to 2017. Panel B shows that public schools dominate rural village education, with an average of 1.58 public schools per village-year compared to 0.21 for private schools. The average nightlight intensity and minimum nightlight intensity is 0.49 and 0.12, respectively. Panel C shows that the average firm entry is 0.96 and the average firm exit is 0.5, with high standard deviations indicating uneven economic dynamics in rural areas. Panel D reports the total treatment intensity across all new branches during the post period for treated villages for 259 treated villages (limited by BSR data available only up to 2016). On average, the total credit amount increases by 35,837.64 thousand rupees. The average baseline literacy and marginal worker ratios are 0.57 and 0.29, respectively, which we use in heterogeneity tests.

3.3 Empirical Specification

To understand the impact of financial development on households' educational investment, we estimate how school-level educational outcomes respond to the introduction of new bank branches. Our baseline model leverages the staggered timing of branch opening in unbanked rural villages, and compares changes in enrollment between the treated schools in villages with new branches and control schools in villages without and those yet to receive branches:

$$Y_{s,v,d,t} = \beta_0 + \beta_1 \text{Post Branching}_{v,d,t} + \alpha X_{s,v,d,t} + \mu_s + \gamma_{d,t} + \lambda_v t + \epsilon_{s,v,d,t} \quad (1)$$

[28] To identify the policy impact of RRBs expansions and alleviate the potential effects from other types of bank branch openings (e.g., public or private sector branching policies), we restrict the sample to villages where only RRB branches expand after 2013. Also, villages with the entry of other types of bank branches only account for less than 1% of the total villages in our sample.

Here, s denotes school, v denotes village, d denotes district, and t denotes year. The main dependent variable $Y_{s,v,d,t}$ is the enrollment — Secondary or Primary grades - of school s in village v, district d, at year t. We use log enrollment plus one in our analysis. Post Branching$_{v,d,t}$ is an indicator variable equal to 1 for years after the school, in village v, district d, receives new bank branch(es) after 2013. $X_{s,v,d,t}$ is control variable for school size characteristics, including log-transformed age and log-transformed total expenditures[29] plus one. μ_s, and $\gamma_{d,t}$ are school fixed effects, and district-by-year fixed effects, which control for time-invariant school characteristics, and unobserved factors that affect all schools in villages within the same district d - year t bracket. We also include village-specific linear time trends, $\lambda_v t$, to account for unobserved village-level characteristics that change linearly over time (Burlig and Preonas, 2024). Standard errors are clustered at the village level.

The coefficient of interest is β_1 in Equation 1, which measures the impact of village-level bank branching on school-level outcomes. The identification assumption is that, in the absence of bank branch entry, school enrollment in the treated villages would have followed the same trends over time as those in control villages. District-by-year fixed effects help mitigate concerns that certain districts may prioritize financial inclusion and actively encourage banks to open branches. These fixed effects also account for district-level shocks or trends in school enrollment that might be correlated with bank branch expansion. Additionally, village-specific linear trends control for time-varying changes within villages that could potentially confound the relationship between bank branch entry and school enrollment.

To analyze the dynamic response of banking access on enrollment over time, we estimate the following distributed lag/lead model:

$$Y_{s,v,d,t} = \beta_0 + \sum_{w=-4}^{w=3} \beta_w \text{Relative Year}_{v,d,w} + \alpha X_{s,v,d,t} + \mu_s + \gamma_{d,t} + \lambda_v t + \epsilon_{s,v,d,t} \tag{2}$$

Here, Relative Year$_{v,d,w}$ is an indicator variable for each village v of district d, the relative year w before or after the entry of new bank branches. The coefficients β_w capture the impact of bank branch opening on the school outcome at each relative year w. Following Equation 1, we include school fixed effect (μ_s), district-by-year fixed effect ($\gamma_{d,t}$), and village specific linear trend ($\lambda_v t$).[30]

[29] Total expenditure is defined as the summation of all funds spent, including the Teachers Grant, School Maintenance Grant, School Development Grant, and expenditures from other funding sources.

[30] We bin the 4 years before the branch opening to $w = -4$ and use the one year before the bank branch entry ($w = -1$) as our benchmark period.

4 Results

4.1 Local Economic Activity and Household Income

Everything that follows rests on a premise that must be established before it can be used: that a new branch in a village that never had one changes the local economy in a way households can feel. This is not automatic. A branch mandated by a regulator rather than opened in pursuit of profit might collect deposits and do little else, or might lend to borrowers outside the village entirely. If that were the case, there would be no expansion in local labor demand, no repricing of a child's time, and no reason to expect schooling decisions to move at all. Table 2 shows that this is not what happens.

Panel A measures the production side. Firm entry rises by 7.7% in treated villages, and firm exit falls by 9.1%. These two margins are economically distinct and it matters that both move. Higher entry means credit is financing businesses that would not otherwise have existed, adding to the stock of local employers. Lower exit means credit is also keeping marginal incumbents alive, preserving employment that would otherwise have disappeared. A village experiencing only the first would show a churning local economy with an ambiguous net effect on labor demand; a village experiencing both is unambiguously expanding. Minimum nightlight intensity rises by 1.2%, and the choice of the minimum rather than the mean is deliberate. Mean luminosity can rise because an already-lit commercial center grows brighter, which would tell us little about the households we study. A rise in the minimum indicates that activity is increasing in the darkest, least developed parts of the treated villages, which is where the families making these schooling decisions live.

Panel B follows the money into households. Using the Consumer Pyramids Household Survey, we find that in districts with above-median branch penetration, household labor income rises by 13.3% and business income by 19.8%; in districts with low penetration, the effect is indistinguishable from zero. The decomposition is informative. Business income captures returns to household enterprise, the family farms and small shops that expand when credit becomes available. Labor income captures wages, and its increase means the expansion is tight enough to bid up the price of labor rather than merely absorbing idle hands at the going rate.

That second fact is the pivot on which the paper turns. A 13.3% increase in household labor income is, from the perspective of a family with an adolescent child, two things at the same time and inseparably so. It is additional resources, which make the fees, uniforms, and supplies of another year of schooling easier to bear. It is also a higher

wage available to any household member willing to work, and a fifteen-year-old is such a member. The literature on transitory income shocks can often hold one of these fixed while moving the other; a rainfall shock raises wages without relaxing credit constraints, and a cash transfer relaxes constraints without raising wages. Bank entry does both, in the same village, at the same moment, to the same household. Which effect dominates cannot be settled by theory and cannot be settled by looking at the average household. It has to be settled by asking where, and for whom, one force outweighs the other. That is the question the rest of the paper takes up.

4.2 Secondary Enrollment

We begin with the children old enough to work. Table 3 reports estimates of Equation 1 for grades 9 through 12, covering students roughly aged fourteen to eighteen.[31] Enrollment falls, and it falls by a great deal. The estimate is 10.5% with school and year fixed effects (Column I), 12.5% once we compare schools within the same state and year (Column II), 13.3% within district and year (Column III, our preferred specification), and 12.0% within block and year (Column IV). Evaluated at the pre-treatment mean of 157 students, the Column III coefficient implies that the average treated school loses about 21 secondary students.

The progression across columns deserves comment. Each successive specification narrows the set of schools a treated school is compared against: first to schools in the same state, then the same district, then the same block of villages. If the decline were being generated by something operating at one of these levels, such as a state changing its education budget, a district building a road, or a block experiencing an agricultural boom, then absorbing that level would attenuate the coefficient toward zero. Instead the estimate is stable, and if anything slightly larger under the tighter comparisons. Whatever is driving secondary students out of school is operating at the village level, which is the level at which branches were assigned.

The timing reinforces this. Figure 3 Panel (a) traces enrollment from four years before branch entry to three years after. In the pre-period, treated and control schools are indistinguishable, statistically and economically, which is the identifying assumption of the design made visible.[32] The decline begins in the year the branch opens and persists at

[31] We follow the classification used in the DISE data.

[32] Following Agarwal and Qian (2014), we also define a binary variable, "pre-branching," equal to one from two to seven years before the shock. As shown in Table A.4, its coefficient is statistically indistinguishable from zero.

between 6% and 23% thereafter. An immediate response is what a labor demand mechanism predicts and what most alternatives do not. Changes in educational aspirations, parental information, or school quality work slowly and would show up as a gradual divergence; a job that becomes available this season is taken this season. Cumulatively, the treated-control gap reaches 69% of baseline enrollment by the third year (Figure A.1), which across the 292 treated villages amounts to roughly 31,720 students.

The persistence matters for how the result should be read. A one-year dip in enrollment during an unusually good harvest is a timing decision, and the student returns. A decline that widens over three years is a different phenomenon: these are students who have left secondary school and are not coming back, and at this age the window for completing secondary education closes permanently.

4.2.1 Robustness

Four threats to this interpretation deserve attention, and we take them in turn.

The first is spillover. If control villages neighboring a treated village also gain access to the branch, the comparison understates the true effect, or, if labor migrates from control to treated villages, it may create a spurious one. Table 4 Column I restricts the control group to villages in blocks containing no treated village, removing the neighbors most likely to be contaminated. Column IV moves in the opposite direction, aggregating treatment to the block so that a block is treated if any village within it receives a branch, which absorbs spillovers into the treatment definition rather than trying to purge them. Both give estimates close to the baseline, suggesting that cross-village leakage is not driving the result.

The second is selection in branch placement. The policy directed branches to unbanked tier 5 and 6 villages, but within that pool banks retained discretion, and they may have chosen villages already on a different economic trajectory. Our baseline addresses this with village-specific linear time trends. Columns II and III go further, interacting baseline village and school characteristics with year fixed effects so that treated and control units are compared only against units that began the period looking alike along observable dimensions.[33] The estimates are unchanged, which does not rule out selection on unobservables but does indicate that the observable dimensions along which banks

[33]Village characteristics include population size, share of main workers, share of scheduled caste/tribe population, access to black-topped roads, power supply, mobile coverage, post offices, and primary health centers, all from the 2011 Census. School characteristics include total enrollment, student-teacher ratio, and access to toilets, libraries, medical check-ups, playgrounds, drinking water, functional electricity, and boundary walls, all as of 2011. We follow Agarwal, Chia and Ghosh (2024).

might have chosen do not account for the enrollment response.

The third is the econometrics of staggered adoption. When units are treated at different times and effects are heterogeneous, the two-way fixed effects estimator can place negative weights on some comparisons and deliver an estimate outside the range of the underlying treatment effects. Table A.2 Panel A reports the diagnostic of De Chaisemartin and d'Haultfoeuille (2020), and Panels B and C re-estimate the average effect using Sun and Abraham (2021) and Callaway and Sant'Anna (2021). All remain close to Table 3, indicating that the weighting problem is not material here.

The fourth is that the design might mechanically produce a negative coefficient for reasons unrelated to banking. We run three placebo exercises. Assigning pseudo-treatment years to villages outside tiers 5 and 6 produces no effect (Table A.3). Drawing 292 pseudo-treated villages at random from the control sample and re-estimating produces a distribution of coefficients centered near zero, with our actual estimate far in its left tail (Figure A.2 Panel A). Randomizing treatment timing within the 292 genuinely treated villages produces the same result (Figure A.2 Panel B), which confirms that the effect comes from the timing of branch entry rather than from something intrinsic to the treated villages.

4.3 Primary Enrollment

We now turn to children too young to work, and the sign reverses. Table 5 shows primary enrollment rising by 3.2% in our preferred specification (Column III), with positive and significant estimates across all four columns. At the pre-treatment mean of 104 students this is roughly three additional children per school. Figure 3 Panel (b) shows the increase emerging at entry and persisting between 2.5% and 6%, and the cumulative gain reaches 13% by the third year (Figure A.1), or about 3,942 additional students.

