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

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**Abstract**

While financial development is widely celebrated for promoting economic growth, we document its adverse effects on human capital formation among the poor. Analyzing 300,000 schools across 160,000 unbanked rural Indian villages, we find that bank entry substantially reduces secondary enrollment while modestly increasing primary enrollment. Credit expansion stimulates local economic activity, raising adolescents' opportunity cost of schooling as they become valuable labor resources, while younger children benefit from relaxed financial constraints. Effects intensify where credit concentrates in labor-intensive agriculture. Economic vulnerability amplifies the decline in secondary schooling, but strong educational culture partially attenuates it. Our findings reveal a critical trade-off: households prioritize immediate income gains over education, suggesting financial development may reduce human capital investment despite expanding economic opportunities.

**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 (Greenwood and Jovanovic, 1990; King and Levine, 1993; Beck, Levine and Loayza, 2000; Burgess and Pande, 2005; Bruhn and Love, 2014). However, its impact on human capital formation—particularly for economically vulnerable households remains complex and potentially contradictory. The expansion of financial services introduces competing economic mechanisms that may either facilitate or discourage educational investments, creating a tension that is especially consequential for families operating near subsistence thresholds. Understanding how financial market expansion affects educational investment decisions at the lower end of the income distribution offers important insights into the broader welfare implications of financial inclusion policies and the channels through which financial development influences long-term economic outcomes.

These competing mechanisms operate through distinct economic channels: as banking services expand into rural communities, they typically stimulate local economic activity, increase household income, and reduce financial constraints—factors that should theoretically 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

[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

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.

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.

(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.

[2]See recent studies such as Chakraborty, Javadekar and Ramcharan (2021), Cramer (2021), 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

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. 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).

Using this empirical design, we have five main results. Our first key finding shows that bank branch entry reduces secondary enrollment by 13.4%. 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 13.4%, representing approximately 21 fewer students per

[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.

treated school—underscores the significant opportunity cost that expanding economic activity imposes on human capital investment decisions for adolescents.

Our second finding reveals that bank branch entry increases primary school enrollment by 3.1%. For younger children, the income effect dominates: when financial constraints are relaxed through improved banking access, households can afford to send younger children to school, particularly when primary education is subsidized under India's Right to Education Act. Unlike older children who can contribute meaningfully to household income, younger children remain too young to participate productively in the expanding local economy. As adults intensify their participation in expanded economic opportunities, schools serve a valuable supervision function for these younger children. The improved household finances from banking access thus enable greater educational access for primary-age children, who face minimal opportunity costs from schooling.

Our third finding examines heterogeneity in credit allocation to understand which types of economic activity most strongly affect educational decisions. We find that the intensity and composition of credit matter substantially. In villages with high credit intensity—those receiving above-median total credit disbursement—secondary enrollment declines by 16.1%, while primary enrollment increases by 4.1%. In contrast, low credit intensity villages show statistically insignificant effects on both primary and secondary enrollment. This pattern suggests 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 insights into the opportunity cost mechanism. In villages where agricultural credit comprises a high share of total lending, secondary enrollment drops dramatically by 23.5%, while primary enrollment increases modestly by 3.5%. Villages with low agricultural credit share experience virtually no change in secondary enrollment and similar primary enrollment gains (3.6%). 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 opportunities for older children to generate household income.[5]

We further examine heterogeneity by borrower type to distinguish between different economic activities. Credit flowing to farmer accounts leads to secondary enrollment declines of 17.2% in high-intensity villages, with primary enrollment rising 3.5%. Similarly, credit to enterprise (firm) accounts in high-intensity villages reduces secondary enrollment by 18.1%. Both channels—agricultural and non-agricultural enterprise activity—generate strong substitution effects for older children, though the agricultural channel appears slightly stronger given the sectoral results. These patterns collectively demonstrate that credit-fueled economic expansion, regardless of whether it flows to farms or firms, creates labor market opportunities that pull adolescents from secondary education.

Our fourth set of results examines the changes in local economic activity that drive these educational responses. Banking access stimulates enterprise growth: firm entry increases by 8.3% while exits decrease by 8.4%—creating a dual effect on educational choices. We also observe an increase in the minimum nightlight intensity, suggesting overall economic gains in the unbanked area.

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 (6.9%) while secondary enrollment shows a smaller, non-significant decline (8.5%). In contrast, villages with low literacy rates experience dramatic secondary enrollment decreases (22.8%). This

[5] The informal sector represents a complementary pathway: using an informal labor absorption capacity measure from the 2013 Indian Economic Census, Table A.7 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.

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 (28.1%), 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.

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, 2015; Chiapa, Prina and Parker, 2016)[9]. Utilizing staggered bank branch expansion in

[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.

[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.

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, and 3 describe the policy background and the data. Section 4 outlines the empirical specification. Section 5 presents results, Section 6 discusses underlying mechanisms, and Section 7 explores factors that buffer or amplify the schooling response. Section 8 concludes.

## 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] Unlike government-owned public sector banks, which operate under broader commercial orientation and could expand in more profitable markets, RRBs are mandated to serve financially excluded populations, including small farmers, agricultural laborers, artisans, and small entrepreneurs,[11] 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[12], 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.

RRB branches operate under Section 23 of the Banking Regulation Act, 1949. It requires RRBs to seek RBI approval for any new branch opening, conditional on fulfilling financial stability criteria[13]. In 2011, RBI first relaxed branch licensing requirements for RRBs. Under this policy, RRBs are permitted to open new branches without prior approval from RBI if they met certain performance-based requirements in their most recent

[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 (2021)), however, rural branch expansion (e.g., India's Regional Rural Banks) receives little attention.

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

[12] 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

[13] Details are in Master Circular on Branch Licensing - Regional Rural Banks (RRBs).

inspection report. These conditions include maintaining a capital to risk weighted assets ratio (CRAR) of at least 9%, net non-performing asset (NPA) below 5%, no default in CRAR or statutory liquidity ratio (SLR) for the last year, net profit in the last financial year, and core banking solutions (CBS) compliant.[14]

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.[15] Specifically, this policy[16] (still in force now) requires each RRB allocate at least 25 percent of their newly opened branches each year to unbanked rural villages — tier 5 or tier 6[17] villages that do not have a physical branch of any scheduled commercial bank.[18]

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 is 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. 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, 2021), economic growth (Young, 2018), firm investment (Kulkarni, Mahajan

[14] See RBI Circular: Branch Licensing Relaxation for further details.

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

[16] More policy details are here.

[17] 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.

[18] 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.