This opposing sign is more than a second result; it is one of the strongest pieces of identification evidence in the paper. Consider the confounders one would ordinarily worry about in a village-level design. A school consolidation, an out-migration episode, a change in how a state records attendance, a local economic collapse, a data processing change at DISE: each of these would push primary and secondary enrollment in the same direction, because each operates on the village or the school rather than on the child's age. To generate a 13.3% decline among adolescents alongside a 3.2% increase among younger children, a confounder would have to act on exactly the margin that separates a child who can work from one who cannot. The opportunity cost mechanism does this by construction. Few alternatives do.

The magnitudes are not symmetric, and the asymmetry should not be glossed over. The 31,720 secondary students lost are roughly eight times the 3,942 primary students gained. Whatever else financial inclusion accomplished in these villages, its net effect on the stock of children in rural schools was strongly negative.

4.3.1 Does the Primary Gain Persist?

The natural question is whether the children drawn into primary school stay there, or whether the primary gain and the secondary loss are the same children observed at different ages. Figure 4 estimates the effect separately for each grade and provides the answer. Enrollment gains peak at grade 4 and then decline almost monotonically, crossing zero and turning negative by grade 6. The aggregate primary effect is therefore driven entirely by grades 2 through 5, and the children who enter do not remain.

Read as a life cycle rather than a cross-section, this is precisely the pattern the trade-off generates. When a branch arrives, a household's improved finances make it feasible to enroll a seven-year-old whose labor is worth nothing and whose schooling is free under the Right to Education Act. Six years later that same child is thirteen, her labor is worth something in an economy that credit has expanded, and the calculation that favored enrollment now favors work. The gain in the lower grades and the loss in the upper ones are not independent findings pointing in opposite directions; they are two observations on a single decision rule applied to children at different ages.

Examination records tell us which children leave. Among upper primary students, where enrollment is falling, Table A.9 Columns (4) through (6) show an 8.7% increase in the number of students sitting and passing upper primary examinations and an 11.3% increase in those passing with merit. Enrollment falls while the count of successful examinees rises, which is only possible if the students who leave are drawn disproportionately from the lower part of the ability distribution. Among primary grades, where enrollment is rising, we find no significant change in the numbers appearing, passing, or passing with merit (Columns (1) through (3)). The marginal children drawn back into school are not academically stronger than those already there, and they are the same children who exit in the upper grades.

This selection has two implications that pull against each other, and we think both are worth stating. On one hand, if the students leaving are those with the weakest academic prospects, the human capital destroyed per student is smaller than the headline enrollment number suggests, and their decision to work may be privately sensible. On the other, these are exactly the children for whom completing secondary school is the dif-

ference between a lifetime of casual labor and a formal sector wage, and they are leaving at the point in the schooling sequence where the private return is concentrated.

5 Mechanism

The results so far establish a pattern: bank entry expands the local economy, raises household income, pulls adolescents out of school, and draws younger children in. We have interpreted this through a trade-off in which credit expansion raises both the resources available for schooling and the wage forgone by attending. That interpretation now has to earn its place, because the same facts are consistent with looser stories. Perhaps enrollment falls because families become able to migrate. Perhaps a branch brings other institutional changes that disrupt schools. Perhaps adolescents leave for reasons unconnected to the local labor market.

The trade-off interpretation makes predictions those alternatives do not, and the four tests in this section are designed around them. If households are weighing immediate earnings against the deferred payoff to finishing school, the decline should be concentrated where that deferred payoff is small, which we test in subsection 5.1. And if the immediate earnings come from credit-financed local production, the decline should scale with how much credit arrives (subsection 5.2), should be sharpest where credit flows to the sectors that actually employ adolescents (subsection 5.3), and should differ according to whether the borrower is a household that farms or a firm that hires (subsection 5.4). Each test uses variation that is measured independently of the enrollment data and fixed before the policy.

5.1 Local Returns to Secondary Education

Begin with the household's problem in its simplest form. A family with a fifteen-year-old chooses between two paths. If she leaves school, she earns a wage now, and the household consumes it now. If she stays, the household forgoes that wage for up to four years and receives in exchange a higher lifetime income for the child, realized only once she enters the labor market as an adult. The decision turns on the size of that premium relative to the wages given up. Where finishing secondary school commands a large wage premium in the local labor market, leaving is expensive and households should resist even a substantial increase in adolescent wages. Where the premium is small, leaving costs little, and a modest improvement in current earning opportunities should be enough

to tip the decision.

This premium varies enormously across Indian districts, for reasons that have little to do with where banks chose to open branches. Districts differ in the industrial composition of their labor markets, in proximity to towns and cities where educated workers are employed, in the depth of their formal sectors, and in the quality of the schooling that a secondary certificate represents. A secondary certificate is worth a great deal in a district with clerical, supervisory, and service employment within commuting distance, and very little in a district where nearly all work is casual agricultural labor paid at a daily rate that does not ask what grade a worker completed.

We measure the premium directly. Using the NSSO 68th round (2011/12), we restrict attention to regular wage and salaried workers aged 18 to 30, young enough that their schooling decisions were made recently and are informative about what a current student faces. We regress log daily wages on an indicator for having completed secondary or higher secondary education, interacted with district indicators, controlling for age, age-squared, and gender. The district-specific coefficient on secondary completion is our proxy for what finishing school is worth locally. We then sort treated districts into terciles of this measure and re-estimate Equation 1 separately within each.

Table 6 reports the result, and the pattern is sharp. The entire secondary enrollment decline is concentrated in low-return districts, where enrollment falls by 19.5% (Column III), significant at the 1% level and roughly half again as large as the average effect. In medium-return districts the coefficient is 4.9% and in high-return districts 12.2%, and neither is statistically distinguishable from zero (Columns I and II). Columns IV through VI repeat the exercise using age and gender fixed effects in the wage regression rather than linear controls, and produce 20.4% in low-return districts against nothing elsewhere. The result does not depend on how we specify the first-stage wage equation.

The economic content of this finding is that households are not responding mechanically to the appearance of work. A credit expansion of similar size, generating similar local labor demand, produces a large withdrawal from school in one district and no detectable withdrawal in another, and what separates them is what a secondary certificate is worth where they live. Families in high-return districts face the same improved earning opportunities for their adolescents and decline to take them, which is what a household protecting a valuable investment would do. Families in low-return districts accept, which is what a household with little to protect would do.

This is also a demanding test in a way that the heterogeneity splits in the next sections are not. The returns measure is built from individual wage data that never enters the en-

rollment regressions, is fixed at a baseline two years before the first branch opened, and varies for reasons related to district labor market structure rather than to bank behavior. It is hard to construct a story in which branch placement, school administration, or enrollment measurement correlates with the local wage premium to secondary education in a way that would generate this pattern.

Two qualifications belong here. First, the Mincerian coefficient recovers the observed wage premium associated with secondary completion, not the causal return to it, since it reflects selection into schooling as well as the payoff to schooling. For our purpose this is arguably the more relevant object: households cannot observe a causal return, and what they can observe is the gap between what educated and uneducated workers earn around them. Still, the measure should be read as the perceived local payoff rather than as a structural parameter. Second, because the split is across districts, this specification absorbs less local variation than our baseline, and the standard errors in the high-return tercile are correspondingly wide. We are confident that the decline is concentrated in low-return districts; we are less able to rule out a modest effect in high-return ones.

5.2 Credit Intensity

If the mechanism runs through credit, the effect should depend on how much credit actually arrives. This is not a trivial prediction. A bank branch is many things besides a source of loans: a building, a source of employment, a signal that the village has been selected for state attention, a place to hold savings safely. Any of these might plausibly affect schooling. But only the lending channel implies a dose-response relationship, because only lending varies substantially in magnitude across villages that all received exactly one branch.

We split treated villages at the median of total credit disbursed in the first year after entry, Rs. 10,448 thousand, and compare each group against control villages that received no branch and no credit. Table 7 shows the gradient clearly. In villages where credit disbursement was above the median, secondary enrollment falls by 15.9% and primary enrollment rises by 4.2%, both significant. In villages below the median, neither effect is statistically distinguishable from zero: secondary enrollment falls by 9.1% with a standard error too large to interpret, and primary enrollment rises by an insignificant 2.8%.

Two features of this pattern are worth drawing out. The first is that the null in low-intensity villages is informative rather than disappointing. These villages received a branch, with everything else a branch brings, and their enrollment did not move. Whatever the branch does as a building, an employer, or a symbol, it is not enough to change

schooling decisions. What changes them is the volume of lending.

The second is that both effects scale together. If the enrollment increase among younger children and the decline among adolescents were driven by separate forces, there would be no particular reason for them to appear and disappear in the same villages. That they do is consistent with a single shock working through two channels: credit large enough to expand production both raises household income, which draws younger children in, and raises local labor demand, which pulls older children out. Below some threshold of disbursement, the expansion is too small to do either.

5.3 Sectoral Allocation

Volume is not the only thing that varies. Credit flowing to different sectors creates different kinds of work, and only some kinds of work are available to a fourteen-year-old. This gives us a sharper test than intensity alone, because it predicts that credit composition should move the secondary enrollment effect while leaving the primary effect broadly intact.

We split treated villages at the median share of credit allocated to agriculture, which is 0.68 in our sample, reflecting how central farm lending is to rural Indian banking. Table 8 shows that in villages where agricultural credit dominates, secondary enrollment falls by 23.0%, nearly twice the average effect and the largest coefficient in the paper outside the vulnerability split. In villages where agricultural credit is a smaller share, the decline is 3.3% and statistically indistinguishable from zero. Primary enrollment, by contrast, rises by a similar amount in both groups: 3.9% where agricultural credit is high and 3.5% where it is low.

The asymmetry between the two age groups is the heart of this test. Agricultural production in rural India has properties that make adolescent labor unusually valuable. It is labor-intensive, so expanding output requires more hands rather than more machines. It is seasonal, concentrating demand into planting and harvest windows when any available worker is worth employing. It requires physical capacity but little formal skill or training, which an adolescent has and a young child does not. And much of it occurs within the household's own farm, so employing a family member requires no hiring decision, no wage negotiation, and no encounter with labor regulation. Credit that expands agricultural production therefore translates almost directly into demand for adolescent time.

Younger children have no comparable role. A seven-year-old contributes little to farm

output however much the farm expands, so her schooling is unaffected by what sector the credit went to. What affects it is whether household income rose, and household income rose in both groups. The composition of credit moves the substitution channel and leaves the income channel untouched, which is difficult to reconcile with any single-channel explanation of our results.[34]

5.4 Borrower Type

A final split asks who received the credit. Lending to a farmer and lending to an enterprise finance different production technologies, and the two should employ adolescent labor differently. Table 9 splits treated villages at the median disbursement to farmer accounts (Columns I and II) and, separately, at the median disbursement to enterprise accounts (Columns III and IV).

The secondary results are close to identical across borrower types. Where farmers borrow heavily, secondary enrollment falls by 17.0%; where enterprises borrow heavily, it falls by 17.9%. In both low-intensity groups the effect is absent. Whatever the borrower, sufficiently large credit expansion pulls adolescents out of school, which is what a labor demand mechanism predicts and what distinguishes it from a story about farming culture specifically.

The primary results differ, and the difference is informative about how each channel operates. Farmer credit raises primary enrollment only at high intensity, by 3.7%, with no effect at low intensity. Enterprise credit does the reverse: primary enrollment rises by 5.8% where enterprise lending is modest and is insignificant where it is large.