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 (2021), Kulkarni, Mahajan and Ritadhi (2023), and Jiao and Mo (2023), employs the policy reform of 2005 to boost bank branches in "underbanked district" with a population-to-branch ratio higher than the national average. But these policies focus on aggregate district-level variation, emphasize scheduled commercial banks excluding RRBs, and target areas with limited — but not absent — financial access.

In our setting, we examine RRBs' branch expansion at a more granular village level. RRBs are mandated to serve rural populations and frequently represent the only source of formal banking in unbanked villages. Since their branch expansion is largely shaped by policy priorities rather than market-driven incentives, this context helps alleviate concerns about endogenous branch placement and supports a clearer identification of causal effects. We further address potential selection concerns in our identification strategy in section 4, and provide supporting robustness tests in section 5.1.1.

# 3 Data

## 3.1 Data

**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.[19] The data provides a wide range of school-level variables, including school characteristics (e.g., location, or school type (public or private schools)), enrollment[20] (across gender and caste), facility conditions, teacher information, and stu-

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

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

dents' 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 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.[21] This data helps us to evaluate changes in the local business environment before

[21] Firm entry includes entities classified as "Active." Firm exit includes those with statuses such as "Dis-

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.

## 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,[22] 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 dy-

solved," "Strike Off," "Under Process of Striking Off," "Under Liquidation," "Liquidated," and "Converted to LLP and Dissolved".

[22] 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.

namics 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.

# 4 Empirical Design

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:

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

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[23] plus one. $\mu_s$, 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-

[23]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.

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 $\beta_1$ 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 $\text{Year}_{v,d,w}$ 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 $\beta_w$ capture the impact of bank branch opening on the school outcome at each relative year $w$. Following Equation 1, we include school fixed effect ($\mu_s$), district-by-year fixed effect ($\gamma_{d,t}$), and village specific linear trend ($\lambda_v t$).[24]

[24]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.

# 5 Results

## 5.1 Bank Branching and Secondary Schooling Response

Following Equation 1, Table 2 presents our main results: the average impact of bank branching on student enrollment for secondary students.[25] The estimates provide strong evidence that bank branch entry brings a significant and large drop in enrollment. Our baseline specification in column I shows a 10% decrease in school attendance. This effect remains robust and economically significant across different combinations of fixed effects. Column II includes state-by-year fixed effects, comparing schools within the same state and year to control for state-specific trends in education spending or economic growth that might drive both bank expansion and enrollment changes. In column III (our preferred specification), we further tighten the comparison by including district-by-year fixed effects, comparing schools within the same district and year to account for district-level infrastructure development and labor market conditions. Specifically, we observe a 13.4% reduction in enrollment. This translates into around 21 fewer students relative to the pre-treatment average enrollments in secondary grades. In Column IV, we impose the most stringent comparison with block-by-year fixed effects to compare treated and control schools within the same block-year bracket. This is to alleviate the concern that block-level differences might correlate with village-level treatment assignment and enrollment changes. The persistence of large, statistically significant coefficients across all specifications strengthens our causal interpretation.

In addition to evaluating the average impact of bank branching on enrollment, we use Equation 2 to understand how enrollment responses evolve from 4 years before to 3 years after the new bank branch entry. Figure 3 Panel (a) shows the path of enrollment changes for secondary students. Before the branch expansion, the enrollment change differences between treated schools and control schools are insignificant both statistically and eco-

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

nomically, supporting the parallel trends assumption in our difference-in-differences design[26]. We observe that enrollment begins to respond immediately after the establishment of the new bank branch, and there is a persistent drop of enrollment — from about 6% to 23%.

Following Agarwal and Qian (2014), we also estimate the cumulative impact of bank branching on enrollment in Figure A.1. In line with the marginal effect in Figure 3, the cumulative enrollment change differences between treated and control schools remain insignificant both statistically and economically before the event, but start to diverge afterward. By the end of 3 years after the shock, the cumulative desrease for secondary grades reaches 69%. In total, this cumulative impact translates into a reduction of nearly 31,720 students (292 × 157.4371 × 0.69) in higher grades across 292 treated villages with bank branch expansion.

### 5.1.1 Robustness

First, in Table 3, we evaluate our baseline specification using alternative definitions of control and treatment. To mitigate potential contamination when control villages are also exposed to treatment — especially those neighboring villages with new bank branches — Columns I and IV redefine control and treated villages as follows. Column I restricts the control villages to those only in blocks different from the treated villages, and Column IV aggregates the treatment at the block level, and a block is being treated if at least one village within that block receives new bank branch(es).

Second, a major challenge in our design is the potential selection issue in our sample of unbanked tier 5 and tier 6 villages. Although our main specification in Equation 1 includes village-specific linear time trends to account for unobserved differential trends across villages, Table 3 columns II-III present two alternative specifications designed to

[26]Following Agarwal and Qian (2014), we also define a binary variable, “pre-branching“, equal to one from 2 to 7 years before the shock, to test the validity of difference-in-differences design. As shown in Table A.4, the coefficient of “pre-branching“ variable is statistically indistinguishable from zero.

compare treated and control units with similar baseline characteristics[27]. In Column II, we introduce year interactions of baseline village characteristics to control for village differences in demographic structure and infrastructure dimensions[28]. This accounts for the possibility that villages with different baseline characteristics may exhibit different trends in outcomes over time. In column III, we implement a similar set of interactions using baseline school characteristics to account for differences in initial enrollment, academic inputs, and school facilities.[29] In both panels, the estimates remain closely aligned with our baseline results in Table 2. Taken together, the robustness tests in Table 3 columns II-III help mitigate bias in the estimated treatment effects arising from non-random branch placement driven by observable village- or school-level characteristics.

Third, to address estimation bias in the staggered difference-in-differences setting, we employ several alternative methods in Table A.2. Panel A reports the diagnostic test proposed by De Chaisemartin and d'Haultfoeuille (2020). Panels B and C present the average enrollment estimates using the approaches of Sun and Abraham (2021) and Callaway and Sant'Anna (2021), respectively. Again, these estimates are closely aligned with our baseline results in Table 2.

Fourth, we perform three additional tests to validate the robustness of our main findings. First, Table A.3 shows the results of a placebo test: we randomly assign pseudo-treatment years to 292 villages in non-tier5 or tier 6 villages. Second, in Figure A.2 Panel A, we randomly select 292 control villages as being "treated", assign them pseudo-treatment years, and re-estimate Equation 1. Third, Figure A.2 Panel B repeats this procedure by assigning pseudo-treatment years within the 292 actual treated villages.

[27] We follow the method used in prior literature; see, for example, Agarwal, Chia and Ghosh (2024).

[28] 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.

[29] 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

## 5.2 Bank Branching and Primary Schooling Response

Based on Equation 1, Table 4 further presents our findings for primary enrollment. After bank branching, we observe modest enrollment gains among younger cohorts. For example, column III shows a 3.06% increase in school attendance. This corresponds to approximately 3 additional students relative to the average number before the bank branch entry. However, such magnitude is substantially smaller than the significant negative impact for secondary students in Table 2 column III.

Figure 3 Panel (b) also shows a persistent increase in enrollment — from approximately 2.5% to 6% — since the year of entry to 3 years thereafter. Besides, by the end of 3 years after the shock, the cumulative increase for primary grades (Figure A.1) reaches 13% relative to the benchmark, translating into an additional 3,942 students (292 × 103.8342 × 0.13) in lower grades. However, the total reduction in secondary enrollment, 31,720, is more than eight times larger than the modest increase in primary enrollment.

### 5.2.1 Is Primary Enrollment Gain Persistent?

To understand the nuances behind primary enrollment responses, Figure 4 presents grade-specific average effects. We observe the largest increase in enrollment occurs at grade 4, followed by a nearly monotonic decline in higher grades. It implies that a) the positive impact on enrollment (Table 4 column III) is driven by grades 2–5, and b) initial gains of enrollment in lower grades do not persist as some marginal students benefiting from the bank branching eventually drop out (negative coefficients for grades 6 and above). The trend is consistent with economic theories of education and labor supply. As children grow older, their opportunity cost of schooling rises, especially in our setting, where bank branching stimulates local economic activity, thereby expanding opportunities for children to contribute to household income through labor. Therefore, higher attrition rates observed in later grades likely reflect the increasing trade-off between continued education and participation in the labor market. Overall, this figure highlights the intensification of attrition as students progress to higher grade levels after the bank branch entry.

### 5.3 Who Drops Out?

To deepen our understanding of dropout patterns, we further examine exam performance in primary and upper primary grades before and after bank branch entry. For upper primary students, where enrollment declines (negative coefficients for grades 7-8, Figure 4), Table A.6 columns (4)–(6) show a significant 9% increase in the number of students taking and passing the upper primary exams, and an 11% increase among those passing with merit. This suggests that lower-performing students are more likely to drop out. For primary grades where enrollment rises (positive coefficients for grades 4-5, Figure 4), we do not find significant changes in the number of students appearing for, passing, or passing with merit in primary-level exams. This implies that the newly enrolled students, or the marginal students returning to school, do not perform better in the exams and are more likely to drop out in subsequent grades.

### 5.4 Heterogeneity: Student gender, caste, and school type

Table 10 Panel A shows the heterogeneity results by student gender. We observe that for both secondary (primary) grades, enrollment drops (increases) simultaneously for both genders. Our results suggest gender-neutral decision-making. And parents weigh labor market opportunities better and withdraw older children—regardless of gender—to enter the labor force to support household income.

In India, caste-based discrimination remains a significant barrier to education and further labor market participation. Thus, next, we investigate whether bank branch expansions affect school enrollment across caste groups differently. Table 10 Panel B classifies students into general and backward castes. For secondary grades, enrollment declines by 13.3% to 14.3% (columns I-II) for both castes. This pattern is in line with the gender-neutral enrollment findings in Panel A that parents from different backgrounds are willing to withdraw older children from school once credit-induced expansion brings in labor market opportunities. For primary grades, expansions significantly increase enrollment by 3.53%, column III, among backward-caste students, suggesting that improved financial services access helps alleviate resource constraints for these households.

We also show heterogeneous effects by school type: public and private schools. Table 10 Panel C shows that dropout among older students in secondary grades is concentrated in public schools (11.6% decrease in column I), while enrollment gains for primary grades occur in private schools (5.78% increase in column IV). We interpret this pattern as reflecting heterogeneous responses to bank expansion across the income distribution. Although families who send their children to private schools tend to be wealthier, they may still face binding credit constraints at the margin for financing tuition and other school expenses. When bank entry alleviates these constraints, these families increase enrollment for younger children in private schools. In contrast, families whose children attend public schools face tighter resource constraints and respond more strongly to new labor market opportunities created by bank expansion, withdrawing older children from school to supplement household income. Hence, the positive enrollment effect for lower grades is concentrated in private schools, while the decline for older grades is driven by public school students.

# 6 Mechanism

In this section, we discuss potential mechanisms through which bank branching influences school enrollment. The central argument is that access to banking services stimulates local economic opportunities, not only alleviating households' financial stress but also repricing adolescent time. First, we examine heterogeneity in credit allocation to understand which types of economic activity most strongly affect educational decisions. Second, we evaluate whether local bank branching boosts economic growth by analyzing firm entry and exit, as well as nightlight intensity.

## 6.1 Bank Branching and Credit Allocation

### 6.1.1 Credit Intensity

To validate credits brought by the new bank branches are driving our results, Table 5 reports the heterogeneity effects of bank branching on enrollment by credit intensity.

Here, we split treated villages based on credit intensity, which is measured as first-year total credit disbursement following bank branch entry, into high or low intensity groups using the median cutoff value (Rs. 10,448 thousand). We observe that enrollment effects are concentrated entirely in high credit intensity villages. In villages receiving above-median credit, secondary enrollment declines by 16.1% (column I), while primary enrollment increases by 4.1% (column II). In contrast, low credit intensity villages show statistically insignificant effects on both secondary enrollment (9.15%) and primary enrollment (2.64%). This pattern indicates that the substitution effect on older children and the income effect on younger children only emerge when credit expansion is sufficiently large to stimulate local economic activity and relax household financial constraints. The null effects in low credit intensity villages suggest that modest credit expansion generates insufficient labor demand or liquidity to alter educational decisions.

### 6.1.2 Sectoral Allocation

To understand which types of economic activity drive our enrollment responses, we examine heterogeneity by credit composition.

Table 6 splits villages based on the share of agricultural credit in total credit allocated, and classify villages as high or low agricultural intensity groups based on whether their ratio is above or below the median value (0.68) among treated villages.

The results show striking differences. In villages where agricultural credit dominates, secondary enrollment falls by 23.5% (column I), while primary enrollment rises by 3.5% (column II). Villages with low agricultural credit share show no significant change in secondary enrollment but similar primary enrollment gains.