We read this as two production technologies with different thresholds. Farm lending works through the household's own operation. A small loan changes little; a large one expands acreage or inputs enough to raise farm income appreciably, which funds a younger child's schooling, while simultaneously requiring more labor, which draws the adolescent in. Both effects therefore appear together and only at high intensity. Enterprise lending works differently. Modest loans to small businesses raise the proprietor's income without expanding operations enough to change hiring, so the income channel operates cleanly and primary enrollment rises. Larger loans finance genuine expansion, and expansion creates work: adolescents are drawn into the family business directly, or

[34]Non-farm family enterprise supplies a parallel pathway. Using a measure of informal labor absorption capacity from the 2013 Economic Census, Table A.10 shows secondary enrollment falling by 10.9% in villages where family-operated non-farm businesses are most prevalent, against no significant effect where they are least. The common feature with agriculture is production inside the household, where an adolescent can be put to work without a formal hire.

into household production as parents redirect their own time toward the enterprise. At that point the substitution effect appears and the primary gain is absorbed.

Taken together, the four tests in this section point in one direction. The enrollment decline requires credit, scales with credit, concentrates where credit creates work an adolescent can do, and, most tellingly, occurs only where the schooling being given up is worth little. This is the signature of a household trading a low-value investment for immediate income, not of a village disrupted by the arrival of a bank.

6 Who Responds, and Where

The mechanism tests explain why households make the trade-off they do. They leave open a related question: which households make it. We consider three dimensions. The first two are properties of the village, both measured at baseline in the 2011 Census and therefore fixed before any branch opened: how much a community values schooling independently of its market return, and how badly its households need income now (Table 10). The third is a property of the child, namely gender, caste, and the type of school she attends (Table 11). The contrast between them turns out to be instructive: the response sorts sharply on the economic circumstances of the household and hardly at all on the identity of the child.

6.1 Local Educational Culture

Panel A of Table 10 sorts villages into terciles by literacy rate, which we take as a proxy for the local educational environment. This is a broad construct, and literacy is capturing several things at once: the education of the parents making these decisions, the presence of adults in the village who completed school and can testify to what it was worth, the information households have about returns, and social norms about what an adolescent's day should contain. In the least literate tercile, secondary enrollment falls by 22.5%. In the middle tercile it falls by 11.8%. In the most literate tercile the coefficient is 7.6% and statistically indistinguishable from zero. Primary enrollment runs the other way, with gains concentrated in high- and medium-literacy villages, at 7.0% and 3.6%, and no significant effect in low-literacy villages.

The gradient is monotone and its interpretation is that educational norms function as a buffer. Villages at every point in the literacy distribution experienced the same kind of credit-driven expansion, and the same improvement in what an adolescent could earn. In

the most literate villages, households declined to act on it. One reading is cultural: where schooling is normal, withdrawing a child is costly in ways that do not appear in a wage comparison. Another is informational: educated parents may have a more accurate view of what completing school pays. We cannot separate these, and we note that literacy is likely correlated with the local return to schooling we measured in subsection 5.1, so the two results are not fully independent.

6.2 Household Financial Vulnerability

Panel B sorts villages by the share of marginal workers, defined as those employed fewer than six months a year, which we use as a proxy for household financial precarity. The gradient here is at least as stark. In the most vulnerable tercile, secondary enrollment falls by 22.6%. In the middle tercile it falls by 11.7%. In the least vulnerable tercile the coefficient is -0.001, indistinguishable from zero and from any economically meaningful effect. Primary enrollment gains appear only in the middle tercile, at 5.1%.

The economics here connects directly to the child labor literature. Households whose members work intermittently have volatile income and little buffer stock, so a rupee today is worth considerably more to them than to a household with stable employment, and a payoff arriving in six years is discounted heavily or not counted at all. Basu and Van (1998) formalize this as a subsistence threshold below which families send children to work. Our result is the comparative static that follows: when a new earning opportunity appears, the households nearest that threshold take it, and the households furthest from it do not. Financial inclusion, in other words, has its largest adverse effect on schooling precisely among the families it was designed to help.

Read alongside Table 6, the picture is coherent and points one way. The secondary decline is largest where the payoff to finishing school is smallest, where educational norms are weakest, and where household finances are most precarious. It vanishes where any one of these conditions is reversed. The adolescents leaving school are not a random sample of rural India; they are concentrated in the villages where the case for staying is weakest and the pressure to earn is strongest, which is also where the long-run cost of leaving is likely to be highest.

6.3 Student Gender, Caste, and School Type

The two splits above show the response varying systematically with the economic circumstances of the village. We now ask whether it also varies with the characteristics of the

child. In a setting where educational access has historically been stratified by gender and caste, one might expect a negative shock to schooling to fall disproportionately on girls, or on backward-caste students, whose position in the schooling system is most marginal. Table 11 shows that it does not, and the absence of these gradients is itself informative about what kind of mechanism we are observing.

Panel A of Table 11 splits by gender. Secondary enrollment falls by 12.9% for boys and 9.9% for girls, and primary enrollment rises by 3.6% and 7.2% respectively. Both age patterns hold for both sexes, and the secondary declines are close enough that we cannot distinguish them. Given the size of India's gender gap in secondary schooling, a mechanism operating through social preference over which child to educate would be expected to show up here. That it does not suggests households are responding to an economic opportunity that is available to sons and daughters alike, and making the withdrawal decision on economic rather than gendered grounds. The larger primary gain for girls is worth noting in passing: when constraints relax, the children brought into school are disproportionately those previously excluded.

Panel B splits by caste, a dimension along which access to both education and labor markets in India remains sharply unequal. Secondary enrollment falls by 14.2% among backward-caste students and by 13.2% among general-caste students, again without a meaningful difference. Primary enrollment rises by 3.7% among backward-caste students and is essentially zero for general-caste students. The asymmetry in the primary result is consistent with the income channel: backward-caste households are poorer on average, so a given relaxation of financial constraints moves more children across the threshold at which schooling becomes affordable. The symmetry in the secondary result says that the opportunity cost of an adolescent's time rose for everyone, and everyone responded.

Panel C splits by school type, which in this setting proxies for position in the local income distribution. The secondary decline is concentrated in public schools, at 11.0%, against an insignificant 7.8% in private schools. The primary gain is concentrated in private schools, at 5.7%, against an insignificant 2.8% in public schools. We interpret this as the same trade-off operating at different points of the distribution. Families who send children to private schools are wealthier but still credit-constrained at the margin for fees and expenses; when a branch relaxes that constraint they respond on the income margin, enrolling younger children in schools that cost money. Families who use public schools are poorer, face the sharper version of the trade-off, and respond on the substitution margin, withdrawing adolescents whose labor has become valuable. The same shock produces a different response depending on which constraint binds harder.

The contrast with the village splits is the point of this section. Literacy and financial vulnerability move the secondary coefficient from zero to more than twenty percent; gender and caste move it hardly at all. The enrollment response does not sort on identity, it sorts on economic position. It is not that certain kinds of children are pulled out of school, but that certain kinds of households face a trade-off sharp enough to act on. That is consistent with everything in section 5, where the variables that mattered were the local return to schooling, the volume of credit, the sector it financed, and the identity of the borrower, and none of them were about who the child was.

7 Adolescent Labor Supply

One step in the argument remains unverified. We have shown that adolescents leave school, that they leave where local labor demand expands, and that they leave where the return to staying is lowest. We have inferred throughout that they leave in order to work. Enrollment data cannot confirm this, since a student who disappears from a school register might be working, migrating, marrying, or idle, and the welfare interpretation differs considerably across these possibilities. Table 12 addresses the question with individual-level data.

We combine the NSSO 68th round (2011/12), which predates the policy, with the Periodic Labour Force Survey of 2017/18, which follows it, and estimate the probability that a rural child aged 14 to 18 reports working as her principal usual activity. Identification comes from comparing districts of differing treatment intensity, where intensity is the fraction of rural villages within a district that received a new branch after 2013. In districts with above-median intensity, the probability that an adolescent is working rises by 3.1 percentage points relative to low-intensity districts (Column I), and by 2.8 percentage points once age fixed effects are included (Column II), following the specification in Shah and Steinberg (2017).

The design here differs from our main specification and the difference is deliberate. Because household surveys do not identify villages, we cannot replicate the village-level comparison, and a treated-versus-untreated district contrast would be vulnerable to the many ways districts differ. Comparing high- and low-intensity treated districts holds constant the fact of exposure to the policy and varies only its depth, which isolates the dose-response relationship between credit penetration and adolescent labor supply. It is the same logic as the credit intensity test in subsection 5.2, applied to a different outcome in a different dataset.

Three percentage points is a substantial reallocation of adolescent time at the extensive margin, and it lines up with the schooling results in both direction and dose. The children who leave school are not disappearing into idleness; they are entering the labor force that credit expansion created. We note the limitations: these are two cross-sections rather than a panel, the unit is the district rather than the village, and the surveys cannot link a particular child's exit from school to her entry into work. What they establish is that in the districts where our enrollment effects are largest, adolescent employment rose, which is the outcome the mechanism requires and which no alternative explanation of the enrollment decline naturally predicts.

With this, the chain is complete. A branch opens in a village that has never had one. Credit flows to farms and small enterprises, which enter, survive, and expand. Local production rises, and with it both household income and the wage an adolescent can command. Families with young children use the income to put them in school. Families with adolescents weigh the wage against what another year of schooling is worth, and in villages where that answer is small, where educational norms are weak, and where the household lives close to the margin, they send the child to work. Financial inclusion delivered on its own terms in these villages. It was paid for, in part, by the secondary schooling of the children who lived in them.

8 Concluding Remarks

This study provides compelling evidence that financial development can have unintended consequences for human capital formation, challenging the conventional wisdom that financial inclusion uniformly enhances welfare outcomes. Through analysis of comprehensive enrollment data from over 305,000 schools across 160,000 villages in rural India, we document that bank branch expansion creates divergent effects on educational investment that vary systematically by age group and socioeconomic status. Bank branch entry increases primary school enrollment by 3.2% while simultaneously reducing secondary and higher secondary enrollment by 13.3%, with the substitution effect—-where expanding economic opportunities pull older children from education into the workforce—dominating income effects particularly among economically vulnerable households. Villages with low literacy rates and high proportions of marginal workers experience the largest declines in secondary enrollment, with reductions reaching 22.5% and 22.6% respectively, as bank branching stimulates local economic activity and raises the opportunity cost of continued schooling for adolescents whose labor becomes increas-

ingly valuable in expanding local enterprises.

The central takeaway from our research is that financial inclusion policies, while successful in promoting economic growth, may inadvertently undermine human capital accumulation among adolescents in economically vulnerable communities. This finding has profound implications for development policy, suggesting that the same mechanisms that generate short-term economic gains may reduce long-term development prospects by limiting educational attainment precisely among those who would benefit most from continued schooling. Our results highlight the inverse U-shaped relationship between economic opportunity and educational investment predicted by theory, demonstrating that modest economic improvements can actually pull children from education into the workforce among households at the bottom of the income distribution. This effect is particularly concerning given that these families constitute a substantial portion of the population in developing economies and are often the primary targets of financial inclusion initiatives.

These findings suggest that financial inclusion policies should be designed with complementary interventions to protect educational investments, particularly for secondary school-aged children in economically vulnerable households. Policymakers should consider conditional cash transfer programs, enhanced educational subsidies for older children, or other mechanisms to offset the increased opportunity costs of schooling that accompany economic development. Furthermore, our research indicates that the timing and design of financial inclusion initiatives matter critically for their ultimate welfare effects. While expanding access to financial services generates important economic benefits, these gains may come at the cost of reduced human capital formation if not carefully managed, especially in communities with weak educational norms where immediate economic gains often outweigh perceived returns to continued education.

This study contributes to the growing literature questioning whether financial development always serves as an unambiguous catalyst for long-term welfare improvements. By focusing on one of the most economically marginalized populations—households in previously unbanked rural villages—we demonstrate that the benefits of financial inclusion are not automatically shared equally across all segments of society or all dimensions of development. Our findings underscore the importance of taking a nuanced, multidimensional approach to evaluating development interventions and highlight that financial development, rather than being a panacea for development challenges, represents one tool among many that must be carefully calibrated and complemented with other interventions to achieve broad-based improvements in human welfare. As policymakers continue to expand financial inclusion globally, our findings serve as an important re-

minder that the ultimate success of these efforts depends not just on increasing access to financial services, but on ensuring that such access translates into sustainable improvements across all dimensions of human development.

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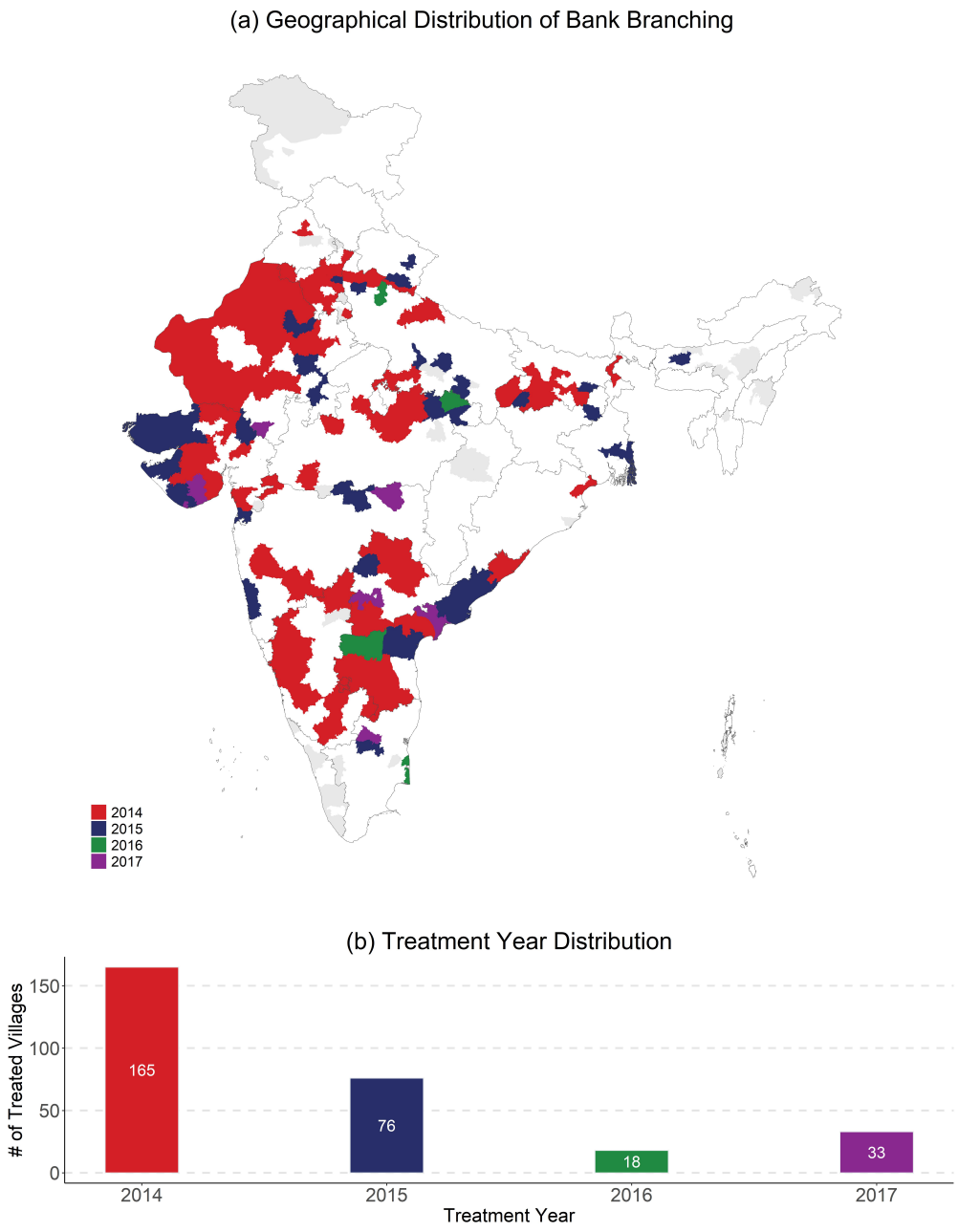
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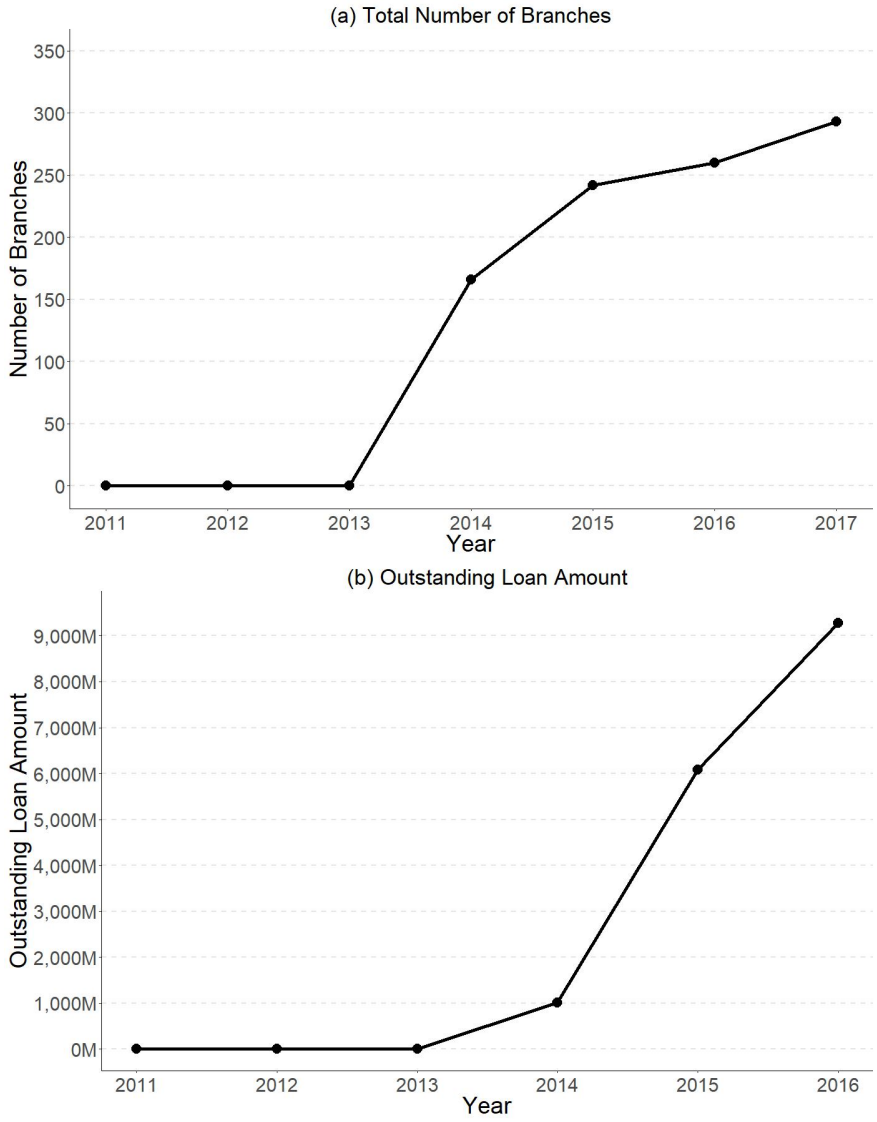
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Figure 1: Distribution of Bank Branching



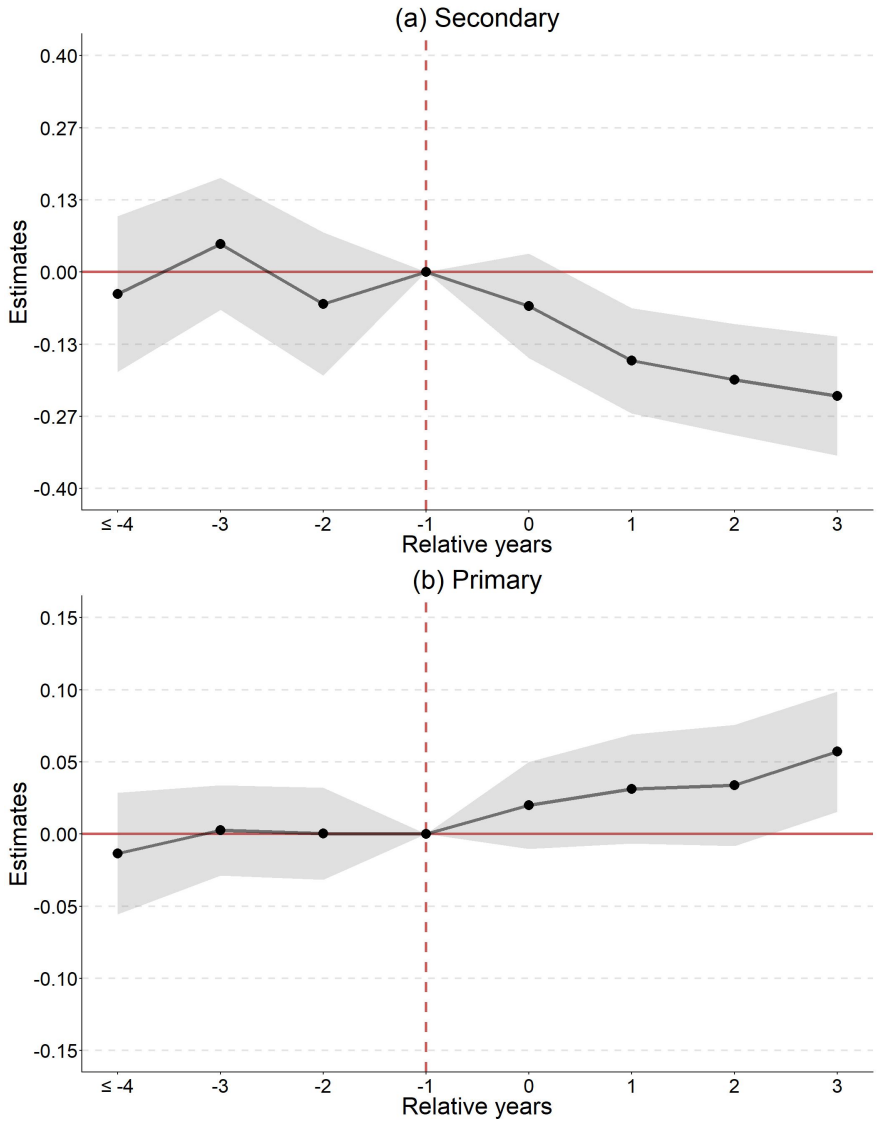
This figure illustrates the geographical and year distribution of places receiving new Regional Rural Bank (RRB) branches between 2014 and 2016. We start with a sample of unbanked tier 5 (population 5,000–9,999 as in Census 2001) and tier 6 villages (population below 5,000 as in Census 2001). Unbanked rural (tier 5 and 6) villages are places without any scheduled commercial bank branches. In July 2013, the Reserve Bank of India (RBI) required RRBs to allocate at least 25% of their new branches to these unbanked rural villages. Panel (a) shows geographical treatment distribution at the district level, and highlights districts with at least one unbanked rural village receiving new RRB branch(es) after 2013. White districts indicate areas with at least one unbanked rural village, while colored districts (red/dark blue/green/purple) represent the earliest treatment year of the unbanked rural villages within that district. Gray districts are areas without any unbanked rural villages in our sample. Panel (b) shows the distribution of event years for 292 treated rural (tier 5 or 6) villages. The number in the middle of each bar indicates the number of rural villages that received new RRB branches in that year.