Why does agricultural credit matter so much? Farming in rural India is labor-intensive and follows seasonal cycles—planting and harvest periods create immediate demand for workers. Adolescents are valuable in these activities: they have the physical capacity to contribute, and their forgone wages are lower than adult workers. When credit expansion stimulates agricultural production, it creates real job opportunities that pull older children out of school. In contrast, younger children have little role in farm labor regardless of how much agriculture expands, which explains why primary enrollment gains are similar across both credit composition groups. The key difference is that agricultural lending generates the type of labor demand that specifically reprices adolescent time.

#### 6.1.3 Heterogeneity by Borrower Type

To further understand the mechanisms, we also examine which types of borrowers drive our enrollment effects.

Table 7 presents credit intensity by borrower type: farmer accounts (columns I-II) and enterprise or firm accounts (columns III-IV). Within each borrower type, we split treated villages using the median of first-year total credit disbursed. The secondary enrollment effects are similar across both channels. When farmers receive large amounts of credit, secondary enrollment drops by 17.2% (column I) while primary enrollment rises 3.5% (column II). Enterprise lending shows a comparable secondary enrollment decline of 18.1% in high-intensity villages (column III). However, the primary enrollment effects differ: low-intensity enterprise credit increases primary enrollment by 5.7% (column IV), while low-intensity farmer credit shows no effect.

These patterns could imply different mechanisms operate across borrower types. Farmer accounts likely involve direct child labor: older children work on expanded family farms while agricultural income allows households to keep younger children in school. This explains why farmer credit only affects enrollment at high intensity, when farm expansion is substantial enough to require adolescent labor. Enterprise credit could work through parental income channels: small business loans generate household income that relaxes budget constraints for primary schooling without creating labor demand. At high intensity, however, enterprise expansion creates opportunities that pull older children from school, either through direct employment in family businesses or by requiring adolescents to substitute for parents' time in household production.

## 6.2 Bank Branching and Economic Activities

Additionally, we evaluate whether bank branching stimulates local economic activity in Table 8. We focus on two proxies: a) private sector firm entry and exit, and b) nightlight intensity for overall economic activities.

Columns I-II show that new bank branch entry increases firm entry by 8.3% (column (1)) and decreases firm exit by 8.4% (column (2)). The simultaneous increase in firm entry and decrease in firm exit indicates that credit expansion enables new business formation while helping existing businesses survive. Column III shows that bank branch expansion raises the minimum nightlight intensity by 1.2% in column III, suggesting overall economic gains in the least-developed areas.

Taken together, bank entry genuinely expands local economic activity rather than simply reallocating existing resources. It also supports our argument that enrollment effects are from actual labor demand creation, when the local economy grows, it generates job opportunities that reprice adolescent time and pull older children from school.

# 7 Understanding Enrollment Response: Buffer and Amplifier

Table 9 reports our heterogeneous enrollment effects using two village-level baseline characteristics: the local educational environment and the extent of household financial constraints. In this section, we aim to understand how village characteristics moderate the substitution and income effects on children's schooling decisions.

In Panel A, we classify villages by their baseline literacy rate, using a tercile cut, to proxy educational environment. The negative effect on secondary enrollment is concentrated in low-literacy areas with a dramatic reduction of 22.8% (column III), where educational norms are weakest, and attenuates in medium-literacy areas, -0.116 (column II). Meanwhile, the positive effect on primary enrollment emerges only in high- and medium-literacy villages (columns IV-V). Taken together, these results highlight that stronger educational norms and values can mitigate, to some extent, the draw of economic opportunities among older students.

In Panel B, we focus on households' financial vulnerability, measured by the share of marginal workers in a village. Similarly, we divide villages into three groups using a tercile cut. In villages with high proportions of marginal workers, secondary enrollment declines substantially, by 28.1% in column I, which is almost twice the drop observed in the medium financial vulnerability villages, 14.5% in column II. Most importantly, in the low-vulnerability villages, where stable employment alleviates financial pressures for households, estimated coefficients are positive across both age groups (columns III and VI), particularly for older cohorts in secondary grades. This implies that alleviating household economic stress could help students remain in school longer.

Overall, Table 9 highlights the moderation role of both educational culture and household financial constraints on children's schooling decisions. In particular, reducing economic pressures appears especially important for mitigating dropouts among older students. When families face lower financial burdens, parents might be more willing to send children to school long enough to realize the long-term benefits of continued education.

# 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 3.1% while simultaneously reducing secondary and higher secondary enrollment by 13.4%, 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 22.8% and 28.1% respectively, as bank branching stimulates local economic activity and raises the opportunity cost of continued schooling for adolescents whose labor becomes increasingly 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 reminder 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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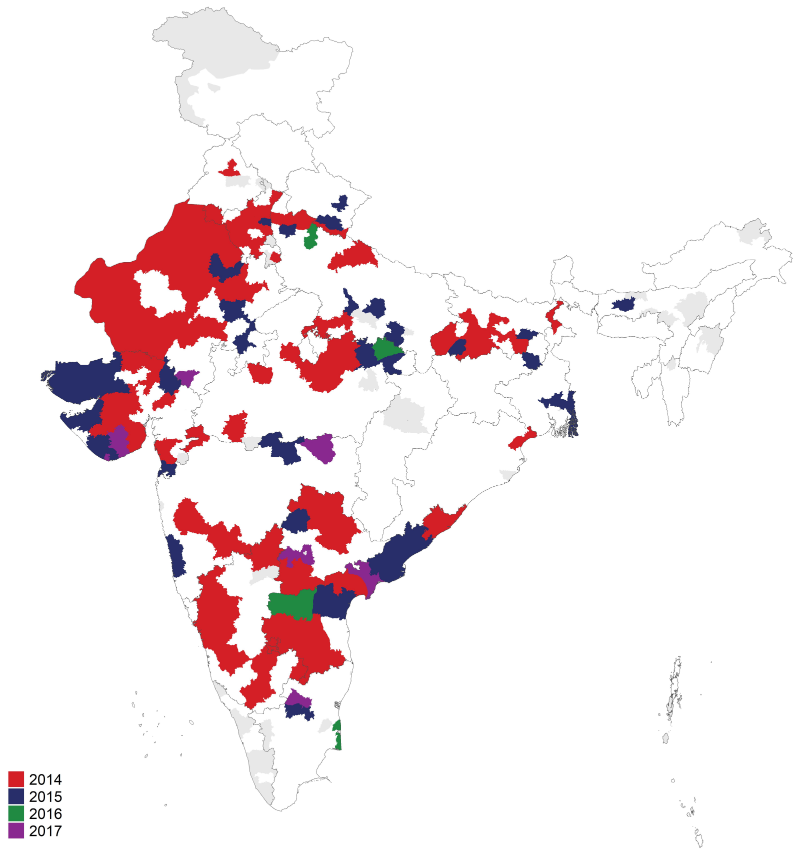
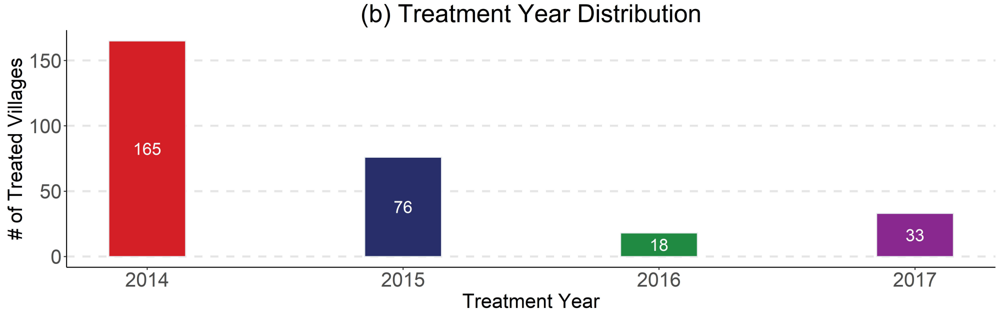
Figure 1: Distribution of Bank Branching