Figure 2: Bank Branching Over Time



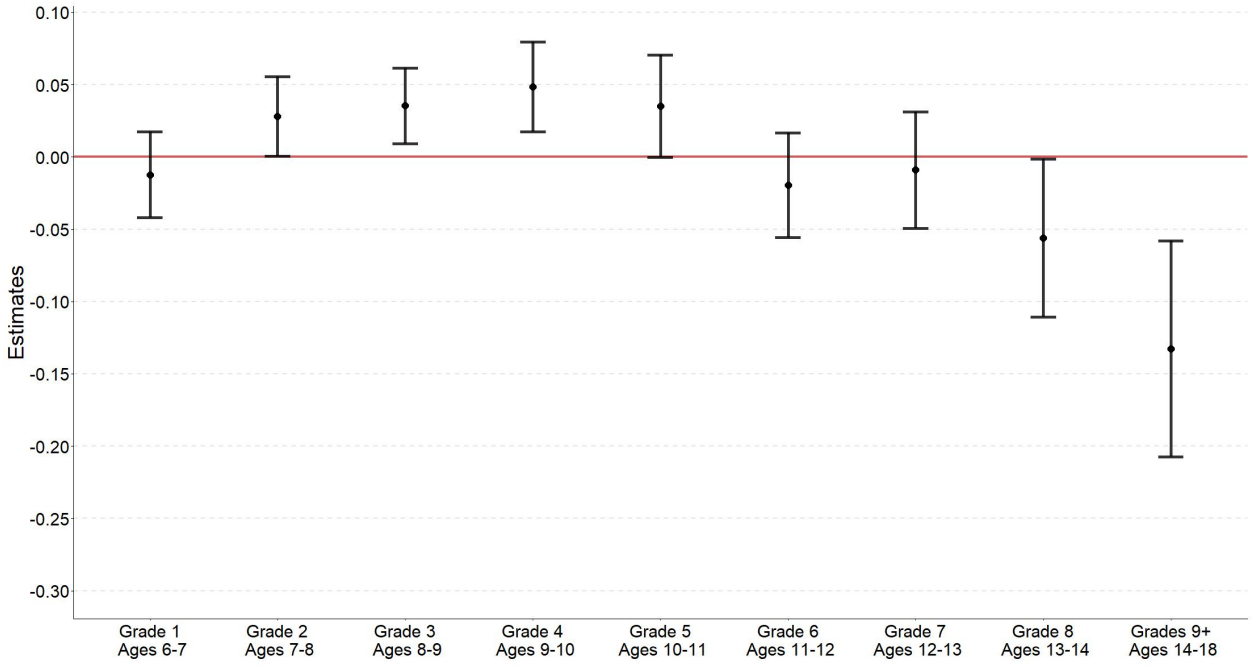
This figure shows the number of newly opened RRBs branches (Panel (a)) and the sum of branch-level outstanding loan amount (Rs.Millions) (Panel (b)) across the treated villages during the sample period. Bank branch entry data is from the Directory of Bank Officials published by the Reserve Bank of India (RBI). Branch-level outstanding loan amount is from the RBI Basic Statistical Return (BSR) 1 until 2016.

Figure 3: Dynamic Response of Bank Branching on Enrollment



This figure illustrates dynamic enrollment changes before and after new bank branch entries, based on Equation 2. Panel (a) shows estimates for secondary (grades 9-12, ages 14–18), while Panel (b) reports results for primary (grades 1-8, ages 6–14). The dependent variable is the log enrollment plus one. Schools are treated if new bank branch(es) open in the village after 2013. The regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends to account for unobserved village characteristics that trend linearly over time. The regressions also control for time-varying school characteristics: age and total expenditures, to capture school size. The shaded gray area represents the 95% confidence interval. The benchmark period is one year before the new bank branch entry.

Figure 4: Heterogeneous Response of Bank Branching on Enrollment by Grade



This figure shows the heterogeneous average changes in enrollment by grade before and after the bank branching from 2011 to 2017. The dependent variable is the log enrollment plus one. Schools are treated if new bank branch(es) open in the village after 2013. The regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends to account for unobserved village characteristics that trend linearly over time. The regressions also control for time-varying school characteristics: age and total expenditures, to capture school size.

Table 1: Summary Statistics

	Mean	Standard Deviation	Observation
Panel A School-Year Panel			
Enrollment - Secondary (Secondary& Higher Secondary)	157.42	177.56	117,483
Enrollment - Primary (Primary & Upper Primary)	103.91	110.61	1,860,872
School Age	37.34	26.09	1,865,613
School Funds Expenditure (Rs.)	10,902.27	25,697.57	1,856,908
Is public school	1	0	1,838,399
Panel B Village-Year Panel			
# of Schools	1.82	1.45	1,026,799
# of Public Schools	1.58	1.17	1,026,799
# of Private Schools	0.21	0.62	1,026,799
Nightlight Intensity - Average	0.49	0.63	855,112
Nightlight Intensity - Minimum	0.12	0.32	855,089
Panel C Pincode-Year Panel			
# of Firms - Entry	0.96	8.00	67,764
# of Firms - Exit	0.50	4.26	67,764
Panel D Village Summary			
Treatment Intensity - Credit Level (Rs. Thousands)	35,837.64	46,119.70	259
Baseline Literacy Rate	0.57	0.14	153,394
Baseline Marginal Worker Ratio	0.29	0.27	153,393

This table summarizes key variables in our sample of tier 5 and 6 villages. We start with a sample of unbanked tier 5 (population 5,000–9,999 as in Census 2001) and tier 6 villages (population below 5,000 as in Census 2001). Unbanked rural (tier 5 and 6) villages are places without any scheduled commercial bank branches. Following 2013, some rural villages receive new bank branches. Schools are treated if new bank branch(es) open in the village after 2013. Panel A reports school-year statistics for 2011–2017 from the District Information System for Education (DISE) data. It includes school enrollments (winsorized at top and bottom 1%), control variables (age & expenditure to proxy size), and school type (public or private). School funds include Teachers Grant, School Maintenance Grant, School Development Grant, and other funds in the DISE data. Panel B presents the statistics of the number of schools and schools by category for 2011-2017 from DISE. Panel B also shows the summary of nightlight intensity (average and minimum) from the Visible Infrared Imaging Radiometer Suite (VIIRS) for 2012-2017, aggregated at the village level. Variables are winsorized at the top and bottom 1%. Panel C summarizes the private firm activities for 2011-2017. The data is at the Pincode-date level from the Ministry of Corporate Affairs (MCA). Panel D reports the total new deposit level and account numbers across all post-treatment years and new branches for each treated village for 2011-2016. The data is from the Reserve Bank of India (RBI) Basic Statistical Return (BSR) 2. Panel D also reports two village baseline characteristics from the 2011 Indian Census. The literacy rate is the proportion of literate individuals to the total village population to proxy village education environment. The marginal labor ratio is the proportion of marginal workers to the total working population in a village, measuring households' economic burden.

Table 2: Average Response of Bank Branching on Local Economic Activity and Household Income

Panel A: Local Economic Activity

	Firm Activity		Nightlight
	log(Number of Firms+1)		log(Intensity+1)
	Entry	Exit	Minimum
	I	II	III
Post branching	0.0766***	-0.0909***	0.0123*
	(0.0277)	(0.0280)	(0.0063)
Pincode FE	Yes	Yes	
District-Year FE	Yes	Yes	Yes
Pincode Time Trend	Yes	Yes	
Village FE	Yes	Yes	Yes
Village Time Trend	Yes	Yes	Yes
# Obs	17,356	17,356	286,731
R-Square	0.7844	0.7026	0.8692

Panel B: Household Income

	Household Income	
	Labor income	*business income*
	I	II
Post branching × High treatment intensity	0.1332***	0.1979**
	(0.0436)	(0.0878)
Post branching	-0.0601	-0.0709
	(0.0365)	(0.0722)
Household FE	Yes	Yes
Year-Month FE	Yes	Yes
# Obs	484,260	61,913
R-Square	0.51619	0.58439

Panel A reports firm entry/exit (MCA data, 2011-2017) and nightlight intensity (VIIRS, 2012-2017) around bank branch entry, with Post Branching equal to 1 after 2013; specifications include Pincode/village and district-year fixed effects with location-specific time trends, and standard errors clustered accordingly. Panel B reports household labor and business income (Consumer Pyramids Household Survey, 2014-2017) for rural households, where High treatment intensity indicates above-median district-level branch penetration; specifications include household and year-month fixed effects, with standard errors clustered at the district level. Both panels restrict to treated districts only. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 3: Average Response of Bank Branching on Secondary Enrollment

	log(Enrollment+1)			
	I	II	III	IV
Post branching	-0.105***	-0.125***	-0.133***	-0.120***
	(0.0395)	(0.0402)	(0.0381)	(0.0400)
School FE	Yes	Yes	Yes	Yes
Year FE	Yes			
State-Year FE		Yes		
District-Year FE			Yes	
Block-Year FE				Yes
Village Time Trend	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes
# Obs	42,617	42,617	42,600	41,527
R-Square	0.8237	0.8317	0.8406	0.8701

This table shows the average enrollment responses (secondary grades 9-12; ages 14–18) before and after the bank branching (Equation 1). The dependent variable is log enrollment plus 1. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives a new bank branch(es) after 2013. All specifications include school fixed effects with additional fixed effects: year (column I), state-year (column II), district-year (column III), and block-year (column IV). The regressions also a) include village-specific linear time trends to account for unobserved village characteristics that trend linearly over time, and b) control for time-varying school size characteristics: age and total expenditures. Standard errors are clustered at the village level and reported in brackets. The average number of students enrolled before receiving the treatment is 157.4371, which is the average enrollment number for treated schools before the shock and for control schools over the full sample period. This table restricts to treated districts only. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 4: Robustness Tests: Alternative Control, Specification, and Treatment

	log(Enrollment+1)			
	Keep Control Villages in Different Blocks	Control Differences in Baseline Characteristics of		Define Treatment at the Block Level
		Village	School	
	I	II	III	IV
Post branching	-0.141***	-0.131***	-0.0799**	-0.0234*
	(0.0397)	(0.0407)	(0.0398)	(0.0140)
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
Village Time Trend	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes
Village Baseline Characteristics × Year Dummies		Yes		
School Baseline Characteristics × Year Dummies			Yes	
# Obs	31,459	40,388	36,021	42,600
R-Square	0.8410	0.8413	0.8555	0.8405

This table presents robustness tests for Table 3 using alternative control villages, specifications, and treatment level. In column I, we removes neighboring control villages within the same block as treated villages. Columns II-III show results using alternative specifications to alleviate the concern that treated and control villages (schools) with different baseline characteristics may evolve differently over time. Specifically, in column III, we have a set of village baseline characteristics interacted with year fixed effects to compare treated and control schools among villages with similar baseline characteristics. Village baseline characteristics include population size, share of main workers, share of scheduled caste/tribe population, access to black-topped roads, power supply, mobile coverage, post offices, and number of primary health centers reported in India Census 2011. The summary statistics of these variables are in Table A.5. Column IV directly compares treated and control schools with similar characteristics by including a set of school baseline characteristics interacted with year fixed effects. School baseline characteristics include total enrollment, student-teacher ratio, access to toilets, libraries, medical check-ups, playgrounds, drinking water, functional electricity, and boundary walls based on school statistics as of 2011. In column V, we aggregate the treatment level at the block, and the treatment year is the earliest year if any village in the block receives new bank branches. The dependent variable is the log enrollment plus one. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives any new bank branch(es) after 2013. Regressions include school fixed effect, village-specific linear time trends, and district-by-year fixed effects. The regressions also control for school size characteristics: age and total expenditures. Standard errors are clustered at the village (Columns I-III) or block (Column IV) level and reported in brackets. This table restricts to treated districts only. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 5: Average Response of Bank Branching on Primary Enrollment

	log(Enrollment+1)			
	I	II	III	IV
Post branching	0.0507***	0.0362***	0.0322**	0.0281*
	(0.0145)	(0.0140)	(0.0139)	(0.0145)
School FE	Yes	Yes	Yes	Yes
Year FE	Yes			
State-Year FE		Yes		
District-Year FE			Yes	
Block-Year FE				Yes
Village Time Trend	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes
# Obs	606,906	606,906	606,906	606,831
R-Square	0.9163	0.9187	0.9209	0.9240

This table shows the average primary enrollment (primary grades 1-8, ages 6–14) responses before and after the bank branching (Equation 1). The dependent variable is log enrollment plus 1. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives a new bank branch(es) after 2013. All specifications include school fixed effects with additional fixed effects: year (column I), state-year (column II), district-year (column III), and block-year (column IV). The regressions also a) include village-specific linear time trends to account for unobserved village characteristics that trend linearly over time, and b) control for time-varying school size characteristics: age and total expenditures. Standard errors are clustered at the village level and reported in brackets. The average number of students enrolled before receiving the treatment is 103.8342, which is the average enrollment number for treated schools before the shock and for control schools over the full sample period. This table restricts to treated districts only. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 6: Understanding Enrollment Response: Local Returns to Secondary Education

	log(Enrollment+1)					
	I	II	III	IV	V	VI
	District Return/Wage to Secondary Completion					
	Age, Age^2, and Gender Controls			Age and Gender FE		
	High	Medium	Low	High	Medium	Low
Post branching	-0.122	-0.0493	-0.195***	-0.109	-0.0560	-0.204***
	(0.1171)	(0.0447)	(0.0418)	(0.1114)	(0.0469)	(0.0419)
School controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Village Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
# Obs	9,464	14,850	11,747	9,782	15,246	11,033
R-Square	0.8630	0.9001	0.8292	0.8678	0.8942	0.8300

This table presents the heterogeneous average enrollment responses of secondary schools based on district-level baseline returns to secondary education. Returns to education are estimated using the NSSO 68th round (2011/12) Survey. The sample is restricted to regular wage/salaried workers aged 18-30. The log daily wage, calculated as weekly regular wage earnings divided by days worked in the reference week, is regressed on an indicator for having completed secondary or higher secondary education, interacted with district indicators. The district-specific coefficient on the secondary indicator serves as a proxy for the local labor market return to completing secondary schooling at baseline. In Columns I-III, the wage regression controls for age, age^2, and gender. In Columns IV-VI, the wage regression includes age and gender fixed effects. In both cases, villages are grouped into High, Medium, and Low categories based on a tercile split among treated districts, with cutoff values of 0.406 and 0.050 in Columns I-III and 0.400 and 0.051 in Columns IV-VI. The dependent variable is log enrollment plus 1. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after 2013. All regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. Standard errors are clustered at the village level and reported in brackets. This table restricts to treated districts only. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 7: Heterogeneous Response of Bank Branching on Enrollment by Credit Intensity

	log(Enrollment+1)	
	Secondary	Primary
	I	II
Post branching * Low credit supply	-0.0909	0.0278
	(0.0655)	(0.0253)
Post branching * High credit supply	-0.1585***	0.0424**
	(0.0527)	(0.0179)
School controls	Yes	Yes
School FE	Yes	Yes
District-Year FE	Yes	Yes
Village Time Trend	Yes	Yes
# Obs	42,981	607,201
R-Square	0.84914	0.92124

This table shows the heterogeneous enrollment responses by intensity of credit. The dependent variable is log enrollment plus 1. Credit intensity is measured as the first-year total credit disbursed in treated villages. Villages are classified as high or low intensity based on whether the number is above or below the median of 10,448 (Rs. thousands) among treated villages. Within each intensity group, schools in treated villages are compared to schools in control villages that received zero credit (without any bank branch entry). Standard errors are clustered at the village level and reported in brackets. This table restricts to treated districts only. ***, **, and * indicate significance at the 1%, 5%, and 10% levels (two-sided), respectively.

Table 8: Agricultural Credit, Farm Labor Demand, and School Enrollment

	log(Enrollment+1)	
	Secondary	Primary
	I	II
Post branching * Low agricultural sector share	-0.0331	0.0351*
	(0.0670)	(0.0205)
Post branching * High agricultural sector share	-0.2304***	0.0391*
	(0.0457)	(0.0208)
School controls	Yes	Yes
School FE	Yes	Yes
District-Year FE	Yes	Yes
Village Time Trend	Yes	Yes
# Obs	115,877	1,850,364
R-Square	0.86228	0.91503

This table shows enrollment responses by agricultural credit concentration. The dependent variable is log enrollment plus 1. Agricultural credit share is the ratio of credit allocated to the agricultural sector over total credits disbursed in the first year after bank entry for treated villages. Villages are classified as high or low sector share based on whether the ratio is above or below the median of 0.68 among treated villages. Within each intensity group, schools in treated villages are compared to schools in control villages that received zero credit (without any bank branch entry). Standard errors are clustered at the village level and reported in brackets. This table restricts to treated districts only. ***, **, and * indicate significance at the 1%, 5%, and 10% levels (two-sided), respectively.

Table 9: Average Response of Bank Branching on Enrollment by Borrowers' Account

	log(Enrollment+1)			
	Farmer Account		Enterprise (Firm) Account	
	Secondary	Primary	Secondary	Primary
	I	II	III	IV
Post branching * Low credit supply	-0.0528	0.0373	-0.0619	0.0576**
	(0.0703)	(0.0270)	(0.0890)	(0.0244)
Post branching * High credit supply	-0.1701***	0.0368**	-0.1792***	0.0237
	(0.0499)	(0.0170)	(0.0398)	(0.0181)
School controls	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
Village Time Trend	Yes	Yes	Yes	Yes
# Obs	42,981	607,201	42,981	607,201
R-Square	0.84914	0.92124	0.84915	0.92124

This table shows the heterogeneous enrollment responses by borrowers' account type: farmer or enterprise. The dependent variable is log enrollment plus 1. Credit intensity is measured as the first-year total credit disbursed in treated villages. Villages are classified as high or low intensity based on whether the number is above or below the median of 4,172 (farmer) or 30 (firm) (Rs. thousands) among treated villages. Within each intensity group, schools in treated villages are compared to schools in control villages that received zero credit (without any bank branch entry). Standard errors are clustered at the village level and reported in brackets. This table restricts to treated districts only. ***, **, and * indicate significance at the 1%, 5%, and 10% levels (two-sided), respectively.

Table 10: Understanding Enrollment Response: Mitigators and Exacerbators

	log(Enrollment+1)					
	I	II	III	IV	V	VI
	Panel A Local Educational Culture - Literacy Rate					
	Secondary			Primary		
	High	Medium	Low	High	Medium	Low
Post branching	-0.0764	-0.118**	-0.225***	0.0696**	0.0359*	0.0361
	(0.0821)	(0.0595)	(0.0516)	(0.0311)	(0.0206)	(0.0263)
# Obs	15,765	14,922	9,560	188,952	201,011	193,370
R-Square	0.8804	0.8422	0.7969	0.9253	0.9181	0.9212
	Panel B Households' Financial Vulnerability - Marginal Labor Ratio					
	Secondary			Primary		
	High	Medium	Low	High	Medium	Low
Post branching	-0.226***	-0.117***	-0.00113	0.0309	0.0508***	0.0137
	(0.0698)	(0.0347)	(0.1194)	(0.0243)	(0.0195)	(0.0391)
# Obs	10,955	17,346	11,978	182,840	220,129	180,380
R-Square	0.8208	0.8427	0.8806	0.9234	0.9172	0.9259
School controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Village Time Trend	Yes	Yes	Yes	Yes	Yes	Yes

This table presents the heterogeneous average enrollment responses based on village baseline characteristics from the 2011 Indian Census. In Panel A, the literacy rate, calculated as the proportion of literate individuals to the total village population, serves as a proxy for educational culture before the shock. Villages are grouped into High, Medium, and Low categories using a tercile split, with the cutoff values of the literacy ratio being 0.52 and 0.64. Panel B shows the marginal labor ratio, defined as the proportion of marginal workers to the total working population in a village, as a measure of household economic burden. Similarly, villages are divided into High, Medium, and Low groups based on a tercile split, with the cutoff values being 0.1 and 0.38. The dependent variable is log enrollment plus 1. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after 2013. All regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. Standard errors are clustered at the village level and reported in brackets. This table restricts to treated districts only. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 11: Heterogeneous Response of Bank Branching on Enrollment by Students' Gender, Caste and School Type

	log(Enrollment+1)			
	I	II	III	IV
	Panel A Student Gender			
	Secondary		Primary	
	Boys	Girls	Boys	Girls
Post branching	-0.129***	-0.0991***	0.0361**	0.0723***
	(0.0386)	(0.0353)	(0.0146)	(0.0192)
# Obs	42,600	42,600	606,906	606,906
R-Square	0.8947	0.8770	0.9249	0.9140
	Panel B Student Caste			
	Secondary		Primary	
	Backward Caste	General Caste	Backward Caste	General Caste
Post branching	-0.142***	-0.132***	0.0368**	-0.00307
	(0.0493)	(0.0443)	(0.0163)	(0.0234)
# Obs	42,598	42,598	606,766	606,766
R-Square	0.8298	0.8943	0.9043	0.8995
	Panel C School Type			
	Secondary		Primary	
	Public	Private	Public	Private
Post branching	-0.110***	-0.0782	0.0280	0.0574*
	(0.0304)	(0.0803)	(0.0171)	(0.0343)
# Obs	25,128	17,030	537,476	64,402
R-Square	0.8514	0.8540	0.9409	0.8211
School controls	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes
Village Time Trend	Yes	Yes	Yes	Yes

This table presents the heterogeneous enrollment responses by students' gender (Panel A), caste (Panel B), and school type (Panel C). The dependent variable is the log enrollment plus one. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after 2013. All regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. Standard errors are clustered at the village level and reported in brackets. This table restricts to treated districts only. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 12: Average Response of Bank Branching on Child Labor