(a) Geographical Distribution of Bank Branching

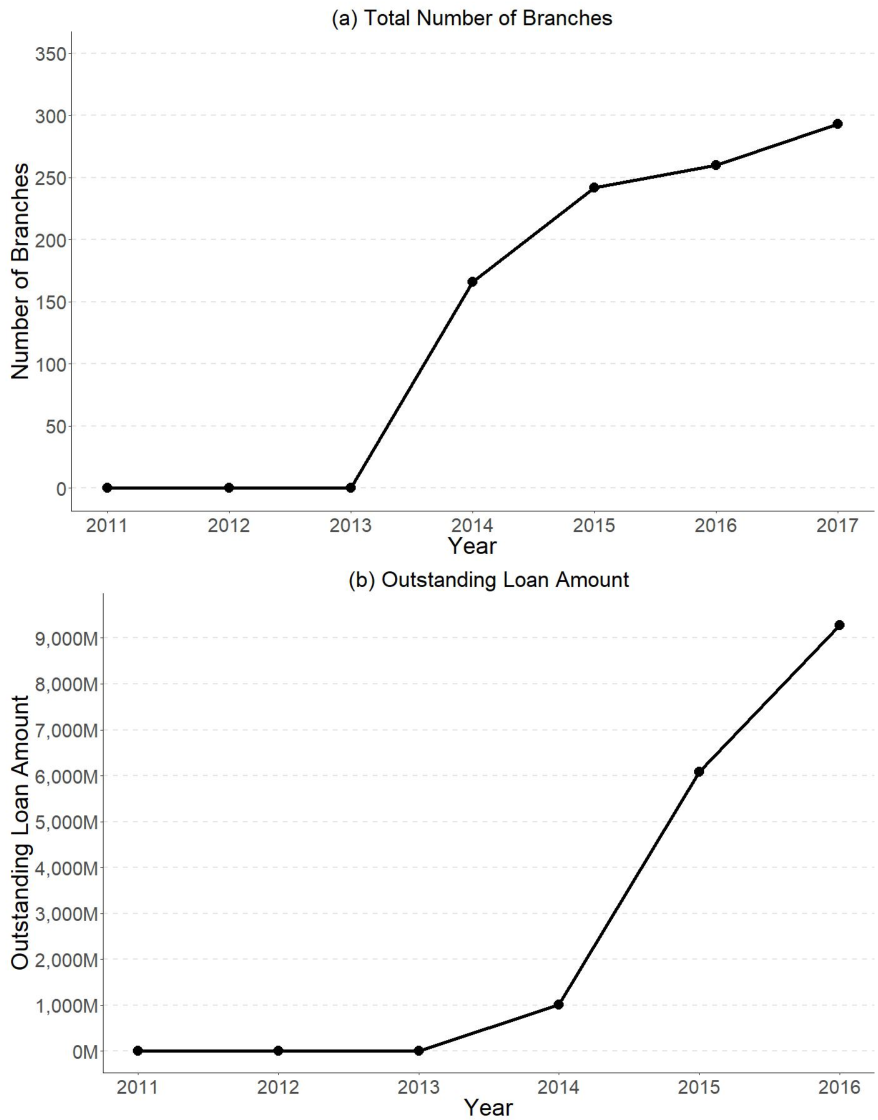



(b) Treatment Year Distribution



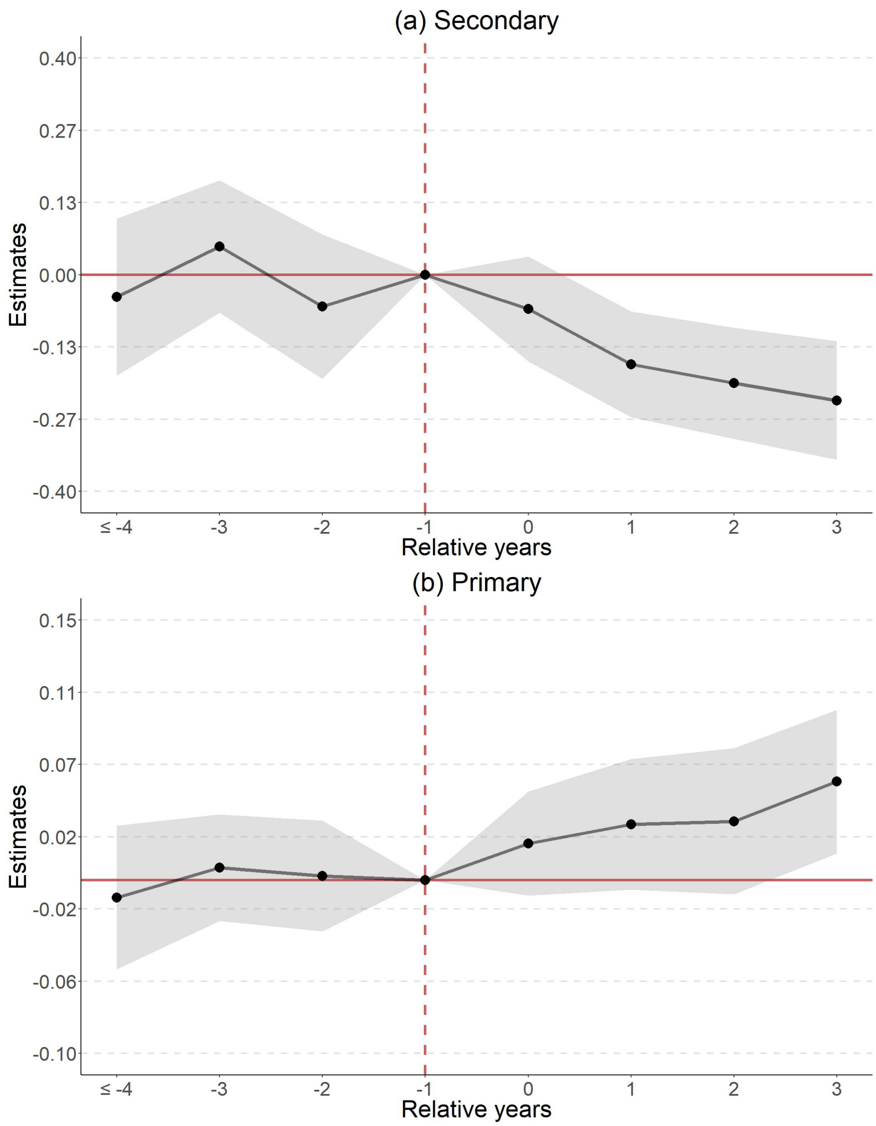

This figure illustrates the geographical and year distribution of places receiving new Regional Rural Bank (RRB) branches between 2014 and 2016. We start with a sample of unbanked tier 5 (population 5,000–9,999 as in Census 2001) and tier 6 villages (population below 5,000 as in Census 2001). Unbanked 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



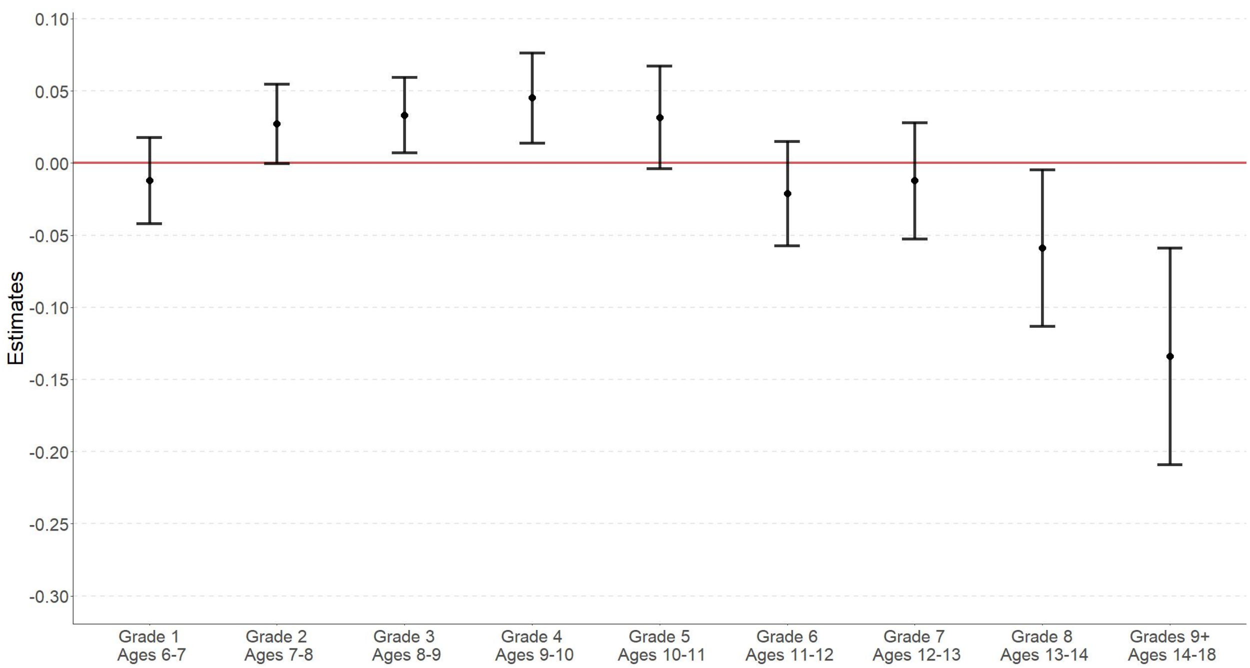

This figure shows the number of newly opened RRBs branches (Panel (a)) and the sum of branch-level outstanding loan amount (Rs.Millions) (Panel (b)) across the treated villages during the sample period. Bank branch entry data is from the Directory of Bank Officials published by the Reserve Bank of India (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 Secondary Enrollment

| | log(Enrollment+1) | | | |
|---|---|---|---|---|
| | I | II | III | IV |
| Post branching | -0.101** | -0.128*** | -0.134*** | -0.121*** |
| | (0.0393) | (0.0402) | (0.0383) | (0.0399) |
| 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 | 112,352 | 112,352 | 112,216 | 109,629 |
| R-Square | 0.8315 | 0.8395 | 0.8523 | 0.8799 |

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. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 3: 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 | -0.143*** | -0.132*** | -0.143*** | -0.0238* |
| | (0.0396) | (0.0406) | (0.0396) | (0.0141) |
| 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 | 101,020 | 106,002 | 95,672 | 112,216 |
| R-Square | 0.8530 | 0.8509 | 0.8640 | 0.8523 |

This table presents robustness tests for Table 2 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. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 4: Average Response of Bank Branching on Primary Enrollment

| | log(Enrollment+1) | | | |
|---|---|---|---|---|
| | I | II | III | IV |
| Post branching | 0.0516*** | 0.0274* | 0.0306** | 0.0265* |
| | (0.0145) | (0.0141) | (0.0139) | (0.0144) |
| 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 | 1,842,789 | 1,842,789 | 1,842,772 | 1,842,278 |
| R-Square | 0.9092 | 0.9116 | 0.9146 | 0.9172 |

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. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 5: Heterogeneous Response of Bank Branching on Enrollment by Credit Intensity

| | log(Enrollment+1) | |
|---|---|---|
| | Secondary | Primary |
| | I | II |
| Post branching * Low credit intensity | -0.0915 | 0.0264 |
| | (0.0656) | (0.0253) |
| Post branching * High credit intensity | -0.1607*** | 0.0407** |
| | (0.0529) | (0.0179) |
| 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 | 0.86226 | 0.91503 |

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. ***, **, and * indicate significance at the 1%, 5%, and 10% levels (two-sided), respectively.

Table 6: Agricultural Credit, Farm Labor Demand, and School Enrollment

| | log(Enrollment+1) | |
|---|---|---|
| | Secondary | Primary |
| | I | II |
| Post branching * Low agricultural sector share | -0.0234 | 0.0355* |
| | (0.0690) | (0.0209) |
| Post branching * High agricultural sector share | -0.2354*** | 0.0352* |
| | (0.0446) | (0.0203) |
| 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. ***, **, and * indicate significance at the 1%, 5%, and 10% levels (two-sided), respectively.