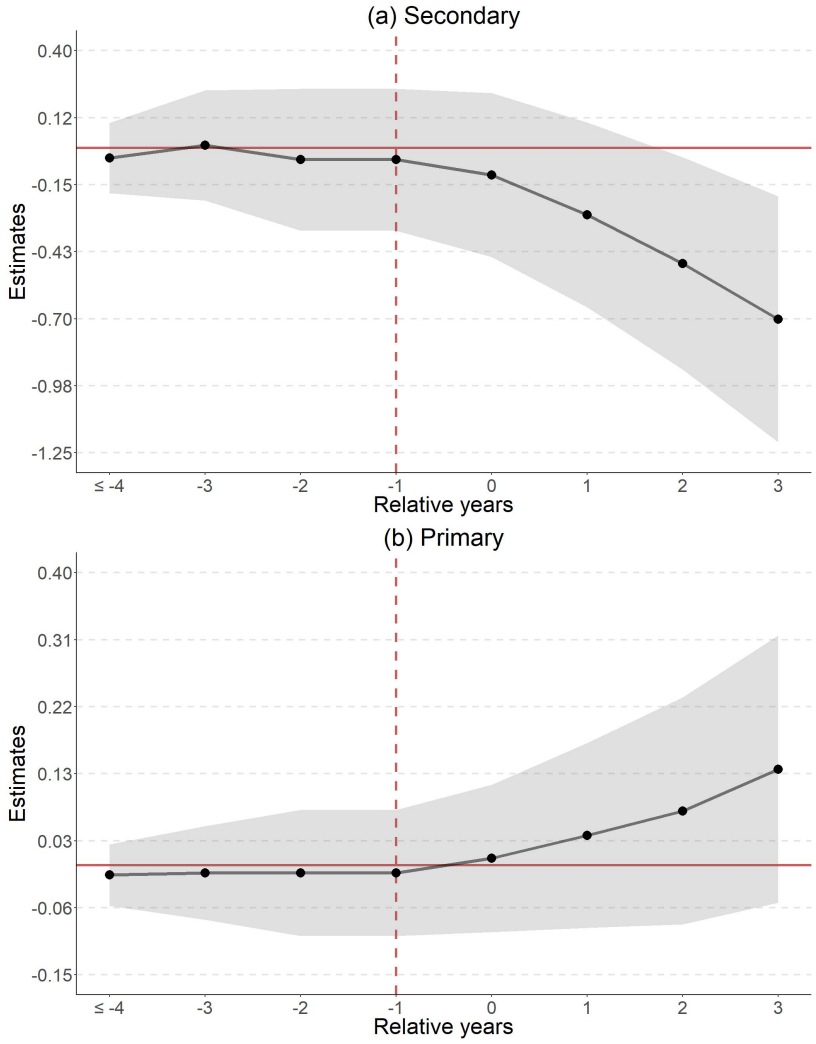
	p(working)	
	I	II
Post branching × High treatment intensity	0.0305**	0.0284**
	(0.0123)	(0.0120)
District FE	Yes	Yes
Year FE	Yes	Yes
Age		Yes
# Obs	11,317	11,317
R-Square	0.04036	0.07675
Sample	Ages 14-18	Ages 14-18

This table shows the effect of bank branching on the probability of working among children aged 14-18. Data are from the NSSO 68th round (2011/12) and the Periodic Labour Force Survey (PLFS) 2017/18. The sample is restricted to rural households, given that the main analysis is among unbanked rural villages. The data is at the child-district-year level. The dependent variable is an indicator equal to one if the child's principal usual activity status is working. Post branching is an indicator variable equal to 1 for the post-treatment period (PLFS 2017/18). High treatment intensity (above median) is an indicator equal to one if the district's treatment intensity, defined as the fraction of treated rural villages within the district that received a new bank branch after 2013, is above the median intensity (0.6%) among all treated districts. All specifications include district and year fixed effects. Columns II additionally includes age fixed effects, following Shah and Steinberg (2017). Standard errors are clustered at the district level and reported in brackets. This table restricts to treated districts only. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Internet Appendix for "Economic Opportunity Costs: Banking Access and Educational (Dis)Investment"

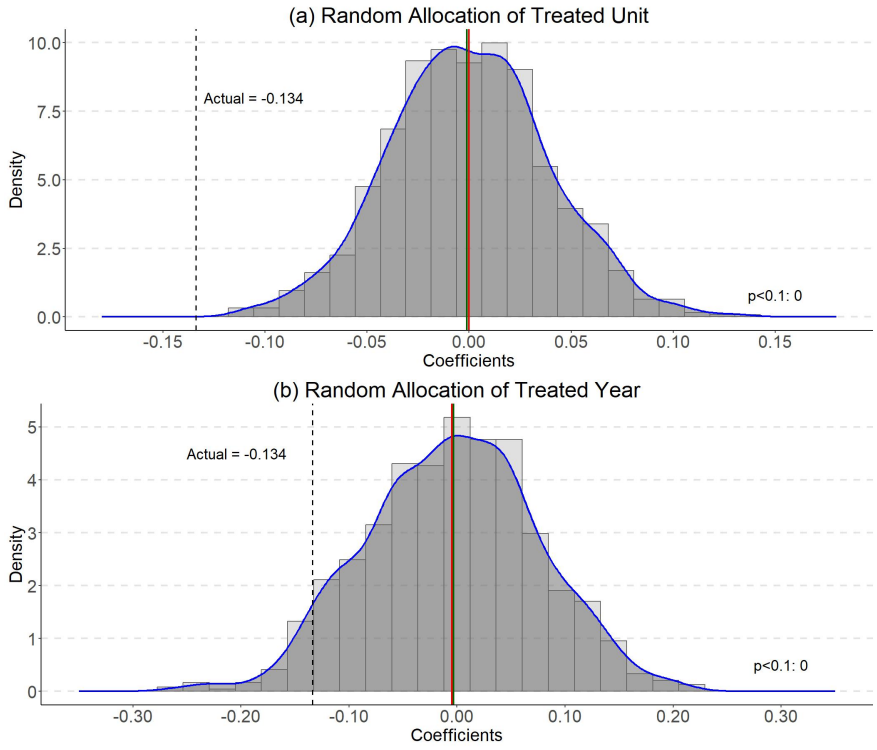
Intended for online publication only

Figure A.1: Cumulative Effect of Bank Branching on Enrollment



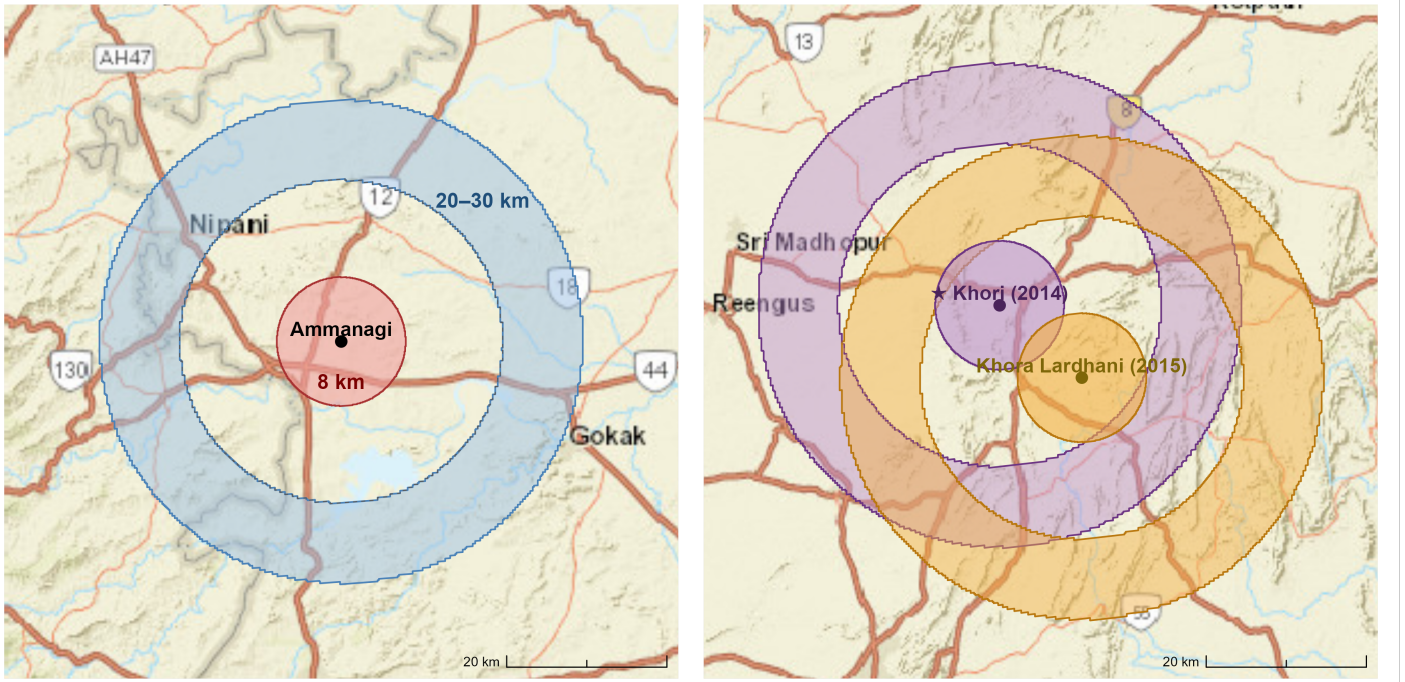
This figure shows the cumulative changes in enrollment around new bank branch entries. The dependent variable is the log enrollment plus one. Panel (a) focuses on secondary grades (ages 14–18), while Panel (b) presents results for primary grades (ages 6–14). Schools are treated if new bank branch(es) open in the village after 2013. The regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. The shaded gray area represents the 95% confidence interval. The benchmark period is one year before the new bank branch entry.

Figure A.2: Placebo Test: Randomization Treated Unit and Year (X)



This figure shows the density distribution of estimated coefficients for "Post Branching" from our placebo test. In (a), we randomly select 292 treated rural villages from the control sample, assign pseudo-treatment years (2011-2017), and estimate Equation 1. In (b), we randomly assign pseudo-treatment years (2011-2017) within the 292 treated villages and estimate Equation 1. The randomization is repeated 1000 times, and the kernel density plots are shown for students in secondary grades. The red vertical line represents the mean, the green line denotes the median, and the black lines show the estimates of our preferred specification in Table 3 Column III.

Figure A.3: Alternative Identification Strategy of Spatial DID: Illustrative Cases of Treatment and Control Assignment



(a) Single-village example

(b) Two-village example

This figure illustrates our treatment and control assignments for our sample of unbanked rural villages, following the empirical design of Fisman et al. (2023), in the simple case of Ammanagi (which has a single bank branch opening) and the more complicated case of two nearby branch openings: Khori (2014) and Khora Lardhani (2015). Villages within the inner 8-kilometer circle of a branch are "treated," while those between 20 and 30 kilometers are "control" villages. Those in the 8-20 kilometer buffer ring are excluded from the sample. When a village falls within the treatment or control zone of more than one branch, it is assigned to its closest branch. If a village falls within the treatment zone of one branch and the control zone of another, it is assigned to the treatment area. In addition, if a branch opens within 8 km of an earlier branch, it is absorbed into the earlier branch's cluster.