Table 7: 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 intensity | -0.0536 | 0.0358 | -0.0638 | 0.0567** |
| | (0.0702) | (0.0270) | (0.0893) | (0.0243) |
| Post branching * High credit intensity | -0.1721*** | 0.0351** | -0.1807*** | 0.0217 |
| | (0.0501) | (0.0170) | (0.0400) | (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 | 115,877 | 1,850,364 | 115,877 | 1,850,364 |
| R-Square | 0.86227 | 0.91503 | 0.86227 | 0.91503 |

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. ***, **, and * indicate significance at the 1%, 5%, and 10% levels (two-sided), respectively.

Table 8: Average Response of Bank Branching on Local Economic Activity

| | Firm Activity | | Nightlight |
|---|---|---|---|
| | log(Number of Firms+1) | | log(Intensity+1) |
| | Entry | Exit | Minimum |
| | I | II | III |
| Post branching | 0.0829*** | -0.0837*** | 0.0123* |
| | (0.0225) | (0.0219) | (0.0063) |
| Pincode FE | Yes | Yes | |
| Pincode Time Trend | Yes | Yes | |
| District-Year FE | Yes | Yes | Yes |
| Village FE | | | Yes |
| Village Time Trend | | | Yes |
| # Obs | 67,438 | 67,438 | 854,271 |
| R-Square | 0.7912 | 0.7109 | 0.8722 |

This table presents the average changes in economic activities before and after bank branch entry. Columns I-II show the response in firm activities. We use private firm data from the Ministry of Corporate Affairs (MCA) at the Pincode-date level between 2011 and 2017. Treatment is then aggregated to the Pincode level, and the event year is the earliest year a village in the Pincode receives new bank branches. The dependent variable is the log number of firm entry/exit plus one. Post Branching is an indicator variable equal to 1 for years in which the Pincode receives new bank branch(es) after 2013. Regressions include Pincode fixed effect, district-by-year fixed effect, and Pincode-specific linear time trends. Standard errors are clustered at the Pincode level and reported in brackets. Column III uses nightlight data from VIIRS between 2012 and 2017, and aggregates the intensity measures at the village level. The dependent variable is log intensity plus one. Post Branching is an indicator variable equal to 1 for years in which a village receives new bank branch(es) after 2013. Regressions include village fixed effect, district-by-year fixed effect, and village-specific linear time trends. 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 9: 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 | -0.0849 | -0.116** | -0.228*** | 0.0686** | 0.0345* | 0.0321 |
| | (0.0781) | (0.0560) | (0.0538) | (0.0314) | (0.0196) | (0.0277) |
| # Obs | 50,795 | 35,101 | 19,266 | 595,373 | 599,518 | 566,270 |
| R-Square | 0.9025 | 0.8454 | 0.7829 | 0.9207 | 0.9102 | 0.9162 |
| | **Panel B Households' Financial Vulnerability - Marginal Labor Ratio** | | | | | |
| | Secondary | | | Primary | | |
| | High | Medium | Low | High | Medium | Low |
| Post branching | -0.281*** | -0.145*** | 0.00760 | -0.0103 | 0.0544*** | 0.0331 |
| | (0.0745) | (0.0458) | (0.1037) | (0.0319) | (0.0176) | (0.0332) |
| # Obs | 26,892 | 44,685 | 33,727 | 564,629 | 656,079 | 540,528 |
| R-Square | 0.8139 | 0.8719 | 0.8936 | 0.9140 | 0.9100 | 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. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

Table 10: Heterogeneous Response of Bank Branching on Enrollment by Students' Gender, Caste and School Type

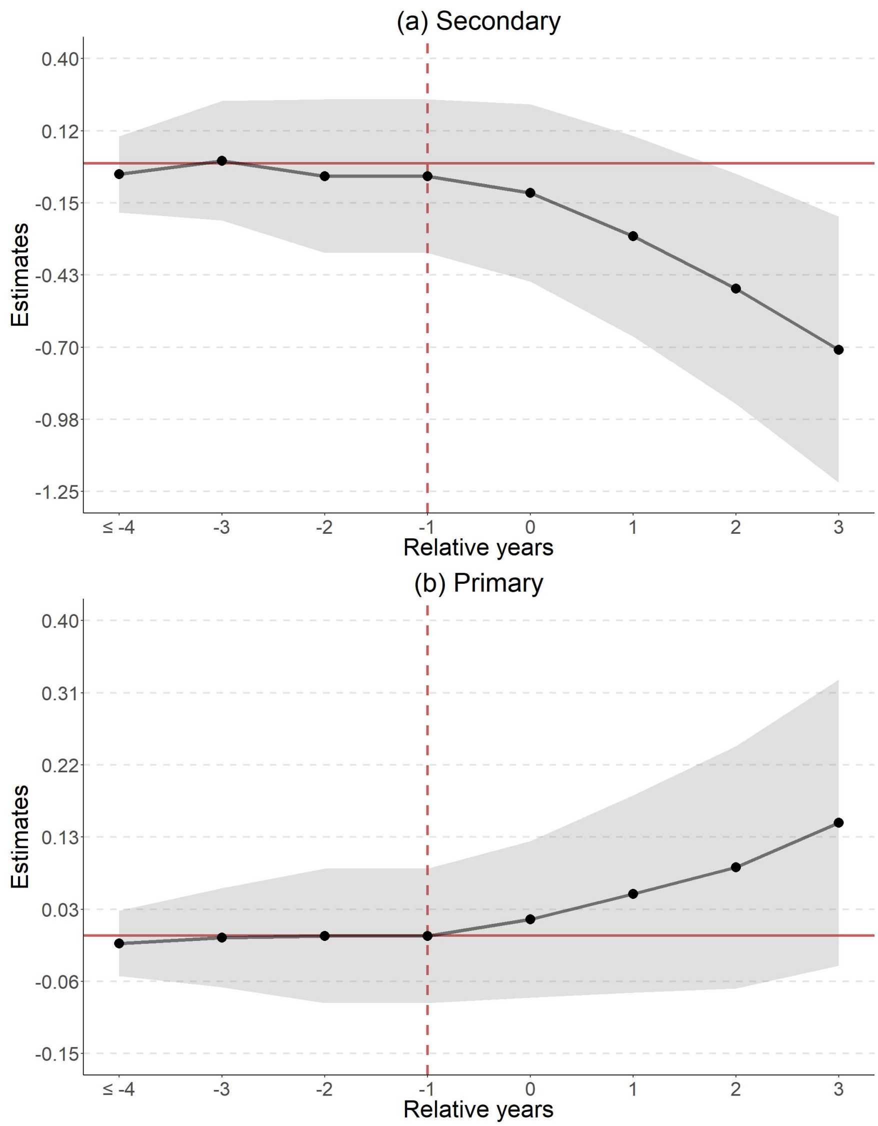
| | log(Enrollment+1) | | | |
|---|---|---|---|---|
| | I | II | III | IV |
| | **Panel A Student Gender** | | | |
| | Secondary | | Primary | |
| | Boys | Girls | Boys | Girls |
| Post branching | -0.130*** | -0.100*** | 0.0348** | 0.0708*** |
| | (0.0388) | (0.0355) | (0.0146) | (0.0192) |
| # Obs | 112,216 | 112,216 | 1,842,772 | 1,842,772 |
| R-Square | 0.9027 | 0.8788 | 0.9197 | 0.9088 |
| | **Panel B Student Caste** | | | |
| | Secondary | | Primary | |
| | Backward Caste | General Caste | Backward Caste | General Caste |
| Post branching | -0.143*** | -0.133*** | 0.0353** | -0.00262 |
| | (0.0496) | (0.0445) | (0.0162) | (0.0234) |
| # Obs | 112,182 | 112,182 | 1,840,747 | 1,840,747 |
| R-Square | 0.8503 | 0.9127 | 0.9066 | 0.9070 |
| | **Panel C School Type** | | | |
| | Secondary | | Primary | |
| | Public Schools | Private Schools | Public Schools | Private Schools |
| Post branching | -0.116*** | -0.0786 | 0.0277 | 0.0578* |
| | (0.0304) | (0.0806) | (0.0171) | (0.0344) |
| # Obs | 58,929 | 51,077 | 1,606,678 | 207,303 |
| R-Square | 0.8669 | 0.8698 | 0.9375 | 0.8264 |
| 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. ***, **, and * denote significance at 1%, 5%, and 10% level (two-sided), respectively.

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

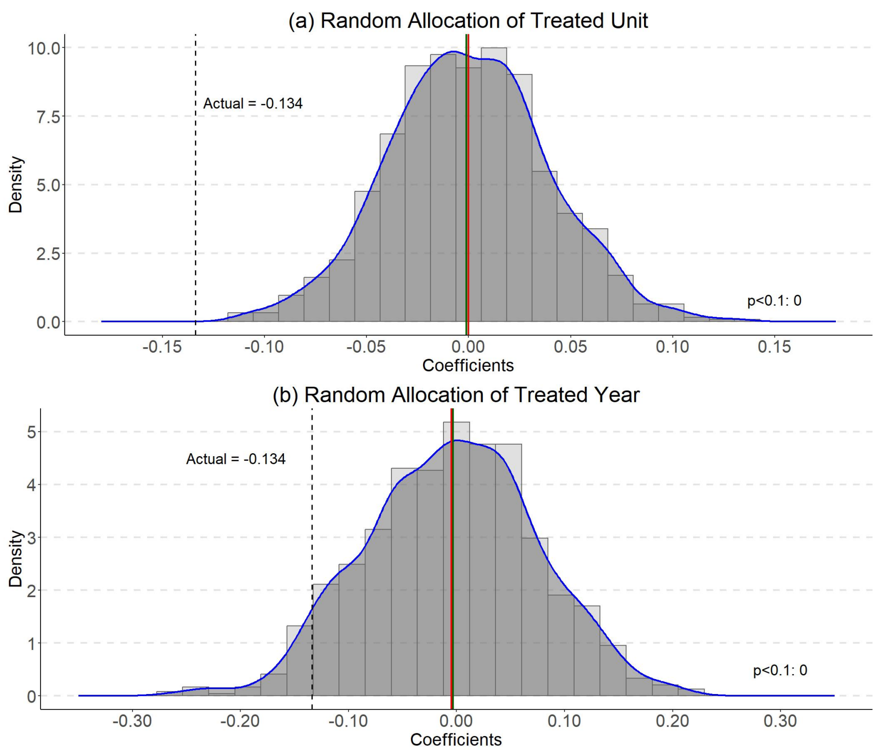
Intended for online publication only

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




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 2 Column III.

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: Village-level Balance Test

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel A Balance Test of Village Characteristics** | | |
| | Treated Villages | Control Villages | Mean Differences: (1)-(2) |
| Population Size | 3876.4596 | 1472.4055 | 2259.7484*** |
| Main Workers (% of Population) | 0.3262 | 0.3319 | -0.0119 |
| Scheduled Caste and Scheduled Tribe (% of Population) | 0.2498 | 0.3748 | -0.0471*** |
| Access to Black Topped Road | 0.9191 | 0.7090 | 0.2363*** |
| Access to Power Supply (All Users) | 0.8382 | 0.6275 | 0.1003*** |
| Access to Mobile Phone Coverage | 0.9007 | 0.8039 | 0.0335 |
| Access to Post Office | 0.2537 | 0.1021 | 0.1694*** |
| No. of Primary Health Centre | 0.2206 | 1.4577 | 0.1808 |
| | **Panel B Mean Differences After Matching** | | |
| | 1-Nearest Neighbor | Coarsened Exact Matching | Mahalanobis Matching |
| # of Treated Villages | 262 | 251 | 254 |
| # of Control Villages | 250 | 9,427 | 251 |
| Population Size | 1177.6130 *** | 1582.3780 *** | 564.7394 ** |
| Main Workers (% of Population) | 0.0014 | -0.0021 | 0.0021 |
| Scheduled Caste and Scheduled Tribe (% of Population) | 0.0030 | 0.0248 *** | 0.0043 |
| Access to Black Topped Road | -0.0032 | -0.0234** | 0.0110 |
| Access to Power Supply (All Users) | 0.0099 | -0.0248** | -0.0072 |
| Access to Mobile Phone Coverage | 0.0005 | -0.0219** | 0.0399 |
| Access to Post Office | -0.0393 | 0.1068 | -0.0160 |
| No. of Primary Health Centre | 0.0083 | 0.1528 | 0.1140 *** |

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 (columns 1 and 2) and mean differences between the treated and control villages within the same block (column 3). Panel B shows the mean differences between treated and control villages after three matching methods: 1-nearest neighbor (column 1), coarsened exact matching (column 2), and mahalanobis matching (column 3). ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively (two-sided).

Table A.6: 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 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 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.7: 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.