Table A.1: Robustness Test: Possion & Level Regression

	Enrollment	
	Possion	Number (level)
	I	II
Post branching	-0.0927***	-10.39***
	(0.0254)	(3.9830)
School FE	Yes	Yes
District-Year FE	Yes	Yes
Village Time Trend	Yes	Yes
School controls	Yes	Yes
# Obs	115,430	112,216
R-Square	0.95701	0.9552

This table shows robustness tests for secondary enrollment responses before and after the bank branching using Poisson regression (to account for the count nature of enrollment in column I), and level specifications (to examine effects in absolute student numbers in column II). Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after 2013. Regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. Standard errors are clustered at the village level and reported in brackets. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table A.2: Robustness Test: Estimates in Staggered Treatment Effect

	log(Enrollment+1)		
	I	II	III
	Negative Weight Test Chaisemartin and D'Haultfœuille 2024	**Sun and Abraham 2020**	**Callaway and Sant'Anna 2021**
% ATTs with negative weights	4%		
Sum of negative weights	-0.0001		
Post Branching		-0.1467***	-0.1402***
		(0.0432)	(0.0432)
# Obs	116,003	116,003	116,003
School controls	Yes	Yes	Yes
School FE	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes
Village-specific linear time trend	Yes	Yes	Yes

This table shows the results of diagnostic tests and alternative approaches to address concerns of biased estimates in staggered difference-in-differences. Column I reports the results of the Diagnostic Test from Chaisemartin and D'Haultfœuille (2020), Columns II-III show the average enrollment estimates using two alternative methods from Sun and Abraham (2021), and Callaway and Sant'Anna (2021). The dependent variable is log secondary enrollment plus one. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after 2013. The regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. Standard errors are clustered at the village level and reported in brackets. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table A.3: Placebo Test: Average Impact of Pseudo Bank Branching on School Enrollment (Non-Tier 5/6 Villages)

	log(Enrollment+1)	
	Secondary	Primary
	I	II
Post Branching	-0.0000583	0.0134
	(0.0326)	(0.0142)
School Controls	Yes	Yes
School FE	Yes	Yes
District-Year FE	Yes	Yes
Village Time Trend	Yes	Yes
# Obs	6,831	54,798
R-Square	0.9261	0.8972

This table shows the average enrollment responses before and after the pseudo-bank branching treatment for schools in non-tier 5 or 6 villages. Pseudo-treatment years (2014/2015/2016/2017) are randomly assigned to 165/76/18/33 villages in other tiers, following the treatment year distribution in Figure 1 Panel (b). The dependent variable is the log enrollment plus one. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after 2013 (pseudo treatment). The regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends. The regressions also control for time-varying school size characteristics. Standard errors are clustered at the village level and reported in brackets. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table A.4: Robustness Test: Average Pre-Treatment Effect

	log(Enrollment+1)	
	Secondary	Primary
	I	II
Pre Branching	-0.0537	0.00220
	(0.0524)	(0.0142)
Post Branching	-0.148***	0.0321**
	(0.0408)	(0.0156)
School Controls	Yes	Yes
School FE	Yes	Yes
District-Year FE	Yes	Yes
Village Time Trend	Yes	Yes
# Obs	112,216	1,842,772
R-Square	0.8523	0.9146

This table shows average enrollment responses before and after the bank branching, including a Pre Branching estimator. Pre-branching is an indicator variable equal to 1 for two years before a rural village receives new bank branch(es). Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after 2013. Regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. Standard errors are clustered at the village level and reported in brackets. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table A.5: Summary Statistics of Village Baseline Characteristics

	I	II	III	IV
	Panel A: Full Sample			
	Treatment	Control	Difference	Difference
# Villages	292	153,122	(pooled)	(within district)
Population size	3876.4596	1472.4055	2404.0541***	2259.7484***
Main workers (% of population)	0.3262	0.3319	-0.0057	-0.0119
Scheduled Caste and Scheduled Tribe (% of population)	0.2498	0.3748	-0.1250***	-0.0471***
Access to road	0.9191	0.7090	0.2101***	0.2363***
Access to power supply	0.8382	0.6275	0.2108***	0.1003***
Access to mobile hone coverage	0.9007	0.8039	0.0968***	0.0335
Access to post office	0.2537	0.1021	0.1515***	0.1694***
Access to health center	0.6765	0.2323	0.4442***	0.3805***
	Panel B: Estimation Sample (secondary schools)			
	Treatment	Control	Difference	Difference
# Villages	143	19,550	(pooled)	(within district)
Population size	4096.7063	2393.5940	1703.1123***	1589.1258***
Main workers (% of population)	0.3462	0.3443	0.0020	-0.0094
Scheduled Caste and Scheduled Tribe (% of population)	0.2415	0.3120	-0.0705***	-0.0406**
Access to road	0.9441	0.8173	0.1268***	0.1159***
Access to power supply	0.8741	0.7191	0.1550***	0.0815***
Access to mobile hone coverage	0.9650	0.9105	0.0545***	-0.0005
Access to post office	0.2448	0.2418	0.0029	0.1064***
Access to health center	0.7902	0.5247	0.2655***	0.1565***

This table presents the balance test for village baseline characteristics from the Indian 2011 Census. Panel A reports the means for treated and control villages in the full sample (columns I and II), the pooled mean difference between treated and control villages (column III), and the mean difference between treated and control villages within the same district for our preferred specification (column IV). Panel B reports the same statistics for the estimation sample, restricted to villages having secondary schools/in the estimation sample. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively (two-sided).

Table A.6: Summary Statistics for Alternative Identification Strategy: Spatial DID

	I	II	III	IV
	Estimation Sample (secondary schools)			
	Treatment	Control	Difference	
Zone Definition	($d \leq 8$km)	($20 < d \leq 30$km)	(pooled)	(within BranchZone)
# Villages	804	1,842		
Population size	2743.7338	2497.1151	246.6187***	323.8061***
Main workers (% of population)	0.3229	0.3497	-0.0269***	-0.0096**
Scheduled Caste and Scheduled Tribe (% of population)	0.2679	0.2802	-0.0123	-0.0058
Access to road	0.8371	0.8290	0.0081	0.0152
Access to power supply	0.7649	0.7720	-0.0071	0.0189
Access to mobile hone coverage	0.9192	0.9197	-0.0005	-0.0032
Access to post office	0.1915	0.1764	0.0151	0.0244
Access to health center	0.6095	0.5890	0.0204	0.0314

This table presents the balance test for village baseline characteristics from the Indian 2011 Census under the alternative spatial difference-in-differences identification strategy illustrated in Figure A.3. The sample is restricted to villages with secondary schools/in the estimation sample. Treated villages are those within 8 kilometers of a bank branch opening (column I), control villages are those between 20 and 30 kilometers from the nearest branch (column II), villages in the 8–20 kilometer buffer ring are excluded from the sample. Columns I and II report the means for treated and control villages, respectively. Column III reports the pooled mean difference. Column IV reports the mean difference within BranchZone, where a BranchZone denotes the set of treated and control villages assigned to a given branch under the rules described in Figure A.3. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively (two-sided).

Table A.7: Alternative Identification Strategy: Spatial DID

Design: Treatment ($d \leq 8$ km), Buffer ($8 < d \leq 20$ km), Control ($20 < d \leq 30$ km)						
	log(Enrollment)					
	I	II	III	IV	V	VI
Treated # Post	-0.0578***	-0.0529***	-0.0510***	-0.0626***	-0.0529***	-0.0511**
	(0.0193)	(0.0178)	(0.0180)	(0.0198)	(0.0180)	(0.0244)
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes					
BranchZone-Year FE		Yes	Yes		Yes	
Pop. Quintile-Year FE			Yes		Yes	
BranchZone-Pop. Quintile-Year FE				Yes		
WorkerRatio. Quintile-Year FE					Yes	
BranchZone-Pop. Quintile-WorkerRatio. Quintile-Year FE						Yes
Village Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
School controls	Yes	Yes	Yes	Yes	Yes	Yes
# Obs	14,658	14,710	14,710	13,703	14,710	11,355
R-Square	0.9149	0.9168	0.9170	0.9269	0.9171	0.9373

This table presents the spatial difference-in-differences estimates of the effect of bank branching on secondary-school enrollment (grades 9–12; ages 14–18), using the alternative identification strategy illustrated in Figure A.3, and following Fisman et al. (2023). The dependent variable is log enrollment. Treated is an indicator equal to one for villages within 8 kilometers of a new bank branch opening after 2013, control villages are those located between 20 and 30 kilometers from the nearest branch, and villages in the 8-20 kilometer buffer ring are excluded from the sample. Post is an indicator equal to one for years following the branch opening. The coefficient of interest is the interaction Treated $\times$ Post. All specifications include school fixed effects, village-specific linear time trends, and time-varying school controls (age and total expenditures). We include additional fixed effects across columns: district-year (column I), BranchZone-year (column II), BranchZone-year and population-quintile-year (column III), BranchZone $\times$ population-quintile $\times$ year (column IV), BranchZone-year, population-quintile-year, and worker-ratio-quintile-year (column V), and BranchZone $\times$ population-quintile $\times$ worker-ratio-quintile $\times$ year (column VI). Standard errors are clustered at BranchZone level and reported in brackets. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table A.8: Average Response of Bank Branching on Household Income

	Household Income	
	Labor income	*business income*
	I	II
Post branching × High treatment intensity	0.1332***	0.1979**
	(0.0436)	(0.0878)
Post branching	-0.0601	-0.0709
	(0.0365)	(0.0722)
Household FE	Yes	Yes
Year-Month FE	Yes	Yes
# Obs	484,260	61,913
R-Square	0.51619	0.58439

This table shows the effect of bank branching on household income. Data is from Consumer Pyramids Household Survey from 2014 to 2017. The sample is restricted to rural households, given that the main analysis is among unbanked rural villages. The data is at the household-district-yearmonth level. The dependent variable is household-level income from two sources: labor and business. Labor income captures earnings from salaried employment including wages, overtime, and bonuses, and business income captures profits from self-employment and informal enterprises including farming, retail, and other own-account activities. Post branching is an indicator variable equal to 1 for the district's post-treatment period after first treatment. High treatment intensity (above median) is an indicator equal to one if the district's total treatment intensity, defined as the fraction of treated rural villages within the district that received a new bank branch during the post-treatment period, is above the median intensity (0.5%) among all treated districts. All specifications include household, and year-month fixed effects. This table restricts to treated districts only. Standard errors are clustered at the district level and reported in brackets. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table A.9: Average Response of Bank Branching on Exam Performance of Students Remaining in Schools

	log(Number of Students+1)					
	Primary Level Examination			Upper Primary Level Examination		
	Appear	Pass	Pass with Merit	Appear	Pass	Pass with Merit
	I	II	III	IV	V	VI
Post Branching	0.0554	0.0566	0.0569	0.0873*	0.0868*	0.113**
	(0.0461)	(0.0456)	(0.0438)	(0.0498)	(0.0486)	(0.0493)
School Controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Village Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
# Obs	1,244,666	1,244,583	1,238,393	473,536	473,509	471,911
R-Square	0.6619	0.6617	0.6354	0.6978	0.6969	0.6599

This table reports students' exam performance before and after bank branching for students who are still in schools. Columns (1) to (3) present changes in primary-level exams (Grades 4 or 5), while columns (4) to (6) show the changes of exam performance in upper primary-level exams (Grades 7 or 8). The dependent variable is log number of students appearing/passing/passing with merit plus one. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after 2013. R-egressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. Standard errors are clustered at the village level and reported in brackets. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table A.10: Family Enterprise Intensity, Informal Labor Absorption, and School Enrollment

	log(Enrollment+1)					
	I	II	III	IV	V	VI
	Secondary			Primary		
	High	Medium	Low	High	Medium	Low
Post branching	-0.109**	-0.150***	-0.0594	0.0363	0.0141	0.0386
	(0.0511)	(0.0454)	(0.1168)	(0.0233)	(0.0189)	(0.0355)
# Obs	31,087	44,312	31,673	553,681	615,623	554,536
R-Square	0.8783	0.8630	0.8393	0.9124	0.9171	0.9168
School controls	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Village Time Trend	Yes	Yes	Yes	Yes	Yes	Yes

This table presents heterogeneous enrollment responses by village-level Family Enterprise Intensity, measured from the 2013 Indian Economic Census (a census of every non-farm establishment in India). Family Enterprise Intensity is defined as the share of non-farm workers in Own Account Establishments, which are businesses run entirely by the owner and family members without any hired outside workers. This measure captures the village's informal labor absorption capacity: in family business-dominated villages, children represent the most accessible and lowest-cost source of additional labor when local businesses expand following bank branching. Villages are classified into High, Medium, and Low groups using a tercile split, with cutoff values of 0.46 and 0.69. The dependent variable is log enrollment plus 1. Post Branching is an indicator variable equal to 1 for years in which the school is in a village that receives new bank branch(es) after academic year 2013. All regressions include school fixed effect, district-by-year fixed effect, and village-specific linear time trends, along with school size controls. Standard errors are clustered at the village level and reported in brackets. ***, **, and * indicate significance at the 1%, 5%, and 10% levels (two-sided), respectively.